Structural Reforms and Economic Growth: A Meta-Analysis

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Structural Reforms and Economic Growth:

A Meta-Analysis

Jan Babecký and Tomáš Havránek*

Abstract

This paper evaluates the impact of structural reforms, mainly liberalization and privatization, on economic growth. To provide stylized facts on how such reforms worked in the past, we quantitatively review 60 studies that estimate the relation between reforms and growth empirically. These studies examine structural reforms carried out in 26 transition and post-transition countries around the world. Our results show that a typical reform caused costs in the short run, but had strong positive effects on long-run growth. Reforms focused on external liberalization proved to be more beneficial than other types of reform in both the short and long run. The findings hold even after correction for publication bias and misspecifications present in some primary studies.

JEL Codes: C83, O11, P21.
Keywords: Bayesian model averaging, growth, meta-analysis, structural reforms, transition economies.

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Nontechnical Summary

In this paper we quantitatively review the empirical literature that examines the relation between structural reforms and economic growth in transition and post-transition countries during the 1990s and 2000s. The empirical studies that we focus on investigate the impact of reforms that can be measured by EBRD and World Bank indices, generally divided into three broad categories: i) internal market liberalization (liberalization of domestic prices and abolition of state trading monopolies), ii) external market liberalization (liberalization of foreign trade, currency convertibility), and iii) private sector entry (small- and large-scale privatization and banking sector reform). The authors of these studies use econometric methods to evaluate how reforms (seen as changes in reform indices) translate into changes in the rate of economic growth in both the short and long run. The definitions of the short and long run are different in different studies, but we label as "short-run" the estimates that correspond approximately to the effect seen within a year since the change in the reform index. We label as "long-run" the estimates of the effect of changes in reform indices on the trend growth rate of GDP.

The effect on GDP is only one of the aspects of the impact of structural reforms, but also the one that is the easiest to measure and that has attracted a great deal of attention in the empirical economics literature. Cultural and social consequences, for example, are more difficult to pin down, but the multidimensionality of possible reform effects suggests that even a zero estimated effect of reforms on GDP growth would not necessarily indicate that the particular reform did not pay off. Moreover, in the literature there are uncertainties about the measurement of reforms, and any reform index is inevitably dependent on judgment. That said, we believe it is useful to examine the vast empirical literature that has investigated this issue to get some idea on the typical reaction of GDP growth following reform implementation and on the differences in the pattern across different types of reform.

For such a heterogeneous literature it is difficult to choose the typical narrative approach to summarize the literature's findings. The evidence on this issue often seems to be mixed (Babecky and Campos, 2011), but we want to find out more about the sources of heterogeneity. For this reason, we employ meta-analysis, the quantitative method of research synthesis. Compared with narrative research surveys, which are typically based on stylized examples from the literature, meta-analysis allows for a more structured discussion concerning the effect that different methodologies have on the implied results of empirical studies. In this paper we take the reported coefficients of the effect of reforms on growth and recompute them to a comparable metric, which captures the statistical strength of the relationship. Next, we evaluate the average reform effect in both the short and the long run. We correct these averages for publication bias (the preference of authors, editors or referees for some particular results) and potential misspecifications in some studies (for example, the failure to employ panel data methods). We also evaluate which types of reform are associated with more benefits in terms of GDP growth.

Our results show that a typical reform was associated with costs in GDP growth in the short run, but led to an increase of long-run growth. These findings relate to the statistical strength of the relationship; back-of-the-envelope calculations suggest that the results correspond to a short-run effect on growth of about minus 0.4 percentage points and a long-run effect of about plus 0.3 percentage points. That is, the positive effects of reforms outweigh the costs after about a year and then continue to contribute to economic growth. Reforms focused on external liberalization (that is, removing trade and capital account controls) appear to have spurred growth more than other types of reform, such as liberalization of domestic prices, privatization, and banking reform, in both the short and the long run. The short-run costs of external liberalization

are about 20% smaller compared to the average across all reforms and the long-run benefits are about 40% larger.

1. Introduction

How do structural reforms affect economic growth and what types of reforms are more conductive to growth than others? The unprecedented process of transformation from a planned to a market economy, resulting in a series of structural reforms of several types carried out in different countries, brings a unique opportunity to examine empirically the link between reforms and economic performance. Indeed, for transition and post-transition economies there is a large number of empirical studies that use a similar measure of reforms, similar types of growth regressions, and similar coverage of countries to estimate the reform effect. Yet the results of these studies vary a lot, ranging from negative to positive estimates for different types of reforms with different time lags.

This empirical observation of large heterogeneity in the estimated effects, labeled the "reform-growth puzzle," has been documented by the survey of Babecky and Campos (2011): about one third of the identified reform effects on growth is positive, one third is negative, and the remaining third is statistically insignificant. Babecky and Campos (2011) suggest that the negative and insignificant reform effects are to some degree associated with the initial costs, while the "expected" positive reform effects tend to materialize in the medium to long run. Apart from this stylized representation, there is also a dozen of method characteristics, among them the type of reform, estimation methods, and empirical specification, that increase the probability of finding either a negative or a positive reform effect.

As recently more studies have become available, one wonders whether the relationship between reforms and growth could be estimated more precisely. In particular, we believe it is useful to asses the relative magnitude of the average reported reform effect and evaluate the differences between the effects of various types of reforms. One difficulty is that, due to different measures of reform and growth, there is no such statistic as the "growth elasticity of reform," which complicates the comparison of estimates across studies. Next, empirical assessments of the reform-growth nexus may be subject to measurement errors, including potential specification errors and preferential publication of a particular type of results (especially those that are statistically significant). These are the issues we address in our study using recent developments in the methods of quantitative research synthesis.

When empirical studies disagree about the size and direction of an effect, tools of quantitative literature reviews become particularly helpful to understand what lies behind the observed variation in the reported results. The quantitative method of synthesizing information from the stock of available literature is called meta-analysis (Stanley, 2001). Developed in medical science, meta-analysis has become widely used in the social sciences, including economics: see, for example, Ashenfelter et al. (1999) for an assessment of returns to education, Rose and Stanley (2005) for an analysis of the effect of common currencies on international trade, and Havranek and Irsova (2011) for evidence on vertical spillovers from foreign direct investment. In the context of the economic growth literature, Doucouliagos and Ulubasoglu (2008) apply meta-analysis to examine the link between democracy and growth, while Nijkamp and Poot (2004) focus on the relation between economic growth and fiscal policy.

One meta-analysis has been done on the reform-growth nexus so far. Babecky and Campos (2011) analyze the variation in the reported effects and relate the variation to study characteristics such as estimation methods, reform measurement, model specification, and study quality. Nevertheless, the analysis of Babecky and Campos (2011) only focuses on differences in statistical significance. Our paper extends the prior research by Babecky and Campos (2011) in

the following aspects: i) we update the data set with studies published between 2009 and 2013 and recompute the reported t-statistics to partial correlation coefficients, which allow us to examine the relative magnitude of the effect of reforms; ii) we correct the average estimates for publication bias, iii) use Bayesian model averaging to address model uncertainty and find the most important factors driving the reported magnitude of the reform effect, and iv) compute the average value of the short- and long-run effect corrected for misspecifications in some primary studies. We corroborate the finding of Babecky and Campos (2011) that reforms focused on external liberalization are more conductive for economic growth; the rest of our findings outlined in the non-technical summary are new because Babecky and Campos (2011) do not examine the magnitude of the reform effect.

The remainder of the paper is organized as follows. Section 2 outlines how the reform effects are usually estimated in the literature and presents an overview of primary studies. Section 3 provides estimates of simple averages of the short- and long-run reform effect. Section 4 performs tests for publication bias and presents estimates of the reform effect corrected for the bias. Section 5 computes the reform effect conditional on "best-practice" methodology used in the literature. Section 7 concludes the paper and outlines suggestions for future research. Appendices present details concerning the Bayesian model averaging exercise employed in the paper.

2. Studies on Reforms and Growth

In the existing empirical studies the effect of structural reforms on economic performance is typically estimated using growth regressions that take the following general form:

$$g = \alpha + \beta R + \delta Z + \epsilon, \tag{1}$$

where g is real GDP growth, R is a measure of reform, Z is a vector of control variables including, for instance, initial conditions, measures of macroeconomic stabilization, institutional development, and factors of production; and ϵ is the error term. Coefficient β represents the estimate of the effect of reforms on growth conditional on the set of control variables Z.

