DO COUNTRIES FALSIFY ECONOMIC DATA STRATEGICALLY? SOME EVIDENCE THAT THEY MIGHT

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Abstract—Using Benford's law, we find evidence supporting the hypothesis that countries at times misreport their economic data strategically. We group countries with similar economic conditions and find that for countries with fixed exchange rate regimes, high negative net foreign asset positions, negative current account balances, or more vulnerable to capital flow reversals, we reject the first-digit law for the balance-of-payments data. This corroborates the intuition of a simple economic model. The main results do not seem to be driven by countries in sub-Saharan Africa or those with low institutional quality ratings.

I. Introduction

CONVENTIONAL wisdom tells us that governments may lie strategically to the public about economic data that they collect and later disseminate. The accusations that Greece and Italy tinkered with their budget deficit figures before joining the euro zone provide anecdotal examples. Argentina has been suspected of understating inflation figures since mid–2007. The Hungarian government, according to its prime minister in a statement that leaked out, lied to the general public about the state of the economy to win the elections in 2006. Ukraine misreported its level of reserves to the International Monetary Fund (IMF) between 1996 and 1998. China is believed to embellish its GDP growth numbers. Even the United States came under scrutiny after GDP growth revisions were consistently negative in the crisis that started in 2008.[1] At times governments are caught red-handed (as Hungary and Ukraine were), but most of the time, it is simply unclear whether the data provided to the public are just inaccurate (because of, say, measurement errors or bad data collection methods) or deliberately altered. In some cases, misinforming economic agents may bring tangible (possibly short-term) gains for a government; for example, Argentina by misstating inflation figures avoided paying out higher interest on government bonds indexed to inflation (which should have constituted in fact a partial default) and raising the wages in the public sector. Greece enjoyed lower borrowing rates (close to Germany's) on its government debt because of its euro zone membership and because investors did not know the entire extent of Greek budget troubles. Given these examples, there are two important questions to ask. Is it possible to find instances when countries or groups of countries engage in misinforming economic agents? Is there some common characteristic that these countries share?

In this paper we use a statistical test based on the distribution of first digits of economic accounting data (also known as Benford's law) to test whether countries falsify the economic figures that they report. Benford's distribution of the first digits arises naturally for many different types of data. This is true for processes with exponential growth or products of independent random variables, that is, in applications for many economic data due to inflation, economic, or population growth in the presence of stochastic shocks. This property is preserved under multiplication by a common factor, so will survive conversions into different currencies. Benford's distribution arises when data from many different economic processes (that is, from many countries) are considered together. Deviations from Benford's distribution may flag data reporting irregularities.

A. *Our Data Set*

In our tests, we use quarterly balance-of-payments (BoP) data from the IMF for years 1989 to 2007, for several reasons.[2] First, these accounting data are available for many countries from the same source. Second, there were instances where countries were either caught (Ukraine) or suspected (Russia, The Gambia, China, Chad, Sierra Leone, Egypt, and others) of misreporting this particular kind of datum. Next, the first digits of each BoP entry should conform with the Benford's law, and the more so when data from multiple countries are considered (when heterogeneity is introduced). Moreover, the data are somewhat standardized (among countries and across time) as they should be prepared by providers according to an IMF manual. They offer a lot of data points relative to other data on economic fundamentals. Also, they are not transformed many times (and if the balance is recorded in U.S. dollars, it is not transformed at all), something that could worsen their statistical properties.[3] Moreover, because the data are quarterly, they include series that are closely watched by investors so that countries may have an incentive to misrepresent them. One would wish naturally to extend

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[1] For Greece's actions before joining the euro zone, see Miller (2004). For a more recent account of releasing dodgy budget statistics, see Barber and Hope (2010). For Italy, see Bream, Blitz, and Norman (2001). Argentina's story is described, for example, in Webber (2008) and O'Grady (2008). The Hungarian case is treated in Wagstyl (2007). Ukraine's history is described in IMF (2000). For accounts for China and the United States, see McSherry (2009).

[2] Section IIIA details why some other statistical series of interest were not chosen for scrutiny.

[3] Nye and Moul (2007) found that economic series transformed many times with imprecise procedures (for example, relying on inaccurate exchange rates) may not obey Benford's law. They advise working with the raw, nominal data.

our tests to other types of macroeconomic data, but the size of the task just for the series at hand is already substantial enough.

B. Detailed Methodology

We first advance theoretical arguments and review the literature on why the economic processes of interest should adhere to Benford's law. Then we show that for our entire data set, we cannot reject a Benford's distribution of the first digits. Before putting our data to a closer test, we develop a simple model (taking into account the characteristics of our data set) that gives insight into why and when countries would strategically misinform investors (economic agents) on the series we study. Our story is simple: a country may want to hide its true state of the world to prevent capital outflows or attract inflows. Next, we group countries in different relevant categories indicated by our model and investigate whether we can reject the hypothesis that the distribution of the first digits of data that they provide complies with Benford's distribution, which would be consistent with data manipulation.

C. Results

Using BoP data, we find evidence that countries with fixed exchange rate regimes, those with high negative net foreign asset positions or negative current account balances, have economic data that have irregular, non-Benford behavior of the first digits. We reject Benford's law for series from many groups of countries that would be more vulnerable to capital flow reversals (those with fixed exchange rates and relatively high capital openness; countries with both current and fiscal deficits). Euro-adopting countries have unusual distributions of first digits in the reported accounts after they join the euro zone. Our main results do not seem to be driven by including countries in sub-Saharan Africa or those with low institutional quality ratings in our samples.

D. Interpretation

Because the rejections of the Benford distribution for the first digits of data occur for specific groups only and not for their complements in the data set (for fixed exchange rate regimes but not floating ones, or countries with low net foreign assets but not those with high net foreign assets), our results are consistent with the hypothesis that vulnerable countries, as indicated by our model, strategically manipulate the data. These rejections are rather category than country specific, as data from many countries for different quarters enter different categories. For example, if a country changes its exchange rate regime or the sign of the current account within the range of our sample, its data will enter different categories for different quarters.[4]

These rejections of Benford's law should not be driven by some perverse statistical features of the data. We correct for possible persistence or statistical breaks in the data by developing a specific randomization procedure and develop its critical test values by simulations. We want our rejections of the null hypothesis to be corroborated by both traditional P-values of chi-square tests, but also with our developed tests based on selecting a fraction of the data at random that we call 1-in-5, 1-in-10, or 1-in-20 criteria. We group many countries sharing similar economic characteristics together so that data heterogeneity and shorter time series for more countries in each group are typically considered than in, for example, a purely geographical division of countries (we want to have at least 1,100 observations so as to be able to use the 1-in-10 criterion). This should allow a better adequation to Benford's law for finite samples (see section IVA).[5] The rejections for some categories (for example, among fixed versus floating division) exist for groups comprising a larger number of countries than the nonvulnerable groups, again favoring the strategic argument.

We doubt that the rejection of Benford's distribution for the first digits of the data for many groups of vulnerable countries according to our model is due to just poor data collection and noise in the data in those countries. Large positions in the errors and omissions section of the BoP data may indicate, of course, that the data collection process in a given country is of low quality or that there is a great amount of illicit or unrecorded transactions, but if there are no manipulations, the first digits of the series (even when incompletely registering transactions) should still adhere to Benford's Law (see section C.2.1 in our 2011 working paper, published as an online supplement, which we refer to as MSWP). When we look at the tests based on the less persistent series, there is little evidence that countries with poor institutions have unexpected first-digit distributions. If the rejection of Benford's distribution were caused by poor data collection, the first-digit distribution of the less persistent series should be also affected. Moreover, robustness checks show that excluding countries in sub-Saharan Africa or those with low institutional quality ratings as measured by World Bank World Governance Indicators (WGI), such as government effectiveness or rule of law, does not change our main conclusions (see section VF).[6] Observing various vulnerable categories, we obtain stronger results for countries with more open capital markets, which tend to be countries with better data collection methods as well. Also, because we find evidence for countries that would appear (at least in the short run, according to our model) to benefit from misinforming investors, we do not think that the data suffer from rounding errors; if that were

[4] We do not claim that necessarily all countries belonging to a particular group are responsible for a discrepancy; this may be driven by a few countries only. We control whether the exclusion of one country at a time changes our results.

[5] For example, the subsets of countries that have fixed exchange rate regimes contain many countries (49 countries with 26.6 quarters on average for the stable sample and 84 countries with 32.6 quarters on average for the full sample), so we believe, based on section IVD, that neither persistence nor structural breaks are driving our results.

[6] Young (2009) questions the quality of growth or price statistics provided by official sources in sub-Saharan Africa. We exclude such countries to show that they do not drive our results.

so, we would find rejections of the null hypothesis across the board. The irregularities may not be caused merely by regular and expected interventions of authorities; for example, if we exclude the item of reserves, which is directly affected by central banks, we still draw the same conclusions for the first digit distributions of countries grouped by fixed and floating exchange regimes.

Relying on our evidence, which supports our economic model, we conclude that countries at times may falsify economic data strategically when it suits their interest. This calls for the inclusion of strategic information provision by governments in models where public signals are important.

E. *Caveats on the Practicality of Our Method*

Unfortunately, due to data limitations of macroeconomic series (too few points per each series), our methods do not allow us to detect which country at what point provided false information. We do not know how grave the possible infractions were. Moreover, we cannot exclude that some of the observed effects would be just price tag effects, though the latter are likely to appear in nominal currency rather than in U.S. dollars. For example, a country might report instead of a deficit on the trade balance of 2.01 billion in a local currency one that is slightly lower, say, at 1.99 billion. Our task is complicated by the fact that countries may not cheat all of the time (serial cheaters might be found out quickly); detecting fraud in one or two quarters when many quarters of the data are present is then unlikely. Out of necessity, we are also working with data that are sometimes revised and sent to the IMF several quarters after initial revelation. The tests also cannot detect some instances of cheating—for example, if a country changes all its items by a common factor or if it changes the numbers preserving the Benford's first-digit distribution. It is also hard to envisage which digit would be favored in any potential infractions; this is because investors may possess some information of their own and the country may not stray too far from either past values or investor expectations in order to appear credible in its announcements.

F. *Literature Review*

The tests we employ are used by accounting practitioners to detect fraud in company accounts (see Nigrini, 1999, for examples). Varian (1972) suggested using Benford's law to test the naturalness of the data and adequacy of the numbers produced by forecasting models. These tests were not widely used in international (macroeconomic) comparisons; there is typically a lack of enough suitable data to boost the statistical power of such tests that render these methods impractical, and until recently (for example, at the time of writing of Varian, 1972) the theoretical processes generating this distribution were not well understood.

To our knowledge, two studies use Benford's distribution to inspect macroeconomic data so far. Nye and Moul (2007) provide evidence (and also simulations of economic quantities) that widely used international macroeconomic data such as the GDP series in the Penn World Tables exhibit first digits that in general conform to Benford's distribution. The data, however, do not conform to Benford's law for non-OECD countries, posing questions about data quality; the alteration of data might come from the source (falsification or simply from an inefficient data collection infrastructure in the considered countries) or inaccurate data transformations. Gonzalez-Garcia and Pastor (2009) follow a similar exercise and extend the analysis to annual IMF data. Their main conclusions are threefold. First, they find that most macroeconomic series they study follow Benford's law. Second, after observing some series for individual countries (GDP for Finland and Japan), they claim that structural breaks in the data may lead to a failure of Benford's law to hold in practice (as they work with a small number of observations) for countries that according to the IMF possess high-quality data.[7] Third, and most interesting, they find no convincing link between independent assessments of data quality collection methods in country groups and rejections of Benford's law for the first digits of data, which may be consistent with our story. None of these papers, however, provides a model as to why countries would misreport data and do so strategically. The focus of these two papers is about assessing the quality of data used in empirical work. In these two studies, countries are grouped based on geographical criteria, which in fact may not be useful (Why should all African countries be lumped together?), whereas we use groupings based on relevant economic criteria, which allows drawing some conclusions on category-specific behavior. Our groupings also should have more data heterogeneity because we consider shorter time series for more countries in each group rather than in a purely geographical division of countries. This should allow better adequation to Benford's law for finite samples (see section IVA). It also appears that the indicated references do not correct for the possible persistence or structural breaks of the economic series, which could lead to false rejections of the conformity of the realized first-digit distribution with Benford's law, while we address this issue; Gonzalez-Garcia and Pastor (2009) notice only the possibility of structural breaks for single series. We sidestep these issues by aggregating data across many countries and using a procedure to select the series at different dates to be tested randomly (see section IVD for details).

From the existing literature, it is clear that the public signal is important for economic agents. The research on countries' truthfulness in information provision, however, is meager to date. The problem is that economic agents cannot readily verify whether the information issued by a government is

[7] This qualification is important; the presence of structural breaks in the data may be due to changes in data collection methodology and a way to blur the measurement of some economic quantities. The discussions about the accuracy of measures of unemployment or the budget deficit under different methodologies are perennial.

of low quality (that is, imprecise) or whether the government in question is supplying deliberately false information.[8] Each government or statistical authority has some leeway in providing the various numbers describing the state of the economy as long as these are not too far off from the public expectations and as long as these subsequently can be put in the errors or omissions sections. Related to our topic is the literature on public signals and transparency under strategic uncertainty, represented by Morris and Shin (2002) or Angeletos and Pavan (2007) and explicitly in the context of a currency attack by Heinemann and Illing (2002). However, the notion of strategic investor misinformation has not been considered in this literature to our knowledge; a central authority can control at best the precision (the variance) of the public signal that the investors get, which is not the same as what we mean here.[9] Note, however, that changing the precision of the signal would not constitute data manipulation and our tests would not pick it up, as the data that would be generated should still then adhere to Benford's law (see the arguments in section IVA). Angeletos, Hellwig, and Pavan (2006) and Tarashev (2007) offer models where the government chooses a signal (the interest rate) to inform the public about the state of its economy. This affects the cost of the agent's later decision. But providing false information is a different issue; the agents may not know well the information policy of the government and (provided that the agents do pay attention to what the government says), this information may affect the belief formation in one way or the other. Extending the work of Obstfeld (1986, 1996) and analyzing a second-generation currency crisis model without any strategic uncertainty, Sbracia and Zaghini (2001) note, without modeling the strategic choices of a central authority, that greater precision of public information may stop an otherwise imminent attack when the state of the economy is bad and that the contrary is true when the state is good. This opens up the question of whether the government would like to use the information that is provided to its advantage. On a different note, Sandleris (2008) argues that a country may repay sovereign debt in order not to reveal unfavorable information about the state of its economy. The literature on the strategic delay of information release is related but does not apply here. The large literature on dynamic inconsistency and limited commitment started by Kydland and Prescott (1977) is not relevant in this context, and neither is the literature on moral hazard with ex post verifiable actions.

Successful misinformation by a government may seem puzzling and is rarely taken into account, as it appears at odds with the assumptions of rational expectations and the economic agents' knowledge about the true state of the world employed in many models.[10] In some models with strategic uncertainty, information is aggregated through individual trades into prices, and hence the government's influence over the information known to market participants may be extremely limited. This, however, may be not true all the time; for example, in a fixed exchange regime where an important price, the exchange rate, is fixed, the shadow price (the real exchange rate) must be calculated based on the statistics provided by the government. A fundamental question is why individuals (knowing that the government may reveal information strategically) would pay attention to public signals at all. However, the basic conclusions of the literature on one-sided private information when signaling is not possible (see for example, Aumann, Marschler, & Stearns, 1995; Crawford & Sobel, 1982; or Benabou & Laroque, 1992) are that this can be the case, and partial information revelation may be an equilibrium. Another question is whether the ability of governments to provide false information is permanent or just temporary; in the long run, serial cheaters may be found out. This paper, by providing some evidence on the strategic character of government release of public signals, provides an incentive to continue and expand theoretical research in these fields.

