



Mark my Words: Information and the Fear of Declaring one's Exchange Rate Regime

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Abstract: This paper investigates the role of a free press and of the circulation of information on the capacity of a country to declare an exchange regime that is different from the regime it *de facto* implements. We put forward consistent evidence that increased press freedom and easier access to information results in a lower probability of untruthfully reporting the regime that is implemented. The finding is resistant to a large set of robustness checks, including controlling for democracy.

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1. Introduction

Exchange rate practices are a field in which words and deeds often fail to coincide. On the one hand, countries that peg their exchange rate may frequently adjust the parity of their currency (Ghosh et al., 1997). As a consequence, their exchange rate policy is similar to a flexible exchange rate regime. On the other hand, countries that officially let their currency float intervene to stabilize it, displaying what Calvo and Reinhart (2002) refer to as the *fear of floating*. The observed discrepancies between announced and implemented policies led to the development of *de facto* classifications of exchange rate regimes, for instance by Reinhart and Rogoff (2004), Levy Yeyati and Sturzenegger (2005), or Shambaugh (2004). These classifications reveal that *de facto* exchange rate practices are often quite different from official *de jure* exchange rate regimes, and put into question the meaningfulness of *de jure* regimes in the study of exchange rate regimes.

The positive literature on the choice of an exchange rate regime quickly took stock of those findings. Studies of the determinants or consequences of the choice of an exchange rate regime, such as Poirson (2001) or Klein and Shambaugh (2008), therefore replaced *de jure* regimes by *de facto* regimes as their dependent variables. Another strand of research tries to explain the observed fear of floating. Hausmann, Panizza and Stein (2001) argue that countries and firms with high unhedged foreign currency denominated debt stand a chance of large balance sheet losses in a devaluation of the local currency. Moreover, countries that officially announce a fixed exchange rate in an environment of mobile capital are more prone to speculative currency crises (Obstfeld and Rogoff, 1995). Following this idea, Genberg and Swoboda (2005) argue that the fear of floating may result from the fear of being subject to speculative attacks if a *de facto* fixed exchange rate regime is announced *de jure*. A managed float may appear as a less risky choice. Policymakers then have an incentive to implement a fixed regime and announce a floating regime at the same time. Others, like Pontines and Rajan (2008), conclude that the fear of floating is essentially a fear of appreciation. Aizenman and Sun (2009) relate the fear of appreciation to a prudential motive. Levy-Yeyati and Sturzenegger (2007) put forward a mercantilist motive.

The aforementioned studies explain why some countries are reluctant to let their exchange rate float freely, or to announce a highly visible exchange rate target, but do not explain why they implement a regime and declare another. For this, one has to specifically study the gap between the observed and declared regimes, as opposed to the chosen regime. Only a handful of papers have addressed that question. Alesina and Wagner (2006) underline the impact of the quality of institutions. Their empirical study suggests that good political institutions drive a policymaker to

deviate from flexible to more rigid regimes, whereas bad institutions drive a policymaker to deviate from fixed to more flexible regimes. Carmignani et al. (2008) follow a similar route, but add social and political to institutional variables as explanatory variables. They show that governments who are subject to an adverse political environment find it more difficult to sustain the commitment to a peg and governments who face social instability unambiguously are more likely to opt for a fear of floating behaviour. Von Hagen and Zhou (2005) focus on economic determinants of regime discrepancies in transition countries, and find that the probability of observing a discrepancy between the *de facto* and *de jure* regime increases when the *de jure* regimes is inappropriate. Finally, according to von Hagen and Zhou (2009), a government displays fear of floating to avoid the political costs of exchange rate crises while preserving some of the benefits of more stable exchange rates.

The common feature of these papers is that they all focus on the motives for a country to declare a *de jure* regime that differs from its *de facto* regime, but overlook the constraints that may limit its scope to do so. The aim of this paper is precisely to investigate one such constraint. More specifically, we investigate the extent to which press freedom and easy access to information constrain a country's declaration of its exchange rate regime.

This is important because there is strong evidence that the media influence the conduct of monetary policy. Havrileski (1995), Maier et al. (2002), or Maier and Bezoen (2004), for instance, document that major central banks respond to media pressure. However, whether or not the exchange rate policy may be affected by the press remains an unaddressed question. Moreover, a vast literature has investigated the relation between democracy and transparency of the political system and exchange rate policy (see Broz, 2002, or Bernhard and Leblang, 1999), but has overlooked the determinants of the truthfulness of a country's declaration.

From a theoretical point of view, the effect of the media freedom and access to information on the propensity to dissimulate the exchange rate regime is *a priori* uncertain. On the one hand, if media essentially seeks to detect and reveal the misreporting of the exchange rate regime, increasing press freedom and the access to information would reduce the propensity of policymakers to declare a regime that differs from the actual one. The media would then act as "watchdogs". On the other hand, if the main role of the media is to transmit the pressure that arises from opponents, interest groups and politicians to policymakers, then increasing media freedom and the access to information will increase the policymakers' propensity to declare a regime that differs from the actual one. The media would then essentially be a means of "pressure".

Which view prevails is, therefore, an empirical issue. Consequently, we test the two views by estimating a series of models in which the dependent variable is the difference between the *de facto* and *de jure* exchange rate regimes, on a panel dataset containing both developed and developing countries. Our contribution to the literature is manifold. First, we extend the debate about the fear of declaring by bringing into the picture the role of information as a constraint on policymakers. We thus extend Alesina and Wagner's (2006) early findings. Second, we contribute to the literature on the role of democracy and transparency of the political system in the exchange rate policy (see Broz, 2002, or Bernhard and Leblang, 1999). Finally, the study addresses the relationship between the media and monetary policy (see Maier et al., 2002, Maier and Bezoen, 2004, Berger et al., 2011).

With these goals in view, the rest of the paper is organized as follows. Section 2 discusses in more detail the theoretical impact of media freedom and access to information on the propensity to truthfully report the *de facto* exchange rate regime. Section 3 describes our empirical strategy. Section 4 displays our findings, while section 5 tests the robustness of our results. Section 6 concludes.

2. Media and exchange rate regime practices: watchdog vs. means of pressure

The relation between press freedom and a country's capacity to conceal its true exchange rate regime is *a priori* ambiguous. It will differ depending on the media playing the role of a watchdog or means of pressure.

2.1. Media as a watchdog

Better informed voters are better able to monitor public decisions and institutional commitments, and use re-election prospects to constrain untruthful policymakers. Therefore, if media coverage is missing because of political oppression or low quality of information technology, one may expect that renegeing on a commitment does not involve serious political sanctions.

An emerging literature provides evidence on the role of the media in shaping economic and political outcomes.¹ Generally, these studies find better governance in countries where the media covers politics actively. At the cross-country level, Djankov et al. (2003) observe that state

¹ Most studies focus on the role of the media on voters' choices and election outcomes (Chan and Suen, 2009, Enikolopov et al, forthcoming, or DellaVigna and Kaplan, 2007).

ownership of the media leads to poor government performance. Brunetti and Weder (2003) report that press freedom has a significant negative effect on corruption. Leeson (2008) finds a positive effect of press freedom on political knowledge, political participation, and voter turnout in European countries. Within countries, Bruns and Himmler (2008) present some evidence that politicians tend to increase public spending in jurisdictions where media coverage is higher. Besley and Burgess (2002) find a positive effect of newspaper circulation on food aid and calamity relief in India. Snyder and Strömberg (2010) provide evidence of the whole process through which the media influences policies in the United States. They observe that elected policymakers that are more closely scrutinized by the media provide better policies and services to their voters. They, for instance, obtain more transfers from the federal budget. Van der Crujisen et al. (2010) focus specifically on monetary policy. Using a survey of Dutch citizens, they observe that obtaining more information through media outlets leads to better knowledge of the central bank's decisions.

If the media monitors public policies, one may contend that a free press and, more generally, easy access to information limit a country's capacity to declare an exchange rate regime that differs from the actual one. A free press can scrutinize data and policymakers' announcements, and analyse their behaviour and policies. A free press is therefore more likely to detect and reveal the exchange rate policy that the policymakers may want to conceal. Better informed voters may even sanction policymakers who try to conceal their true exchange rate policy. The same holds for investors in the foreign exchange market who will not believe official announcements if they are contradicted by the press. Trying to conceal the true exchange rate policy then becomes futile. Accordingly, if the press plays the role of what Berger et al. (forthcoming) refer to as watchdogs, press freedom should simply deter policymakers from trying to conceal the true exchange rate regime. Conversely, lower media coverage due to political oppression or low quality of information technology, may give policymakers more leeway to declare an exchange rate regime that differs from the regime that is actually implemented.

2.2. Media as means of pressure

The media does not only convey information to the general public about the deeds of policymakers. They also convey information from the general public to policymakers. Accordingly, they are a means of pressure on the latter.

There is evidence that central banks respond to media pressure. Havrileski (1995) reports evidence that the US Federal Reserve responds to pressure to raise or decrease interest rates,

measured by the number of press articles asking for an increase or a cut in interest rates. Maier et al. (2002) apply Havrileski's method to the German Bundesbank, and find a significant relation between the opinions expressed in the press by the financial sector and the central bank's interest rate decisions. Dalle Nogare and Vassali (2008) obtain similar results for the Bank of Italy before the EMU. Maier and Knaap (2002) refine the results of Maier et al. (2002) by distinguishing pressure, defined as in Havrileski (1995), from support, defined as the number of published articles expressing support for the Bundesbank. They show that the marginal impact of pressure, be it to raise or decrease interest rates, decreases when support is higher. Maier and Bezoen (2004) also confirm that the Bundesbank could be pressured by interest groups through the press. Comparing it with the European Central Bank, they also argue that pressure is stronger on national than supranational central banks.

The common theme of these findings is that the press relays the pressure from the public to policymakers. Therefore, more press freedom should result in more pressure on monetary authorities. To cope with, or respond to, increased pressure, a country's policymakers may declare an exchange rate regime that differs from its *de facto* regime. Changing the exchange rate *de jure* requires less means and involves fewer consequences in terms of policy outcomes. The consequence of the view of media as a means of pressure is that greater media freedom and better access to information should result in more frequent discrepancies between *de facto* and *de jure* exchange rate regimes.

3. Data and econometric strategy

The view of the media as a watchdog predicts that more press freedom and a better access to information should result in a lower propensity to declare a *de jure* exchange rate regime that differs from the *de facto* regime, while the view of media as a means of pressure predicts the opposite. Which view prevails is an empirical matter. In this section, we describe the strategy we carry out to decide between the two views. We present our dependent variables, then describe and discuss the key explanatory variables. Finally, we list control variables.

3.1. De jure vs. de facto exchange rate regimes

The *de jure* classification of exchange rate regimes is provided by the IMF and extensively explained in its Annual Report on Exchange Rate Arrangements and Exchange Restrictions. The *de jure* exchange rate regime is based solely on information provided by national authorities, either via

IMF agents working in central banks or via contacts between IMF and these authorities. Until 1999 that regime was the only one to be provided by the IMF. No instances were set up to verify or correct the declarations of central banks.

Because they suspected that the classification of the IMF may not reveal actual exchange rate regimes, some scholars developed *de facto* classifications.² Reinhart and Rogoff (2004), Shambaugh (2004), and Levy Yeyati and Sturzenegger (2005) provide such classifications. Levy-Yeyati and Sturzenegger develop a cluster analysis based on exchange rate, base money and foreign reserves fluctuations.