Specification (1) in its most basic form was applied by earlier studies, which examined the effect of reforms on growth in a cross-section framework, using average values over a certain period of time, for example, five to eight years (de Melo et al., 1997; Heybey and Murrell, 1999; Krueger and Ciolko, 1998, among others). Subsequently, specification (1) was extended into a panel framework to address time dynamics, potential endogeneity of reforms, and different measures of reforms (for instance, the level versus the speed of reforms). A typical panel version of equation (1) used by studies in our sample takes one of the three following forms:

$$g_{it} = \alpha + \beta(R_{it} - R_{it-1}) + \delta R_{it-1} + \gamma Z_{it} + \epsilon_{it}, \tag{2}$$

$$g_{it} = \alpha + \beta R_{it} + \delta R_{it-1} + \gamma Z_{it} + \epsilon_{it}, \tag{3}$$

$$g_{it} = \alpha + \beta R_{it} + \gamma Z_{it} + \epsilon_{it}, \tag{4}$$

where the sub-indices i and t denote the country and the time period. Specifically, t denotes the year of the sample since all the reviewed studies work with yearly data, and the average number

of countries in the panels is about 24. Notice that the coefficients β in equations (1) through (4) are different (the constant terms and other coefficients being different as well).

One important difference in the effect of reforms on growth in specifications (1) to (4) concerns the horizon considered, namely, the difference between the short- and long-run effect. The *long-run* (cumulative) effect of structural reform on growth is measured by: (i) coefficient β in equation (1) estimated in a cross-section over a period of several years; (ii) coefficient δ in equation (2); the sum of coefficients β and δ in equation (3); and coefficient β in (4). The *short-run* (contemporaneous) effect of reform on growth is captured by (i) coefficient β in equation (1) if it is estimated for a given year; (ii) coefficient β in equation (3); and (iii) coefficient β in equation (2), although in this case the explanatory variable is a change in reform as opposed to the reform level in other specifications. Thus, we can distinguish whether the reform effect on growth is an immediate one (within a year) or whether it corresponds to a longer horizon.[1]

Furthermore, coefficient β could be different even for the same type of specification depending on whether the variables enter the equation in logarithms or in absolute values (or as a combination of both), and on the units of measurement if absolute values are used. Compared to the studies estimating, for example, the wage elasticity or employment elasticity, the literature evaluating the effect of reform on growth does not have such a term as "reform elasticity," which complicates the comparison of results across studies. One way of converting the estimates from different studies to a common metric is to record the estimated sign of the effect. This was done by Babecky and Campos (2011) in their meta-analysis—but we choose a different approach, described in the next section.

Table 1: List of Primary Studies

Abed and Davoodi (2002)	Fidrmuc and Tichit (2009)	de Melo et al. (1997)
Ahrens and Meurers (2002)	Fischer and Sahay (2001)	de Melo et al. (2001)
Apolte (2011)	Fischer and Sahay (2004)	Merlevede (2003)
Aslund et al. (1996)	Fischer et al. (1996a)	Mickiewicz (2005b)
Aziz and Westcott (1997)	Fischer et al. (1996b)	Mickiewicz (2005a)
Beck and Laeven (2006)	Fischer et al. (1998)	Nath (2009)
Borensztein et al. (1999)	Gillman and Harris (2010)	Neyapti and Dincer (2005)
Bower and Turrini (2009)	Godoy and Stiglitz (2006)	Pääkkönen (2010)
Cerović and Nojković (2009)	Havrylyshyn et al. (2001)	Pelipas and Chubrik (2008)
Christoffersen and Doyle (2000)	Havrylyshyn and van Rooden (2003)	Piculescu (2003)
Cieslik and Tarsalewska (2013)	Hernandez-Cata (1997)	Polanec (2004)
Cungu and Swinnen (2003)	Heybey and Murrell (1999)	Radulescu and Barlow (2002)
Denizer (1997)	Iradian (2009)	Radziwill and Smietanka (2009)
Eicher and Schreiber (2010)	Josifidis et al. (2012)	Raimbaev (2011)
Eschenbach and Hoekman (2006)	Kim and Pirttila (2003)	Rapacki and Prchniak (2009)
Falcetti et al. (2002)	Krueger and Ciolko (1998)	Sachs (1996)
Falcetti et al. (2006)	Lawson and Wang (2005)	Selowsky and Martin (1997)
Fidrmuc (2001)	Lejko and Štefan Bojnec (2012)	Staehr (2005)
Fidrmuc (2003)	Loungani and Sheets (1997)	Stuckler et al. (2009)
Fidrmuc and Tichit (2004)	de Macedo and Martins (2008)	Wolf (1999)

Notes: Both published and unpublished studies are included. The search for primary studies was terminated on May 1, 2013.

The selection of studies was performed using three criteria. A suitable study must (i) cover transition economies, (ii) report estimates of the reform coefficients and their t-statistics (or

[1] One of the referees wonders whether a reform can have any immediate effect in the given year if it is implemented at the end of that year; indeed such measurement issues are likely to bias the short-term effect toward zero.

standard errors), and (iii) contain details on the estimation methodology, type of reform, and country and period coverage. Primary studies were searched using EconLit, SSRN, RePEc, and Google Scholar, using the keywords "reform," "growth," and "transition economies." Next, the search was extended to the references contained in the identified studies and to their citations. For each reported coefficient a set of several dozen characteristics was recorded, including data and estimation methods, type of reform, measure of reform dynamics, control variables, and publication characteristics (details are provided in Section 5). In total, 60 studies issued since 1996 are included, both published and unpublished; they contain 537 empirical estimates of the effect of various types of structural reform on growth in transition economies. The list of studies is provided in Table 1.[2]

In the next section we propose a refined measure of the reform effect on growth which captures both the magnitude and significance of the effect. This measure allows us to explicitly estimate the average reform effect and subsequently to construct an estimate of the effect corrected for publication bias and misspecifications in some primary studies.

3. Estimating the Average Effect

Because the regression coefficients associated with the reform effect reported in primary studies are not always comparable, due to different units and transformations of the variables employed, it is necessary to use the corresponding t-statistics as a starting point. The t-statistics, however, do not represent a standardized measure of the effect of structural reforms on economic growth, since they depend on the number of degrees of freedom available for estimation in the primary study. Hence, t-statistics cannot be directly aggregated; we need to standardize them. A standardized measure of statistical association, commonly employed in meta-analysis (for example, Djankov and Murrell, 2002; Doucouliagos and Laroche, 2009), is the partial correlation coefficient, computed in the following way:

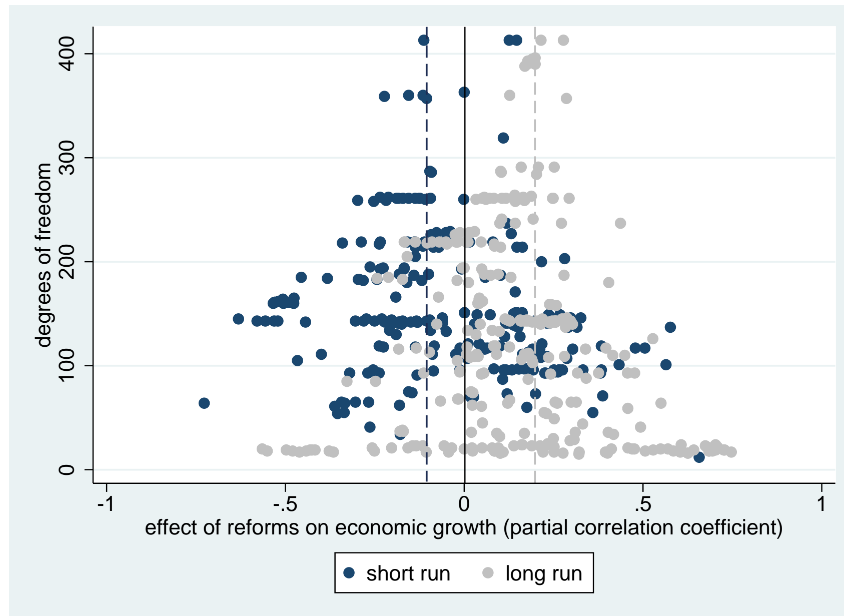
$$r = \frac{t}{\sqrt{t^2 + df}}, \tag{5}$$

where r denotes the partial correlation coefficient corresponding to the effect of reforms on growth, t denotes the t-statistic, and df denotes the number of degrees of freedom available for estimation in the primary study. The partial correlation coefficient is limited to the interval $[-1, 1]$. The standard error of the partial correlation coefficient can be computed as $SE = r/t$. The data set enables us to construct 245 partial correlation coefficients for the short run and 292 coefficients for the long run.