G. *Organization of the Paper*

The paper is constructed as follows. First, we discuss the reasons for which countries would want to misreport economic data. In section II, we lay out a model of international financial flows and country incentives to misinform investors about their state of the economy. In section III, we justify the choice of our data set and describe the data we use. In section IV, we discuss the theory behind Benford's law and the statistical methodology for our data set. Section V reports and interprets the evidence, and section VI concludes. The proofs of the results stated in section II are included in the appendix. Other material, such as extended proofs, full sample results, and robustness checks, is in MSWP.

II. Misreporting Economic Data to International Investors: A Model

We provide a parsimonious open economy model where we study the incentives of countries to provide false information to investors. We study a one-shot game of incomplete information. (All proofs are relegated to the appendix). Suppose first there is a continuum of identical countries indexed $i \in [0, 1]$ with a population of immobile workers $L_i \equiv 1$ and endowed with immobile capital installed $C_i \equiv C > 0$. There are in total $\overline{K}$ units of mobile capital in the world; for simplicity, these are owned by some agents that have no

[8] Institutions like the IMF and Eurostat can verify at least partially the information that is given out by the country, but this does not solve the problem entirely.

[9] A different notion is that of vagueness, when a policymaker would release a set of signals containing the true one to be informative in the equilibrium. See Stein (1989), based on the model of Crawford and Sobel (1982). Gelos and Wei (2005) show empirically that funds invest less in less transparent countries.

[10] The Billion Prices Project by Alberto Cavallo and Roberto Rigobon is an initiative by private agents to gather information in lieu of government agencies (in this case, on the evolution of prices). This way of substituting the government, however, is limited by what can be observed by economic agents themselves. In the case of the BoP or GDP data, a similar project is hard to envisage.

domicile but have to invest this capital in at least one of the aforementioned countries. There are free capital flows among all countries and perfectly competitive markets for the factors of production within each country. The remuneration of factors equals their marginal productivity.

To model a country's incentives, suppose that the news about each country's BoP data can be either good (g) or bad (b) reflecting the underlying state of a country's economy.[11] For each country $i \in [0, 1]$, we denote its state by $s_i \in \{g, b\}$. The state of the economy is modeled as a multiplicative productivity parameter, and denoting by A a common parameter for all countries, one has $A(g) > A(b)$. Each country has the same technology and the statistical process generating a period's news, as well as $\Phi > 0$, the present discounted value of punishment for being caught lying.[12]

There is only one period. For all countries, a good state occurs with a common probability $\hat{\rho}$, which is drawn from some distribution ρ on $[0, 1]$. Since there is a continuum of countries, this results, by the law of large numbers, in respective fractions $\hat{\rho}$ and $1 - \hat{\rho}$ of the countries being in good and bad states. The distribution ρ is common knowledge, but the actual probability $\hat{\rho}$ of a good state is initially unknown to the countries and investors. The sequence of moves is the following. Each country $i \in [0, 1]$ learns its own true state of productivity $s_i \in \{g, b\}$ and then declares it (truthfully or not) as $d_i \in \{g, b\}$ to investors simultaneously with other countries; we denote by $\tilde{\rho}$ the fraction of countries declaring a good state. Investors observe country declarations and then make decisions on where and how much to invest. We denote by $K_i(d_i, \tilde{\rho})$ the total capital invested in country i (including the immobile capital C). Then the period ends, investments are realized, and the payouts (including punishments, if any) are made.

We assume that the production function in country i,

$$\begin{aligned} Y_i &= A(s_i)\ F\big(K_i(d_i, \tilde{\rho}), L_i\big) \\ &= A(s_i)\ \big(K_i(d_i, \tilde{\rho})\big)^{\alpha}\ \big(L_i\big)^{1-\alpha}, \end{aligned} \tag{1}$$

is Cobb–Douglas with the capital intensity $0 < \alpha < 1$. As $L_i = 1$ for all countries i, we do not analyze the model in per capita form. Let $R_i(s_i, d_i, \tilde{\rho})$ be the return to a country, and let $ER_i(s_i, d_i) = \mathbb{E}\big[R_i(s_i, d_i, \tilde{\rho})\big]$ be its expectation (with respect to the distribution ρ). We assume that each country i cares about its gross national product (the remuneration of domestic factors), $R_i = w_i L_i + r_i C_i$ where w_i is the wage and r_i is the return to (domestically installed and owned) capital. With the above assumption on the production function, this translates into

$$\begin{aligned} &R_i(s_i, d_i, \tilde{\rho}) \\ &\quad = A(s_i)\left((1-\alpha)\big(K_i(d_i, \tilde{\rho})\big)^{\alpha} + \alpha\big(K_i(d_i, \tilde{\rho})\big)^{\alpha-1} C\right). \end{aligned} \tag{2}$$

Country i's payoff is increasing in the state of the world s_i and in the installed (immobile) capital C.

We discuss only an equilibrium when a fraction of countries always lies, but investors analyze the declarations, obtain information from these, and make rational investment decisions nevertheless.[13]

In this equilibrium, countries that receive a good state $s_i = g$ report truthfully g; a fraction $\varphi \in [0, 1]$ of countries that receive a bad state $s_i = b$ declare a good state g knowing that they will obtain in expectations a penalty Φ; the rest of those receiving the bad state $(1 - \varphi)$ declare it as such. As we show in proposition 6 (in the appendix), at equilibrium, the investors deduce φ from the knowledge of Φ. Since, in addition, countries' strategies do not depend on $\hat{\rho}$, which is unknown to them when making the declarations, one has that $\tilde{\rho} = \hat{\rho} + \varphi(1 - \hat{\rho})$, and investors learn the true $\hat{\rho}$ from observing $\tilde{\rho}$. Investors wish to maximize their ex ante return from investment (this will be the characteristic of any equilibrium); hence, they will allocate capital $\overline{K}$ so that the marginal return from investment in countries declaring a good or bad state of the world is equalized: at equilibrium, the invested capital $K_i(g, \tilde{\rho})$ and $K_i(b, \tilde{\rho})$ do not depend on the countries i (given that $C_i \equiv C$ for all i), and their common values $K(g, \tilde{\rho})$ and $K(b, \tilde{\rho})$ satisfy[14]

$$\begin{aligned} &\alpha\left(\frac{\hat{\rho}}{\tilde{\rho}}A(g) + \frac{\varphi(1-\hat{\rho})}{\tilde{\rho}}A(b)\right)\big(K(g, \tilde{\rho})\big)^{\alpha-1} \\ &\quad = \alpha\ A(b)\ \big(K(b, \tilde{\rho})\big)^{\alpha-1}. \end{aligned} \tag{3}$$

Existence of this equilibrium is shown in the appendix. When Φ is high enough, all countries always make a truthful declaration about the state of their productivity and $\varphi = 0$.

Proposition 6 (stated in the appendix) guarantees the existence of a one-to-one mapping $\varphi \in (0, 1) \mapsto \Phi$, and the following result indicates that this mapping, and its inverse, are decreasing: the weaker the punishment, the more that countries lie.

Proposition 1. *The fraction $\varphi \in (0, 1)$ of liars at equilibrium strictly increases as the punishment Φ decreases.*

[11] The model is stylized. The application to the BoP data we use can be understood that good or bad data about the BoP will increase or decrease potential flows if declared truthfully by a country and the country desires (given the production functions and its objective to maximize GNP) to attract as many flows as possible. Good data about the BoP may mean that a country is less risky and offers higher returns to potential investors given the same level of flows.

[12] This punishment may not occur with certainty. In reality, it may be very difficult for investors collectively to learn that a country misstated its information.

[13] The "babbling" equilibrium, as in games of cheap talk (Crawford & Sobel, 1982), where countries randomize their declarations and investors ignore the messages sent by countries, does not exist here. This is because there is a positive punishment Φ, so countries that would randomly lie would be punished: lying has consequences.

[14] We assume that the endowments of capital C_i are such that even when being in the bad state b, each country would receive some international flows.

In the analyzed equilibrium, investors make their decisions rationally; there is some information transmission between countries and the investors, and some fraction of countries is lying about their state of the world.

We now perform some comparative statics and interpret the implications of the model for the data. We use the implications stemming from these propositions to partition countries into different groups that should display different behavior in manipulating (or not) their messages.

Proposition 1 does not handle the case if there were different groups of countries with different levels of punishment (the size of which is common knowledge). Investors would view differently each group of countries, but still they would want to equalize the ex ante return to capital on both groups. Suppose there are two groups that vary in the punishment costs of misreporting, respectively denoted by $\underline{\Phi} < \overline{\Phi}$:[15]

Proposition 2. *Countries in the group with the lower punishment cost $\underline{\Phi}$ have a higher fraction of liars at equilibrium.*

Let Θ be the cost of abandoning a fixed currency regime for country i if the period capital flows were low after the country truthfully declared $s_i = b$. This means, ceteris paribus, given a penalty Φ, that the effective penalty for misreporting for such a country is lower when the true state is $s_i = b$. Hence, proposition 2 shows that if the countries have the same level of installed capital C, a country with a fixed exchange rate would more likely provide false information than a country with a more flexible currency arrangement if they had the same openness to capital flows. Also, if some countries are facing higher punishments because of their previous misreporting or sovereign defaults, proposition 2 tells us they would be less likely to misrepresent their statistics.

We now show that countries with less installed capital (lower C_i) and therefore relying more on foreign investment will have higher incentives to lie. For clarity of exposition we again assume that there are only two groups of countries, with respective installed capitals $\underline{C} < \overline{C}$.

Proposition 3. *Countries in the group with the lower installed capital $\underline{C}$ have a higher fraction of liars at equilibrium.*

When we turn to our data (see section III for a description), we will interpret the last observations in the following way. Countries with high negative asset positions (that is, relying strongly on foreign capital), having a need to finance their negative current account or a fiscal deficit and at the same time open to capital flows, should have higher incentives to misreport their true state of the economy.

In what we have assumed so far, capital flows were unrestricted for the mobile international capital. Suppose now that there are barriers to capital flows to some countries, so that only a fraction $\tau < 1$ of any investment is obtained ex post. Then, ceteris paribus, a country with no barriers will have higher incentives to misinform investors in the bad state of the world because the resulting relative capital flows (when claiming the good versus the bad state) will be more important:

Proposition 4. *Countries with lower barriers to capital flows have a higher fraction of liars at equilibrium.*

When the economic state of the world (realized $\hat{\rho}$) is lower than the expectation of ρ, the fraction of liars among all countries is higher:

Proposition 5. *The lower the $\hat{\rho}$, the more countries are lying.*

This may not be the necessarily true during all "crisis" years when the fraction of countries receiving a bad state of productivity is high, if such an outcome was expected.

A. *Punishments (possible extensions)*

We assumed that the punishment Φ is constant and that the interaction is one shot only. A more complex model could be delivered; for example, investors may punish a country that lied by not investing in it in the future for at least some time. We chose not to model such an interaction, as in repeated games, there are many punishment structures that could sustain different equilibria. Which one investors would use is an empirical question that was not resolved even in the oft-studied sovereign debt literature. We also did not do it for parsimony. Benabou and Laroque (1992) provide a model with the stage game with similar features to ours (though with players' payoff functions that are easier to analyze) where many different equilibria with repeated interactions are characterized depending on the initial assumptions.[16]

III. A Convenient Data Set: IMF's Balance-of-Payments Statistics

A. *Choice of Data Set*

We choose a data set to make detecting misreporting (if any occurs at all) possible given our methodology. We seek economic series that could be manipulated by the government and at the same time easily observed by investors. The indicators in question should also theoretically follow Benford's law. For our tests to have rejection power (see section IV), we require a lot of comparable data. Certain other characteristics are also desirable for a data set to be adequate (explained below).

For theoretical reasons later described in section IV as properties P1 to P3, we do not expect inflation, interest

[15] The arguments in propositions 2 to 4 can be extended to countably many different groups.

[16] These authors studied the problem of an informed agent that, except for passing her private information to other noninformed traders, can trade on this information herself. The game studied there is a game of cheap talk when regarded as a one-stage game, and then a repeated game analysis is provided.

rates, the fiscal deficit (as a percentage of the GDP), or any indicators that are growth rates with some time-invariant expectation (GDP growth, changes in industrial production) to follow Benford's law. These series do not satisfy property P1 stated in section IV. In addition, for such series, it might also be difficult to obtain enough heterogeneity so that property P2 works. In contrast, we expect that money supply, public debt (stock), budgetary entries (data in levels), output (level data), and BoP entries should follow the law. Taking data from many countries (distributions) should increase adherence to the Benford's law (by property P2; see section IV).

There are few statistics for GDP or industrial production available for each quarter for a wide range of countries, and some of these (fiscal data, for example) are available only yearly. Comparable public sector debt data for most of the covered countries in the Public Sector Debt Statistics database from the World Bank/IMF are available only since 2009. For external debt in the World Bank/IMF Quarterly External Debt Statistics, more extensive quarterly coverage for countries starts with 2003 to 2005.[17] With a small number of observations in the data sets, it is difficult to group countries according to their economic characteristics and reduce persistence by considering randomly selected subsets.

A different issue is whether the data are measured (as a flow, for example) or artificially constructed (as an index).[18] Industrial production and price levels are indices, so are not "natural" data. Geometrically increasing sequences like the BoP entries and fiscal data are convenient and guarantee on a theoretical side conformity to Benford's law. Ideally, we would like measures that are calculated using similar methodologies from country to country and that do not involve many transformations of the data. Moreover, some macroeconomic data are only one-of-a-kind and so do not comprise comprehensive accounting data sets. Finally, possible persistence in the series may make rejection of Benford's law easier.

Data sets on BoP, fiscal data, and money supply satisfy most of these requirements. We chose to investigate quarterly BoP data from the IMF balance of payments statistics (BOPS) from the International Financial Statistics (IFS) database for the period 1989 to 2007.[19] They had some additional advantages over other data sets.[20] Available quarterly, these are accounting data containing net values, which helps counter the problem of persistence that could lead to easier rejections of Benford's distribution for the first digits in finite samples (see section IVD). These data are provided by statistical agencies of IMF member states and should be prepared in a standard way, using the IMF's (1993) BoP manual. The data are in U.S. dollars, which means that they were minimally transformed (for the importance of this, see Nye & Moul, 2007) given the fact that most international transactions are conducted in this currency. Moreover, many countries keep track of the BoP in U.S. dollars (among them are Poland, Romania, and Taiwan). The fact that the BoP data are converted to U.S. dollars should preserve the Benford's distribution of the digits if the original distribution indeed is Benford (section IVA), but the distribution would remain non-Benford if it were such initially. Currency conversion may also help to avoid some structural breaks like currency reforms, hyperinflation, or rapid disinflation that would appear in the domestic currency data.

Why would countries manipulate the BoP data? BoP data are crucial for fixed exchange rate regimes and countries with high net foreign liabilities. In a fixed exchange regime, the value of the information coming from the BoP is even higher to agents as the exchange rate (which would otherwise contain aggregate information about the underlying transactions) in a credible regime is expected not to change. A sustained current account imbalance may point to a misalignment of the real exchange rate that may be (abruptly) corrected by a devaluation or depreciation of the country's currency. A depletion of reserves without a possibility of borrowing liquidity may lead to a BoP crisis as in the literature started by Krugman (1979) or to a currency attack. Moreover, investors obtain valuable information from the BoP that helps them assess the returns to capital (and their riskiness) within the country in the nearest future.[21] For example, changes in reserves or changes in the volume and composition of the current account items have implications for the ability to retire capital (for possible capital flow restrictions in the future), the probability of a successful speculative attack (that would diminish the valuation of the assets held in the local currency), or the solvency (or possible illiquidity) of the country and the firms located there and potential macroeconomic adjustment needed in case of a rapid correction of any current account imbalances. Reinhart and Rogoff (2009) include among the best few predictors of banking and currency crashes (compiling from the rich literature on early crisis indicators), respectively, the short-term capital inflows to GDP ratio current account balance or investment, current account balance to GDP ratio, and exports. In the view of this, a government may want to lie on the BoP

[17] The troubling lack of availability (with speculations why such transparency could be caused by) of precise debt data even historically is highlighted, for example, by Reinhart and Rogoff (2009).