However, our dependent variable is neither the *de jure* nor the *de facto* classification, but the discrepancy between the two. To build that variable, we therefore need both the *de jure* classification and a *de facto* classification. Our main classification of *de facto* exchange rate regimes is provided by Reinhart and Rogoff (2004, hereafter R&R). It is the classification that has been used in the few studies that have tried to explain the discrepancies between announced and implemented exchange rate regimes, such as Alesina and Wagner (2006), Carmignani et al. (2008) or Hossain (2009). R&R rely on official exchange rates and black market rates to develop their “natural classification”. Their classification is particularly well suited to the question we address. The first reason is that the R&R classification depends on a detailed analysis of country chronologies, the official exchange rate but also the dual market rate. The existence of parallel exchange rates remains an important issue in developing countries, and results from interventions through capital controls. Second, the mixed *de jure-de facto* classification method leads R&R to verify exchange rate regime announcements and specifically correct the cases where they are disconnected from exchange rate policies. Unlike other classifications such as Levy Yeyati and Sturzenegger (2005), this does not depend on a specific statistical method that is sometimes difficult to extend and leads to inconclusive cases and sometimes doubtful results. As noted by von Hagen and Zhou (2009), the R&R classification is a better reflection of actual market conditions because it rests on a larger set of indicators, and results from a careful study of individual countries.³

We define the *de jure* and the *de facto* exchange rate regimes on a binary base. We thus focus on the fixed versus flexible exchange rate regime alternative, and overlook more subtle differences,

² Because the IMF may interfere with domestic policies and the choice of exchange rate arrangements, especially in times of crisis (Dreher and Vaubel, 2001, Dreher 2006, Dreher and Walter, 2010), a country’s authorities may have an incentive to declare one exchange rate regime and implement another.

³ The classification developed by Levy-Yeyati and Sturzenegger (2005) is sometimes used as the principal *de facto* classification. Nevertheless their method is criticized by some authors (Klein and Shambaugh, 2008, Eichengreen and Razo-Garcia, 2006,). See Tavlas et al. (2008) for further information.

like the difference between a pure and a dirty float, or the difference between a peg and a crawling peg. This choice leads to a larger consensus between the *de facto* classifications, as reported by Klein and Shambaugh (2010). According to the binary classification, fixed exchange rate regimes include exchange rate regimes with no separate legal tender, currency boards, pegged exchange rates, crawling pegs, and exchange rates with crawling bands. Flexible exchange rate regimes include managed and independently floating exchange rate regimes. The freely falling cases that arise in the R&R classification are dropped, because they correspond to episodes of hyperinflation, and do not necessarily reflect a dissimulation of the implemented regime. Moreover, they may be outliers, due to the special conditions that arise in such situations.

We merge the information contained in the *de jure* and *de facto* classifications to produce three dummy variables capturing the discrepancy between the implemented and the announced regimes. The first measure is a dummy variable that takes the value one if there is a difference between the *de facto* exchange rate regime and the *de jure* exchange rate regime of a country. This variable pools deviations towards greater flexibility with those towards greater fixity. It is therefore a general measure of the untruthfulness of countries' declarations to the IMF, and we label it the "dissimulating country" variable.

We refine that general measure by distinguishing deviations towards more flexibility and deviations towards more fixity. We do so by splitting the observations in our sample into two subsamples: one contains countries that are *de facto* implementing a floating exchange rate and the other those that are *de facto* implementing a fixed exchange rate. In the first subsample, the dependent variable is therefore equal to one if a country that *de facto* pegs its exchange rate declares a float. It accordingly signals "dissimulating fixers". In the second subsample, the dependent variable is equal to one if a country that *de facto* lets its exchange rate float declares a peg, and signals "dissimulating floaters". Table 1 provides a clearer view of the classification of countries.

***Insert table 1 about here ***

The dummy variable opposes the first to the second diagonal of Table 1. Namely, it opposes countries that transparently declare their exchange rate regime to those that dissimulate it. When we focus on the first subsample, we consider the first column of Table 1, *de facto* fixers. We thus distinguish transparent and dissimulating fixers. The second subsample focuses on the second column of Table 1, *de facto* floaters, and therefore opposes transparent and dissimulating fixers.

Note that in the two specific definitions of discrepancy between exchange rate regimes *de jure* and *de facto*, we start from the *de facto* regime and check whether the declared regime is in line with the *de facto* one. We could have chosen the reverse strategy by classifying countries according to their *de jure* regimes first. The strategy used here is more appropriate for several reasons. First, the question at hand is to determine under which conditions countries announce an exchange rate regime that is different from the implemented regime and not their inability to sustain a given regime. Had we classified observations according to the *de jure* regime first, we would simply test the ability of countries to commit to a given exchange rate regime. Second, the way in which we characterize deviations is in line with the timing of countries' declarations to the IMF because the *de jure* classification is announced either during the year or ex-post (see IMF: Annual Report on Exchange Rate Arrangements and Exchange Restriction). Third, our strategy implies a particular view of the hierarchy of the two decisions that a country has to make. It considers that the key decision is to choose the *de facto* exchange rate regime, and that declaring it to the IMF is an ancillary decision. Indeed, implementing an exchange rate policy requires drastic steps that a policymaker needs to prepare carefully before conducting the policy. Simply announcing a regime requires virtually no preparation. It therefore makes sense to split countries according to their *de facto* regime. Moreover, our classification of observations allows us to make sure that we are indeed focusing on the decision to report the true exchange rate regime, and not the choice of the exchange rate regime. Even in the extreme case, if countries did not care about the regime that they announce, and announced it randomly, we would simply test the determinants of the choice of an exchange rate, as opposed to the determinants of the truthful declaration of the exchange rate regime.⁴ The descriptive statistics and the correlation matrix of the studied variables are provided in the appendix.

3.2. *Press freedom and the circulation of information*

To test the view of the media as a watchdog against the view of the media as a means of pressure, we need to relate the choice to truthfully report the exchange rate regime to a set of variables measuring press freedom and the access to information.

Press freedom is a general concept that is not easily quantified. We therefore proxy it by several indicators. We use the Press Freedom Index compiled by Freedom House (2010), as the

⁴ Reinhart et Rogoff (2004) put forward that the IMF official classification is only “a little better than random”.

main indicator of media freedom. This index is the most comprehensive available evaluation of media freedom. It is compiled by experts, and based on several sources: international and regional specialists, the finding of human rights and press freedom organizations, official reports of governments and multilateral bodies and a variety of domestic and international news media. Their analysis is based on 23 questions and 109 indicators grouped into three categories: the legal environment, the political environment, and the economic environment. The legal environment category includes “an examination of both the laws and regulations that could influence media content and the government’s inclination to use these laws and legal institutions to restrict the media’s ability to operate” (Freedom House, 2010). It encompasses, among others, legal and constitutional guarantees of press freedom or the penalties for libel and defamations. The political environment category evaluates “the degree of political control over the content of news media” (Freedom House, 2010). The editorial independence of the media and censorship, both official and self-imposed, are also taken into consideration by experts. Finally, the economic environment category assesses the extent to which economic constraints influence the media’s actions, such as media ownership or subsidies by the State or other actors. To ease the interpretation of the results, we recode the Freedom House index in such a way it increases with press freedom. A country’s overall press freedom score is then located on a scale of 0 (no media freedom) to 100 (perfectly free media). This variable covers up to 196 countries, from 1980 to 2010.

A second category of explanatory variables concerns the circulation of information, or more generally the ease with which the public can access information about policy decisions and political decision making processes. The main sources of information are directly related to the media: the Internet, newspapers, television, telephone or radio. As the role of the Internet has dramatically increased over the past 20 years, turning it into a major source of information, our favourite measure of access to information is thus the percentage of Internet users (ICT indicators Data 2010, International Telecommunication Union). It reflects the technological development of information diffusion and access to information. It is based on available national statistics and ad hoc surveys.

The sign of the relation between press freedom and the probability to untruthfully report the exchange regime will reveal whether the watchdog or the means of pressure view of the media holds. If the watchdog view holds, we will observe a negative relation between press freedom or access to information on the one hand, and the probability to untruthfully announce the exchange rate regime on the other hand. Conversely, if the means of pressure view holds, we should observe a positive relation.

3.3. Control variables

We control for level of development, size, financial and trade openness and the level of democracy. The level of development is captured by the log of the constant price purchasing power parity adjusted gross domestic product per capita (Penn World Table 6.3). The behaviour of developed and developing countries may differ because of differences in their institutional development and the credibility of their authorities.

A country's size is measured by its population (in millions). This variable is routinely controlled for in studies of the determinants of the exchange rate regime, because a country's size determines its propensity to be a price taker or price maker.

Trade openness is measured by the ratio of exports plus imports to GDP. It captures the fact that an open country pays more attention to the *de facto* stability of its exchange rate. Hence it is more likely to deviate from a flexible regime *de jure* and less likely to deviate from a fixed regime *de jure*.

The indicator of financial openness is taken from Chinn and Ito (2008). It controls for openness in cross-border financial transactions. A country with less stringent capital controls will find it more difficult to peg its exchange rate.

Democracy is measured by Cheibub et al.'s (2010) dummy variable. It is meant to capture the influence that voters have on policymakers and on the policy-making process. In a democratic system, policymakers are exposed to the sanction of voters, and will therefore avoid highly visible deviations. This argument applies in particular to the deviation from a fixed exchange rate regime, but not to the deviation from a flexible exchange rate regime. A flexible exchange rate regime does not necessarily involve a high volatility of the exchange rate. The effect of democracy is thus not well defined.

Finally, all regressions include year dummies, to control for unobserved global trends. Country fixed effects appear inappropriate in this study, because exchange rate arrangements are generally stable and time invariant for most countries.

As the dependent variable is a binary variable, we estimate a binary logit model. It is estimated with robust standard errors clustered at the country level, to control for serial correlation and for heteroskedasticity across countries.⁵

⁵ Following similar studies, like Alesina and Wagner (2006), we do not control for endogeneity in our baseline regressions. The motivation is that, although policymakers who are dissimulating their exchange rate regime would

4. Empirical results

Our findings are displayed in Table 2. The first three columns report baseline regressions relating the deviations from announced regime to media freedom. The following three columns report the result of the regressions in which the access to information is the main explanatory variable. In Table 2, the results pertaining to control variables confirm previous studies. Regression (2.1) reveals that countries with a higher level of development are less likely to dissimulate their exchange rate regime. However, development has an asymmetric effect on deviations from the announced regime across *de facto* regimes. Its coefficient is positive in columns (2.2) and (2.5), and negative in column (2.3). This implies that more developed countries are more likely to dissimulate their exchange rate regime when they float *de facto*, and less likely to dissimulate their exchange rate regime when they peg *de facto*. These results are in line with previous studies (Alesina and Wagner, 2006 or Hossain, 2009).

The results related to the democracy index confirm the findings of Alesina and Wagner (2006): democracy is positively correlated with the probability of being a dissimulating fixer, but negatively with the probability to be a dissimulating floater. This suggests that democratic countries have an incentive to reduce uncertainty by lowering the fluctuation of their exchange rate in a flexible regime, but may support high political costs in devaluing the announced exchange rate in a fixed regime.⁶ Size, as measured by population, and trade and financial openness have no significant effect at standard levels of significance.

***Insert Table 2 about here ***

Turning now to our variables of interest, the coefficient of the media freedom index is negative and highly significant at conventional levels of significance. In other words, increasing media freedom has a negative effect on the probability of concealing one's *de facto* regime.

probably prefer the press to remain silent about it, it is unlikely that they will determine the control they have over the media just to accommodate their decision to hide or reveal the country's true exchange rate regime. Indeed, in many countries the policymaker in charge of monetary policy tends to have little control over media policy. Moreover, media freedom is mainly a consequence of countries' political systems, history and culture, and cannot be easily modified, as pointed out by Leeson (2008). Thus, considering these variables of interest as exogenous to our baseline model seems reasonable. We will, however, address potential endogeneity problems in the robustness checks section.

⁶ Using the Polity IV index to assess democracy leads to similar results.

The second variable of interest captures the circulation of information. Its coefficient is again negative and highly significant at standard levels. Improving the access to information tends to discourage policymakers from misreporting the exchange rate regime. In all regressions, measures of media freedom and circulation of information are inversely related to the propensity to untruthfully report to the IMF the *de facto* exchange rate regime.