We illustrate the collected reform effects, converted to partial correlation coefficients, in Figure 1. The figure depicts reform effects on the horizontal axis and the number of degrees of freedom used in the estimation (which can be thought of as a measure of estimation precision) on the vertical axis. Such a figure is usually called a funnel plot: if all estimates measure the same effect, the most precise ones will be concentrated near the underlying reform effect, while the imprecise ones will be widely dispersed. Therefore, the cloud of the estimates should form an inverted funnel with the tip pointing up at the underlying reform effect. Nevertheless, the funnel depicted in Figure 1 apparently has two peaks, which suggests heterogeneity; in other words, the collected estimates seem to cover two distinct effects.

[2] Some studies were not included because they do not use the typical reform indices; for example, Stankov (2013) or Stankov (2012).

Figure 1: Reforms Hurt in the Short Run, but Spur Long-Run Growth



Notes: The figure shows a scatter plot of all reported estimates of the reform effect. The vertical axis measures the number of degrees of freedom available for estimation in each model. The dashed lines denote averages of the 10 estimates with the most degrees of freedom for the short and long run.

Table 2: Estimating the Average Reform Effect

	Short run			Long run		
Method	Estimated effect	95% confidence interval		Estimated effect	95% confidence interval	
Simple average	-0.052	-0.084	-0.021	0.146	0.118	0.173
Fixed effects	-0.081	-0.091	-0.072	0.135	0.125	0.145
Random effects	-0.056	-0.087	-0.025	0.143	0.122	0.164

Notes: "Estimated effect" denotes the estimated partial correlation coefficient for the relation between structural reforms and economic growth. "Simple average" is the unweighted arithmetic average of all estimates. "Fixed effects" is the average weighted by the inverse of the standard error of the partial correlation coefficient. "Random effects" is the average weighted by the inverse of the standard error of the partial correlation coefficient; additionally, heterogeneity among estimates is taken into account.

Indeed, when the short-run effects are separated from the long-run ones in Figure 1, it is clear that the cloud of the estimates consists of two overlapping funnels. Most of the precise estimates of the short-run effect are negative, while for the long-run effect the precise estimates are positive. This simple analysis suggests that, on average, structural reforms carried out in the past in transition countries had non-negligible costs in the short run, but fueled growth in the long run. In what follows, we need to examine the short-run and long-run effects separately.

The intuition given by Figure 1 is confirmed by the simple arithmetic averages reported in Table 2: the estimated averages are -0.05 for the short run and 0.15 for the long run. The results hardly change when more specialized meta-analysis techniques are used: namely, the fixed-effects estimator and random-effects estimator (see Borenstein et al., 2009). The fixed-effects estimator weights the partial correlation coefficients using the inverse of their standard errors. This "precision weighting" is commonly applied in meta-analysis; if the weights were instead based on the number of observations or degrees of freedom of the underlying model, the results would be very similar. The implied averages are -0.08 for the short run and 0.14 for the long run. Finally, the random-effects estimator explicitly assumes that the underlying reform effects estimated in different models may vary. Allowing for heterogeneity in this way yields results broadly similar to the previous two methods: the average is -0.06 for the short run and 0.14 for the long run.

All averages estimated in Table 2 are different from zero at the 1% level of significance; the short-run effect of reforms on growth is negative, while the long-run effect is positive. Nevertheless, it remains to be shown whether these effects are actually important in practice. According to Doucouliagos's guidelines for the importance of partial correlation in economics (Doucouliagos, 2011),[3] values of partial correlation smaller than 0.07 in absolute value denote no important effect, values between 0.07 and 0.17 denote a small effect, values between 0.17 and 0.33 denote a medium effect, and values larger than 0.33 denote a strong effect.

In our case, the estimated short-run average suggests a negative, but small (or even negligible) effect of structural reforms on economic growth in the short run. For the long run, the estimated average effect of reform on growth is positive and stronger, but still falls into the category of "small" effects. The estimates reported in this section, however, do not take into account that different estimates may have different probabilities of being reported (the problem is usually referred to as *publication bias*) and that models estimating the effect of reforms are of different quality (*heterogeneity*). Both issues may have important consequences for the estimates of the underlying effect, and we discuss them in turn in the following sections as we refine our estimates of the effect of reforms.

4. Consequences of Publication Bias

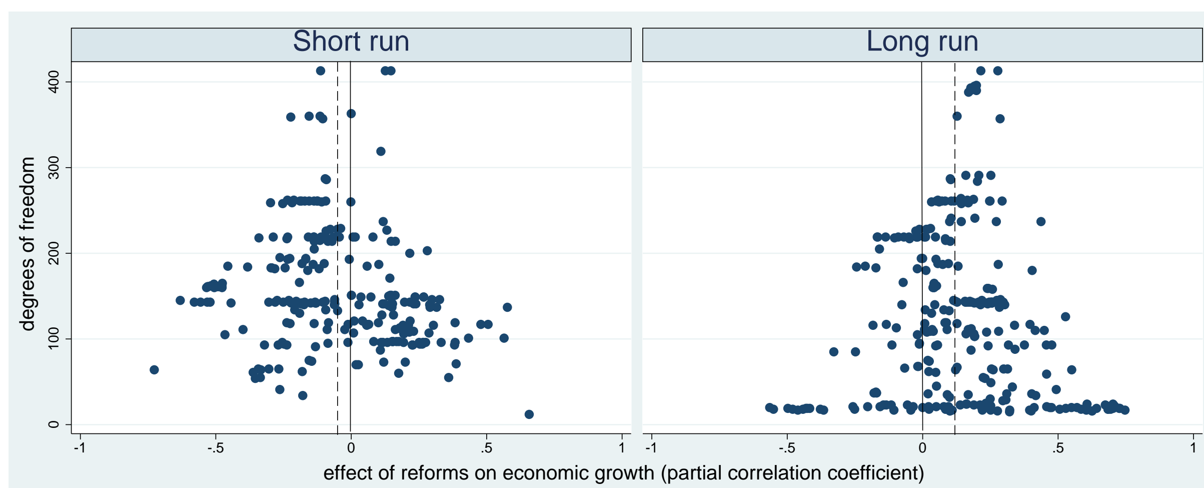
It has long been recognized that scientific results showing a certain direction or statistical significance may be more likely to get published than others, the other results often ending in a "file drawer" (Rosenthal, 1979). The problem has been found especially strong in empirical economics, as documented by, for example, Card and Krueger (1995), Görg and Strobl (2001), Havranek (2010), Rusnak et al. (2013), and Havranek et al. (2012). A recent survey of meta-analyses conducted in economics (Doucouliagos and Stanley, 2013) documents that most areas of empirical economics are affected by publication bias to a certain degree.

[3] Doucouliagos (2011) provides an update of Cohen's guidelines for the importance of the effect size in the social sciences (Cohen, 1988).

Most commonly, the bias manifests as a preference for results that are statistically significant or consistent with a major theory (Stanley, 2005). While the problem is usually labeled "publication" bias, it concerns unpublished manuscripts as well, since authors may use the sign of their estimates as a specification check, and discard those with the "wrong" (that is, unintuitive) sign. Therefore, publication bias is a complex phenomenon stemming from the preferences of authors, editors, and reviewers.

Publication bias can seriously distort the estimates of the average effect taken from the literature, because if the bias is present, some types of results become systematically underrepresented, their correctness or incorrectness notwithstanding. For example, Stanley (2005) shows how the average price elasticity of water demand reported in the literature is exaggerated fourfold due to publication bias. In the literature on reforms and growth, we have perhaps less reason to expect publication bias, since both positive and negative effects of reforms are theoretically possible, particularly when comparing short-run costs versus long-run benefits. On the other hand, since the topic is politically attractive, researchers with a political agenda may implicitly prefer strong results—positive or negative, depending on their ideological view. Some researchers may simply like to report "good news" in contrast to negative or insignificant estimates. For example, in the literature on the effects of foreign direct investment on the productivity of domestic firms in transition and developing countries, strong publication bias toward positive results has been found (Havranek and Irsova, 2012). If a similar tendency is present in the literature on reforms and growth, the average effects estimated in the previous section must be corrected for publication bias.

Figure 2: Funnel Plots Suggest Slight Publication Bias



Notes: The dashed lines denote averages of all reported estimates for the short and long run. In the absence of publication bias, the funnels should be symmetrical with respect to the line representing the average estimate.