[18] Nye and Moul (2007) advise against using indices for testing the goodness of fit to Benford's law. Statistical methodologies used in constructing such indices and seasonal adjustment may worsen the "naturalness" of the data.

[19] The data were accessed at the IMF website on December 31, 2009.

[20] In further work, we analyze various fiscal data sets to uncover patterns of rejection of Benford's law. Most fiscal data sets contain yearly data, which leads to relatively few data points available for a large number of countries.

[21] For example, Standard and Poor's (2008) analyzes the structure of the current account, the composition of capital flows, and reserve adequacy to assess a sovereign's rating. It uses indices composed of different items of the BoP (balance on trade, FDI, portfolio flows, reserves) in its assessments. Imperfect as it may be, the sets of indicators published in the economic press may be telling as to what draws the most attention. For example, in *The Economist*, the set of indices covered on a regular basis include GDP change (%), industrial production change (%), consumer prices change (%), unemployment rate (%), trade balance (flow), current account (flow and % of GDP), budget balance (% of GDP), and interest rates (%). In the IMF World Economic Outlook issues, the main indicators include output, inflation, fiscal balances, trade volumes and prices, current account, capital, and financial account transactions.

data, for example, to steer the market expectations to a good equilibrium (as in second-generation currency crisis models), avoid capital flight, or attract investment to cover a current account or a fiscal deficit.

There are several instances of countries that were suspected of changing their BoP entries or caught doing so.[22] Russia and The Gambia were charged of being guilty of overstating reserves, with Ukraine (see IMF, 2000) being the most blatant case. Chad and Sierra Leone were not disclosing external arrears that they ran in breach of the agreements with the IMF. China was accused of understating its trade surpluses. Analysts and commentators point in several cases to doubts about the veracity and accuracy of data (Egypt in 2005, but many other instances can be found) when the size of the net errors and omissions may seriously alter the current account balance shown by a country (for example, putting it into a negative territory). All of these cases, except for China (the country is not in the IFS data for 1989–2007), are found in countries that were at the time of the accusations dependent on capital flows or international aid.

B. *Data Description*

For our BOPS data set, we chose the initial date of 1989 because prior to that year, the data are available for few countries and international capital flows were then relatively small; also, after 1989, many new countries emerged that start providing statistics. We use quarterly data that investors often take into account while making their decisions. Yearly data occur with a lag and may not be as informative (so may not be actively falsified); moreover, we would get four times fewer data points. Monthly data are available for only a few countries on a regular basis.

We take all the net figures from the BoP so as to lower the problem of persistence.[23] We consider this to be an innocuous but a helpful fix. Net values like the current or financial account balances are given by many countries to investors before any gross items are available. Theoretically (see section IVA), a difference of almost all two exponential sequences (so, net values) will also have a Benford behavior of the first digits. We consider all entries (also net errors and omissions); because the BoP items should sum up to 0, a typical manipulation would require changing at least two series. Net errors and omissions is an item that can be inflated to hide unfavorable current account data ("Egypt 2004/5," 2005). All in all, we have at most thirteen entries per country for each quarter, so 76 quarters of data, at most 988 observations for each country.[24] More detailed statistics for the BoP are not available for a large set of countries and often come with a considerable lag.[25]

We have data for 103 countries for the period 1989 to 2007 with 69,287 observations on the first digits. Because the maximum number of observations is then 101,764 (103 countries with 13 items reported over 76 quarters), a considerable number of observations are missing. Many of these missing values appear not to be random, stemming from the fact that countries did not report all BoP entries (especially those that were small) for several consecutive quarters (or at all) at the beginning of the studied period, but started doing so regularly with time. In the first quarter of 1989, we have 517 observations; for the last quarter of 2007, we have 1,167. An example of an entry that has this pattern is "Financial Derivatives: Net," with only 3 countries reporting it in the first quarter of 1989 and 45 countries in the last quarter of 2007. We find 32,456 missing observations that are missing in consecutive quarters (so that only 21 of the missing observations occur in one quarter only). Among them, we lose at least 4,628 observations only due to the fact that 24 countries were created within the sample period so obviously could not report the data prior to their independence (and possibly for some years afterward). Although some countries report the data for a few years or a few quarters only, with breaks for unknown reasons, we do not consider the missing observations a major obstacle. Even if some countries may have not reported data for strategic motives (which we cannot outrule), this would work against our method of detecting irregular behavior. Examples of obtained empirical distributions of the first digits of the BoP data for different countries are reported in figure 1.

We also use a subset of the data containing only the countries reporting BoP data for at least one quarter and one series a year between 1995 and 2007, keeping only the observations corresponding to the latter period of time. This subset will be referred to as the *stable sample*; *full sample* will refer to the entire data set. The stable sample is a more complete data set with 66 countries between 1995 and 2007, with 41,245 observations out of 44,616 all possible values.[26] Some additional data that we use to provide conditions for slicing the samples (country investment positions and WGI, for example) are available for this later period only. When we scrutinize yearly data only, the number of observations per year remains roughly constant. The countries included may also provide higher-quality data. For example, Poland is excluded from this sample because it did not report the data between 1996 and 2000 when it was changing and improving the statistical

[22] For Russia, Jack (1999); for China, Ruskin (2003). For Chad, The Gambia, and Sierra Leone, see the IMF press releases from 2003, 2005, and 2006, respectively. Doubts about Egypt's figures are "Egypt 2004/5" (2005).

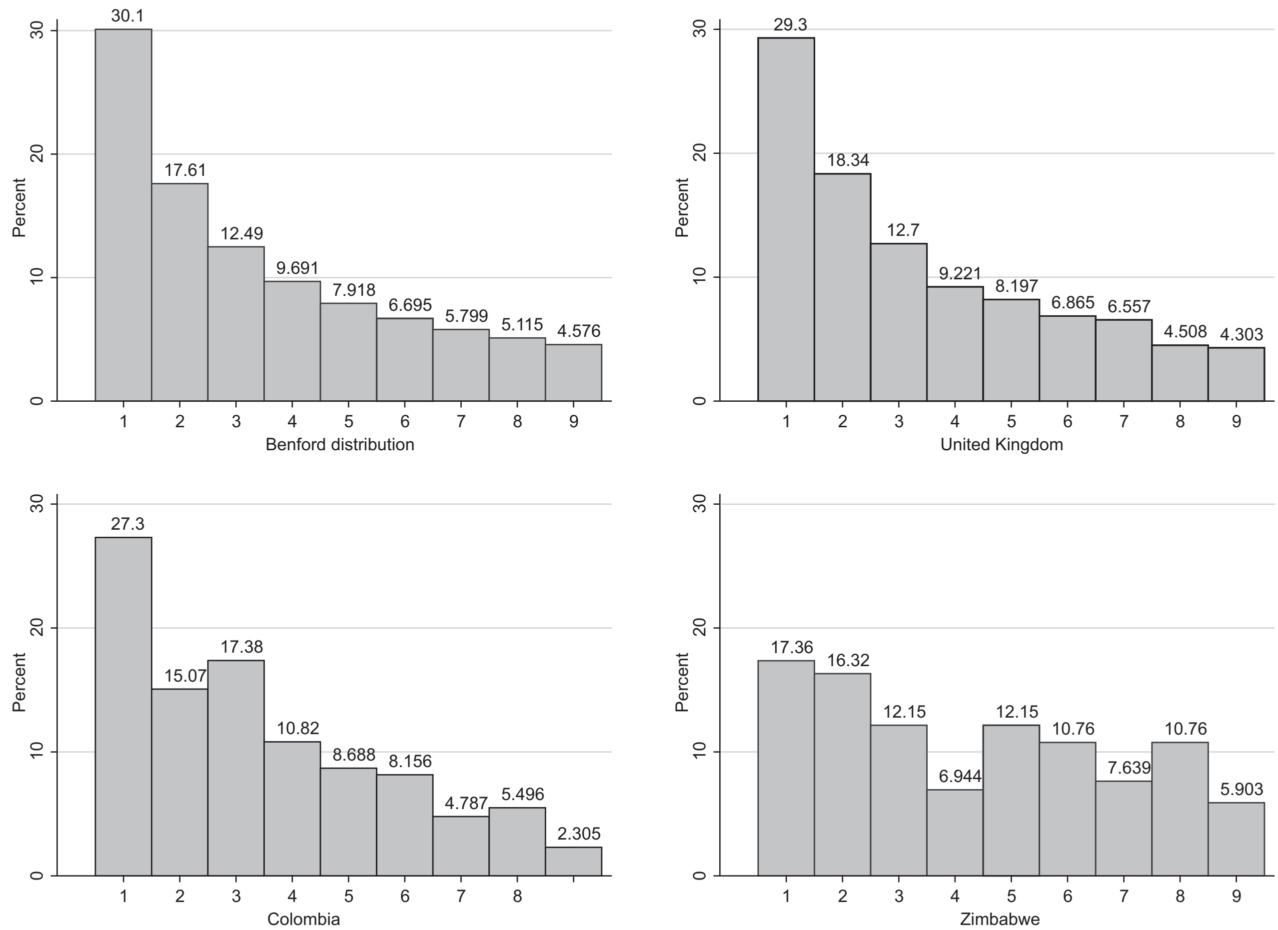
[23] Some items, like net errors and omissions or, for many countries, capital account: net are reported only in net values.

[24] These are current account: net; goods: net; services: net; income: net; current transfers: net; capital account: net; financial account: net; direct investment: net; Portfolio investment: net; other investment: net; financial derivatives: net; reserve assets: net; net errors and omissions. We have not investigated any non-net series.

[25] We do not use the data supplied directly to investors in real time, but what is given to the IMF with some lag. Nevertheless, the IMF is an important lending institution that has an influence on the financial conditions in many, especially non-OECD, countries. The data submitted to the IMF are also used by market participants in their analyzes.

[26] Choosing 1994 as the initial year would give us only 61 countries with more incomplete data, while picking 1996 would raise the number of countries in the sample only to 70. In both cases, the alternative choice would lead to fewer observations being included in the stable sample. Because we need as many countries and quarters as possible given the arguments advanced in section IV, the choice of the 1995–2007 sample is a natural one.

FIGURE 1.—EMPIRICAL DISTRIBUTIONS OF THE FIRST DIGITS OBTAINED FOR VARIOUS INDIVIDUAL COUNTRIES



methodology of BoP reporting. The stable sample is thus our preferred one.

To categorize countries according to their international investment position, we use the updated and extended version of the External Wealth of Nations Mark II database developed by Lane and Milesi-Ferretti (2007). The exchange rate regimes (and euro adoption data) were classified based on information of the IMF in various issues of its "Annual Report on Exchange Arrangements and Exchange Restrictions" (IMF, 1989–2008) and the data provided by fund staff. We opted for the de facto classifications of the IMF. What matters for us is the end-of-period (quarter) status of the regime. Data for fiscal balances and inflation were taken from the IMF. Data for institutional quality measures were taken from the World Bank's WGI database (Kaufmann, Kraay, & Mastruzzi, 2009) and those for country ratings from the Institutional Investor. Data on sovereign defaults and banking crises are from Reinhart and Rogoff (2009).

The basic characteristics of the variables in the full and stable samples do not differ substantially (table 1). The major exception is in the exchange rate regimes: a higher fraction of the observations is available for the purely floating regimes in the stable sample than in the full sample (36.3% versus 28.2%, respectively) and a lower fraction for fixed exchange rate regimes (37.5% versus 45.8% respectively). The countries in the stable sample have on average slightly higher liabilities but higher net foreign assets and better institutional rankings.

IV. Balance-of-payments Data and Benford's Law

We first recall why the numbers contained in the BoP data should theoretically follow a specific distribution called Benford's law and then show that this is empirically the case.

A. Benford's Law and Its Properties for Macroeconomic Data

Newcomb (1881) and Benford (1938) independently observed empirically and described a distribution of numbers called since Benford's law. The predicted occurrences of the first digits are given, for all $j \in \{1, \dots, 9\}$, by $\theta_j = \log_{10}(1 + 1/j)$.[27] The values of these frequencies θ_j are given in table 2 and are plotted in the top left graph of figure 1.

To date, we know of one property and two families of processes that lead the data to be distributed according to

[27] Benford's law also describes the frequencies of occurrences of the next digits, which we do not use here for the lack of a sufficient number of observations.

TABLE 1.—DATA DESCRIPTION FOR THE FULL AND STABLE SAMPLES

	Full Sample					Stable Sample				
	Number of Observations	Mean	SD	Minimum	Maximum	Number of Observations	Mean	SD	Minimum	Maximum
First digit of BOP figure	69,287	3.43	2.46	1	9	41,245	3.40	2.45	1	9
Stable sample = 1	69,287	0.60		0	1					
Fixed exchange rate = 1	69,287	0.46		0	1	41,245	0.38		0	1
Floating exchange rate = 1	69,287	0.28		0	1	41,245	0.36		0	1
Exchange arrangement with no separate legal tender = 1	67,150	0.0179		0	1	41,245	0.0093		0	1
Currency board arrangement = 1	67,150	0.0372		0	1	41,245	0.0522		0	1
Conventional peg to a single currency = 1	67,150	0.1554		0	1	41,245	0.1168		0	1
Conventional peg to a composite = 1	67,150	0.0444		0	1	41,245	0.0319		0	1
Pegged exchange rate within horizontal bands = 1	67,150	0.0967		0	1	41,245	0.0761		0	1
Crawling peg = 1	67,150	0.0686		0	1	41,245	0.05		0	1
Crawling band = 1	67,150	0.047		0	1	41,245	0.0485		0	1
Managed floating with no predetermined path for the exchange rate = 1	67,150	0.2454		0	1	41,245	0.2514		0	1
Independently floating = 1	67,150	0.2874		0	1	41,245	0.3638		0	1
Debt liabilities in year (−1) / GDP (−1)[a]	67,881	0.73	0.67	0.04	26.88	40,583	0.76	0.67	0.06	6.94
Equity liabilities in year (−1) / GDP (−1)[a]	69,287	0.10	0.29	0	4.27	41,245	0.13	0.34	0	4.27
Liabilities excluding FDI in year (−1) / GDP (−1)[a]	67,837	0.83	0.87	0.06	26.88	40,583	0.90	0.94	0.06	11.21
Liabilities in year (−1) / GDP (−1)[a]	67,837	1.09	1.00	0.07	27.21	40,583	1.19	1.10	0.20	11.92
Net foreign asset position in year (−1) / GDP (−1)[a]	67,837	−0.39	0.55	−25.66	1.96	40,583	−0.37	0.51	−3.94	1.96
Net foreign asset position excluding FDI in year (−1) / GDP (−1)[a]	67,837	−0.23	0.52	−25.34	2.51	40,583	−0.20	0.48	−3.85	2.51
Current account balance positive in the quarter = 1	69,204	0.35		0	1	41,209	0.37		0	1
WGI control of corruption (rank)	50,372	56.39	29.24	0.49	100	38,088	59.94	29.91	0.49	100
WGI government effectiveness (rank)	50,460	60.04	27.23	1.90	100	38,176	63.57	27.55	1.90	100
WGI political stability (rank)	50,460	51.18	29.20	0.96	100	38,176	53.34	30.14	0.96	100
WGI rule of law (rank)	50,460	56.22	29.01	1.43	100	38,176	60.12	29.39	2.86	100
WGI regulatory quality (rank)	50,460	60.14	27.03	0	100	38,176	64.09	26.92	0	100
WGI voice and accountability (rank)	50,460	58.61	27.31	0	100	38,176	62.20	26.85	0	100
Institutional Investor country ranking	64,999	51.27	25.78	4.50	96.4	39,334	55.18	26.30	6	96

TABLE 1.—(CONTINUED)

	Full Sample					Stable Sample				
	Number of Observations	Mean	SD	Minimum	Maximum	Number of Observations	Mean	SD	Minimum	Maximum
OECD member prior to 1989 = 1	69,287	0.29		0	1	41,245	0.32		0	1
Africa and Middle East = 1	69,287	0.14		0	1	41,245	0.10		0	1
CIS member = 1	69,287	0.12		0	1	41,245	0.14		0	1
Eastern Europe = 1	69,287	0.15		0	1	41,245	0.17		0	1
Developed Europe + Western offshoots + Japan = 1	69,287	0.29		0	1	41,245	0.32		0	1
Latin America = 1	69,287	0.16		0	1	41,245	0.15		0	1
Asia excluding Middle East and CIS = 1	69,287	0.16		0	1	41,245	0.17		0	1
Inflation year/year, quarterly, in %	67,145	46.905	594.44	−11.96	28284	41,033	11.484	52.225	−11.96	1715.6
Euro is the currency in the quarter = 1	69,287	0.0667		0	1	41,245	0.1033		0	1
Bank crisis in the country in the year = 1	69,287	0.0365		0	1	41,245	0.0288		0	1
A country defaulted in the period 1969–2007 = 1	69,287	0.2711		0	1	41,245	0.2798		0	1

The indications of = 1 flag variables with values in {0,1}; the value 1 indicated that the given condition is fulfilled. The reported means then correspond to proportions. No standard deviation is reported.
[a] See Lane and Milesi–Feretti (2007).