An important remark is that the effect of media freedom and improved circulation of information on the propensity to untruthfully report the exchange rate regime is qualitatively the same regardless of the *de facto* exchange rate regime. Greater media freedom and access to information both tend to deter policy makers from concealing their country's *de facto* exchange rate regime regardless of the nature of that regime. This is evidence that media freedom and the circulation of information reflect a constraint imposed on a country's policymakers. This is in line with the view of media as a watchdog. Furthermore, one should bear in mind that those results are obtained while controlling for the level of democracy. Accordingly, they are not due to press freedom and access to information capturing the effect of democracy, but signal the independent effect of press freedom and access to information. This lends additional credibility to the interpretation of the media as a watchdog.

To substantiate these findings, it is important to assess the marginal effect of the variables of interest (press freedom and access to information) on the propensity to declare a regime that is different from the regime the country implements. In a non-linear model, the marginal effect of a variable differs with each observation, which makes the interpretation of the coefficient difficult. Using the estimates reported in columns 4.1 and 4.4, we compute the marginal effect of press freedom and access to information for each observation, analyse its density function, and then consider the mean or the median of those effects.

***Insert Figure 1 about here ***

The density function of the marginal effect of press freedom on the probability to dissimulate one's exchange rate regime is plotted in Figure 1. It shows that the marginal effect of increasing press freedom on the propensity to dissimulate one's exchange rate regime is negative for every observation in our sample. The median marginal effect is approximately equal to -0.0064, which means that increasing press freedom by one point of the rescaled press freedom index decreases the probability of dissimulating one's exchange rate regime by 0.64 percentage point. To illustrate this

effect, let us assume that Libya improves its press freedom by one standard deviation of the Freedom House index. Its index would rise from 4 to 28.53, which is close to the level of Malaysia.⁷ All things being equal, the probability of dissimulating its exchange rate regime would approximately decline from 0.36 to 0.26 in our model.

***Insert Figure 2 about here ***

The density function of the marginal effect of the access to information on the propensity to dissimulate one's exchange rate regime is displayed in Figure 2. This figure shows that the marginal effect is negative across the whole sample. The median marginal effect is approximately equal to -0.015. This means that increasing the number of Internet users by one percentage point results would decrease the probability of choosing a regime *de jure* that is different from the regime *de facto* by 1.5 percentage points. To illustrate this effect, let us assume that Libya improves its percentage of Internet access by one standard deviation of the Internet access index in our sample. Its index would rise from 5.13% to 25.26%, and would approach the level of Argentina. All things being equal, the probability of dissimulate its exchange rate regime would approximately decline from 0.40 to 0.24 in our model.

We complement the interpretation of our results by plotting the predicted probability of dissimulating one's exchange rate regime against the press freedom index. Figure 3 shows that low levels of press freedom are linked to higher, but also more dispersed, probabilities of dissimulating. This is in line with the view that a free press acts as a limitation to announcing a regime that is different from the implemented regime. Namely, in countries where the press is not free, policymakers can, but do not have to, dissimulate their exchange rate regime. If they have no incentive to dissimulate it, they will truthfully report the regime they *de facto* implement. Conversely, policymakers facing a free press have little choice but to report their exchange rate regime truthfully. Their exchange rate practices are, therefore, more homogeneous than those of countries where the media are under control.

***Insert Figures 3 and 4 about here ***

***Insert Figures 5 and 6 about here ***

⁷ Libya is the country in which the press is the most constrained in our sample in 2007.

Finally, Figure 5 plots the predicted probability of dissimulating one's exchange rate regime against the percentage of Internet users. It displays the same features for the measure of access to information as Figure 3 for press freedom.

5. Robustness checks

In the previous tables, we considered press freedom and access to information in distinct regressions. Though related, the two phenomena are independent. In other words, the media freedom index and the access to information index may be considered as complements rather than substitutes. Table 3 therefore uses the two variables as regressors in the same regression. The results are similar to those of the baseline regressions. An increase in the level of access to information significantly decreases the probability of dissimulating one's exchange rate regime at standard levels of confidence. The effect of press freedom on the probability of dissimulating the exchange rate regime is confirmed in the subset of countries that de facto fix their exchange rate regime, but is insignificant in the other regressions. This is probably due to multicollinearity problems, given the high correlation between press freedom and Internet access.⁸

As media freedom and access to information can only be proxied, one may be concerned that the results be driven by a specific measure of the variable of interest. We therefore replaced the Freedom House Index and the percentage of Internet users by two alternative indicators.

An alternative indicator of press freedom is provided by the international non-governmental organization Reporters Without Borders, hereafter RWB. The index is built calling on various sources: freedom of expression groups, journalists, researchers, legal scientists, and human rights activists. It is based on a questionnaire featuring 40 criteria mainly linked to violations directly affecting journalists and news media. We recoded the indicator to rank countries on the same scale as the Freedom House Index. It covers up to 175 countries but only from 2002 to 2009. This indicator is also used as a proxy by Leeson (2008).

To reflect press freedom, or the lack thereof, we also used the number of journalists killed while doing their work during combat or dangerous situations such as street protests. The organization Committee to Protect Journalists (CPJ) has computed this international variable since 1992. These deaths may however be due to war situations and political instability. Furthermore,

⁸ The coefficient of correlation between the two variables is 0.44.

countries with political instability attract considerable media coverage. As a consequence, the correlation of that variable with the exchange rate regime may be caused by the circumstances, and not by press freedom. We therefore only use this variable as a complement and interpret the corresponding results with caution. The variable is rescaled to increase with press freedom.

The results obtained with the alternative measures of press freedom are displayed in Table 4. They broadly confirm the results obtained with the Freedom House index: the coefficient of the RWB index is always negative and significant at conventional levels. In other words, greater press freedom always results in a smaller probability of concealing the country's *de facto* exchange rate regime, regardless of the nature of that *de facto* regime.

***Insert Table 4 about here ***

We also considered two alternative measures of the circulation of information: daily newspaper circulation (ICT indicators Data 2010, International Telecommunication Union), and mobile phone subscriptions (World Development Indicators, 2009), as provided by the United Nation Statistics Division. These indicators are constructed in a way similar to the indicator of the number of Internet users. Newspaper circulation is an alternative measure of the circulation of information, but may also capture the circulation of propaganda publications, which is why we only use the measure as a robustness check. The results obtained with the alternative measures of information circulation are displayed in Table 5. They again confirm the results obtained with the access to information index: easier access to information reduces a country's propensity to conceal its *de facto* exchange rate regime.

***Insert Table 5 about here ***

The definition of *de facto* exchange rate regimes may be critical to our results. One may therefore be concerned that they may be sensitive to the classification used. We addressed this concern by using an alternative exchange rate regime classification. We used the one developed by Shambaugh (2004) and transformed by Klein and Shambaugh (2008). Shambaugh's classification is based on whether the official exchange rate stays within a small band around the base currency for a

given period.⁹ This classification leads to one of the largest available datasets. It unfortunately takes into account neither the possibility to choose a multilateral peg nor the unofficial exchange rate, which is why we only use it as a robustness check. However, the results obtained with the alternative classification broadly confirm the findings described in the paper. Increased access to information reduces the propensity to dissimulate one's exchange rate regime. The results obtained with the alternative classification are displayed in Table 6. The results concerning media freedom are insignificant, but do not contradict previous findings.

***Insert Table 6 about here ***

So far, we have imposed a hierarchical structure to our dependent variable. We have implicitly assumed that the choice to truthfully declare one's exchange rate regime is subsequent to, or constrained by, the choice of the *de facto* exchange rate regime. The choice of exchange rate arrangement may, however, also be represented as a choice between four independent strategies: fixed regime *de jure* and *de facto* (transparent fixer), flexible regime *de jure* and fixed regime *de facto* (dissimulating fixer), fixed regime *de jure* and flexible regime *de facto* (dissimulating floater), and finally, flexible regime *de jure* and *de facto* (transparent floater). This setting results in four discrete outcomes, which cannot be ordered. Here, the relevant statistical model is a multinomial probit model.¹⁰ We perform two regressions using respectively the effect of press freedom (Freedom House index) and access to information (Internet users) as the main independent variable. The reference category is the case where both *de jure* and *de facto* are fixed. The results obtained with the multinomial probit model are displayed in Tables 7 and 8 according to the base outcome. Regression 7.1 shows that if press freedom increases, the probability of being a dissimulating fixer decreases in comparison with the probability of being a transparent fixer. The coefficients of press freedom in the other two columns are insignificant. Concerning the effect of access to information (regression 7.2), the results are similar for dissimulating and transparent fixers. Furthermore, if the access to information improves, the probability of being a transparent floater increases significantly more than the probability of being a dissimulating floater. As a consequence, these results confirm the negative impact of the media on the propensity to dissimulate one's exchange rate regime and

⁹ The classification developed by Shambaugh (2004) is also slightly modified to include single-year pegs in the paper of Klein and Shambaugh (2008). This modified dataset will be used in the robustness tests.

¹⁰ We prefer a multinomial probit to a multinomial logit model to avoid the problems linked to the property of the independence of irrelevant alternative (IIA). However, both models provide exactly the same conclusion.

the watchdog view. Moreover, Table 8 and, more particularly regression 8.2, show us that the strategy to transparently float becomes increasingly preferred to the strategy to transparently fix, when access to information improves. The coefficient associated with the three alternative arrangements is significant and negative when the base outcome is the transparent floater arrangement. Accordingly, the probability of choosing the transparent floating exchange rate regime increases with the access to information.¹¹

***Insert tables 7 and 8 about here ***

In previous regressions, we neutralized the effect of episodes of hyperinflation by dropping the corresponding observations. As a robustness test, we rerun our baseline specifications including episodes of hyperinflation, considered as flexible regimes. The results are displayed in Table 9. Just as in benchmark findings, increased press freedom and access to information decrease the propensity to announce a regime that is different from the *de facto* regime.

***Insert Table 9 about here ***

Another concern was that *de facto* exchange rate regimes, and consequently our dependent variables, are estimated. As a result, standard errors may be underestimated, thereby biasing statistical inference, and leading to spurious significant coefficients. We, therefore, reran all our estimations again using bootstrapped standard errors. The results are displayed in Table 10 and are similar to those of the baseline models.

***Insert Table 10 about here ***

An additional concern was that the results pertaining to press freedom may be subject to reverse causality. A policymaker dissimulating the true exchange rate regime could subsequently decide to silent the press to prevent it from revealing the dissimulation. In that case, the lack of press freedom would result from, and not allow, the dissimulation of the true exchange rate regime. Limiting press freedom just to keep the true exchange rate secret seems *a priori* unlikely. However,

¹¹ This statement is confirmed by marginal effects at the mean and at the median. The marginal effect of the access to information is significant and positive for the reference category, the transparent floating regime, but insignificant or negative for the other categories. Results are available on request.

the possibility cannot be completely ruled out a priori. Similar arguments may hold for the access to information. Another endogeneity problem appears if we consider a third variable influencing both dependent and independent variables. The transparency of the central bank may, for instance, influence both press freedom, through a better communication of the central bank, and the exchange rate strategy, through a change of the central bank objectives. We therefore check the robustness of our results to reverse causality in two ways. First, we lag the variables of interest by one year.¹² The results are displayed in the Table 11. They still show a strong negative impact of greater press freedom and access to information on a country's propensity to dissimulate its exchange rate regime.

***Insert Table 11 about here ***

Second, we address endogeneity by using instrumental variables estimations. We use the shares of Catholics, Muslims, and Protestants in the population as instruments for the press freedom and the share of English speakers in the population for the access to information. Hallin and Mancini (2004) and Leeson (2008) stress that press freedom is to a large extent historically and culturally determined, and religion is an important dimension of a country's cultural heritage. On the contrary, it is difficult to believe that religion may directly influence the choice of a particular exchange rate regime, or of the decision to report it to the IMF. The effect of a country's religious composition is therefore bound to run through its impact on press freedom, which makes it a suitable instrument. Moreover, those variables are clearly beyond the reach of monetary policymakers. They are therefore independent from, and predetermined to, the decision to untruthfully report the country's exchange rate regime choice. Religious shares may therefore also be used to determine causality. The data are extracted from the World Values Survey. We used the share of English speakers to instrument the access to information because the Internet access and development is historically linked to the English language. This instrument is closely related to the Internet development but much less to the mobile phones development. The data are provided by Crystal (2003) and some governmental institutions through national analysis. As multiple instruments are used, it is convenient to use the Two-Stage Least Square estimation method.¹³ We thus reran the baseline model of the fear of declaring in a cross section with their respective

¹² We also considered a lag of two years. The results were unchanged.