To test for publication bias, the funnel plot introduced in the previous section can be used (Egger et al., 1997). Figure 2 shows separately the short- and long-run effect of reforms on growth. In the absence of publication bias, the funnels should be symmetrical with respect to the line representing the average estimate. In other words, all imprecise estimates should have the same probability of being reported, and in that case the average effect would also represent the underlying reform effect. If, in contrast, publication bias plagues the literature, positive or negative

estimates would be underrepresented, and the funnel would become asymmetrical.[4] Moreover, if statistically significant results were preferred to insignificant ones, the funnel would become hollow, since estimates that are small in magnitude and that are estimated with low precision get low t-statistics.

The funnels depicted in Figure 2 are relatively symmetrical when compared to funnels typically reported in economics meta-analyses (Doucouliagos and Stanley, 2013), but some signs of publication bias are still present. Both funnels are a little skewed—to the right for the short-run effect and to the left for the long-run effect. The simple averages are smaller in absolute value than the values of estimates with the highest precision. The funnels thus present some evidence for a slight preference for positive results in the case of the reported short-run effects and for negative results in the case of the long-run effects. Moreover, the funnel corresponding to the short-run effects seems to be relatively hollow, suggesting publication bias against insignificant results. But since the visual test of publication bias is inevitably subjective, more formal analysis is necessary to ascertain whether or not the bias is important.

The formal test of publication bias builds on Card and Krueger (1995) and Egger et al. (1997): in the absence of publication bias, the estimated size of the partial correlation coefficient should not be correlated with its standard error. If, in contrast, estimates of the reform effect are selected for publication because of their significance or sign, the relation will become significant. This idea is formalized by the following regression:

$$r_i = r_0 + \beta_0 \cdot Se(r_i) + u_i, \tag{6}$$

where r_i is the partial correlation coefficient derived from the i-th primary study, r_0 denotes the underlying partial correlation corrected for publication bias, $Se(r_i)$ denotes the standard error of r_i, and β_0 measures the direction and magnitude of publication bias.[5] Nevertheless, regression (6) is likely to be heteroskedastic, because the explanatory variable is a sample estimate of the standard deviation of the response variable. To ensure efficiency, the regression is usually estimated by weighted least squares (Stanley, 2005, 2008), where the precision of the estimates (the inverse of the standard error) is taken as the weight. In meta-analysis, this estimator is usually called fixed effects, similarly to the estimator of the simple average introduced in the previous section (now only the term capturing publication bias is added). To check the sensitivity of our results, we also employ a robust method, iteratively re-weighted least squares (Hamilton, 2006, pp. 239–256). Finally, because the estimated reform effects are extracted from many studies, and different studies report different numbers of estimates, in the third specification we cluster standard errors at the study level.

The results of the test for publication bias and the underlying effect corrected for the bias are reported in Table 3. According to all three methods, publication bias is not statistically significant for the estimates of the long-run reform effect, and consequently the corrected effect is very close to the simple average (approximately 0.1). In contrast, publication bias is significant at the 1% level in the fixed-effects and robust estimations for the short-run effect, although it becomes less significant when standard errors are clustered at the study level. In that case, the p-value corresponding to β_0 in (6) reaches 0.051. Nevertheless, the test for publication bias is

[4] The funnel has to be symmetrical not with respect to the zero line, but with respect to the top of the funnel (the most precise estimates). Therefore, the funnel can be symmetrical even if all reported estimates have the same sign.

[5] In contrast to the partial correlation coefficient, the t-statistic cannot be used as the response variable in this specification because it depends on the number of degrees of freedom, which is correlated with the standard errors.

Table 3: Test of Publication Bias

	Short run			Long run		
	Fixed	Robust	Clustered	Fixed	Robust	Clustered
Publication bias (coef. β_0)	4.137***	4.179***	4.137*	0.313	0.265	0.313
	(0.947)	(0.961)	(2.036)	(0.290)	(0.300)	(0.586)
Effect beyond bias (Constant)	-0.394***	-0.395***	-0.394**	0.110***	0.116***	0.110*
	(0.073)	(0.074)	(0.164)	(0.025)	(0.026)	(0.056)
Observations	245	245	245	292	292	292

Notes: The response variable is the effect of reforms on economic growth (partial correlation coefficient). Standard errors in parentheses. "Fixed" denotes the estimates by weighted least squares; weighted by the inverse of the standard error of the partial correlation coefficient. "Robust": estimated by iteratively re-weighted least squares. "Clustered": estimated by weighted least squares; standard errors clustered at the study level. ***, **, and * denote significance at the 1%, 5%, and 10% levels.

known to have low power (Egger et al., 1997; Stanley, 2005), so estimates of β_0 on the borderline of statistical significance still indicate evidence of publication bias. More importantly, the corrected estimates of the short-run reform effect are consistent and significant at the 5% level across all three methods: they reach -0.39, which is approximately four times more than the simple averages reported in the previous section.

Therefore, after correction for publication bias, the long-term effect of an average reform on economic growth is still positive and small according to Doucouliagos's guidelines. In the short run, however, reforms seem to bring considerable costs in terms of economic performance: the value of the short-run partial correlation equal to -0.39 would be classified as a "strong" effect according to Doucouliagos's guidelines.

5. Consequences of Heterogeneity

The primary studies in our sample employ a variety of different methods to estimate the effect of structural reforms on economic performance. The studies differ in terms of the quality of the data and econometric techniques used, for example. If these differences have a systematic influence on the estimated reform effect, we need to take it into account and adjust the average estimate presented in the previous section.

The heterogeneity in the estimates of the reform effect was examined and discussed in detail in Babecky and Campos (2011); in this paper we use the variables capturing study design to estimate the average effect conditional on the "best practice" from the literature. We have identified 32 variables describing the characteristics of the data and methods used in the primary studies, the type of the reform index employed, the measure of dynamics, specification characteristics, and publication characteristics. All variables are explained and summarized in Table 4.

The data and method characteristics include information on whether a panel or cross-sectional data set is used and whether endogeneity is taken into account. Variables capturing the type of reform index include dummy variables for the institutions producing the index (the World Bank, the EBRD, or a combination of both; further details on the type and occurrence of reform indices are provided below). The category "measure of dynamics" captures, for example, whether the lagged dependent variable is used in the regression and whether time dynamics are controlled

for. Specification characteristics include, among others, dummy variables for the control for the initial conditions, stabilization, and institutional development. Publication characteristics capture the affiliation of the authors (academia or policy institutions), the number of citations of the study, and the type of publication (a journal article or a working paper).

Table 4: Description and Summary Statistics of Explanatory Variables

		Short run		Long run	
Variable	Description	Mean	Std. dev.	Mean	Std. dev.
prec	The precision of the estimated partial correlation coefficient (the inverse of the standard error)	12.666	2.675	10.535	4.456
Data and methods					
panel	= 1 if the model uses panel data.	0.996	0.064	0.750	0.434
endo	= 1 if the model used is 2SLS, 3SLS, GMM, or cointegration.	0.298	0.458	0.274	0.447
fixed	= 1 if fixed effects estimation is used (or country dummies).	0.318	0.467	0.144	0.352
k	The number of explanatory variables.	13.449	10.478	10.048	9.768
start	The first year of the sample.	8.155	2.271	7.801	3.052
tspan	The number of years in the sample.	7.963	3.437	8.452	4.526
Type of reform index					
ebrd	= 1 if the reform index originates from the EBRD only.	0.453	0.499	0.620	0.486
comb	= 1 if a combination of EBRD and WB indices is used.	0.163	0.370	0.151	0.358
lii	= 1 if internal and/or price liberalization components are used as a reform measure.	0.069	0.255	0.048	0.214
lie	= 1 if external liberalization components are used.	0.069	0.255	0.045	0.207
lip	= 1 if privatization and banking reform components are used.	0.110	0.314	0.082	0.275
margeff	= 1 if *lii*, *lie*, and *lip* are used in the same specification.	0.118	0.324	0.068	0.253
av	= 1 if the average (simple or weighted, or simple sum) of *lii*, *lie*, and *lip* is used.	0.645	0.480	0.798	0.402
cli	= 1 if the Cumulative Liberalization Index from the World Bank is used.	0.008	0.090	0.082	0.275
Measure of dynamics					
lagdep	= 1 if the lagged dependent variable is used in the regression.	0.184	0.388	0.154	0.362
speed	= 1 if speed is the measure of reform.	0.241	0.428	0.205	0.405
lags	= 1 if both contemporaneous and lagged reform variables are used.	0.620	0.486	0.534	0.500
time	= 1 if time dynamics are controlled for.	0.167	0.374	0.195	0.397
Specification characteristics					
ic	= 1 if the initial conditions are controlled for.	0.718	0.451	0.791	0.407
ic12	= 1 if the first cluster and/or second cluster of the initial conditions from de Melo et al. (1997) is used.	0.278	0.449	0.250	0.434
nic	The number of types of controls for the initial conditions.	1.624	1.916	1.740	1.637
stabil	= 1 if stabilization is controlled for.	0.910	0.286	0.726	0.447
nstab	The number of types of controls for stabilization.	1.518	0.939	1.086	0.925
infl	= 1 if inflation is controlled for.	0.824	0.381	0.616	0.487
inst	= 1 if institutional development is controlled for.	0.216	0.413	0.260	0.440
ninst	The number of types of controls for institutional development.	0.229	0.449	0.411	0.871
fact	= 1 if factors of production are controlled for.	0.294	0.456	0.229	0.421
nfact	The number of types of controls for factors of production.	0.318	0.517	0.264	0.513
pubpr	= 1 if the study separates the effect of reform on the public and private sectors.	0.065	0.248	0.048	0.214