TABLE 2.—BENFORD'S LAW ($\theta_1, \ldots, \theta_9$) FOR THE FIRST DIGITS

j	1	2	3	4	5	6	7	8	9
θ_j	30.1%	17.6%	12.5%	9.7%	7.9%	6.7%	5.8%	5.1%	4.6%

Benford's law: processes with exponential growth and variations around them (differences of such processes or random exponential growths), taking random samples of random distributions, and scale-invariant (unit-invariant) processes. We label these properties P1, P2, and P3, and this is how we refer to them in the remainder of this paper.

We first provide a brief overview of the classes of processes that lead to Benford's law (mathematical details and further references may be found in section C.2.1 in MSWP). The applicability of the Benford's law to macroeconomic data has been discussed in Nye and Moul (2007) and Gonzalez-Garcia and Pastor (2009), based on properties P1 to P3 and those of artificially generated sequences.

P1—Processes with Exponential Growth. The simplest generating model is formed by (deterministic) geometric sequences $\{a, a^2, \ldots, a^n\}$ with ratio a such that $\log_{10} a$ is irrational.[28] They lead to sets of data that conform more and more closely to Benford's law as n grows (Diaconis, 1977). Almost all geometric sequences lead to Benford's law as the set of rational numbers has a null measure within the set of all real numbers. This implies that in practice, all geometric sequences linked to real data lead the data to obey Benford's law. This result extends to differences of such geometric processes. For given ratios a and b (such that $\log_{10} a$ is irrational) and any real numbers α and β, the sets $\left\{\alpha a - \beta b,\ \alpha a^2 - \beta b^2,\ \ldots,\ \alpha a^n - \beta b^n\right\}$ conform more and more closely to Benford's law as n grows (lemma 2.4 in Kaynar et al., 2010). Finally, all of these deterministic results extend to certain sequences of random variables (X_j) and (Y_j), with the respective replacement in the expressions above of a^t and b^t by $X_1 X_2, \ldots, X_t$ and $Y_1 Y_2, \ldots, Y_t$. The sequences (X_j) and (Y_j) can be formed, for instance, by i.i.d. random variables, each having a common characteristic function φ such that $\varphi(2\pi k) \neq 1$ for all natural integers $k \neq 0$. This condition is satisfied for almost all distributions. Many other extensions and relaxations of the condition of independence or identical distribution exist.[29]

Given this, we may consider macroeconomic statistics that are obtained in some multiplicative way. Due to economic, population, or price growth, such multiplicative sequences of macroeconomic statistics for a given country should, if observed long enough, result in a collection of numbers with the distribution of first digits abiding by Benford's law. For instance, the series of nominal GDP (such as exports, FDI

[28] Otherwise the sequence of the first digits is cyclic and has asymptotic proportions that are rational and thus are different from the ones of Benford's law. Consider the simple example when $\log_{10} a = 2/3$, in which case, the sequence is $10^{2k/3}$ and contains only elements with first digits equal to the first digits of $10^{2/3}$, $10^{4/3}$, and 100, that is, 4, 2, 1; the asymptotic repartition of the first digits is uniform for these three numbers.

[29] See section C.2.1 in MSWP for an overview of some recent results.

flows) figures can be considered but not the series of the GDP (such as exports, FDI flows) growth rates, since the latter correspond to realizations of random variables X_j above. For example, in a stationary economy with constant real GDP (such as exports, FDI flows), this should be the case if long enough time series are considered when the inflation in each period is, for example, drawn from a log-normal distribution with a given mean, say 1, and a constant small variance.[30]

These results extend to net values: the difference of multiplicative sequences of macroeconomic statistics such as exports or imports (the trade balance) would generate Benford behavior of the first digits. In the same way, mismeasurement—for example consistent underreporting of some quantities—will still lead to Benford sequences as long as the nonmeasured part (say, illicit trade) can be represented by sequences discussed above. Various extensions can be modeled as special cases of the multiplicative form discussed above, including the case of structural breaks (that could be associated with deep economic crises or large shocks) or the presence of errors in data collection.[31]

P2—Random Samples of Random Distributions. Hill (1995b) considered the case of data sets with numbers chosen at random according to a two-step procedure, choosing first at random a distribution over the positive real numbers and then drawing k numbers according to it, and then repeating the whole procedure a large number n of times; he provided natural conditions on the distribution of the random probability distribution for Benford's law to occur when n grows. Janvresse and de la Rue (2004) showed that it suffices to consider random probability distributions taking values in the family of uniform distributions over intervals.

This means that data coming from many different distributions are much more likely to conform to Benford's law (as observed by Benford himself). Aggregating macroeconomic data of several countries preserves the conformity to Benford's law and should even increase it. This is important to remember, especially when the number of data points per country is too small (the series are too short or when breaks occur) for property P1 to lead to the conformity to Benford's law per se. Property P2 works best when the measured quantities differ in scales that may arise naturally, for example, in terms of country size. It should work well for our BoP data set, as the scale of the items on the BoP balances clearly is determined by the size of the economy in question. It could lead, for example, to price levels to converge to Benford's law, especially if they do not all start at the same initial value (for example, indices with different base years for each country are preferred).

P3—Scale invariance. Pinkham (1961) and Hill (1995a) proved that Benford's law is a unique law such that the distribution of the first digits of numbers drawn at random according to it is stable when the drawn numbers are multiplied by a common multiplicative factor. This means that the distribution of first digits according to Benford's law is preserved even if data for a country were provided in the home currency and then converted into U.S. dollars (the case for the BoP data). The multiplicative factors used to proceed to these conversions are random and change with time; however, since we are interested in quantities that can be written in multiplicative forms, the impact of the conversion into U.S. dollars (the value of the exchange rate) can be encompassed in the sequence of the X_j, which should describe the multiplicative evolution of the quantity of interest as well as the one of the exchange rate. This way, a conversion of a sequence adhering to Benford's law in one currency to another should lead to a Benford sequence.

More on the applicability to macroeconomic data. The most thorough study of conformity to Benford's law of data in macroeconomics to date is by Nye and Moul (2007). They showed through simulations that long enough sequences of nominal GDPs for a set of fictitious countries formed sequences of numbers whose first digits obey Benford's law, provided that the random economic factors (various growth rates) were set in a proper way. They then illustrated on the Penn World Tables data set that some subgroups of countries (OECD countries on the one hand, African countries on the other) are such that the data set obtained by aggregating their GDP data conformed to Benford's law. They also argued that various transformations of nominal data (inflation adjustments, creation of purchasing power indices, and others) may alter the quality and lead to sets of numbers no longer conforming to Benford's law. They used Benford's law as a test of the naturalness of the data, as is the case in other fields, such as accounting. They conclude that "broadly speaking, country GDP figures should be consistent with Benford's distribution when countries are heterogeneous in their initial levels (e.g., population, per-capita income, etc.) and then grow."

Gonzalez-Garcia and Pastor (2009), based on the same IFS data set as ours, find that in general, data conform to Benford's law when countries are grouped based on geographical criteria. They in particular also consider the same BoP data set as we do, with entries transformed into U.S. dollars. They point out that the obtained conformities are hardly related to measures of the quality of the data (as indicated by independent experts) and also explain the atypical behavior of series for some countries by structural breaks. Their tests are based, however, on annual observations and use fewer data points, as we do, thus probably not being able to ensure that n is large enough when calling for property P1. In addition, they

[30] This will also be true not only in an economy with (zero on nonzero) GDP growth but even with stochastically varying real GDP as long as the increases or variations can be modeled as the result of successive multiplications by i.i.d. random variables.

[31] Suppose that each period, a break can occur independent from the past with a constant (small) probability. In this case, the X_j are given by a two-step randomization, the first step determining whether a break occurs and the second one providing the adequate multiplicative factor, drawn at random according to a distribution depending on the regime. The same two-step modeling can be applied in the presence of errors in data collection. If the errors can be described in some random multiplicative manner, the obtained sequences will obey Benford's law.

TABLE 3.—ASSESSMENT OF THE GLOBAL CONFORMITY OF THE DATASET TO BENFORD'S LAW

	Number of Countries and Quarters Involved		All Series		Less Persistent Series		Rejection Rates (%)		
Observations Corresponding to	Number of Countries	Average Number of Quarters	p-Value (in %)	Number of Observations	p-Value (in %)	Number of Observations	1-in-20 Criterion	1-in-10 Criterion	1-in-5 Criterion
Full sample	103	57.2	64.4	69,287	66.4	30,075	6.5	6.1	5.4
Stable sample	66	51.9	14.6	41,245	14.8	18,066	5.5	6.9	8.5
OECD countries	22	74.0	92.9	20,105	94.2	8,970	3.7	3.7	2.9
Non-OECD Countries	81	52.6	25.0	49,182	76.1	21,105	6.1	5.6	5.8

Significant at *5%, **2%, and ***1%.

do not correct for the persistence issues we mention in the next section. Given the short data span and the frequency, they may not have enough observed cycles.

B. *The Chi-Square Test of Goodness of Fit to Benford's Law*

Such a goodness-of-fit test compares the empirical frequencies $\widehat{\theta}_j$ of the digits $j \in \{1, \ldots, 9\}$ of the numbers arising in a given data set to the theoretical frequencies θ_j prescribed by Benford's law via the statistic

$$D^2 = N \sum_{j=1}^{9} \frac{\left(\theta_j - \widehat{\theta}_j\right)^2}{\theta_j},$$

where N denotes the total number of the available first digit observations. The statistic D^2 converges in distribution to a chi-square distribution with 8 degrees of freedom as $N \to \infty$. Practitioners think of the distribution of D^2 as already close enough to this limit whenever $N \geq 30$ and $N\theta_j \geq 5$ for all $j \in \{1, \ldots, 9\}$ (van der Vaart, 1998). These conditions are in our case $N \geq 5 \big/ \min_j \theta_j = 5/\theta_9 \geq 110$. This approximation is used for P-values associated with values of the statistic D^2 in the tables. The test is powerful; it is easy to show by concentration-of-the-measure techniques (see Rivoirard & Stoltz, 2009) that the probability of not rejecting any distribution of the first digits other than Benford's law when the first digits indeed conform to Benford's law is exponentially small in N, where the constant in the exponent depends on the discrepancy between the alternative distribution tested and Benford's law.

The idea of detecting manipulations in the data by tests of conformity to Benford's law is now well established in auditing and accounting, after the seminal article of Nigrini (1996) and the successful use of his methodology by the authorities of the City of New York, leading to the detection of fraud in seven companies (see Nigrini, 1999, for an extended review of many other cases). The methodology there is to use as the data set to be tested all quantities appearing in accounts (the values of all individual transactions); the theoretical justification behind this is idea is mainly property P2. Other occurrences and practical uses of Benford's law can be found in Hill (1995b); one can cite, among others, stock market data (Ley, 1996) and census statistics as occurrences and the design of more efficient computers as a possible use.

C. *Global Conformity of Our Proposed Data Set to Benford's Law*

We first use chi-square tests of goodness of fit to show that our data set indeed conforms globally to Benford's law, as suggested by properties P1 to P3 and the existing literature. We consider here the full and stable samples, as well as those formed by OECD and non-OECD countries. The first and second columns of table 3 indicate the number of countries encompassed by the economic condition considered, as well as the average number of quarters per country. In the third column, we report the corresponding p-value and in the fourth column the value of N. Since the p-values are much larger than any conventional threshold for rejecting a null hypothesis, the data set considered globally (and all other subsets here) conforms to Benford's law.

D. *Conformity of Typical Subsets of the BOPS Data Set to Benford's Law*

In this paper we consider subsets of the entire BoP data set defined by some economic conditions and show that some of them, for appropriately chosen conditions, lead to subsets whose distributions of the first digits do not conform to Benford's law. But before doing this, we need to show that in the BOPS data set, typical (most of the randomly drawn) subsets do obey the latter law.

As we detail below, a persistence issue appears in a significant fraction of the data series, which is compensated for by the heterogeneity among countries. Subsets corresponding to a small number of countries or containing many consecutive or close-in-time quarters of these countries typically do not conform to Benford's law while the ones corresponding to the choice of a large number of countries and a few number of quarters per country do. This is in line with properties P1 and P2: property P1 may require very large sample sizes, while property P2 indicates that smaller sample sizes for each country can be compensated for by grouping the data corresponding to different countries.

Persistence. We illustrate persistence in the first line of table 4, where we estimated the probability that when choosing a given number C of countries at random from our data set, the data subset formed passes the goodness-of-fit test against Benford's law at the 5% level. This table shows that a significant fraction of the subsets formed by C countries,

TABLE 4.—REJECTION RATES ACCORDING TO NUMBER OF COUNTRIES AND QUARTERS

Number of Quarters	Number C of Countries Chosen at Random								
	$C = 1$	$C = 10$	$C = 20$	$C = 30$	$C = 40$	$C = 50$	$C = 60$	$C = 70$	$C = 80$
All quarters[a]	43.1%	39.8%	35.4%	32.4%	29.6%	24.5%	16.1%	14.0%	7.8%
$M = 40$ quarters[b]			27.7%		22.0%		12.9%		7.0%
$M = 30$ quarters[b]			25.0%		16.9%		11.9%		8.5%
$M = 20$ quarters[b]			16.2%		14.3%		9.6%		7.3%
$M = 10$ quarters[b]			11.2%		9.6%		9.2%		6.2%

[a]Rejection rates at the 5% level of the hypothesis of conformity to Benford's law on data subsets corresponding to given numbers C of countries chosen at random. Estimated by performing 1,000 random draws for each value of C.
[b]Rejection rates at the 5% level of the hypothesis of conformity to Benford's law on data subsets corresponding to given numbers C of countries and quarters M thereof, all chosen at random. Estimated by performing 1,000 random draws for each value.

with $C \leq 70$, cannot be said to be distributed according to Benford's law. Only when taking a large number of countries (say, $C = 80$) does one get subsets that typically conform to Benford's law.