¹³ We also tested the instrumental variable probit model and the results are identical.

instruments. Except for the regression concerning mobile phone subscriptions, Anderson canonical correlations tests reject the null hypothesis that estimated models are underidentified, meaning that the excluded instruments are correlated with the endogenous regressor. The Cragg-Donald F-Statistic and the Stock and Yogo weak ID test show that the bias of the IV estimator is less than 10% of the OLS bias with the press freedom variables, less than 15% with the Internet access variable but more than 30% with the mobile phone subscriptions variable. Sargan-Hansen tests reject the hypothesis that the error terms are correlated with instruments. This confirms that religious shares appear to be suitable instruments for press freedom in our regressions. The share of English speakers appears to be a suitable instrument for the regression concerning Internet access but not mobile phone subscriptions. The results are displayed in Table 12 and confirm the negative effect of press freedom and access to information on the fear of declaring. Lack of data prevented us from analysing the effect on subsamples. The IV regressions confirm the impact of Internet access but not the impact of mobile phone subscriptions on the propensity to truthfully report one's exchange rate regime. Nevertheless, according to the Durbin-Wu-Hausman test, the only variable of interest that could be considered as endogenous is Internet access. In the other cases, baseline models should be preferred to 2SLS estimates, which lends additional credibility to our estimations.

***Insert Table 12 about here ***

The dissimulating fixer's arrangement is often described in the literature as being essentially present in emerging countries. Table 13 reports regressions completed with a dummy variable for emerging countries. It shows that the effect of our variables of interest on the propensity to dissimulate the exchange rate regime remains negative and statistically significant at standard levels.

***Insert Table 13 about here ***

As a final test, we decomposed the sample into a series of cross sections corresponding to each year, to check whether the marginal effect of press freedom and access to information evolved over time. The point estimates of the coefficients are displayed in Figures 7 and 8 with the confidence intervals in dotted lines. This shows that the marginal impacts of press freedom and access to information were either significantly negative or insignificant at standard levels of

significance over the period of study, which confirms our key finding. A common feature of those figures is that the period during which the impact of press freedom and access to information was insignificant corresponds to the aftermath of the Asian crisis of 1997. An interpretation of this is that the crisis resulted in noticeable devaluations that forced countries to allow their exchange rate to float. It can therefore be interpreted as an exogenous shock that forced countries to abandon their fear of floating strategies, thereby severing the link between press freedom and access to information and the propensity to hide the true exchange rate regime. The impact of the crisis was, however, limited in time and the standard impact of media freedom and access to information was observable until the crisis, and reappeared shortly thereafter.

***Insert Figures 7 and 8 about here ***

Overall, the finding that greater press freedom and an easier access to information result in a lower probability of concealing the *de facto* exchange rate resists to a large set of robustness checks. Our results therefore provide evidence of a watchdog effect of the media on exchange rate practices.

6. Conclusion

This paper has investigated the constraint that media freedom and access to information impose on the capacity of a country's authorities to conceal their *de facto* exchange rate regime. It provides robust evidence that free media and a better access to information decrease the propensity to declare a *de jure* regime that differs from the *de facto* regime. This influence is observed both for countries that *de facto* peg their exchange rate, and countries that *de facto* let their currency float. These findings are not due to an indirect effect of democracy, because the degree of democracy is systematically controlled for. They are robust to using several measures of media freedom and access to information, to alternative definitions of the *de facto* exchange rate regime. They are not driven by reverse causality and, overall, pass a series of robustness tests.

Our results accordingly provide strong support for the view that the media acts as a watchdog that monitors and reveals the authorities' policies. Moreover, they show that the influence of the media extends to exchange rate policies and to the transmission of information to the IMF by its member countries.

From a practical perspective, our results imply that the reliability of reported exchange rate regimes depends on press freedom. Policymakers and practitioners who have a stake in knowing a

country's exchange rate regime should be particularly careful when using a country's official exchange rate regime when that country muzzles the press. Such countries are likely to in fact implement another exchange rate regime than the one they announce.

While we have focused on the role of media freedom as a constraint on exchange rate policy, the paper leaves a number of questions for further research. First, the reason why a country's authorities may untruthfully reveal its exchange rate regime remains open, and calls for formal theoretical research. One possibility would be to relate the decision to conceal the *de facto* exchange rate regime to the interaction between the authorities and the private sector. A complementary route would be to study in more detail the role of that decision in the interaction between national authorities and the IMF and other international organizations. Second, the benefits from concealing the *de facto* exchange rate regime have not been studied yet. Determining if they exist, and how they are distributed within the dissimulating country, should improve our understanding of the determinants of such behaviour.

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Appendix

***Insert tables A1 and A2 about here ***

Tables

Table 1: Fear of declaring exchange rate regime

		<i>de facto exchange rate regime</i>	
		Fixed	Floating
<i>de jure exchange rate regime</i>	Fixed	Transparent fixers (1)	Disimulating floaters (3)
	Floating	Dissimulating fixers (2)	Transparent floaters (4)

Table 2: Baseline logit model

	Press freedom			Access to information		
	(2.1) Being a dissimulating country	(2.2) Being a dissimulating floater	(2.3) Being a dissimulating fixer	(2.4) Being a dissimulating country	(2.5) Being a dissimulating floater	(2.6) Being a dissimulating fixer
Freedom House Index	-0.0181** (0.009)	-0.0124 (0.019)	-0.0258** (0.011)			
Internet users (%)				-0.0377*** (0.011)	-0.0753** (0.032)	-0.0306** (0.013)
GDP per capita (log)	-0.2936* (0.150)	0.6561** (0.286)	-0.3630* (0.197)	-0.1344 (0.157)	0.7514* (0.391)	-0.2610 (0.198)
Population	0.0007 (0.001)	-0.0280 (0.022)	0.0009 (0.001)	0.0010 (0.001)	-0.0159 (0.012)	0.0012 (0.001)
Trade openness	0.0015 (0.003)	-0.0021 (0.006)	-0.0016 (0.004)	0.0018 (0.003)	0.0056 (0.005)	-0.0026 (0.004)
Democracy dummy	0.2550 (0.348)	-1.785** (0.754)	0.8454* (0.434)	-0.0741 (0.275)	-1.737*** (0.601)	0.2557 (0.331)
Financial Openness	0.0796 (0.104)	-0.4722 (0.320)	0.1650 (0.116)	0.0649 (0.097)	-0.2869 (0.266)	0.1458 (0.113)
Constant	1.0702 (1.504)	-5.943** (2.907)	1.2817 (1.937)	0.5770 (1.470)	-5.0749 (3.656)	1.9415 (1.840)
Number of countries	165	62	139	165	75	143
Years sample	1994-2007	1994-2007	1994-2007	1989-2007	1989-2007	1989-2007
Observations	1,793	461	1,332	1,973	532	1,441
Correctly classified	65.64%	89.37%	64.41%	66.04%	85.53%	63.15%

Note: Robust standard errors are clustered by country and are shown below coefficients. All regressions include year fixed effect.

The dependent variable is equal to 1 if there is a difference between the regimes de jure and de facto and to 0 otherwise. The sample in columns 2.1 and 2.4 is the whole sample. The sample consists of *de facto* floaters only, in columns 2.2 and 2.5. The sample consists of *de facto* fixers only, in columns 2.3 and 2.6.

*** significant at 1%, ** significant at 5%, * significant at 10%

Table 3: Baseline logit model

	Press freedom and access to information		
	(3.1) Being a dissimulating country	(3.2) Being a dissimulating floater	(3.3) Being a dissimulating fixer
Freedom House Index	-0.0118 (0.009)	-0.0156 (0.021)	-0.0230** (0.011)
Internet users (%)	-0.0392*** (0.012)	-0.0937** (0.037)	-0.0241* (0.014)
GDP per capita (log)	-0.0878 (0.171)	1.2176*** (0.369)	-0.2583 (0.222)
Population	0.0008 (0.001)	-0.0236 (0.019)	0.0009 (0.001)
Trade openness	0.0025 (0.003)	0.0043 (0.006)	-0.0010 (0.005)
Democracy dummy	0.2360 (0.329)	-1.4938** (0.653)	0.8590** (0.421)
Financial Openness	0.1439 (0.105)	-0.2356 (0.309)	0.2059* (0.121)
Constant	0.7682 (1.414)	-9.6462*** (3.641)	3.1750* (1.918)
Number of countries	163	61	138
Years sample	1994-2007	1994-2007	1994-2007
Observations	1,743	451	1,292
Correctly classified	68.31%	89.8%	63.98%

Note: Robust standard errors are clustered by country and are shown below coefficients. All regressions include year fixed effect. The dependent variable is equal to 1 if there is a difference between the regimes de jure and de facto and to 0 otherwise. The sample in columns 3.1 is the whole sample. The sample consists of *de facto* floaters only, in columns 3.2. The sample consists of *de facto* fixers only, in column 3.3.

*** significant at 1%, ** significant at 5%, * significant at 10%

Table 4: Alternative measures of press freedom

	Press freedom					
	(4.1) Being a dissimulating country	(4.2) Being a dissimulating floater	(4.3) Being a dissimulating fixer	(4.4) Being a dissimulating country	(4.5) Being a dissimulating floater	(4.6) Being a dissimulating fixer
RWB index	-0.0302** (0.014)	-0.0613* (0.034)	-0.0409*** (0.016)			
Killed journalists				-0.3151 (0.194)	0.2725 (0.306)	-1.1331*** (0.269)
GDP per capita (log)	-0.6396*** (0.202)	1.0415 (0.703)	-0.6790*** (0.251)	-0.3469** (0.139)	0.4974* (0.274)	-0.4267** (0.179)
Population		-0.1055** (0.046)	-0.0002 (0.002)	0.0008 (0.001)	-0.0226 (0.023)	0.0009 (0.001)
Trade openness	0.0004 (0.004)	-0.0165** (0.008)	-0.0006 (0.005)	0.0017 (0.003)	-0.0003 (0.005)	-0.0016 (0.004)
Democracy dummy	0.1272 (0.436)	-3.123** (1.320)	0.8585* (0.498)	-0.1911 (0.282)	-1.818*** (0.647)	0.1686 (0.342)
Financial openness	0.1373 (0.150)	-0.5259 (0.368)	0.1799 (0.166)	0.0460 (0.099)	-0.4578 (0.317)	0.1316 (0.113)
Constant	6.777*** (1.885)	-1.2972 (4.575)	7.978*** (2.213)	2.534** (1.162)	-3.4412 (2.325)	3.221** (1.509)
Number of countries	141	39	110	165	71	141
Years sample	2002-2007	2002-2007	2002-2007	1992-2007	1992-2007	1992-2007
Observations	789	195	594	1,968	516	1,452
Correctly classified	69.46%	93.85%	68.69%	64.48%	87.02%	63.50%

Note: Robust standard errors are clustered by country and are shown below coefficients. All regressions include year fixed effect.