Continued on next page

Table 4: Description and Summary Statistics of Explanatory Variables (continued)

		Short run		Long run	
Variable	Description	Mean	Std. dev.	Mean	Std. dev.
Publication characteristics					
journal	= 1 if the study is published in a refereed journal.	0.465	0.500	0.565	0.497
lgoog_pa	The logarithm of the number of citations per year from Google Scholar.	1.751	1.114	1.840	1.004
authaff	= 1 if all the authors are from academia.	0.502	0.501	0.568	0.496

Source of the data: Primary studies estimating the effect of structural reforms on economic growth. For the explanation of the differences among the reported short-run effects, variables *panel*, *lii*, and *cli* are not used: the variation in these variables is too low or they are perfectly correlated with other variables.

Regarding the type of reform measures, the studies surveyed analyze the effects on growth of structural reforms as captured by the World Bank cumulative liberalization index (available for the years 1989–1997), the EBRD average reform index (available for the period from 1991 to the present at yearly frequency), or a combination of both. Furthermore, some studies use individual reform components, namely, internal and/or price liberalization (*lii*), external liberalization, that is, the liberalization of trade and capital flows (*lie*), and privatization and banking reform (*lip*).

Table 5 shows the correspondence between the individual components of the World Bank and EBRD reform indices. While the EBRD classification of reform measures has gradually become more detailed (currently distinguishing 11 various areas, including infrastructure and financial sector reform), those selected categories listed in the right column of Table 5 are commonly viewed in the literature as compatible with the World Bank's three types of reform. Along with these individual components, the average reform index (*av*), constructed as the average (simple or weighted) or the sum of the above reform components, is also used in a number of studies. Moreover, we create a dummy variable characterizing marginal reform effects (*margeff*) to distinguish cases where the individual reform components (*lli*, *lie*, and *lip*) are used jointly in the same regression.

Table 5: Correspondence Between the World Bank and EBRD Reform Indices

	Reform index origin	
Reform type	World Bank	EBRD
lii	Internal markets liberalization (liberalization of domestic prices and abolition of state trading monopolies)	Price liberalization
lie	External markets liberalization (liberalization of foreign trade and currency convertibility)	Trade and foreign exchange liberalization
lip	Private sector entry (small- and large-scale privatization and banking reform)	Small-scale privatization Banking reform and interest rate liberalization Large-scale privatization

Notes: Details on the World Bank indices are provided in de Melo et al. (1997). For the EBRD reform indicators, see the EBRD Transition Reports, which are available at yearly periodicity at www.ebrd.com.

The occurrence of alternative reform measures in the studies surveyed can be seen in the mean values of the statistics in Table 4. Indeed, since we code reform-type indices as dummy variables (taking the value of 1 if the corresponding reform index was used in the regression, and 0 otherwise), the mean values of the summary statistics presented in Table 4 illustrate the proportion of the corresponding reform indicators in the total number of observations, for the short-run (third column) and long-run effects (fifth column). For example, the average of the individual reform components (*av*) is used in 64.5% of regressions capturing the short-run effect and 79.8% of regressions for the long-run effect, while the occurrence of *lii*, *lie*, and *lip* is much less frequent, varying between 6.9% and 11% of regressions for the short-run effect and 4.5%–8.2% for the long-run effect.

We intend to explain the differences in the partial correlation coefficients corresponding to the reported reform effects. To be specific, we need to plug the variables capturing heterogeneity into equation (6) to get the following general model:

$$r_i = r_0 + \beta_0 \cdot Se(r_i) + \gamma \cdot \text{Study design} + v_i, \tag{7}$$

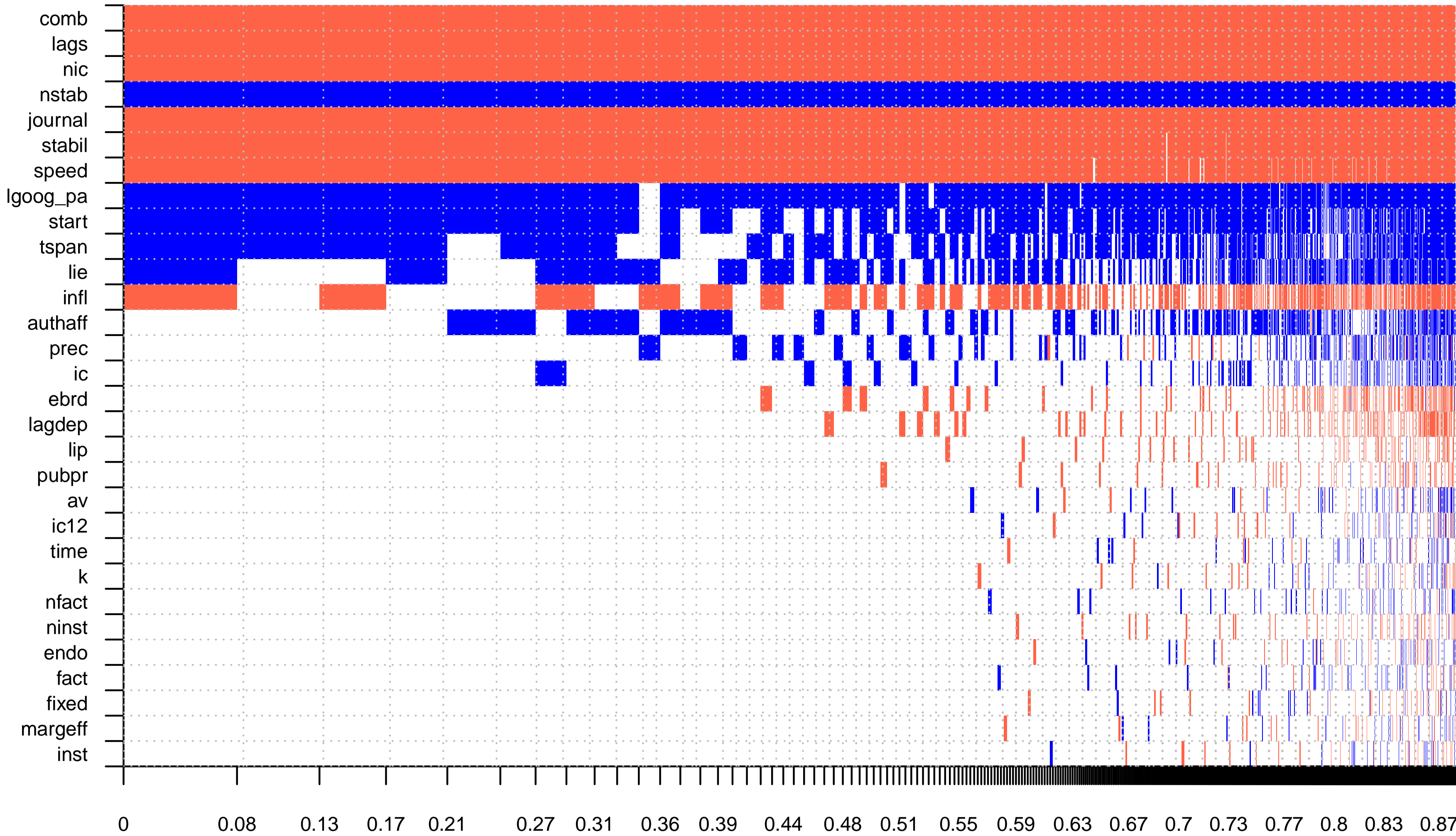
where *Study design* denotes a vector of variables listed in Table 4. The specification still controls for publication bias ($\beta_0 \cdot Se$), but the estimate of the underlying reform effect, r_0, now becomes conditional on the values of the variables explaining heterogeneity. To correct for heteroskedasticity, we still consider regression (7) in the fixed-effects form, that is, weighted by precision (as explained in Section 4).