In the subsets grouped by economic conditions, fewer countries will be typically involved—fewer quarters too, and this will be the key. Indeed, as is shown in the bottom part of table 4, the rejection rates become smaller as either the number C of involved countries increases or the number M of quarters picked at random for each country decreases.

This can be explained by the persistence of data from one quarter to another or structural breaks in the series for individual countries (section C.2.2 in MSWP). It is crucial to deal with it when a small number C of countries is involved (otherwise enough heterogeneity is introduced for the aggregated data to conform to Benford's law). We use net items from the IMF BOPS database to reduce persistence as much as possible, but obviously an extra treatment is needed.

One solution is to select only the less persistent series (section C.2.3 in MSWP). By doing so, however, one may not detect the manipulations on the most crucial series that investors watch and analyze, which can be the most persistent ones as well (for example, the balance on the current account or goods). Some persistent series in fact may also be such because they are manipulated. This is why we focus on the fact that conformity to Benford's law is more likely when the data subset is formed randomly by taking few quarters only (relatively to the number of countries), as in table 4.

A methodological remedy: Considering random subsubsets. We need to quantify whether a large enough proportion of the data subsets conforms to Benford's law for the whole data set to be declared as abiding by Benford's law. This is why we introduce the following new statistical methodology.

To assess the conformity of a subset D of the data, we do not consider all country–quarter pairs of the corresponding subset, but take a small fraction f of them at random and test the conformity of the obtained random subset to Benford's law (at the 5% level), a fact that we denote by R_D, which is therefore a Bernoulli random variable. We repeat this procedure a large number of times, say, 1,000 times, by fixing D but choosing different subsubsets of it at random. The average rejection rate will be an indicator of conformity to Benford's law without the bias due to persistence

TABLE 5.—ESTIMATES OF THE QUANTILES OF THE DISTRIBUTION OF THE RANDOM VARIABLE

Cardinality of D	Estimated Quantiles			
	90%	95%	98%	99%
$N = 1{,}200$	5.5	6.3	7.2	8.0
$N = 2{,}000$	7.0	7.9	9.1	9.5
$N = 5{,}000$	6.9	7.8	9.0	9.8
$N = 10{,}000$	6.9	7.5	8.7	9.9
$N = 30{,}000$	6.8	7.5	8.7	9.5
$N = 50{,}000$	6.8	7.7	9.0	9.7
Conclusion: Upper bounds				
$N \geq 2{,}000$	7.0	8.0	9.0	10.0

$\overline{R}_{D,1000}$ when f equals 10% and the subset D has a cardinality of N and is drawn at random according to Benford's law. Estimates were obtained by drawing D at random 1,000 times and computing the associated values of $\overline{R}_{D,1000}$.

(and due to the randomness to any structural breaks in the data). Formally, the procedure gives rise to the random variables $R_{D,1}, R_{D,2}, \ldots, R_{D,1000}$, which, conditional, to D, are i.i.d. according to a Bernoulli distribution whose parameter is denoted by q_D. We consider the empirical mean $\overline{R}_{D,1000}$ of these random variables as the statistic of interest.

The heuristics behind this procedure is to reduce efficiently the average number of consecutive quarters at hand per country, thus weakening persistence, and to consider q_D instead of the p-value quantifying the goodness of fit of D against Benford's law. The repetition of the procedure aims at obtaining a stable result given D. Formally, we mean that

$$\left[\overline{R}_{D,1000} \pm 1.96\sqrt{\frac{\overline{R}_{D,1000}\left(1-\overline{R}_{D,1000}\right)}{1\,000}}\right] \tag{4}$$

is a confidence interval at a 95% confidence level for q_D, where, given that the typical realized values of $\overline{R}_{D,1000}$ are around 0.10, the precision of the estimation of the rejection rate q_D is about $\pm 2\%$.

To use this new criterion, we first determine the typical values of the statistic $\overline{R}_{D,1000}$ when $f \in \{5\%, 10\%, 20\%\}$ and D is a set drawn at random from Benford's distribution with a given size N.[32] Results are in table 5, which provides estimates of the quantiles of the underlying distribution according for $f = 10\%$, the value mostly used in this

[32] Of course, in this case, the expectation of $\overline{R}_{D,1000}$ (with respect to the choice of D and the random subset) is 5%, but we want a sharper idea of its distribution, namely, good estimates of its tail.

paper (the quantiles for $f = 5\%$ and $f = 20\%$ can be found in table XIV in MSWP). These estimates were constructed by running 1,000 times the above procedure on randomly generated sets D. This involved computing 10^6 goodness-of-fit tests per cell; because of the computational cost, no sharper estimates based on more repetitions are given.

This method provides another test of conformity to Benford's law of subsets D. This test, given D, compares the obtained value of $\overline{R}_{D,1000}$ to the above quantiles and is then able to associate a p-value with the hypothesis of conformity to Benford's law. This is done by identifying in which interval of the last line of table 5 (corresponding to the chosen f) this value lies in. For instance, if the realized value of the statistic $\overline{R}_{D,1000}$ is 8.5 and f was chosen equal to 10%, the corresponding p-value is between 1% and 5%, or if the realized value is 5.4 and f was chosen equal to 5%, the corresponding p-value is larger than 5%.

The subsamplings in proportions $f \in \{5\%, 10\%, 20\%\}$ are called, respectively, 1-in-20 (when $f = 5\%$), 1-in-10 (when $f = 10\%$), and 1-in-5 (when $f = 20\%$) criteria. We include the 1-in-10 criterion in our main results as this already delivers good randomness of the sampled data, but is not as demanding in terms of the required observations as the 1-in-20 criterion.[33]

E. *Our New Methodology and the Conformity of the BOPS Data Set*

We include all the criteria discussed above (as well as the one using less persistent series described in MSWP) in table 3 and find a goodness-of-fit of the first digits of the BoP data to Benford's law, confirming our findings from section IVC. This is true for the full and stable samples. It does not appear important whether we restrict the samples to include only OECD members (as of 1989) or not. This is very important, as we do not find that non-OECD countries have non-Benford distributions of the first digits as an entire group taken together, which is different from the results of Nye and Moul (2007) on the Penn World Table data set. We further investigate groups of countries that may have driven the results of Nye and Moul (2007) in sections VE and VF.

V. Empirical Results

We discuss here in detail the results for our preferred stable subsample of countries that provided data at least once each year in the period 1995 to 2007 and back them up with the results from the full sample and other robustness checks. We perform traditional chi-square goodness-of-fit tests and our developed 1-in-10 randomized tests on the subsamples, grouping country-quarter pairs by economically meaningful characteristics to detect any irregularities in the first-digit behavior. The null hypothesis is that the first digits of the data are drawn from Benford's distribution. We consider that the null is rejected if it is so for both tests. Based on our model in section II, we believe that some groups of countries have higher incentives to tweak their BoP statistics than others. Fixed exchange rate regime countries may be wary of providing truthful information about the developments in the BoP because of a fear of an attack on their currency that unfavorable information may trigger. Countries with high net foreign asset positions relative to GDP and those having negative current account balances or fiscal deficits may fear that too much negative information in the BoP data may spur a nervous reaction of the part of investors and cause capital outflows, which they would like to avoid.

In grouping countries, we investigate subsets that are the unions of various conditions. While investigating the specificity of countries partitioned according to their exchange rate regimes, current account, or fiscal balances, we also look at subgroups of countries that have relatively lower net foreign assets, excluding foreign direct investment to GDP (in tables, denoted as NFA_EXCL_FDI_GDP) or higher foreign equity liabilities to GDP ratio (EQ_LIAB_GDP). (Additional criteria, for example, for the exchange rates, are presented in tables in MSWP). For the international investment positions, we took the top 75% of countries ranked in terms of negative net foreign assets or foreign liabilities.[34] In all cases, we use the previous year's figures for grouping countries. So, for example, to test for a group of countries with fixed exchange rate regimes that also have low net foreign assets (excluding FDI) in the stable sample, we took all the countries with a ratio of net foreign assets (excluding FDI) to GDP lower than -0.0201 in the preceding year that also had a fixed regime.[35] As a measure of capital openness, we used the ratio of foreign equity liabilities to GDP. A relatively higher value of this indicator shows that a country is de facto more open to private capital flows than others, and hence may be also more prone to capital outflows.[36]

The amount of data that we were able to gather allows for grouping countries into many different subsamples; however, with many restricting conditions, these subsamples become small, containing few countries with many quarters of data, which would lead to easy rejections of Benford's distribution due solely to persistence, as discussed in section VD. In such cases we want to rely on the 1-in-10 condition but

[33] To ensure that for each chi-square goodness-of-fit test that needs to be performed we have at least 110 observations (as mentioned above), at least 1,100 and 2,200 data points are respectively needed to compute the 1-in-10 and 1-in-20 criteria.

[34] In the robustness checks, we also use the top 90%, 80%, 66.66%, or 50% of countries (not reported).

[35] The quantiles of a given economic quantity (for example, net foreign assets to GDP) were computed by considering all the available values of the quantity as countries and quarters vary. At most one value of the quantity per country-quarter pair of the sample (full or stable) was considered: none when the quantity was unavailable and one when the data contained information about it. For this reason, the number of available observations in, for instance, the top 10% or lower 10% of the country-quarter pairs for this criterion may differ; the difference is due to the grouping according to the criterion by itself.

[36] We did not use the total portfolio liabilities (equity and debt), as many debt liabilities for a country consist of foreign-denominated debt traded outside country borders. There are also fewer data points for this measure.

TABLE 6.—RESULTS FOR FIXED AND FLOATING EXCHANGE RATE REGIME COUNTRIES: MAIN RESULTS, STABLE SAMPLE

Economic Conditions						
Exchange Regime	Other Conditions	Number of Countries	Average Number of Quarters	Number of Observations	Global p-Value (in %)	1-in-10 Criterion
Floating		39	30.9	15,003	37.6	5.8
Fixed		49	26.6	15,488	0.5***	13.3***
All	NFA_EXCL_FDI_GDP ≤–0.0201	59	43.0	30,480	3.2*	8.4*
Floating	NFA_EXCL_FDI_GDP ≤–0.0201	34	27.8	11,702	35.4	5.6
Fixed	NFA_EXCL_FDI_GDP ≤–0.0201	44	21.0	11,099	0.9***	10.5***
All	EQ_LIAB_GDP > 0.0098	51	43.5	27,339	72.0	4.1
Floating	EQ_LIAB_GDP > 0.0098	31	35.1	13,631	77.9	3.6
Fixed	EQ_LIAB_GDP > 0.0098	38	17.8	8,316	0.00004***	21.7***

The threshold values are given by quartiles (first or third) of the given quantities within the sample. Significant at *5%, **2%, and ***1%.

then need at least 1,100 observations in the sample, as discussed in section IVD. Hence we are unable to slice the data very finely, which prevents us from testing our hypotheses in small subsamples of country groups that may have higher incentives to cheat.

Robustness Checks for Our Findings. We offer several robustness checks of our main goodness-of-fit tests (most are shown in MSWP). First, we typically test whether, for the complement of the set for a given restricting condition (or an alternative regime in the case of an exchange rate regime), we reject Benford's distribution for the first digits as well or not. Such a rejection would make our claim of the relevance of a potential category for grouping countries and detecting data manipulation vacuous. Second, we seek a confirmation of our results for the stable sample in the full sample as well. Next, we want to know whether it is only one country that drives the result for a particular category. This may indicate that out of the whole group, only one country is either providing false information or suffers from a dire problem of persistence in its quarterly data. The fact that such a country would drive the results would invalidate the generality of the claim. This number of countries driving the results, which we label the stability index, is quantified by the number of countries that, when excluded from the subset, lead to the acceptance of the null hypothesis at a level of 10%; that is, we count the number of countries such that when they are excluded one at a time, the p-value associated with the goodness-of-fit test to Benford's law increases from less than 5% to more than 10%. We would like our stability index to be 0 when we expect to reject the null hypothesis. We also test for the rejection of Benford's distribution for the less persistent items and offer results for different criteria based on random subsubsets (1-in-5 and 1-in-20). Finally, we rerun the tests for our principal results, excluding countries with poor governance to ensure that the worst countries in terms of institutional rankings are not responsible for our findings.

A. *Different Exchange Rate Regimes*

In table 6 (see also tables XVIII to XXI in MSWP), we present the tests for the conformity of the first digits of the BoP data with Benford's distribution for country-quarter pair groups in terms of their exchange regimes at the end of a quarter. The left-hand column presents the characteristics of the groups considered. (Additional groupings are presented in tables 9 and 10.)

Any exchange rate regime where the intervention of the central bank was substantial (a currency board, a conventional peg, a crawling peg, a crawling band) was labeled in our data set as "fixed."[37] Such a wide range of regimes was classified together so that we had a large number of countries. We label a regime as "floating" when it was deemed by the IMF as being independently floating.[38] It is to note that the currencies that were part of the European exchange rate mechanism I or II in a given quarter are considered by the IMF to belong to the fixed category prior to the introduction of the euro and as floating afterward.

First, we observe that the null hypothesis of conformity to Benford's law is rejected for fixed exchange rate countries for both the stable and full samples at a 1% significance level. The subsets of countries that have fixed exchange rate regimes contain many countries (49 countries with 26.6 quarters on average for the stable sample and 84 countries with 32.6 quarters on average for the full sample), so we believe, based on section IVD, that neither persistence nor structural breaks are driving our results. The 1-in-10 criterion confirms this. The important thing to notice is that any irregular behavior detected here may not be country specific, as many countries in our samples (27 and 40, respectively, for the stable and full samples) change the fixed exchange rate regime to floating (or the other way round) at least once.

Moreover, after imposing more restrictive conditions, groups of countries with fixed exchange rate regimes have distributions of the first digits for which the hypothesis of Benford's distribution is rejected at a 1% level for all series.

[37] Consequently, for example, crawling band regimes prior to 1998, when the IMF adopted a new classification, were coded as being fixed rather than managed floating.

[38] The remainder of the countries have regimes that are difficult to classify into either category. For some managed exchange rate regimes ("managed floating with no predetermined path for the exchange rate" in the IMF nomenclature), it is difficult to make a call as to whether it is a floating regime; we preferred to make only conservative assessments. Second, we classify here countries when they do not have any own legal tender (Panama, El Salvador, or Ecuador). Indeed, in our tests, countries with regimes that are neither fixed nor floating exhibit different behavior from the two "ideal" regimes (see section IVB).

The 1-in-10 criterion confirms our findings, as these statistics are always greater than 10. More precisely, the level of significance of the rejection increases considerably when we refine the picture and consider countries that belong to the upper 75% in terms of the (negative) net foreign assets (with or without foreign direct investment), equity liabilities, and the size of their total foreign liabilities (shown in MSWP). This is also true for fixed exchange rate regime countries that have negative current account or fiscal balances (tables 9 and 10). This supports our hypothesis that countries with fixed exchange rate regimes that would be more sensitive to capital flows have an interest in tinkering with the signals that they send out to investors. In particular, the rejection rate of Benford's distribution for countries with fixed exchange rate regimes and relatively liquid equity markets within our sample (which means they need to be de facto open to individual financial flows) is 0.00004% in the stable sample and 0.0007% in the full sample. This occurs for a large number of countries (38 and 58, countries respectively) in this category with an average data span of 17.8 and 24.2 quarters. Our findings square with the model that we presented in section II. Countries with fixed exchange rate regimes may wish to misreport data, and among these countries, those that have more negative net asset positions or higher openness to capital flows have higher incentives to do so.