The dependent variable is equal to 1 if there is a difference between the regimes de jure and de facto and to 0 otherwise. The sample in columns 4.1 and 4.4 is the whole sample. The sample consists of *de facto* floaters only, in columns 4.2 and 4.5. The sample consists of *de facto* fixers only, in columns 4.3 and 4.6

*** significant at 1%, ** significant at 5%, * significant at 10%

Table 5: Alternative measures of the access to information

	Access to information					
	(5.1) Being a dissimulating country	(5.2) Being a dissimulating floater	(5.3) Being a dissimulating fixer	(5.4) Being a dissimulating country	(5.5) Being a dissimulating floater	(5.6) Being a dissimulating fixer
Mobile subscriptions (/100 people)	-0.0271*** (0.006)	-0.0404* (0.022)	-0.0258*** (0.007)			
Daily newspaper circulation (/1000 people)				-0.0054** (0.002)	-0.0107 (0.007)	-0.0057** (0.003)
GDP per capita (log)	0.0069 (0.157)	0.9118** (0.381)	-0.0777 (0.196)	-0.6187*** (0.215)	-0.3153 (0.797)	-0.3228 (0.254)
Population	0.0008 (0.001)	-0.0172 (0.013)	0.0014 (0.001)	-0.0001 (0.001)	-0.1226** (0.050)	-0.0005 (0.001)
Trade openness	0.0018 (0.003)	0.0035 (0.005)	-0.0030 (0.004)	0.0053* (0.003)	0.0076 (0.008)	-0.0030 (0.004)
Democracy dummy	-0.0124 (0.278)	-1.922*** (0.571)	0.3906 (0.340)	0.3633 (0.323)	0.9843 (1.334)	0.5127 (0.374)
Financial openness	0.0137 (0.096)	-0.3654 (0.259)	0.1260 (0.115)	-0.0902 (0.109)	-0.4068 (0.368)	-0.1969 (0.130)
Constant	0.5065 (1.250)	-6.282** (2.584)	-0.1695 (1.684)	5.289*** (1.821)	3.0981 (6.645)	3.2836 (2.103)
Number of countries	165	75	143	80	25	60
Years sample	1989-2007	1989-2007	1989-2007	1999-2004	1999-2004	1999-2004
Observations	2,206	610	1,596	333	101	232
Correctly classified	64.51%	86.39%	65.04%	74.47%	90.10%	73.71%

Note: Robust Standard errors are clustered by country and are shown below coefficients. All regressions include year fixed effect.

The dependent variable is equal to 1 if there is a difference between the regimes de jure and de facto and to 0 otherwise. The sample in columns 5.1 and 5.4 is the whole sample. The sample consists of *de facto* floaters only, in columns 5.2 and 5.5. The sample consists of *de facto* fixers only, in columns 5.3 and 5.6

*** significant at 1%, ** significant at 5%, * significant at 10%

Table 6: Alternative definition of the *de facto* exchange rate regime

	Press freedom			Access to information		
	(6.1) Being a dissimulating country	(6.2) Being a dissimulating floater	(6.3) Being a dissimulating fixer	(6.4) Being a dissimulating country	(6.5) Being a dissimulating floater	(6.6) Being a dissimulating fixer
Freedom House Index	-0.0072 (0.010)	-0.0080 (0.015)	-0.0120 (0.017)			
Internet users (%)				-0.0205* (0.011)	-0.0368* (0.020)	-0.0021 (0.019)
GDP per capita (log)	0.1722 (0.171)	0.6674*** (0.248)	-0.2917 (0.312)	0.3396* (0.175)	0.9161*** (0.342)	-0.2526 (0.300)
Population	0.0017*** (0.001)	0.0007 (0.001)	0.0063 (0.013)	0.0017*** (0.001)	0.0004 (0.001)	0.0106 (0.017)
Trade openness	0.0030 (0.004)	0.0061 (0.009)	0.0027 (0.006)	0.0027 (0.004)	0.0070 (0.007)	0.0011 (0.007)
Democracy dummy	-0.4943 (0.355)	-1.225** (0.585)	0.1362 (0.634)	-0.6537** (0.293)	-1.299*** (0.485)	-0.2646 (0.434)
Financial Openness	-0.0867 (0.112)	-0.2355 (0.174)	0.1240 (0.195)	-0.1279 (0.105)	-0.2253 (0.153)	0.0659 (0.193)
Constant	-3.054* (1.751)	-7.206*** (2.607)	1.5085 (2.986)	-3.548** (1.594)	-8.134** (3.249)	0.9755 (2.629)
Number of countries	141	102	111	141	97	112
Years sample	1994-2007	1994-2007	1994-2007	1989-2007	1989-2007	1989-2007
Observations	1,174	610	564	1,356	724	632
Correctly classified	78.28%	80.33%	79.08%	76.7%	78.73%	80.38%

Note: Robust standard errors are clustered by country and are shown below coefficients. All regressions include year fixed effect.

The dependent variable is equal to 1 if there is a difference between the regimes *de jure* and *de facto* and to 0 otherwise. The sample in columns 6.1 and 6.4 is the whole sample. The sample consists of *de facto* floaters only, in columns 6.2 and 6.5. The sample consists of *de facto* fixers only, in columns 6.3 and 6.6

*** significant at 1%, ** significant at 5%, * significant at 10%

Table 7: Multinomial probit model (base outcome: fixed exchange rate regime *de jure* and *de facto*)

	Press freedom			Access to information		
	(7.1) Fixed regime <i>de jure</i> and flexible regime <i>de facto</i> (diss. Fixer)	(7.1) Flexible regime <i>de jure</i> and fixed regime <i>de facto</i> (diss. Floater)	(7.1) Flexible regime <i>de jure</i> and <i>de facto</i> (trans. Floater)	(7.2) Fixed regime <i>de jure</i> and flexible regime <i>de facto</i> (diss. Fixer)	(7.2) Flexible regime <i>de jure</i> and fixed regime <i>de facto</i> (diss. Floater)	(7.2) Flexible regime <i>de jure</i> and <i>de facto</i> (trans. Floater)
Freedom House Index	-0.0182** (0.008)	-0.0029 (0.012)	-0.0050 (0.010)			
Internet users (%)				-0.0198** (0.009)	-0.0164 (0.014)	0.0205*** (0.008)
GDP per capita (log)	-0.2614* (0.147)	0.2552 (0.163)	0.0718 (0.169)	-0.2241 (0.151)	0.1764 (0.197)	-0.1810 (0.181)
Population	0.0009 (0.001)	-0.0048 (0.005)	0.0004 (0.001)	0.0013 (0.001)	-0.0035 (0.005)	0.0007 (0.001)
Trade openness	0.1287 (0.091)	-0.1965 (0.142)	0.0583 (0.101)	0.1006 (0.086)	-0.1781 (0.144)	0.0127 (0.096)
Democracy dummy	0.5974* (0.311)	-0.4099 (0.460)	0.6839* (0.402)	0.1985 (0.261)	-0.3682 (0.316)	0.5051* (0.303)
Financial Openness	-0.0015 (0.003)	-0.0061 (0.004)	-0.0082 (0.006)	-0.0014 (0.003)	-0.0043 (0.004)	-0.0090 (0.006)
Constant	2.9327*** (1.133)	-2.4762* (1.423)	-0.6138 (1.394)	1.2904 (1.389)	-1.9185 (2.013)	0.7506 (1.589)
Number of countries	165	165	165	165	165	165
Years sample	1994-2007	1994-2007	1994-2007	1989-2007	1989-2007	1989-2007
Observations	1,793	1,793	1,793	1,973	1,973	1,973

Note: Robust standard errors are clustered by country and are shown below coefficients. All regressions include year fixed effects. The dependent variable corresponds to the number between brackets described in the table 1. The base outcome is a fixed exchange rate regime *de jure* and *de facto*.

*** significant at 1%, ** significant at 5%, * significant at 10%

Table 8: Multinomial probit model (base outcome: flexible exchange rate regime *de jure* and *de facto*)

	Press freedom			Access to information		
	(8.1) Fixed regime <i>de jure</i> and <i>de facto</i> (trans. Fixer)	(8.1) Fixed regime <i>de jure</i> and flexible regime <i>de facto</i> (diss. Fixer)	(8.1) Flexible regime <i>de jure</i> and fixed regime <i>de facto</i> (diss. Floater)	(8.2) Fixed regime <i>de jure</i> and <i>de facto</i> (trans. Fixer)	(8.2) Fixed regime <i>de jure</i> and flexible regime <i>de facto</i> (diss. Fixer)	(8.2) Flexible regime <i>de jure</i> and fixed regime <i>de facto</i> (diss. Floater)
Freedom House Index	0.0050 (0.010)	-0.0131 (0.009)	0.0022 (0.013)			
Internet users (%)				-0.0205*** (0.008)	-0.0403*** (0.009)	-0.0369*** (0.014)
GDP per capita (log)	-0.0718 (0.169)	-0.3332** (0.153)	0.1834 (0.169)	0.1810 (0.181)	-0.0431 (0.163)	0.3573* (0.197)
Population	-0.0004 (0.001)	0.0005 (0.001)	-0.0053 (0.005)	-0.0007 (0.001)	0.0006 (0.001)	-0.0042 (0.005)
Trade openness	-0.0583 (0.101)	0.0704 (0.103)	-0.2548* (0.153)	-0.0127 (0.096)	0.0879 (0.098)	-0.1908 (0.155)
Democracy dummy	-0.6839* (0.402)	-0.0865 (0.372)	-1.0938** (0.533)	-0.5051* (0.303)	-0.3065 (0.295)	-0.8732** (0.347)
Financial Openness	0.0082 (0.006)	0.0068 (0.005)	0.0021 (0.006)	0.0090 (0.006)	0.0076* (0.005)	0.0048 (0.006)
Constant	0.6138 (1.394)	3.5465*** (1.213)	-1.8624 (1.493)	-0.7506 (1.589)	0.5397 (1.389)	-2.6692 (1.942)
Number of countries	165	165	165	165	165	165
Years sample	1994-2007	1994-2007	1994-2007	1989-2007	1989-2007	1989-2007
Observations	1,793	1,793	1,793	1,973	1,973	1,973

Note: Robust standard errors are clustered by country and are shown below coefficients. All regressions include year fixed effects. The dependent variable corresponds to the number between brackets described in the table 1. The base outcome is a flexible exchange rate regime *de jure* and *de facto*.

*** significant at 1%, ** significant at 5%, * significant at 10%

Table 9: Baseline logit model including episodes of hyperinflation

	Press freedom			Access to information		
	(9.1) Being a dissimulating country	(9.2) Being a dissimulating floater	(9.3) Being a dissimulating fixer	(9.4) Being a dissimulating country	(9.5) Being a dissimulating floater	(9.6) Being a dissimulating fixer
Freedom House Index	-0.0149* (0.009)	0.0020 (0.018)	-0.0258** (0.011)			
Internet users (%)				-0.0385*** (0.010)	-0.0534** (0.024)	-0.0306** (0.013)
GDP per capita (log)	-0.3010** (0.145)	0.5044* (0.276)	-0.3630* (0.197)	-0.1066 (0.150)	0.6872* (0.376)	-0.2610 (0.198)
Population	0.0009 (0.001)	-0.0168 (0.014)	0.0009 (0.001)	0.0011 (0.001)	-0.0146 (0.009)	0.0012 (0.001)
Trade openness	0.0013 (0.003)	-0.0023 (0.006)	-0.0016 (0.004)	0.0022 (0.003)	0.0023 (0.005)	-0.0026 (0.004)
Democracy dummy	0.1957 (0.346)	-1.751** (0.723)	0.8454* (0.434)	-0.0914 (0.263)	-1.571*** (0.528)	0.2557 (0.331)
Financial Openness	0.0873 (0.103)	-0.4198 (0.272)	0.1650 (0.116)	0.0867 (0.093)	-0.2760 (0.247)	0.1458 (0.113)
Constant	1.1502 (1.464)	-4.1659 (2.710)	1.2817 (1.937)	0.1762 (1.413)	-5.805* (3.482)	1.9415 (1.840)
Number of countries	165	74	139	165	90	143
Years sample	1995-2007	1995-2007	1995-2007	1989-2007	1989-2007	1989-2007
Observations	1,788	559	1,332	2,091	650	1,441
Correctly classified	66.72%	87.84%	64.41%	67.58%	86.15%	63.15%

Note: Robust standard errors are clustered by country and are shown below coefficients. All regressions include year fixed effect.