It is not reasonable to estimate a regression including all 32 explanatory variables. At the same time, no theory can help us select which variables could matter for the reform effect and which should be omitted. This is an example of model and parameter uncertainty, common in meta-analysis, and can be addressed by a method called Bayesian model averaging (BMA; for example, Fernandez et al., 2001; Sala-i-Martin et al., 2004; Ciccone and Jarocinski, 2010). BMA has been used in meta-analysis, for instance, by Moeltner and Woodward (2009) and Irsova and Havranek (2013).

BMA estimates many regressions with the possible subsets of all explanatory variables on the right-hand side and constructs a weighted average over these regressions. The weights used in the BMA estimation are the so-called posterior model probabilities. The posterior model probability can be thought of as a measure of the fit of the model, analogous to the adjusted R-squared: the models that fit the data best get the highest posterior model probability, and those that fit the worst get the lowest. Moreover, for each explanatory variable we can compute the posterior inclusion probability, which represents the sum of the posterior model probabilities of all models that contain this particular variable. In other words, the posterior inclusion probability expresses how likely it is that the particular variable should be included in the "true" regression. For the estimation of the BMA exercise we use the `bms` package available in R (developed by Feldkircher and Zeugner, 2009, who also provide a detailed explanation of BMA). More details on the BMA procedure employed in this paper are available in Appendix Appendix B.

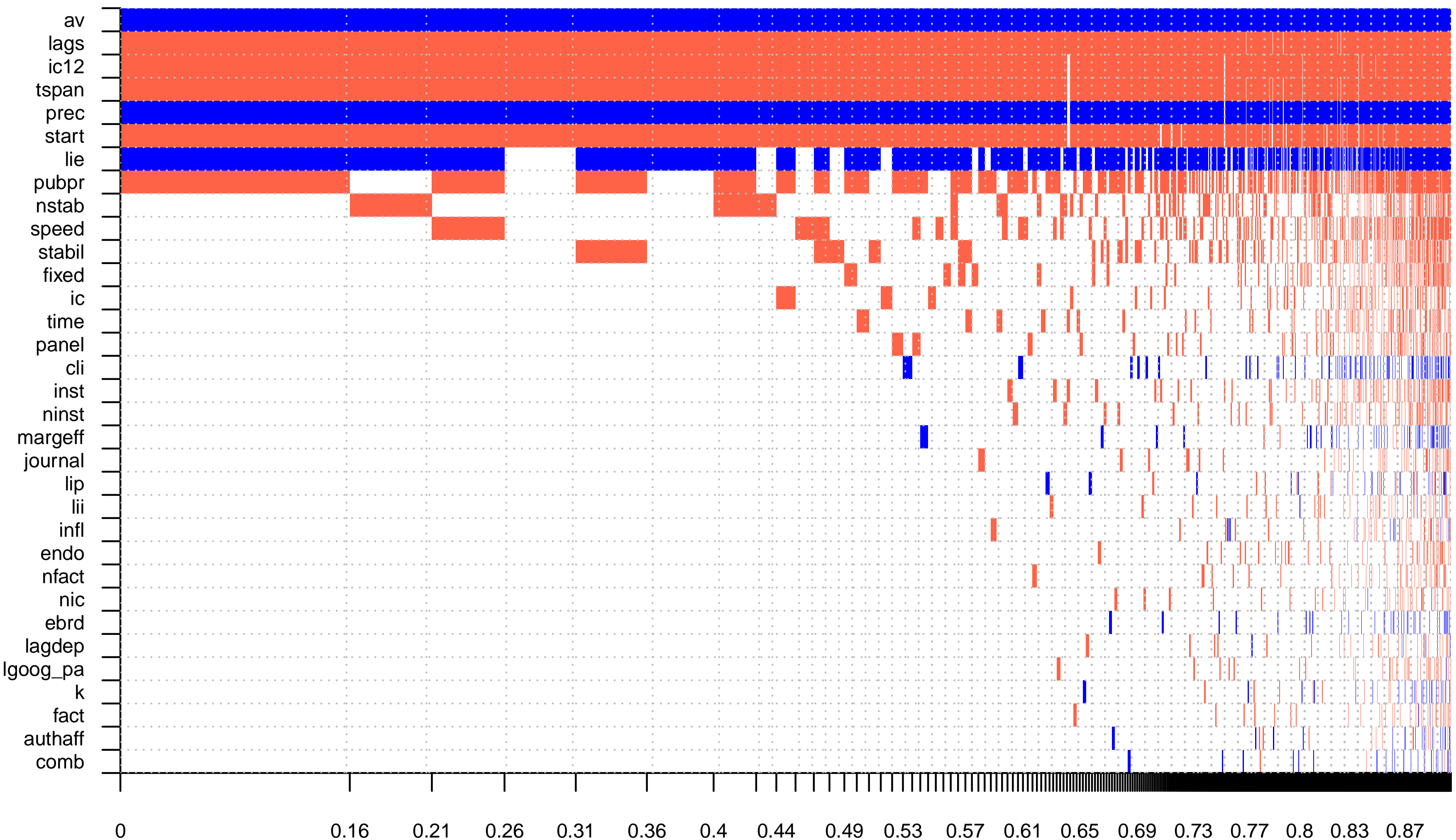
The results of the BMA estimation for the short and long run are reported graphically in Figure 3 and Figure 4; different regressions estimated by BMA are depicted as different columns. If the cell for a variable is blank, the variable is not included in the regression. If the cell is blue (darker in grayscale), the variable is included and the estimated sign is positive; similarly, if the cell is red (lighter in grayscale), the variable is included and the estimated sign is negative. The width of the columns represents the weight for each regression. The variables are sorted by

Figure 3: Bayesian Model Averaging, Model Inclusion (Short Run)



Notes: Response variable: the effect of reforms on economic growth in the short run (partial correlation coefficient). The acronyms of the explanatory variables are explained in Table 4. Columns denote individual models; variables are sorted by posterior inclusion probability in descending order. Blue color (darker in grayscale) = the variable is included and the estimated sign is positive. Red color (lighter in grayscale) = the variable is included and the estimated sign is negative. No color = the variable is not included in the model. The horizontal axis measures cumulative posterior model probabilities.

Figure 4: Bayesian Model Averaging, Model Inclusion (Long Run)



Notes: Response variable: the effect of reforms on economic growth in the long run (partial correlation coefficient). The acronyms of the explanatory variables are explained in Table 4. Columns denote individual models; variables are sorted by posterior inclusion probability in descending order. Blue color (darker in grayscale) = the variable is included and the estimated sign is positive. Red color (lighter in grayscale) = the variable is included and the estimated sign is negative. No color = the variable is not included in the model. The horizontal axis measures cumulative posterior model probabilities.

their posterior inclusion probabilities: most models that include the variables at the top of the figure belong among the good models (in terms of the posterior model probability), while the models that include the variables at the bottom of the figure usually do not fit the data well.

Some variables are important (that is, have a posterior inclusion probability higher than 50%) for the estimates of the reform effect in both the short and long run. These are *lie* (a dummy variable capturing the type of the reform index, namely, external liberalization), entering with a positive sign, and *lags* (a dummy variable capturing whether both contemporaneous and lagged reform variables are used in the model), entering with a negative sign. Moreover, these variables affect the estimates of the short- and long-run reform effect in the same direction. Some other variables are important either only for the short-run estimates (for example, *comb*, a dummy variable reflecting whether a combination of the EBRD and World Bank indices is used) or only for the long-run estimates (for example, *tspan*, the number of years included in the data set), and some do not seem to be important at all.

Our intention is to use the results concerning the sources of heterogeneity to improve our estimate of the underlying reform effect (r_0). Instead of selecting one of the regressions (the columns in Figure 3 and Figure 4) and building our analysis on this specification, BMA uses the weighted average of all regressions; the numerical details on the weighted average of the coefficients for each variable are reported in Table A1 and Table A2 in Appendix Appendix A. To estimate the underlying reform effect, we need to select the preferred value for each explanatory variable and plug it into equation (7), using the regression coefficients given by BMA (the coefficients for variables with a low posterior inclusion probability are very close to zero). In other words, from the literature we create a synthetic model with best-practice methodology, the largest data set, and maximum quality characteristics.