These results survive our stress tests (shown in tables XX to XXI in MSWP). For the stable and full samples, the hypotheses of Benford's distribution of the first digits are still rejected even when we exclude from the sample each country one-by-one (as the value 0 for the stability indexes indicates), which means that several countries in a given group may be responsible for the result. Even when we run the tests on less persistent series of the BoP, we still get rejections of the hypothesis of the first-digit distribution being Benford at a 5% level except for the full sample for the fixed exchange rate regimes that have more liquid equity markets or the entire unconditioned group.

The 1-in-5 and 1-in-20 criteria tell the same story as the 1-in-10 criterion. These provide a strong indication that the rejections may not be due to the persistence of the data. We also run the tests excluding the entries on the reserve assets, which in fixed exchange rate regimes may experience large movements due to the readiness of the central bank to buy and sell currency at a prespecified rate (column 4 in tables XX to XXI in MSWP). This is important, as this is the only item in the BoP that a central bank can control directly. It does not seem thus that the unusual distribution of the first digits for fixed regimes comes from the activity in this entry and hence directly from central bank intervention. Taking different quantiles for the conditions preserves all of the results for the top 90%, 80%, and 66.66% of the conditions and most for the 50% (however, we start having few observations here for some conditions). The picture does not change when we condition on contemporaneous year values of the ratios of net foreign asset to GDP or liabilities to GDP.

Our results for fixed exchange rate regimes contrast strongly with similar tests for the floating exchange rates regimes, for which the null hypothesis cannot be rejected even for one category and when we use the same conditions on the investment positions of a country (see tables 6, 9, and 10). Notice also that the number of countries with floating regimes included in each sample is always lower than for the fixed regimes in the same category; again this may be an indication that the persistence issue does not matter here and that properties P1 to P3 are at work.

It seems odd that our results would occur only due to the specificity of the economic processes under a fixed exchange rate regime. The fact that some items of the BoP, like changes in reserves, may be more variable in a fixed exchange rate regime' should not matter (and indeed, when we exclude the reserves entry, the conclusions are not different) as long as the processes generating the data are well behaved and conditions P1 to P3 are preserved. In a fixed exchange regime, there are still inflation and economic and population growth that would lead the BoP data to evolve according to a process that leads to Benford's law, and we still aggregate across many countries. Some less vulnerable fixed exchange regimes' country groups do exhibit first digit distributions for which the hypothesis that these are drawn from Benford's distribution cannot be rejected—for example for countries with positive fiscal balances (table 10) OECD countries as of 1989 (line 5 of tables XVIII to XIX in MSWP), countries with relatively low ratios of liabilities to GDP in the sample (not shown), or those with institutional investor ratings above 68 (not shown).[39] Also, the rejections of the null hypothesis for vulnerable countries with fixed exchange rate regimes (those more indebted or being more open to capital flows) are stronger than for fixed regimes that are not vulnerable (if the null for the latter is rejected at all). Another question thus arises: Can this be driven by less developed countries that, for example, score badly in terms of the quality of institutions?[40] This again seems unlikely, as we obtain increases in the strength of rejections (lower *p*-values, higher 1-in-10 criteria) when adding further conditions. In particular, we obtain the strongest rejections for countries with fixed regimes and most internationally open capital markets, typically a feature of more developed countries (see also section VG for further discussion and tests of this issue).

It thus appears that among countries with fixed exchange rate regimes, there might be some (but probably not all) that strategically misinform investors because the distribution of the first digits of the BoP data they report is unusual. This is true especially for countries that may face higher outflows due to the stock of liabilities they owe to the outside world. It becomes even more so for countries that are de facto open in

[39] According to Reinhart and Rogoff (2009), such countries are advanced economies that are credible debtors.

[40] When we split the countries at the median of their WGI scores, it turns out that fixed exchange rate regimes are adopted equally often by both groups created in this way. (See table XLVII in MSWP.)

Table 7.—Results for Finer Categories of Exchange Rate Regimes: Stable Sample

Economic Conditions and Type of Exchange Rate Regime	Number of Countries	Average Number of Quarters	Number of Observations	Global p-Value (in %)	1-in-10 Criterion
All observations					
Currency board arrangement	4	43.3	2,152	17.4	8.8*
Conventional peg	38	20.7	9,273	22.8	5.2
Crawling peg or band	16	21.4	4,063	0.6***	13.8***
Managed floating	32	27.8	10,370	8.6	8.4*
Independently floating	39	30.9	15,003	37.6	6.0
Quarters for Which EQ_LIAB_GDP > 0.0098					
Currency board arrangement	4	22.8	1,142	0.1***	[a]
Conventional peg	29	14.6	5,237	1.7**	6.1
Crawling peg or band	11	14.8	1,937	0.000003***	26.0***
Managed floating	21	21.5	5,392	0.5***	9.7**
Independently floating	31	35.1	13,631	77.9	4.5

The threshold values are given by quartiles (first or third) of the given quantities within the sample. Conventional peg: "conventional peg to a single currency or a composite or pegged exchange rate within horizontal bands." Managed floating: "managed floating with no predetermined path for the exchange rate." Significant at *5%, **2%, and ***1%.
[a] Not enough data.

terms of capital flows (and have higher equity to GDP liabilities than other countries). There is an economic rationale as to why this may be the case: in a fixed exchange rate regime, the aggregate information contained in the behavior of the exchange rate is missing, and investors need to rely more on the information provided by a country, which may want to misreport it in order to put itself in a more favorable light.

Subcategories of exchange rates. We report here further results of our tests among finer IMF exchange rate categories. These tests are less reliable, as we have fewer countries in each group, and persistence may be a larger problem. We present two studies in table 7 (see supplementary tables XXII to XXV in MSWP). First, we compare different exchange rate regimes. In the "all observations" section of table 7, we observe that Benford's law is rejected for the first digits of data in both chi-square and 1-in-10 tests from countries with crawling peg regimes. Using our 1-in-10 criterion, we also find rejections at a 5% level for countries with currency board and managed float regimes. This, however, may not be the best metric to measure countries with vulnerability of their exchange rate regime. For example, Cape Verde, a country with a conventional peg regime throughout our sample, has very little capital flow as measured by equity to GDP held by foreigners (a maximum of 0.0055% in the sample). Therefore, we additionally scrutinize countries that are de facto more open to capital flows (lower part of table 7) and therefore may be more vulnerable to any currency attacks or rapid capital movements. Interestingly, now we reject the Benford distribution for the first digits for all fixed exchange rate types and managed float regimes as well when using p-values from chi-square tests and all except conventional pegs using the 1-in-10 criterion.[41] This means that we get rejections for the countries that manage their currencies and are vulnerable to capital flows. This is also true for countries with currency board regimes—and these are the same four countries providing data as in the stable sample—but for different (shorter) time periods when they experience higher capital flows. On the full sample, for all fixed exchange rate regimes with higher capital flows, the null can be rejected at a 5% level in both chi-square tests and the tests based on the 1-in-10 criterion.

[41] For the currency board regimes, due to a lack of a sufficient number of observations, we could not calculate the 1-in-10 criterion. The 1-in-20 criterion that could be calculated strongly points to rejection at 1% level.

B. *Countries with High Negative Foreign Asset Liabilities*

In table 8 (see also tables XXVI to XXIX in MSWP) we show the p-values of tests for different centiles of the data when countries are ranked according to the ratios of net foreign assets excluding foreign direct investment (FDI) stocks to GDP for the stable sample. This is the most likely measure of the capital stock that could rapidly move out of a country; FDI flows, which are largely immobile in the short run, are excluded, and we take the net value of assets. We use the previous year's value for the current year quarters to group countries.

For the first digits of the BoP data for the 10% and 25% of countries with the worst net foreign asset position (excluding FDI) in the stable sample, the null hypothesis is rejected at the 5% significance level, with the 1-in-10 criterion confirming this (for the top 10% countries on the full sample). These results survive several robustness checks (shown in tables XXVIII to XXIX in MSWP). First, for the complement of the sets defined by our conditions, we cannot reject the null hypothesis of conformity to Benford's law. With the exclusion of one or more countries, we still reject Benford's distribution for the previously flagged categories as indicated by the stability indexes. In addition, we obtain a rejection of Benford's distribution for the first digits of the data for the 10% most indebted countries on the stable sample also for the less persistent series. The 1-in-20 and 1-in-5 indicators give the same indication as the 1-in-10 criterion.

We conclude that countries that have a high ratio of liquid indebtedness to GDP provided, in contrast to others, BoP data with an unusual distribution.

We do not get consistent and strong results in terms of the observed patterns for country groups created using other

TABLE 8.—RESULTS FOR COUNTRIES GROUPED BY NET FOREIGN ASSET LIABILITIES, EXCLUDING FOREIGN DIRECT INVESTMENT: MAIN RESULTS, STABLE SAMPLE

Economic Conditions: NFA_EXCL_FDI_GDP Quantile Level	Value	Number of Countries	Average Number of Quarters	Number of Observations	Global p-Value (in %)	1-in-10 Criterion
≤10% quantile	−0.581	18	19.3	4,003	0.003***	14.4***
>10% quantile	−0.581	64	47.1	36,580	31.8	5.6
≤20% quantile	−0.432	24	28.5	8,008	0.1***	10.0**
>20% quantile	−0.432	63	42.5	32,575	39.3	6
≤25% quantile	−0.379	30	28.5	10,086	0.1***	9.9**
>25% quantile	−0.379	62	40.4	30,497	40.7	7
≤50% quantile	−0.193	48	35.4	20,234	7.3	7.5
>50% quantile	−0.193	46	36.1	20,349	35.3	5.2
≤75% quantile	−0.02	59	43	30,480	3.2*	8.4*
>75% quantile	−0.02	30	27.5	10,103	37.8	5.2
≤80% quantile	0.015	60	45	32,494	6.7	7.7
> 80% quantile	0.015	29	22.8	8,089	52.1	5.5
≤90% quantile	0.144	63	48.1	36,527	8.5	6.9
>90% quantile	0.144	15	21.9	4,056	15.8	5.7

Significant at *5%, **2%, and ***1%.

TABLE 9.—RESULTS FOR COUNTRIES GROUPED BY THE SIGN OF THEIR CURRENT ACCOUNT BALANCES: MAIN RESULTS, STABLE SAMPLE

Economic Conditions						
Sign of the Current Account	Other Conditions	Number of Countries	Average Number of Quarters	Number of Observations	Global p-Value (in %)	1-in-10 Criterion
CA ≥ 0		61	20.6	15,360	15.6	5.2
CA < 0		63	34.3	25,849	0.2***	10.9***
[No condition]	Fixed exchange	49	26.6	15,488	0.5***	13.3***
CA ≥ 0	Fixed exchange	37	10.1	4,494	29.6	5
CA < 0	Fixed exchange	45	20.4	10,958	0.007***	15.6***
[No condition]	Floating exchange	39	30.9	15,003	37.6	5.8
CA ≥ 0	Floating exchange	31	16.7	6,590	69.8	5.6
CA < 0	Floating exchange	35	19.6	8,413	5.2	7.5
[No condition]	NFA_EXCL_FDI_GDP ≤–0.0201	59	43.0	30,480	3.2*	8.4*
CA ≥ 0	NFA_EXCL_FDI_GDP ≤–0.0201	48	17.5	10,230	46.7	4.1
CA < 0	NFA_EXCL_FDI_GDP ≤–0.0201	57	29.8	20,250	0.1***	11.5***
[No condition]	EQ_LIAB_GDP > 0.0098	51	43.5	27,339	72	4.1
CA ≥ 0	EQ_LIAB_GDP > 0.0098	43	22.9	12,228	40.5	4.9
CA < 0	EQ_LIAB_GDP > 0.0098	48	25.6	15,111	2.0*	9.2**

The threshold values are given by quartiles (first or third) of the given quantities within the sample. Significant at *5%, **2%, and ***1%.

measures of investment positions. In particular, for the full sample, we obtain rejections of Benford's distribution of the first digits also for countries with the highest net foreign asset positions and the lowest liabilities to GDP ratio, although we get similar results as for countries with net foreign assets (excluding FDI) for the stable sample. These series may be less pertinent for our tests. For example, the fact that a country has a high ratio of total liability to GDP, like Switzerland, may not be an indicator that it is vulnerable if it has high assets as well. It may also mean that we cannot condition the data well enough; for example, we do not control for many features, like differences in technology, that may affect the payoffs from misreporting. The data on liabilities may not be free of error, and these are data that we have on a yearly basis only. Therefore, if the data set heterogeneity is large, the picture may be blurred with these less precise measures.

C. *Countries with Negative Current Account Balances*

We report in table 9 (see also tables XXX to XXXIII in MSWP) the results of tests for countries with negative current account positions in a given quarter (labeled CA in the tables).

By taking into account the p-values and the 1-in-10 criterion, we observe that in both the stable (p-value of 0.2% and 1-in-10 criterion at a 1% significance level) and the full sample (p-value of 5.6% and 1-in-10 criterion indicating rejection at less than a 2% significance level), we obtain a rejection of the conformity of the first digits of the BoP data to Benford's distribution for countries that have negative current account positions in a given quarter. Sixty-three of 66 countries at some point in time have a negative current account figure (99 out of 103 for the full sample). This shows that the rejection of this condition is not due to country-specific data dissemination practices but rather to what is reported when the country in question is borrowing from the rest of the world in a quarter. The same is true when we select countries with the highest level of net indebtedness (with or without FDI), fixed exchange rates, or fiscal deficits (shown in table 10) that are running negative current accounts.

The robustness tests are presented in tables XXXII to XXXIII in MSWP. In general, they confirm our findings

TABLE 10.—RESULTS FOR COUNTRIES GROUPED BY THE SIGN OF THEIR FISCAL BALANCES: MAIN RESULTS, STABLE SAMPLE

Economic Conditions: Fiscal Balance	Economic Conditions: Other Conditions	Number of Countries	Average Number of Quarters	Number of Observations	Global p-Value (in %)	1-in-10 Criterion
FISC ≥ 0		39	23.4	11,163	1.8**	10.9***
FISC < 0		63	38.7	29,175	0.5***	12.0***
[No condition]	Fixed exchange	49	26.6	15,488	0.5***	13.3***
FISC ≥ 0	Fixed exchange	22	12.0	3,133	70.6	5.0
FISC < 0	Fixed exchange	47	20.9	11,694	0.02***	16.4***
[No condition]	Floating exchange	39	30.9	15,003	37.6	5.8
FISC ≥ 0	Floating exchange	18	22.5	5,061	27.0	6.8
FISC < 0	Floating exchange	35	22.6	9,854	15.5	7.1
[No condition]	CA < 0	63	34.3	25,849	0.2***	10.9***
FISC ≥ 0	CA < 0	35	12.8	5,458	16.7	6.1
FISC < 0	CA < 0	59	27.9	19,654	0.007***	17.5***
[No condition]	CA ≥ 0	61	20.6	15,360	15.6	5.2
FISC ≥ 0	CA ≥ 0	28	16.4	5,693	6.7	8.2*
FISC < 0	CA ≥ 0	57	13.7	9,497	13.4	7.2
[No condition]	NFA_EXCL_FDI_GDP ≤–0.0201	59	43.0	30,480	3.2*	8.4*
FISC ≥ 0	NFA_EXCL_FDI_GDP ≤–0.0201	33	17.0	6,897	1.0***	8.8*
FISC < 0	NFA_EXCL_FDI_GDP ≤–0.0201	53	35.9	22,766	2.5*	9.9**
[No condition]	EQ_LIAB_GDP > 0.0098	51	43.5	27,339	72.0	4.1
FISC ≥ 0	EQ_LIAB_GDP > 0.0098	29	25.0	8,982	6.2	7.4
FISC < 0	EQ_LIAB_GDP > 0.0098	46	31.8	18,037	7.4	8.1*

The threshold values are given by quartiles (first or third) of the given quantities within the sample. Significant at *5%, **2%, and ***1%.

with some caveats. For the main condition, negative current account position in a quarter, after excluding countries one-by-one from the sample we still always reject Benford's distribution for the first digits of the data for the stable sample. It seems that some additional conditioning (for example, choosing countries with high ratios of liabilities to GDP, with or without FDI accounted for) have in fact little importance in characterizing countries with unusual first-digit distributions, as we reject the null hypothesis for the complement of the set. The 1-in-5 and 1-in-20 criteria confirm the findings of the 1-in-10 criterion.