The dependent variable is equal to 1 if there is a difference between the regimes de jure and de facto and to 0 otherwise. The sample in columns 9.1 and 9.4 is the whole sample. The sample consists of *de facto* floaters only, in columns 9.2 and 9.5. The sample consists of *de facto* fixers only, in columns 9.3 and 9.6

*** significant at 1%, ** significant at 5%, * significant at 10%

Table 10: Baseline logit model with bootstrapped standard errors

	Press freedom			Access to information		
	(10.1) Being a dissimulating country	(10.2) Being a dissimulating floater	(10.3) Being a dissimulating fixer	(10.4) Being a dissimulating country	(10.5) Being a dissimulating floater	(10.6) Being a dissimulating fixer
Freedom House Index	-0.0181** (0.008)	-0.0124 (0.029)	-0.0258** (0.012)			
Internet users (%)				-0.0377*** (0.010)	-0.0753** (0.036)	-0.0306** (0.014)
GDP per capita (log)	-0.2936* (0.165)	0.6561 (0.689)	-0.3630* (0.208)	-0.1344 (0.170)	0.7514 (0.458)	-0.2610 (0.218)
Population	0.0007 (0.002)	-0.0280 (0.044)	0.0009 (0.012)	0.0010 (0.002)	-0.0159 (0.029)	0.0012 (0.013)
Trade openness	0.0015 (0.003)	-0.0021 (0.016)	-0.0016 (0.006)	0.0018 (0.004)	0.0056 (0.008)	-0.0026 (0.005)
Democracy dummy	0.2550 (0.351)	-1.7852 (1.327)	0.8454* (0.466)	-0.0741 (0.253)	-1.737** (0.871)	0.2557 (0.353)
Financial Openness	0.0796 (0.134)	-0.4722 (0.467)	0.1650 (0.134)	0.0649 (0.089)	-0.2869 (0.312)	0.1458 (0.123)
Constant	2.293* (1.238)	-5.1325 (4.323)	3.442** (1.517)	0.9479 (1.340)	-6.2297 (3.963)	2.3299 (1.904)
Number of countries	165	62	139	165	75	143
Years sample	1994-2007	1994-2007	1994-2007	1989-2007	1989-2007	1989-2007
Observations	1,793	461	1,332	1,973	532	1,441
Correctly classified	65.64%	89.37%	64.41%	66.04%	85.53%	63.15%

Note: Robust standard errors are estimated using 50 bootstrap replications. They are clustered by country and are shown below coefficients. All regressions include year fixed effect.

The dependent variable is equal to 1 if there is a difference between the regimes de jure and de facto and to 0 otherwise. The sample in columns 10.1 and 10.4 is the whole sample. The sample consists of *de facto* floaters only, in columns 10.2 and 10.5. The sample consists of *de facto* fixers only, in columns 2.3 and 2.6

*** significant at 1%, ** significant at 5%, * significant at 10%

Table 11: Baseline logit model with lagged press freedom and access to information

	Press freedom			Access to information		
	(11.1) Being a dissimulating country	(11.2) Being a dissimulating floater	(11.3) Being a dissimulating fixer	(11.4) Being a dissimulating country	(11.5) Being a dissimulating floater	(11.6) Being a dissimulating fixer
Freedom House Index	-0.0163* (0.009)	-0.0046 (0.019)	-0.0266** (0.010)			
Internet users (%)				-0.0418*** (0.012)	-0.0963** (0.046)	-0.0340** (0.014)
GDP per capita (log)	-0.3279** (0.151)	0.5908** (0.301)	-0.3967** (0.199)	-0.1732 (0.158)	0.7147* (0.399)	-0.2937 (0.198)
Population	0.0007 (0.001)	-0.0298 (0.022)	0.0008 (0.001)	0.0009 (0.001)	-0.0162 (0.012)	0.0012 (0.001)
Trade openness	0.0014 (0.003)	-0.0021 (0.005)	-0.0015 (0.004)	0.0018 (0.003)	0.0067 (0.006)	-0.0024 (0.004)
Democracy dummy	0.2079 (0.347)	-1.950** (0.773)	0.8702** (0.431)	-0.0853 (0.277)	-1.745*** (0.610)	0.2582 (0.333)
Financial Openness	0.0772 (0.105)	-0.5102 (0.313)	0.1688 (0.118)	0.0711 (0.098)	-0.2623 (0.275)	0.1454 (0.114)
Constant	3.126*** (1.184)	-4.469* (2.379)	4.308*** (1.576)	0.5734 (1.698)	-5.1641 (3.910)	2.0501 (1.840)
Number of countries	164	61	138	165	73	142
Years sample	1995-2007	1995-2007	1995-2007	1990-2007	1990-2007	1990-2007
Observations	1,707	439	1,268	1,890	506	1,378
Correctly classified	65.38%	89.75%	65.14%	66.24%	86.36%	64.37%

Note: Robust standard errors are clustered by country and are shown below coefficients. All regressions include year fixed effect.

The dependent variable is equal to 1 if there is a difference between the regimes de jure and de facto and to 0 otherwise. The sample in columns 11.1 and 11.4 is the whole sample. The sample consists of *de facto* floaters only, in columns 11.2 and 11.5. The sample consists of *de facto* fixers only, in columns 2.3 and 2.6

*** significant at 1%, ** significant at 5%, * significant at 10%

Table 12: Instrumental variable regressions (2SLS estimators)

	Press Freedom		Access to information	
	Being a dissimulating country (12.1)	Being a dissimulating country (12.1)	Being a dissimulating country (12.1)	Being a dissimulating country (12.1)
Freedom House Index	-0.0069*			
	(0.004)			
RWB index		-0.0129**		
		(0.006)		
Internet users (%)			-0.0218*	
			(0.013)	
Mobile subscriptions (/100 people)				0.0435
				(0.065)
GDP per capita (log)	-0.0966**	-0.1225***	0.1333	-1.3343
	(0.042)	(0.044)	(0.214)	(1.674)
Population	0.0000	-0.0002	0.0001	0.0005
	(0.000)	(0.000)	(0.000)	(0.001)
Trade openness	-0.0006	-0.0004	-0.0002	-0.0021
	(0.001)	(0.001)	(0.001)	(0.003)
Democracy dummy	0.0213	0.0462	0.1213**	-0.1510
	(0.025)	(0.033)	(0.060)	(0.251)
Financial Openness	0.0558	0.0805	0.1088	-0.2848
	(0.138)	(0.131)	(0.146)	(0.507)
Constant	1.564***	2.423***	-0.4720	10.4509
	(0.301)	(0.437)	(1.676)	(12.228)
Year considered	2005	2005	2005	2005
Observations	157	139	88	85
R-squared	0.42	0.41	0.17	-1.49
Anderson can. corr. test (p-value)	0.0000	0.0000	0.0035	0.4116
Cragg-Donald F-Stat (rel. biais)	< 5%	< 10%	< 15%	> 30%
Sargan- Hansen test (p-value)	0.3057	0.4013	.	.
Durbin-Wu-Hausman test (p-value)	0.6073	0.1819	0.0626	0.1599

Note: Robust standard errors are shown below coefficients. All regressions include year fixed effect.

The dependent variable is equal to 1 if there is a difference between the regimes de jure and de facto and to 0 otherwise. The shares of Catholics, Muslims, and Protestant in the population are used as instruments for the regressions 13.1 and 13.2. The share of English speakers in the population is used as instrument for the regressions 13.3 and 13.4. *** significant at 1%, ** significant at 5%, * significant at 10%

Table 13: Baseline logit model controlling for emerging countries.

	Press freedom			Access to information		
	(13.1) Being a dissimulating country	(13.2) Being a dissimulating floater	(13.3) Being a dissimulating fixer	(13.4) Being a dissimulating country	(13.5) Being a dissimulating floater	(13.6) Being a dissimulating fixer
Freedom House Index	-0.0180** (0.009)	-0.0032 (0.018)	-0.0253** (0.011)			
Internet users (%)				-0.0380*** (0.011)	-0.0645*** (0.023)	-0.0342*** (0.013)
GDP per capita (log)	-0.2995* (0.153)	0.8425*** (0.303)	-0.4148** (0.202)	-0.1492 (0.159)	1.1443*** (0.396)	-0.2863 (0.201)
Population	0.0007 (0.001)	-0.0133 (0.013)	-0.0004 (0.001)	0.0007 (0.001)	-0.0117 (0.008)	-0.0002 (0.001)
Trade openness	0.0015 (0.003)	-0.0024 (0.005)	-0.0015 (0.004)	0.0020 (0.003)	0.0015 (0.004)	-0.0024 (0.004)
Democracy dummy	0.2525 (0.349)	-1.7303*** (0.782)	0.9162** (0.453)	-0.0720 (0.275)	-1.6442*** (0.549)	0.3774 (0.337)
Financial Openness	0.0818 (0.106)	-0.7161** (0.308)	0.1647 (0.118)	0.0730 (0.099)	-0.5964** (0.259)	0.1486 (0.116)
Emergent Countries	0.0815 (0.388)	-2.7089** (1.247)	1.4175** (0.566)	0.2326 (0.378)	-2.4236*** (0.844)	1.5702*** (0.564)
Constant	2.9131** (1.170)	-6.1974*** (2.344)	4.2684*** (1.600)	0.6648 (1.478)	-8.0215** (3.586)	1.3394 (1.850)
Number of countries	165	62	139	165	75	165
Years sample	1994-2007	1994-2007	1994-2007	1989-2007	1989-2007	1989-2007
Observations	1,793	461	1,332	1,973	532	1,441
Correctly classified	65.59%	90.02%	66.07%	66.04%	85.53%	63.15%

Note: Robust standard errors are clustered by country and are shown below coefficients. All regressions include year fixed effect.

The dependent variable is equal to 1 if there is a difference between the regimes de jure and de facto and to 0 otherwise. The sample in columns 12.1 and 12.4 is the whole sample. The sample consists of *de facto* floaters only, in columns 13.2 and 13.5. The sample consists of *de facto* fixers only, in columns 2.3 and 2.6

*** significant at 1%, ** significant at 5%, * significant at 10%

Figures:

Figure 1: Density function of the marginal effect (Freedom House index)

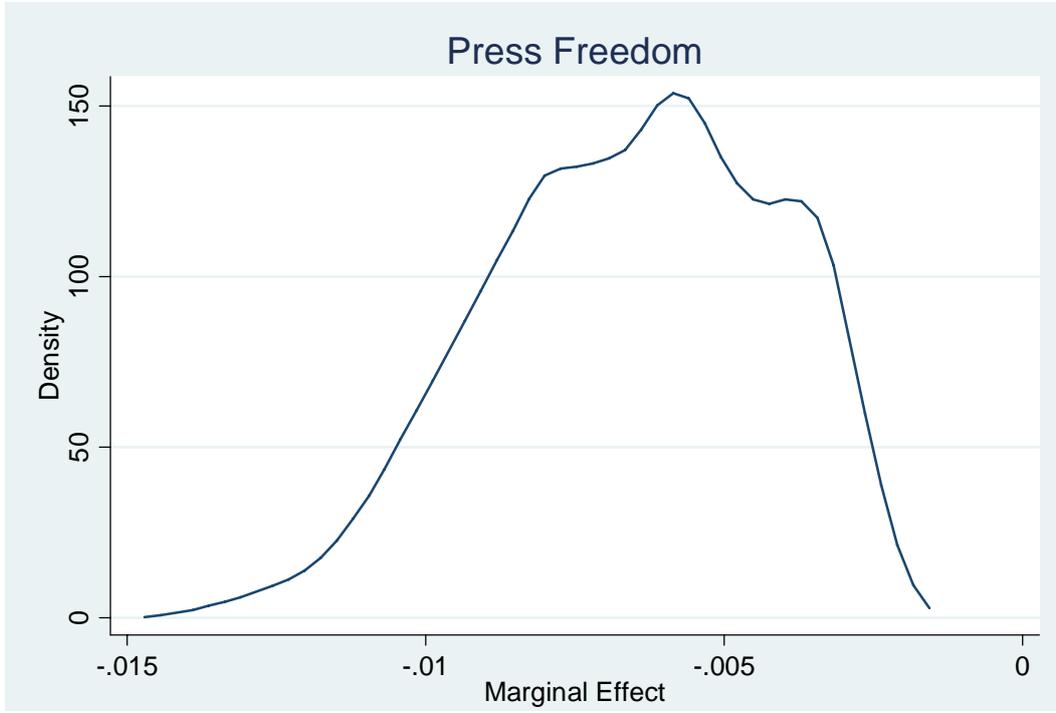


Figure 2: Density function of the marginal effect (% of Internet users)