Of course, the authors of primary studies have different views on how best practice in this literature should look, but some aspects of methodology would be preferred by most evaluators. We prefer panel-data models over cross-sectional models (that is, we plug in value 1 for the corresponding dummy variable), models explicitly addressing endogeneity, and models employing country-level fixed effects. We prefer it if the study uses data on the reform index from both the World Bank and the EBRD and if it takes into account internal, external, privatization, and banking reform components (not only a subset of those). We prefer models controlling for time dynamics, the initial conditions, stabilization, inflation, institutional development, and factors of production. We also plug in sample maxima for the number of types of control for the initial conditions, the number of types of control for stabilization, the number of types of control for institutional development, and the number of types of control for factors of production. Finally, we prefer studies published in peer-reviewed journals and plug in the sample maximum for the number of citations. All other variables are set to the sample means.

The improved estimate of the reform effect for the short run is -0.38, which means virtually no change compared with the case when we only corrected the simple average for publication bias. In contrast, the improved estimate of the long-run effect is 0.27, which is almost three times larger than the estimate in the previous section. Both effects are statistically significant at the 5% level, and the numbers are robust to marginal changes in the definition of best practice. All in all, when we correct for both publication bias and misspecifications, according to Doucouliagos's guidelines the short-run effect of an average structural reform on economic growth would be classified as "strong," while the resulting category for the long-run effect is "medium."

6. Discussion of the Magnitude of the Reform Effect

We have noted that one of the advantages of this paper over the previous meta-analysis by Babecky and Campos (2011) is our ability to estimate the strength of the reform effect (the other advantages being adjustment for publication selection bias, correction for misspecifications, use of Bayesian methods to address model uncertainty, and an updated data set). Babecky and Campos (2011) use t-statistics and experiment with three categories of reform effects: statistically significant and negative, statistically insignificant, and statistically significant and positive. We use partial correlation coefficients, which represent a statistical measure of the strength of the underlying economic relationship and, in contrast to t-statistics, do not increase with the number of degrees of freedom and are therefore comparable across studies. Ideally we would like to measure the economic effect directly, but elasticities of GDP growth with respect to changes in reform indices are not available.

For the classification of partial correlation coefficients into "small," "medium," and "strong" effects we use the guidelines of Doucouliagos (2011). In the guidelines Doucouliagos (2011) collects 22,000 partial correlation coefficients reported in empirical economics. The thresholds are determined according to the distribution of the coefficients: if the coefficient is smaller than 75% of all empirical estimates reported in economics, it is classified as not being important at all. If the coefficient lies between the 25th and 50th centile of the reported effects, it is classified as small. The coefficient is classified as medium if it lies between the 50th and 75th centile, and as large if it is greater than the 75th centile of partial correlation coefficients in empirical economics. In sum, the classifications of Doucouliagos (2011) are relative to the size of the effects that economists typically find.

There are two reasons why in this case we cannot use elasticities, the preferred summary statistic of economic meta-analyses. First, different studies use different functional forms, which means that the reported estimates of reform effects are not directly comparable. Second, as Barlow (2006, p. 509) put it concerning the EBRD indices: "A score of 4 of an index should not be regarded as indicating double a score of 2." An increase in the index indicates an improvement in the characteristic in question, but is not necessarily proportional to the previous value. For example, the price liberalization index is defined as taking value 1 if "most prices are formally controlled by the government" (EBRD, 2004), value 2 if there is "some lifting of price administration; state procurement at non-market prices for the majority of product categories," and value 3 if there is "significant progress on price liberalization, but state procurement at non-market prices remains substantial." An improvement from value 1 to value 2 represents a 100% increase in the index, but may actually be easier than a move from value 2 to value 3 (a 50% increase).

Bearing the two limitations in mind, we believe it could still be interesting to try to compare the results of studies on the relation between reforms and growth summarized in our meta-analysis with the effects of other macroeconomic shocks and policies.[6] Such a comparison requires judgment on several key parameters and should therefore be taken with a grain of salt. First, for any meaningful estimate we need the elasticity of growth with respect to reforms, which cannot be directly obtained for the reasons described in the previous paragraph. As Doucouliagos (2011) notes, there should be a positive relationship between the elasticity and the partial correlation coefficient, but the exact form of the relationship is uncertain. We use the data set of what we believe is the largest meta-analysis conducted in economics so far, Havranek

[6] We thank an anonymous referee for suggesting this analysis.

et al. (2013),[7] and regress the elasticities reported there on partial correlations to get some idea about the relationship. The regression yields a coefficient on partial correlations of 1 with an intercept of 0.34, and we will use these estimates in our analysis although we realize that the relation may be field-specific.

The estimates imply a short-run elasticity of growth with respect to reforms of -0.72 and a long-run elasticity of 0.61. Next, we need an estimate of the percentage change in reform indices due to typical reforms. Changes in EBRD reform scores of about 1/3 are relatively common, as illustrated in EBRD (2004, p. 7): for example, such an improvement in the reform index represented "approval of new competition law and creation of independent competition authority" in Albania, "adoption of a new bankruptcy act, amendments to the law on public companies and the introduction of measures to improve the effectiveness of the judiciary" in Croatia, or "significant privatizations over the past year, including the oil and gas company Petrom and other energy assets" in Romania. If we take the midpoint of the range of the indices, such reforms reflect a 13% improvement of reform scores. Using the estimated elasticities and assuming a transition country with a 4% trend growth rate, we find that a standardized reform (a 13% improvement of reform scores) translates into a decrease of short-term growth by 0.4 percentage points and an increase in the long-term growth rate by 0.3 percentage points. Our results from the previous section also indicate that reforms affecting the index of external liberalization are more beneficial than other types of reforms: the estimated effects for external liberalization compared to privatization are about 20% smaller in the short run and 40% larger in the long run.

7. Conclusion

In this paper we examine the link between structural reforms and economic growth in transition and post-transition economies using the results of 60 empirical studies published in the period 1996–2013. Our intention is i) to obtain robust estimates of the typical reaction of GDP growth to reforms and ii) to examine the differences between the effects of different reforms. We summarize the reform effect by employing partial correlation coefficients, which capture both the statistical significance of the effect and its magnitude. We find that, on average, in the short run reforms lead to significant costs in terms of output growth (back-of-the-envelope calculations suggest costs of about 0.4 percentage points), while in the long run the effect of reforms on economic performance is positive and substantial (of about 0.3 percentage points). Our results, building on the body of available empirical studies, thus corroborate the stylized fact that it takes time for the benefits of structural reforms to materialize. Nevertheless, the positive effects of reforms outweigh the costs after about a year and then continue to contribute to economic growth.

The type of reform determines how fast benefits materialize and how strong they are. The results reported by primary studies allow us to control for several reform measures, namely, the origin of the index (EBRD, World Bank, or a combination of both) and the type of the index (internal liberalization, external liberalization, privatization, the average of the above three components, their marginal effects, and the cumulative liberalization index). Among these alternative measures, external liberalization shows more positive effects on growth compared to other types of reform: the short-run costs of reforms focused on external liberalization are about 20% smaller; the long-run benefits are about 40% larger.

[7] Havranek et al. (2013) collect 2,735 estimates of the elasticity of intertemporal substitution in consumption from 169 studies and their data set is available at meta-analysis.cz/substitution.

One direction for future research could be to explore the mechanism through which external liberalization (that is, removing trade and capital account controls) affects growth, and the interactions among reform components—the complementarity of reform. Moreover, as documented in EBRD (2011), there is still substantial potential for improving upon the reforms implemented in a number of transition countries. In this paper we only review the so-called first-generation structural reforms (mainly liberalization and privatization), since these are the ones covered by most of the existing literature on transition economies. As more empirical evidence on the effects of second-generation reforms (for example, enterprise governance, institutional change, and competitiveness) becomes available, evaluation of the effects of such reforms in the meta-analysis framework may prove a promising avenue for further research.

One caveat should be kept in mind when interpreting the results of the present study: a meta-analysis can only filter out misspecifications that have been overcome by a sufficient number of researchers. If a misspecification is shared by the entire literature and influences the estimates in a systematic way, meta-analysis will give biased results. The measurement of reforms, for example, has been especially controversial, and new measures have recently been proposed (Campos and Horvath, 2012). Nevertheless, until the new measures have been employed by a sufficient number of researchers, they cannot be explored using meta-analysis tools.