Overall, the rejections we find seem to indicate that countries requiring the financing of their borrowing (as their current account balances are negative) may be willing to misinform investors, especially among the countries in the stable sample.

D. *Countries with Fiscal Deficits*

We group countries also according to whether they had a positive general government fiscal balance in a given year (labeled FISC in the tables). Results are shown in table 10 (as well as tables XXXIV to XXXVII in MSWP).

The evidence here is weaker than in the case of countries grouped according to other economic criteria. Global p-values for country groups with either a positive or negative fiscal balance are below 5%. For the countries with positive balances, this result is, however, not confirmed for the full sample, and for the stable sample, the result is driven by two countries, exclusion of either of which removes the rejection. Because Benford's distribution is rejected for the complement of the sample as well, we do not conclude that a negative fiscal balance by a country itself is consistent with any strategic data manipulation. However, further investigation shows interesting patterns.

When we study the grouping of countries according to a fixed or floating exchange rate regime, we find a rejection of the Benford distribution of the first digits for the BoP data for countries with a negative fiscal balance and fixed exchange rates (a p-value of 0.02%, corroborated on the full sample and by our 1-in-10 criterion). In contrast, countries with a fixed exchange rate regime but reporting a budget surplus display a high goodness of fit to Benford's law (a p-value of 70.6%). We do not reject Benford's law for the first digits of data from countries with floating exchange rates either.

When we condition countries according to deficits or surpluses in their current account and fiscal balances (therefore creating four groups), we reject the Benford's law only for the group of countries with both a negative current account and a fiscal deficit (p-value of 0.007%).[42] This group contains the most countries and observations among the four possible groups according to this partitioning in the sample. The finding is confirmed for the full sample and by our 1-in-5, 1-in-10, and 1-in-20 conditions. Countries with twin deficits—in the current account and with negative fiscal balances—would be the most vulnerable among the four possible groupings, ceteris paribus: they would be in need of borrowing to cover the fiscal deficit and doing so (on the macroeconomic aggregate level) from international investors.

These results for the two groupings noted survive our robustness checks. The stability index is equal to 0, meaning

[42] For the group with positive fiscal and current account balances, the 1-in-10 criterion based on studying randomly selected subsets of the data points to a rejection at a 5% level with the similar pattern for the full sample. This is not confirmed by the p-value, so we do not consider it as a category for which we have a rejection.

TABLE 11.—RESULTS FOR COUNTRIES GROUPED BY WGI INDICATORS, ALL INDICATORS: MAIN RESULTS, STABLE SAMPLE

Indicator	Corruption		Government Effectiveness		Political Stability		Rule of Law	
Parsing According to the Indicator	Global p-Value (in %)	1-in-10 Criterion	Global p-Value (in %)	1-in-10 Criterion	Global p-Value (in %)	1-in-10 Criterion	Global p-Value (in %)	1-in-10 Criterion
Top 25%	92.5	5.6	86.7	4.0	22.6	6.8	77	3.5
Bottom 75%	14.4	5.7	11.9	6.6	2.0*	9.4**	7.6	6.8
Top 50%	88.7	4.4	79.1	3.6	92.2	4.3	51.5	5.7
Bottom 50%	0.6***	10.0**	11.5	7.0	0.6***	9.9**	1.2**	10.7***
Top 75%	66.3	4.7	64.4	5.4	64.8	4.9	82.2	4.1
Bottom 25%	5.6	9.1**	2.6*	8.9*	0.02***	14.8***	3.1*	8.1*

Significant at *5%, **2%, and ***1%.

that no one country is driving the rejections. The 1-in-5 and 1-in-20 criteria give the same indications as the 1-in-10 criterion. The results are corroborated on the full sample.

Although for either the global partition or among the 75% of countries with the lowest in-sample NFA (without FDI) we get consistent rejections for both groups of countries (with either positive or negative fiscal balances), we find two highly interesting patterns nevertheless: countries with negative fiscal balances and fixed exchange rate regimes or current account deficits display unnatural patterns in their first-digit data.

E. *Quality of Institutions and Data Provision*

We also study how institutional quality affects the behavior of the first digits of provided data. For this purpose, we use various WGI from the World Bank and report the results in table 11 (main results for all indicators, on the stable sample). Detailed tables for all WGI indicators along with the robustness checks are omitted from the main text but are available in MSWP.

Both the p-values from the goodness-of-fit tests and the 1-in-10 criterion indicate that countries ranked below the 50th percentile (and below the 25th as well) in the WGI data set in terms of corruption, political stability, and the rule of law and below the 25th percentile in terms of government effectiveness (in terms of the global WGI data set) exhibit BoP data with first digits that have non-Benford behavior. It thus appears that countries with poor institutions provide data of low quality. This is a finding similar to Nye and Moul (2007), who show that in samples of economic data from the Penn World Tables for non-OECD countries, the hypothesis of Benford's distribution of the first digits is rejected (but remember that for our BOPS data, this is not the case for the entire group of non-OECD countries, as witnessed by table 3). For rankings in terms of voice and accountability and regulatory quality, we obtained no consistent and interesting results.

For all the categories flagged above, the rejection of Benford's distribution for the complement of the set (that is, for countries with better institutional rankings) of the first digits fails. The exclusion of one of the countries from the samples does not drive the results mentioned above, except for the lower 25th percentile of government effectiveness in the stable sample. For tests run on less persistent series, we can reject Benford's distribution at the 10% significance level for the countries ranking in the lowest 25% in terms of government effectiveness and political stability for both samples and for those ranking most corrupt for the stable sample; the evidence based on these is weak. The 1-in-20 and 1-in-5 criteria give rejections for the same categories, albeit sometimes at different conventionally accepted significance levels, as the 1-in-10 criterion.

Our results lead us to believe that countries with poor institutions produce BoP data with irregular first digit behavior. However, we do not have a story about why such countries would manipulate the data. The answer may be that it is not due to poor data collection procedures or methodologies.[43] First, even high measurement errors should in practice cause the data to have first digits obeying Benford's law if the errors are well behaved, in view of property P1 (see section IVA, as well as the example described in the next section). Second, when we look at the tests based on the less persistent series, there is little evidence that countries with poor institutions have odd first-digit distributions. If the rejection of Benford's distribution were caused by poor data collection, the first-digit distribution of the less persistent series should be also affected. Thus, a simple explanation for the rejections for these groups of countries may be that their data exhibit persistence. Another story may be that the institutions in these countries have lower scruples or fewer control mechanisms (so that they rightly are ranked as having bad institutional quality) that prevent data falsification. A different point is that such countries may find themselves more likely in a situation that encourages misreporting. For example, as witnessed by table XLVII in MSWP, countries with poor institutions in our sample typically are more indebted to foreigners, adopt floating regimes less often, and are more likely to have negative current account and fiscal balances.

F. *Are Poor Countries with Bad Statistical Agencies Driving Our Main Results?*

A valid question in this context is whether our main findings in sections VB to VE are driven by bad data collection

[43] Gonzalez-Garcia and Pastor (2009) compare the quality rankings of data collection methods of various countries with the rejections of the Benford's distributions of their macroeconomic series, finding no consistent pattern between the two.

TABLE 12.—STABILITY OF THE FINDINGS WITH RESPECT TO COUNTRIES WITH BAD STATISTICAL AGENCIES: MAIN RESULTS, STABLE SAMPLE

Economic Conditions	Number of Countries	Average Number of Quarters	Number of Observations	Global p-Value (in %)	1-in-10 Criterion
Floating exchange	39	30.9	15,003	37.6	5.8
Same condition without countries in the lowest 25% of GE	37	30.8	14,245	46.1	5.8
Fixed exchange	49	26.6	15,488	0.5***	13.3***
Same condition without countries in the lowest 25% of GE	46	22.5	12,464	0.8***	9.9**
Floating exchange and EQ_LIAB_GDP > 0.0098	31	35.1	13,631	77.9	3.6
Same conditions without countries in the lowest 25% of GE	31	33.8	13,142	85.6	4.9
Fixed exchange and EQ_LIAB_GDP > 0.0098	38	17.8	8,316	0.00004***	21.7***
Same conditions without countries in the lowest 25% of GE	36	16.0	7,129	0.0002***	18.4***
NFA_EXCL_FDI_GDP > – 0.3793	62	40.4	30,497	40.7	7
Same condition without countries in the lowest 25% of GE	58	37.0	26,332	42.0	5.9
NFA_EXCL_FDI_GDP ≤ – 0.3793	30	28.5	10,086	0.1***	9.9**
Same condition without countries in the lowest 25% of GE	26	22.5	7,121	4.0*	6.2
CA ≥ 0	61	20.6	15,360	15.6	5.2
Same condition without countries in the lowest 25% of GE	55	19.1	12,991	37.4	5.8
CA < 0	63	34.3	25,849	0.2***	10.9***
Same condition without countries in the lowest 25% of GE	60	28.8	20,960	6.7	8.1*
FISC ≥ 0	39	23.4	11,163	1.8**	10.9***
Same condition without countries in the lowest 25% of GE	34	23.1	9,708	44.9	4.6
FISC < 0	63	38.7	29,175	0.5***	12.0***
Same condition without countries in the lowest 25% of GE	60	32.7	23,877	26.0	5.2

The statistics are computed for each set of economic conditions with and without countries in the lowest 25% of government effectivement (GE). The threshold values are given by quartiles (first or third) of the given quantities within the sample. Significant at *5%, **2%, and ***1%.

procedures employed in some countries. Moreover, Young (2009) claims that many entries for data in the UN or World Bank data sets, for example, for sub-Saharan countries, for growth or price-level data are not based on actual measurements by governments but on extrapolations or from other sources. Hence, the data that we might be working with could be subject to errors during collection or be actually ad hoc or artificially created by various agencies.

We are not aware of how grave the problem of mismeasurement of BoP entries is for the countries included in our sample and are not aware of the IMF's extrapolating the BoP data that are obtained from monetary and statistical authorities of countries.[44] We note that the data we use are available for only 103 countries for the period 1989 to 2007, and for many countries (and data series) there are gaps. We have fourteen sub-Saharan countries (including South Africa) in the full sample but only four (including South Africa) in the stable sample. If we look at all WGI indicators characterizing the countries in the data, we have countries that boast on average governance scores that are higher than the means for the entire WGI data sets (which contains for 2007 up to 215 entries). For example (as witnessed by table 2), for the most relevant WGI indicator in this respect, government effectiveness, the average rank value for the full sample is 60 and for the stable sample 63.5 (whereas in the WGI data set, it is by construction 50).

Theoretically, errors in measurement should not matter for our results as long as they can be represented by an appropriate stochastic process (see section IVA). For example, if the goods balance is increasing exponentially but its multiplicative variations are measured with independent errors that are identically distributed (possibly according to a distribution that is centered around 1 or according to positively or negatively skewed distributions with respect to 1, which could account for repeated over- or underestimation of some quantities), the series should adhere to Benford's law if enough data points are observed.[45]

In order to address the issue empirically, we reran our basic results excluding sub-Saharan countries or those with bad governance scores. We present the results for the most relevant categories in table 12 for the stable sample (see also tables XLVIII to LI in MSWP). We see that our basic result on the behavior of the first digits of the BoP statistics for fixed versus floating regimes does not change, and the strength of the rejection of the null hypothesis for the fixed regimes is similar when we exclude only sub-Saharan countries (keeping South Africa) on the basis that these countries might not have developed capabilities for clean data collection and when we exclude countries ranked in the lowest 25% in terms of government effectiveness in the WGI data. If we look at some additional conditioning, considering, for example, the exchange rate regime and the openness to equity flows, again the picture is the same. In this case, we are not surprised: very few countries with bad governance have high equity liabilities and also fixed exchange rates (the average government effectiveness rank for countries with a fixed exchange rate in the stable sample and equity liabilities/GDP> 0.0098 is 70.8). The results are confirmed for the stability index for these results (0), as well as if we look at only the less persistent series. Again we obtain a rejection for the fixed exchange rate regimes but not for floating ones. If we exclude sub-Saharan countries from the group of countries that have the lowest net

[44] Curcuru, Thomas, and Warnock (2009) argue that the mismeasurement of the BoP entries even for the United States is severe.

[45] Section C.2.1 in MSWP indicates, however, that these assumptions of independence or identical distribution can be strongly relaxed; more general random errors can thus be considered.

foreign assets to GDP ratio (excluding foreign direct investment liabilities), the rejection patterns are confirmed. The evidence is weaker when we exclude from the same group the countries with the lowest government effectiveness, but we still obtain a rejection at 5% when we scrutinize the p-value. For the countries with a negative current account in a given quarter, the null hypothesis is rejected at 0.1% level when we exclude sub-Saharan countries and at 6.7% when we exclude those with the lowest government effectiveness scores. The results for countries grouped by fiscal balances are the only ones for which we cannot obtain rejections after countries with poor institutional scores are dropped from the sample. Nevertheless, for the groupings of interest for which we claimed rejections in section VE—countries with negative fiscal balances and fixed exchange rates or countries with twin deficits—we still reject the Benford distribution of the first digits of the data when we exclude countries ranked in either the lowest 25% in terms of government effectiveness or the sub-Saharan countries (except South Africa) for the stable sample at least a 5% level (results not shown).

Based on our discussion, we do not believe that our results are driven by poor countries with bad data collection procedures.

G. *Other Conditions for Defining the Subsets of the Data*

We have tried other conditioning of the data in our search of unusual reporting patterns.[46] First, we studied (akin to section VC) whether countries with more capital openness (defined as high ratios of foreign equity liabilities to GDP) exhibit non-Benford distribution of their first digits of the BoP data. Scrutinizing the tails of data sorted according to this measure, we find no results that this is the case.

Next, for countries that joined the euro zone, we inspected the period in which they had to maintain a fixed exchange rate regime and satisfy certain economic performance criteria for admission. We do not find any evidence that the BoP data are unusual for up to five years prior to joining the zone. However, interestingly, we find that the Benford distribution is rejected for the first digits of the BoP statistics for these countries after joining the euro zone. For the full sample, the p-value is 0.08% and the 1-in-10 criterion 14.6 (both statistically significant at 1% level) with 4,623 observations and 11 countries (Belgium and Luxembourg are not in our data set). For the stable sample, the p-value is 0.18% with a 1-in-10 criterion of 14.1 (4,259 observations and 10 countries). There are two explanations in the context of our model. First, adherence to the common currency caused capital flows barriers to fall. Second, the cost of leaving the euro zone is high, so the fear of a substantial attack that would force a country to leave the joint currency arrangement might induce countries to misreport the statistics more often.