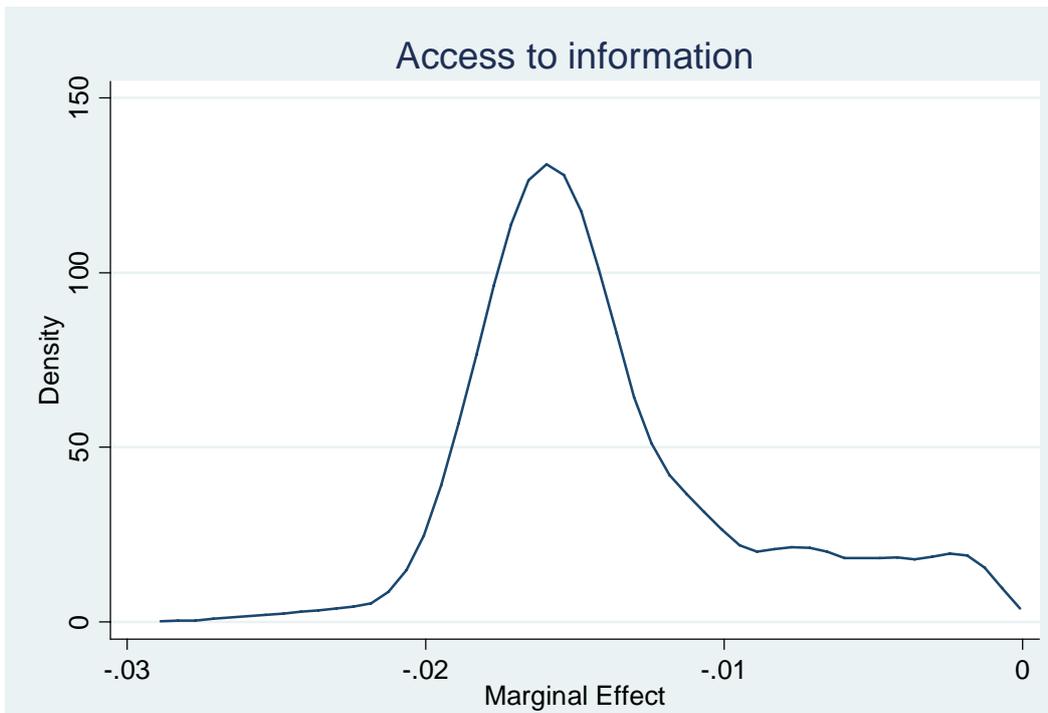


Figure 3: Predicted probability of dissimulating one's exchange rate regime vs. the press freedom index (model 2.1).

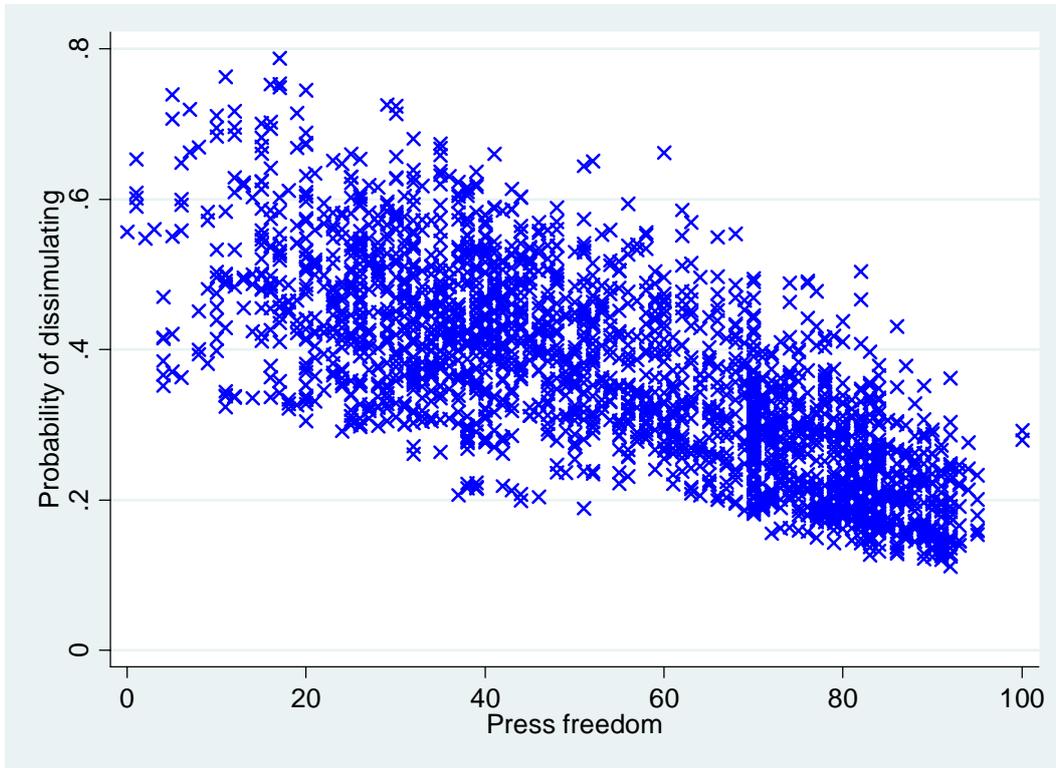


Figure 4: Predicted probability of dissimulating one's exchange rate regime according to press freedom deciles.

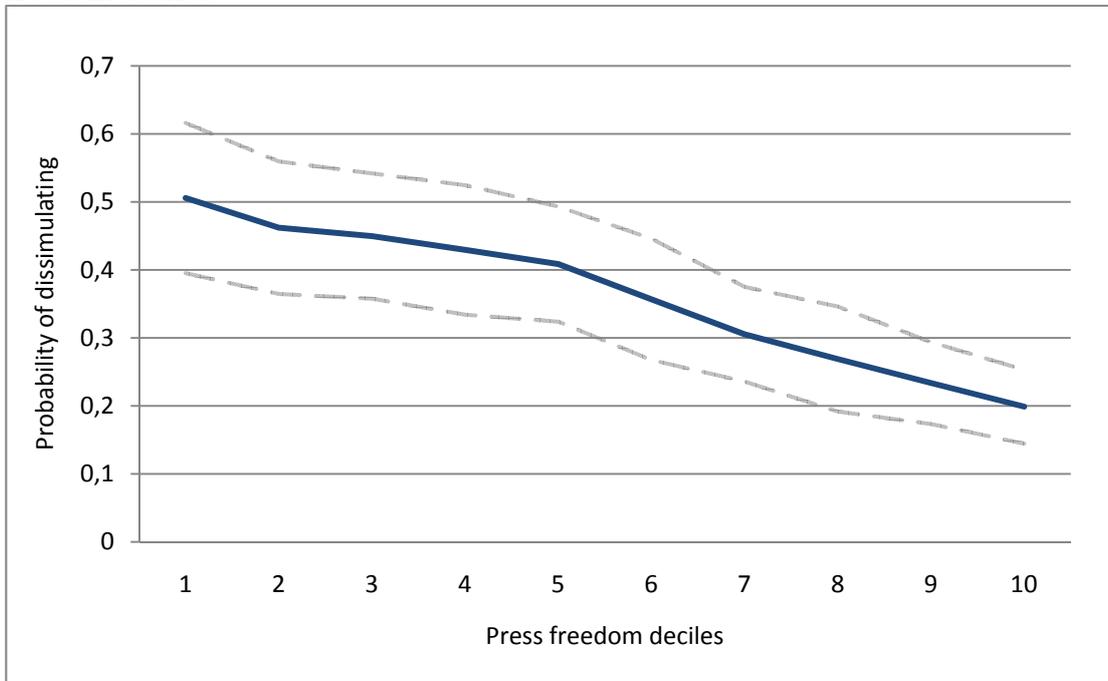


Figure 5: Estimated probability of dissimulating one's exchange rate regime vs. Internet access (%) (model 2.4)

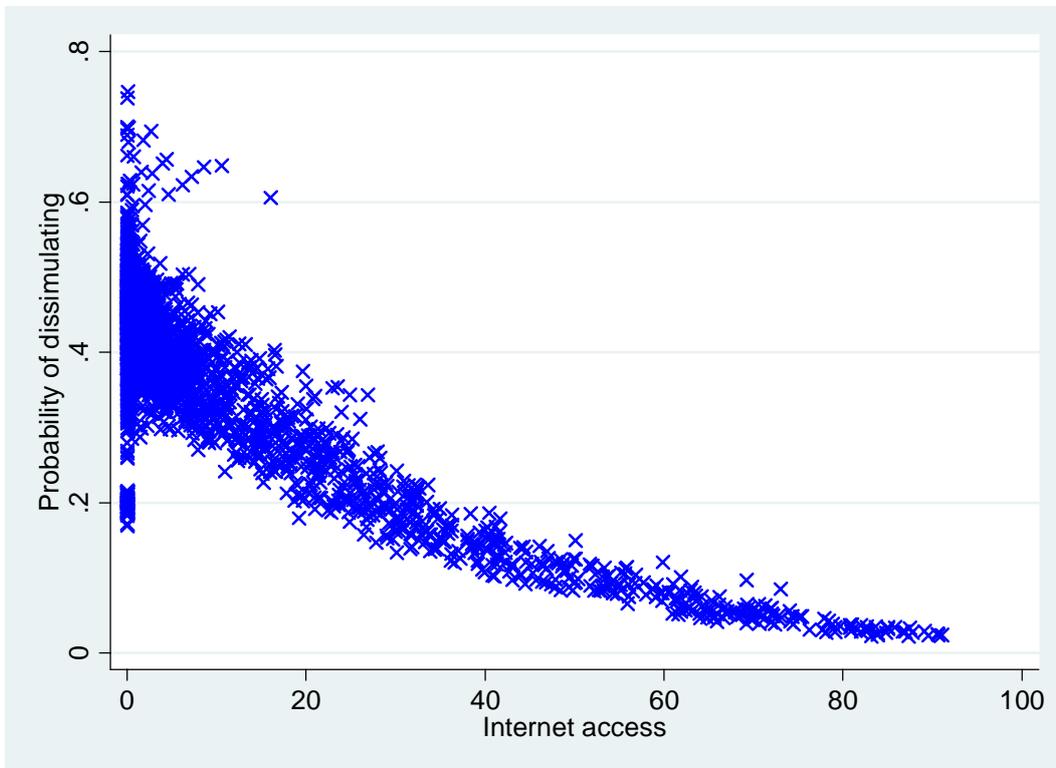


Figure 6: Predicted probability of dissimulating one's exchange rate regime according to Internet access deciles.