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Appendix A. Results of BMA

Table A1: Explaining the Differences in the Estimates of the Reform Effect (Short Run)

Variable	PIP	Posterior mean	Posterior std. dev.	Cond. pos. sign
prec	0.155	0.0308	0.0906	0.889
Data and methods				
endo	0.024	0.0000	0.0043	0.444
fixed	0.022	0.0000	0.0043	0.427
k	0.025	0.0000	0.0002	0.384
start	0.820	0.0156	0.0089	1.000
tspan	0.677	0.0062	0.0049	0.999
Type of reform index				
ebrd	0.088	-0.0058	0.0224	0.007
comb	1.000	-0.2400	0.0383	0.000
lie	0.555	0.0677	0.0687	1.000
lip	0.037	-0.0015	0.0106	0.008
margeff	0.022	-0.0001	0.0055	0.435
av	0.033	0.0005	0.0071	0.715
Measure of dynamics				
lagdep	0.079	-0.0036	0.0145	0.000
speed	0.977	-0.0949	0.0278	0.000
lags	1.000	-0.1303	0.0262	0.000
time	0.026	0.0003	0.0058	0.639
Specification characteristics				
ic	0.104	0.0058	0.0197	0.997
ic12	0.027	-0.0002	0.0060	0.469
nic	1.000	-0.0661	0.0067	0.000
stabil	0.995	-0.2702	0.0642	0.000
nstab	1.000	0.1336	0.0177	1.000
infl	0.518	-0.0525	0.0571	0.000
inst	0.021	0.0000	0.0063	0.400
ninst	0.025	-0.0005	0.0062	0.043
fact	0.023	0.0002	0.0053	0.759
nfact	0.024	0.0004	0.0053	0.903
pubpr	0.035	-0.0016	0.0123	0.029
Publication characteristics				
journal	1.000	-0.1371	0.0269	0.000
lgoog_pa	0.958	0.0672	0.0276	1.000
authaff	0.365	0.0353	0.0521	0.997

Notes: Estimated by Bayesian model averaging. Response variable: the effect of reforms on economic growth in the short run (partial correlation coefficient). The acronyms of the explanatory variables are explained in Table 4. PIP = posterior inclusion probability. Cond. pos. sign = probability that the sign estimated for the corresponding variable is positive. The posterior mean is analogous to the estimate of the regression coefficient in a standard regression; the posterior standard deviation is analogous to the standard error of the regression coefficient in a standard regression.

Table A2: Explaining the Differences in the Estimates of the Reform Effect (Long Run)

Variable	PIP	Posterior mean	Posterior std. dev.	Cond. pos. sign
prec	0.985	0.4270	0.1022	1.000
Data and methods				
panel	0.047	-0.0042	0.0218	0.000
endo	0.019	-0.0004	0.0046	0.000
fixed	0.065	-0.0027	0.0118	0.000
k	0.013	0.0000	0.0001	0.557
start	0.966	-0.0136	0.0042	0.000
tspan	0.987	-0.0186	0.0038	0.000
Type of reform index				
ebrd	0.014	0.0002	0.0034	0.946
comb	0.011	0.0001	0.0025	0.825
lii	0.020	-0.0010	0.0098	0.045
lie	0.812	0.1170	0.0675	1.000
lip	0.021	0.0001	0.0093	0.592
margeff	0.024	0.0012	0.0110	0.890
av	1.000	0.2520	0.0265	1.000
cli	0.042	0.0037	0.0210	1.000
Measure of dynamics				
lagdep	0.014	-0.0002	0.0033	0.125
speed	0.192	-0.0101	0.0229	0.000
lags	0.995	-0.1448	0.0265	0.000
time	0.056	-0.0022	0.0103	0.000
Specification characteristics				
ic	0.061	-0.0026	0.0115	0.000
ic12	0.991	-0.0933	0.0232	0.000
nic	0.015	-0.0001	0.0009	0.034
stabil	0.166	-0.0091	0.0223	0.000
nstab	0.198	-0.0050	0.0111	0.000
infl	0.018	-0.0003	0.0040	0.208
inst	0.042	-0.0017	0.0096	0.000
ninst	0.035	-0.0006	0.0038	0.000
fact	0.012	-0.0001	0.0030	0.041
nfact	0.016	-0.0003	0.0034	0.000
pubpr	0.598	-0.0801	0.0732	0.000
Publication characteristics				
journal	0.021	-0.0004	0.0038	0.000
lgoog_pa	0.013	-0.0001	0.0013	0.033
authaff	0.011	0.0000	0.0022	0.549

Notes: Estimated by Bayesian model averaging. Response variable: the effect of reforms on economic growth in the long run (partial correlation coefficient). The acronyms of the explanatory variables are explained in Table 4. PIP = posterior inclusion probability. Cond. pos. sign = probability that the sign estimated for the corresponding variable is positive. The posterior mean is analogous to the estimate of the regression coefficient in a standard regression; the posterior standard deviation is analogous to the standard error of the regression coefficient in a standard regression.

Appendix B. Diagnostics of BMA

Table A3: Summary of BMA Estimation (Short Run)

Mean no. regressors	*Draws*	*Burn-ins*	*Time*
11.7273	$2 \cdot 10^6$	$1 \cdot 10^6$	7.408883 minutes
No. models visited	*Modelspace*	*Visited*	*Topmodels*
400, 300	$1.1 \cdot 10^9$	0.037%	96%
Corr PMP	*No. Obs.*	*Model Prior*	*g-Prior*
0.9998	245	random	BRIC
Shrinkage-Stats			
Av= 0.9989			

Notes: The "random" model prior refers to the beta-binomial prior advocated by Ley and Steel (2009): prior model probabilities are the same for all possible models; in other words, we do not a priori prefer any particular model size. We set the Zellner's g prior following Fernandez et al. (2001).

Figure A1: Model Size and Convergence (Short Run)

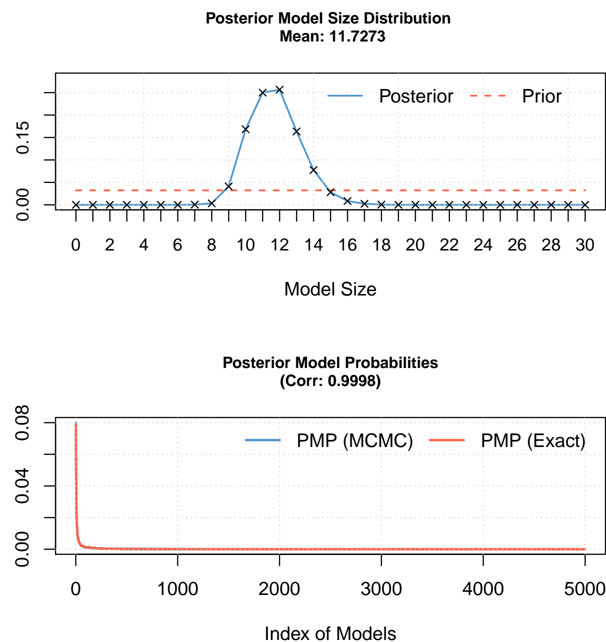
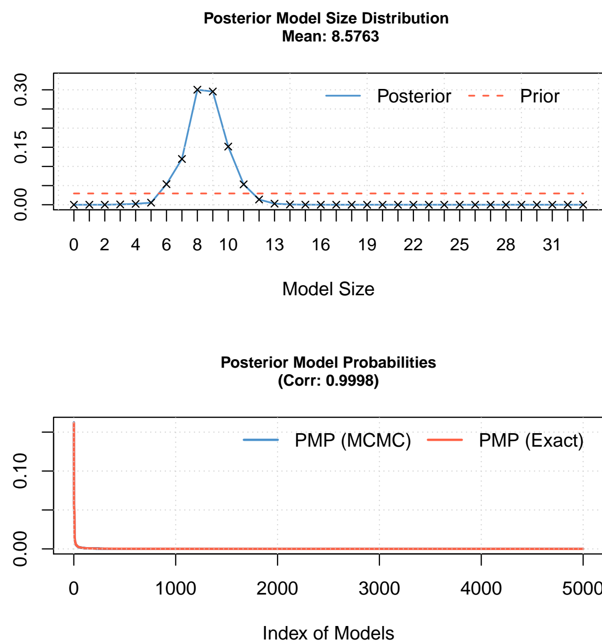


Table A4: Summary of BMA Estimation (Long Run)

Mean no. regressors	*Draws*	*Burn-ins*	*Time*
8.5763	$2 \cdot 10^6$	$1 \cdot 10^6$	6.829383 minutes
No. models visited	*Modelspace*	*Visited*	*Topmodels*
289,356	$8.6 \cdot 10^9$	0.0034%	96%
Corr PMP	*No. Obs.*	*Model Prior*	*g-Prior*
0.9998	292	random	BRIC
Shrinkage-Stats			
Av= 0.9991			

***Notes**:* The "random" model prior refers to the beta-binomial prior advocated by Ley and Steel (2009): prior model probabilities are the same for all possible models; in other words, we do not a priori prefer any particular model size. We set the Zellner's g prior following Fernandez et al. (2001).

Figure A2: Model Size and Convergence (Long Run)



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