We also investigated countries with episodes of sovereign default and banking crises (data on their timing obtained from Reinhart & Rogoff, 2009). For countries that defaulted on their sovereign debt within our sample period, we do not find rejections of the null hypothesis for one-, two-, or three-year brackets preceding a default (results shown in table LIII in MSWP). The first digits of the statistics released by countries three, five, ten, and twenty years after default appear to conform with the Benford distribution. This is consistent with investors' threatening a more severe punishment for future default (or misinformation) but also with lower capital flows after a default occurs. After default, a country's actions may also be scrutinized more closely by investors or the IMF. Note that default for the countries in our sample is observed on average for less developed countries with worse governance scores (for example, the mean government effectiveness WGI ranking among countries five years after default is 31, much lower than the average of 60 for the entire sample). This favors our strategic argument and is against the main competing explanation that countries with poor institutions are responsible for our results. However, we find evidence that within one year after enduring banking crises, countries report suspicious distributions of the BoP data. For these, we can reject the hypothesis that the first digits conform to Benford's distribution at 0.2% with 5,094 observations on the full sample and 5.6% for the stable sample (2,487 observations).

There seems to be no evidence that countries suffering substantial falls in their institutional investor country credit ratings, either contemporaneous, lead or lagged, offer suspect statistics.

We found no strong results for countries grouped by the level of their year-to-year inflation, though our 1-in-10 criteria reject the Benford distribution for the first digits of statistics from countries with an inflation rate higher than 10% for both the stable and full samples at a 2% level.

We inspect our data for main categories on a year-by-year basis in section C.4.1 in MSWP. We find rejections of the Benford distribution for many vulnerable country groupings for 1996, 1997, and 2006. For 1997, one can rationalize the finding in favor of proposition 5: an unexpected global crisis put many countries in a position where they misreported their data. We do not know, however, what investors' expectations were in 1997 (and for the other years as well), so we do not draw any conclusions from this exercise.

H. *Summary of the Empirical Results*

We find results consistent with the hypothesis that countries strategically provide manipulated data to economic agents. We observe rejections of Benford's distribution for the

[46] For geographically based country groupings (see section C.4.2 in MSWP), we rely on the 1-in-10 criterion as the persistence issue may be severe. We find that countries in Africa and the Middle East (for both the full and stable samples), Latin America, and Eastern Europe (for the stable sample) have suspect distributions of the first digits. This should be taken with caution, as there are relatively few countries in each group with many quarters of data each, which may make rejecting the null hypothesis easier (tests on the least persistent series do not confirm any of the findings). There may be also too little heterogeneity for property P2 to be able to compensate for the possible persistence issues.

first digits of data issued by groups of countries that are more vulnerable to high capital outflows. From the main results shown in tables 6, 8, 9, and 10, we find such a pattern in ten out of thirteen groupings with the three inconclusive ones involving partitions using fiscal balances. These rejections are rather category than country specific, as data from many countries for different quarters enter different categories. We obtain the strongest (also most robust) results for countries with fixed exchange rate regimes, though we also find that countries with the highest levels of net indebtedness and those that were running current account deficits have first digits that have unusual, non-Benford distributions. The Benford distribution is rejected for the first digits of the BoP statistics for euro-adopting countries after these join the euro zone. These findings confirm the viability of the intuition developed by the simple model in section II. However, we do not find general results for countries that were de facto more open to capital flows. This may be due to the fact that we are unable to capture the extent of their vulnerability very finely; but for some subgroups of countries (for example, those with fixed exchange rate regimes and relatively high de facto openness), we still get very strong rejections of the null hypothesis. The interesting finding on the fixed exchange rate regime countries may show that the public is more readily misinformed by governments when there is a larger scope for misinformation—for example, when the market prices aggregating private information are missing. We find also evidence that countries with weak institutions provide data that are non-Benford distributed; moreover, it seems that they are not driving our main results in sections VA to VD.

VI. Conclusion

In this paper we took a glimpse at the dark side of the moon of government statistics. We conclude that country-quarter pairs that correspond to economic situations in which countries would have higher incentives to misinform investors lead to different distributions of the corresponding first digits of the balance-of-payments data than Benford's distribution, while for other country-quarter pairs, the data conform to this distribution. This may be because there is a different underlying process that rules the generation of balance-of-payments distributions in country-quarter pairs for vulnerable countries, which we think is unlikely. It may be simply that such countries at times falsify their balance-of-payments data in these quarters. This partial evidence on the strategic character of misreporting points out that models in which governments emit public signals (for example, the discussion on central bank transparency) should seriously consider the possibility that this signal may be intentionally misleading at times. On the policy side, this paper calls for the need to establish independent statistical agencies akin to that of independent central bankers. Some countries that failed in having such an independent agency, like Argentina and Greece, were caught red-handed in altering economic data that they disseminated to the public. The possible welfare implications of misinformation should also be investigated; the actions of Argentina and Greece show that economic gains to governments, even if short run, may exist and be substantial. In further work, we would like to extend our analysis to fiscal data from many data sets.

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APPENDIX A

A. Incentives to Misreport Economic Data: Proofs of the Stated Results

We provide in this appendix the proofs of the results stated in section II.

A.1. Existence and Properties of the Equilibrium with Lying

Investors wish to maximize their ex ante return from investment; hence, they will allocate capital $\overline{K}$ so that the marginal return from investment in all countries is equalized, no matter whether they declare a good or bad state of the world. This leads, on the one hand (when $C_i \equiv C$ for all countries i), to the stated fact that the capital $K_i(s_i, \tilde\rho)$ invested in country i depends not on i but solely on s_i and $\tilde\rho$, and, on the other hand, that returns are equal for countries declaring good or bad states, as in equation (3). As $\tilde\rho \geq \hat\rho$, investment return in countries that declare a good state of the world is lower than if all these countries would in fact be enjoying a good state. Equality (3) needs to be a feature of any efficient allocation; then investors are indifferent between investing marginally more in one group of countries than in the other. Note also that equation (3) indicates that the ratio Ξ of $K(g, \tilde\rho)$ to $K(b, \tilde\rho)$ equals, at equilibrium,

$$\Xi(\tilde\rho, \varphi) = \frac{K(b, \tilde\rho)}{K(g, \tilde\rho)} = \left(\frac{\hat\rho A(g) + \varphi(1-\hat\rho)A(b)}{\tilde\rho A(b)}\right)^{1/(\alpha-1)} = \left(1 - \frac{\hat\rho\big(A(g) - A(b)\big)}{\hat\rho A(g) + \varphi(1-\hat\rho)A(b)}\right)^{1/(1-\alpha)}, \tag{A1}$$

with $\Xi(\tilde\rho, \varphi) < 1$. Given this and the global mobile capital supply $\overline{K}$, we can find the capital values $K(b, \tilde\rho)$ and $K(b, \tilde\rho)$, the stock of capital in countries declaring a bad and good state, respectively. Indeed, the market clearing condition in the capital market is

$$\int_{[0,1]} \big(K(b, \tilde\rho) - C\big)\, \mathbb{I}_{\{d_i=b\}}\, \mathrm{d}i + \int_{[0,1]} \big(K(g, \tilde\rho) - C\big)\, \mathbb{I}_{\{d_i=g\}}\, \mathrm{d}i = (1-\tilde\rho)K(b, \tilde\rho) + \tilde\rho K(g, \hat\rho) - C = \overline{K}\,.$$

After some algebra and by substituting the equality $\tilde\rho = \hat\rho + \varphi(1-\hat\rho)$, we can show that

$$K(g, \hat\rho) = \frac{\overline{K} + C}{\hat\rho + (1-\hat\rho)(\varphi + \Xi - \varphi\Xi)}\,.$$

We now show that $\varphi + \Xi - \varphi\Xi$ increases as φ increases, which implies that $K(g, \hat\rho)$ then decreases; the derivative of the considered expression with respect to φ indeed equals $(1-\Xi) + (1-\varphi)\,\partial\Xi/\partial\varphi$, where both summands are positive as $\Xi < 1$ and as $\Xi(\tilde\rho, \varphi)$ is increasing in φ, as can be seen from its closed-form expression. The interpretation is that as the fraction φ of liars among countries with $s_i = b$ increases, the capital invested in countries declaring a high state of the world declines; this is because the return from investment in these countries falls.

Countries. At equilibrium, in the interior when $\varphi \in (0, 1)$, countries are indifferent between lying and reporting truthfully the state of the world when their state is $s_i = b$. When making their decision, they do not know $\hat\rho$; hence, their strategies depend on the information they have—that is, on their own state of the world and the knowledge of the distribution of ρ. The first step is to analyze the payoffs of countries given in equation (2). Since we showed that the functions K_i giving the invested capital did not depend on i, the same is true for the return functions R_i, which we will simply denote by R in the sequel (the same holds for the expected returns ER). A country that has a good state of the world $s_m = g$ will not declare a bad state of the world; it would not only receive less capital from investors (as these make decisions based on declarations only and $K(g, \tilde\rho) < K(b, \tilde\rho)$), but also be punished. In other words, $R(b, g, \tilde\rho) - R(g, g, \tilde\rho) < 0$. To analyze the incentive of countries with a bad state of the world, we introduce the gap $\Delta(\tilde\rho) = R(b, g, \tilde\rho) - R(b, b, \tilde\rho)$. Because of the antagonist effects of the punishment and the increase of invested capital when lying, this gap can in principle be positive, negative, or null.

Lemma 6. *The gap in payoffs $\Delta(\tilde\rho)$ is strictly decreasing in φ, that is, decreases as the proportion φ of liars is higher.*

Proof. By definition of Ξ and by equation (2), one gets that

$$\Delta(\tilde\rho) = A(b)\Big((1-\alpha)\,\big(K(g, \tilde\rho)\big)^{\alpha}(1-\Xi^{\alpha}) + \alpha\, C\,\big(K(g, \tilde\rho)\big)^{\alpha-1}(1-\Xi^{\alpha-1})\Big).$$

But since Ξ increases as φ increases (as can be seen from its closed-form expression) and since the above expression of Δ shows that it is a decreasing function of Ξ, the conclusion follows.

The higher the φ, the more liars given any $\hat{\rho}$, and the lower the capital that is invested in countries declaring g. Hence, there is a lower incentive to lie when the country in fact has a true state b. The following result is immediate by taking expectations (with respect to $\tilde{\rho}$) in the lemma.

Proposition 6. *The expected gap $ER(b,g) - ER(b,b)$ is strictly decreasing in φ.*

In particular, the mapping $\varphi \mapsto ER(b,g) - ER(b,b)$ is one-to-one. If there exists a $\varphi \in (0,1)$ such that $ER(b,g) - ER(b,b) = \Phi$, then this φ is unique, and the investors can deduce it from the value of Φ. In particular, they are then able to precisely deduce the value of $\hat{\rho}$ given the fraction of countries $\tilde{\rho}$ declaring a good state of the world. Since the strategies of countries do not depend on $\hat{\rho}$, unknown to the countries when making the declarations, and since $\tilde{\rho} = \hat{\rho} + \varphi(1-\hat{\rho})$, the investors can learn the true $\hat{\rho} = (\tilde{\rho}-\varphi)/(1-\varphi)$. If $\varphi \in (0,1)$, then countries with $s_m = b$ are indifferent between declaring truthfully and misreporting. If $\varphi = 1$, then all countries with $s_m = b$ find it more profitable to lie than to declare truthfully despite the expected punishment Φ. If $\varphi = 0$, then all countries declare truthfully. This is the case whenever Φ is large enough (larger than the expected gap in payoffs). All in all, we showed that countries have the correct incentives given Φ and the parameters of the environment (the stock of mobile capital, productivity differences between the good and the bad state, and so on).

Proof of Proposition 1. When $\varphi \in (0,1)$ at equilibrium, one has $ER(b,g) - ER(b,b) = \Phi$. But proposition 6 precisely stated that $\varphi \mapsto ER(b,g) - ER(b,b)$ is decreasing, so the inverse mapping $\Phi \mapsto \varphi$ of what happens at equilibrium is decreasing as well.

A.2. Comparative Statics: Proofs of Propositions 2–5.

The simplest proof is of proposition 5, since this result does not rely on partitioning the countries into two groups.

Proof of Proposition 5. Countries make the choice whether to report g or b based on the properties of the distribution of ρ, as at the moment of making the decision, they do not know the actual fraction $\hat{\rho}$ of countries enjoying a good state. Hence, at a given equilibrium (for a given value of φ), a constant fraction φ of those that obtained a b state lies and the fraction of liars in the entire population is $\varphi(1-\hat{\rho})$, which is decreasing in $\hat{\rho}$ as stated.

The framework for all other propositions to be proved is to split countries into two groups, with respective proportions ξ and $1-\xi$, for which it is easy to see—following the same lines as above—that at equilibrium, they share the same characteristics. This defines a common invested capital function $\underline{K}$ or $\overline{K}$ in each group, a common expected return function $\underline{ER}$ or $\overline{ER}$, a common cheating rate $\underline{\varphi}$ or $\overline{\varphi}$, and so forth. The observed proportion of countries declaring a good state is now

$$\tilde{\rho} = \hat{\rho} + (1-\hat{\rho})\big(\xi\underline{\varphi} + (1-\xi)\overline{\varphi}\big).$$

The respective proportions of countries not truthfully declaring their states in each group are given by

$$\frac{(1-\hat{\rho})\underline{\varphi}}{\hat{\rho} + (1-\hat{\rho})\underline{\varphi}} \quad \text{and} \quad \frac{(1-\hat{\rho})\overline{\varphi}}{\hat{\rho} + (1-\hat{\rho})\overline{\varphi}}.$$

As above, at equilibrium, investors can determine $\underline{\varphi}$ and $\overline{\varphi}$ and infer $\hat{\rho}$ from $\tilde{\rho}$.

Proof of Proposition 2. We are interested in the behavior of the countries when there is lying in equilibrium, that is, when $\underline{\varphi}$, $\overline{\varphi} \in (0,1)$ at equilibrium. In this case, as follows from an immediate adaptation of the arguments above in each of the groups, $\underline{ER}(b,g) - \underline{ER}(b,b) = \Phi$ and $\overline{ER}(b,g) - \overline{ER}(b,b) = \Phi$, where each gap is still a decreasing function of the respective proportions $\underline{\varphi}$ and $\overline{\varphi}$ of liars (see proposition 6). The conclusion follows.

The proofs of the last two propositions are relegated due to their length to MSWP, section C.1; We provide here only a sketch of how these are constructed.

Sketch of proof of Proposition 3. In any equilibrium, investors demand that investment bring ex ante the same return at the margin in either group of countries, regardless of the declared state of the world. We use this condition and find that the following must be true: $\overline{\varphi} < \underline{\varphi}$ if and only if $\overline{K}(g,\tilde{\rho}) > \underline{K}(g,\tilde{\rho})$ for at least one realization $\hat{\rho}$.[47] To show that indeed $\overline{K}(g,\tilde{\rho}) > \underline{K}(g,\tilde{\rho})$ we use that for equilibria in the interior, $\underline{ER}(b,g) - \underline{ER}(b,b) = \Phi$ and $\overline{ER}(b,g) - \overline{ER}(b,b) = \Phi$ must hold, take a difference of the two conditions and substitute.

Sketch of Proof of Proposition 4. We use a similar line of reasoning as in the above proof. From the ex ante return equalization on any group of countries, it can be shown that the stated $\overline{\varphi} < \underline{\varphi}$ holds if and only if $\overline{K}(b,\tilde{\rho})/\underline{K}(b,\tilde{\rho}) < \overline{K}(g,\tilde{\rho})/\underline{K}(g,\tilde{\rho})$ for at least one realization $\hat{\rho}$. We use the same property of the interior equilibrium $\underline{ER}(b,g) - \underline{ER}(b,b) = \Phi$ and $\overline{ER}(b,g) - \overline{ER}(b,b) = \Phi$ to show that the right-hand side of the equivalency condition holds.

[47] It can be shown that it will then hold for all realizations of $\hat{\rho}$.