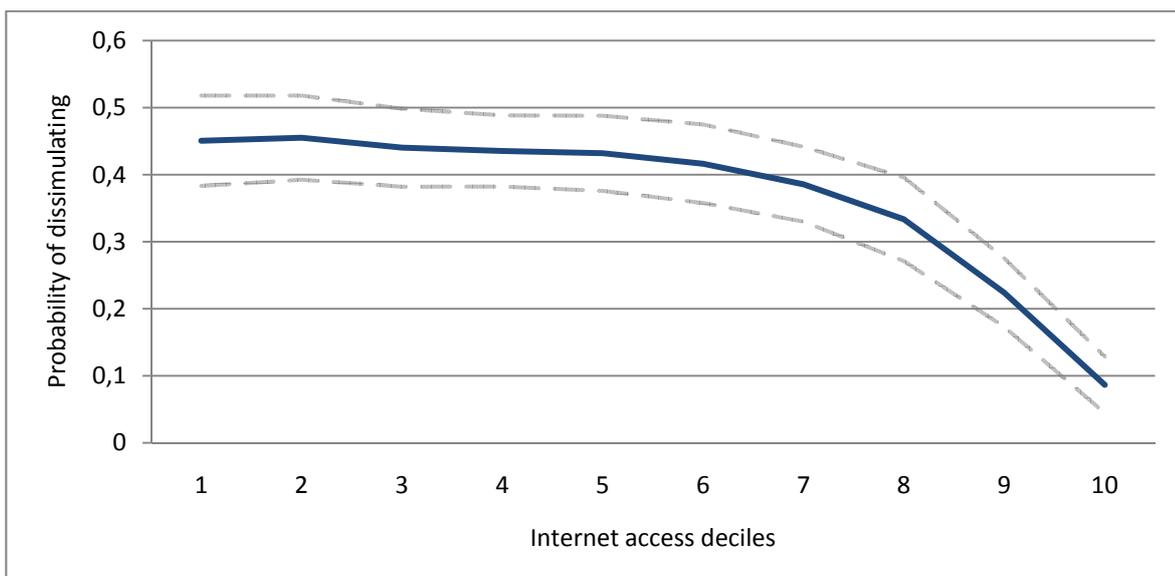
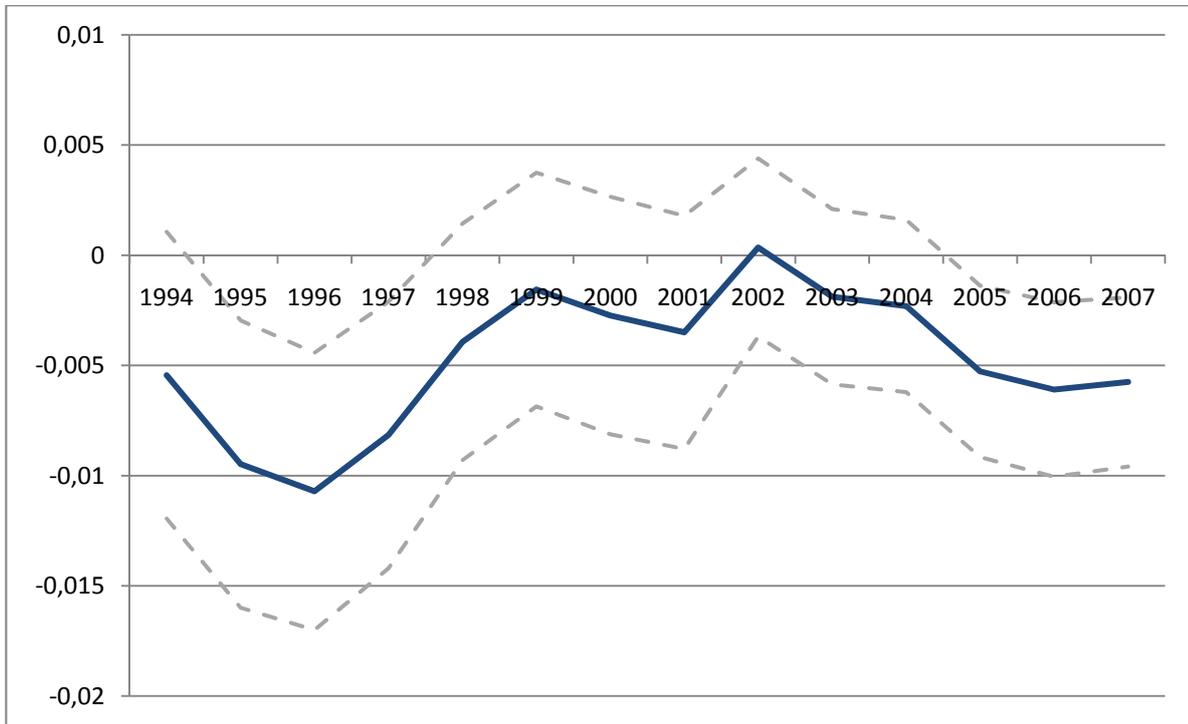
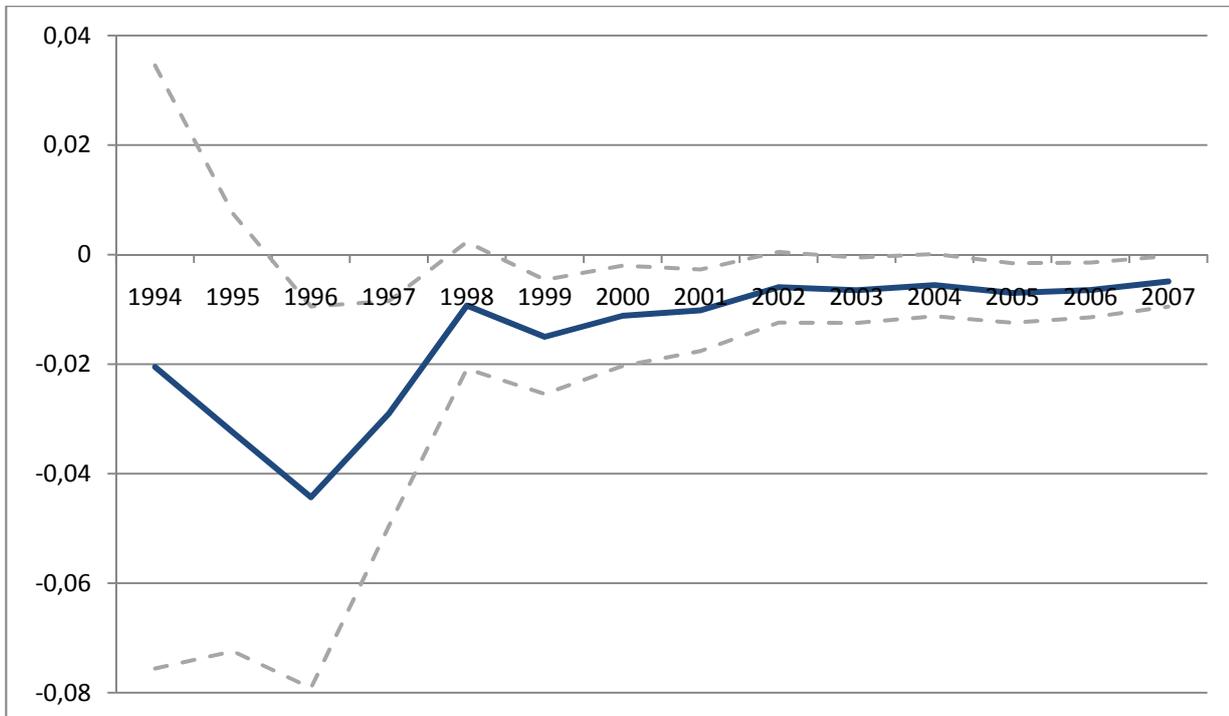


Figure 7: Marginal effect of the Freedom House index in the different cross sections (based on model 2.1)



Note: Confidence intervals at the 90% level

Figure 8: Marginal effect of Internet Users (%) in the different cross sections (based on model 2.4)



Note: Confidence intervals at the 90% level.

Table A1: Descriptive statistics

Variables	Obs.	Mean	Std. Dev.	Min	Max
Dependent variables					
Reinhart –Rogoff (RR) classification (0 = peg, 1 = floating regime)	4865	0.25	0.43	0.00	1.00
IMF classification (0 = peg, 1 = floating regime)	4274	0.36	0.48	0.00	1.00
Shambaugh classification (SH) (0 = peg, 1 = floating regime)	4864	0.47	0.50	0.00	1.00
Dissimulating countries (RR)	4156	0.34	0.47	0.00	1.00
Dissimulating countries (S)	3112	0.28	0.45	0.00	1.00
Dissimulating floaters (RR)	1164	0.46	0.50	0.00	1.00
Dissimulating fixers (RR)	2992	0.29	0.45	0.00	1.00
Dissimulating floaters (S)	1576	0.42	0.49	0.00	1.00
Dissimulating fixers (S)	1536	0.14	0.35	0.00	1.00
Press freedom variables					
Freedom House Index	2804	54.13	24.53	0.00	100.00
RWB index	1293	75.80	20.79	0.00	100.00
Killed journalists	3591	-0.23	1.46	-33.00	0.00
Access to information variables					
Internet users (%)	3410	12.57	20.13	0.00	93.46
Mobile subscriptions (/100 people)	6909	10.72	26.24	0.00	208.68
Daily newspaper circulation (/1000 people)	416	143.71	147.04	0.00	580.79
Control variables					
GDP per capita (log)	6631	8.55	1.16	5.03	11.62
Population (in millions)	7220	32.65	130.95	0.01	1321.85
Trade openness	6631	79.59	50.21	1.09	456.56
Democracy dummy	9115	0.44	0.50	0.00	1.00
Financial openness	5705	0.00	1.52	-1.83	2.50
Instrumental variables					
Share of Catholics	207	32.85	36.25	0.00	99.10
Share of Protestants	204	14.77	23.43	0.00	99.80
Share of Muslims	207	21.43	35.06	0.00	99.99

Table A2: Pair wise correlation matrix

Variables	(a)	(b)	(c)	(d)	(e)	(f)	(g)	(h)	(i)	(j)	(k)	(l)	(m)	(n)	(o)	(p)	(q)	(r)	(s)	(t)
(a) RR	1.00																			
(b) IMF	0.24	1.00																		
(c) SH	0.37	0.46	1.00																	
(d) Dissimulating RR	0.16	0.38	0.25	1.00																
(e) Dissimulating SH	0.08	-0.15	0.31	0.22	1.00															
(f) Freedom House	0.06	-0.03	0.02	-0.21	-0.14	1.00														
(g) RBW index	0.13	-0.05	0.02	-0.25	-0.21	0.83	1.00													
(h) Killed Journalists	-0.05	-0.05	-0.07	-0.09	-0.03	0.11	0.17	1.00												
(i) Internet users	0.13	-0.03	-0.01	-0.25	-0.12	0.44	0.46	0.07	1.00											
(j) Mobile subscriptions	0.02	0.03	-0.02	-0.18	-0.11	0.33	0.45	0.05	0.86	1.00										
(k) Newspaper circulation	0.36	0.05	0.08	-0.37	-0.18	0.55	0.39	0.09	0.82	0.73	1.00									
(l) GDP per capita (log)	0.04	0.07	0.05	-0.06	0.02	0.47	0.34	0.06	0.54	0.41	0.72	1.00								
(m) Population	0.06	0.16	0.10	0.08	0.09	-0.08	-0.21	-0.06	-0.03	-0.01	-0.07	-0.08	1.00							
(n) Trade openness	-0.12	-0.15	-0.24	-0.07	0.00	0.08	0.02	0.04	0.22	0.24	0.14	0.22	-0.20	1.00						
(o) Democracy dummy	0.00	0.22	0.13	-0.02	-0.08	0.70	0.60	0.06	0.29	0.25	0.42	0.41	0.02	0.01	1.00					
(p) Financial openness	-0.07	0.13	-0.04	-0.12	-0.11	0.38	0.48	0.06	0.44	0.38	0.56	0.50	-0.06	0.22	0.23	1.00				
(q) Share of Catholics	-0.04	-0.10	.	-0.18	.	0.34	0.35	0.06	0.12	0.15	.	0.15	-0.10	0.00	0.39	0.23	1.00			
(r) Share of Protestants	0.18	-0.10	.	-0.16	.	0.47	0.37	0.09	0.41	0.20	.	0.16	-0.09	0.01	0.16	0.08	-0.16	1.00		
(s) Share of Muslims	-0.12	0.02	.	0.19	.	-0.53	-0.42	-0.18	-0.33	-0.22	.	-0.22	-0.02	-0.08	-0.46	-0.19	-0.49	-0.34	1.00	
(t) Emergent countries	0.18	0.21	0.20	0.10	0.03	0.02	-0.03	-0.11	-0.01	0.03	-0.15	0.05	0.38	-0.20	0.00	-0.09	0.03	-0.14	-0.04	1.00