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The "Thin film of gold": monetary rules and policy credibility

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We ask whether developing countries reap credibility gains from submitting policy to a strict monetary rule. We look at the gold standard era, 1880–1914, to test whether adoption of a rule-based monetary framework such as the gold standard increased policy credibility, focusing on sixty independent and colonial borrowers in the London market. We challenge the traditional view that gold standard adherence was a credible commitment mechanism rewarded by financial markets with lower borrowing costs. We demonstrate that for the poor periphery—where policy credibility is a particularly acute problem—the market looked behind "the thin film of gold".

Monetary policy can be used to achieve short-term objectives such as boosting economic activity, lowering unemployment or financing budget deficits. Yet if the market anticipates policy makers' temptation to resort to surprise actions and doubts the commitment to sound policies, it will demand to be compensated for this risk. In an environment of low policy credibility, interest rates will be higher than they would otherwise be. This is a short version of the well-known time-inconsistency theorem of monetary policy first pointed out by Kydland and Prescott (1977) and elaborated by Barro and Gordon (1983). On the other hand, if policy credibility is high, the expectations of the public will remain well anchored even in the face of adverse conditions as markets do not expect policy makers to put medium-run macroeconomic stability at risk. Achieving and maintaining policy credibility is hence a crucial task for monetary policy-makers around the world.

While the advantages of credibility are evident, it is much less clear how credibility can be built in developing countries whose economies are often subject to shocks and have little reputational track record to build credibility on. A theoretically appealing solution in such cases is to pre-commit policy to a binding rule. Such a policy rule ties the hands of politicians who might otherwise be prone to use monetary policy for short-run gains in output or to run excessive fiscal deficits. For example, the adoption of a currency board or a comparable rule-based framework would reduce the risks faced by investors, encouraging them to lend more at lower rates. Policy-makers in the developing world could thus enhance credibility in the eyes of international markets by importing stability from abroad (Chang and Velasco 2000). In the past decade, developing countries have often been advised to opt for a "hard peg" for this reason (Calvo and Mishkin 2003).

Did the classical gold standard during the first era of financial globalization, between 1880 and 1914, work as such a binding policy rule? One key implication of the interpretation of the gold standard as a policy rule was that countries that adopted the gold standard should, *ceteris paribus*, have been charged lower interest rates in the international

capital market because of the credibility effects conferred by gold standard adherence. An empirical study by Obstfeld and Taylor summarized the case for the gold standard as a successful policy rule before 1914:

> Gold was apparently a good enough seal of approval by itself, and risk was priced without much reference to public debt levels, the terms of trade, or whether the country was part of the British Empire. (Obstfeld and Taylor 2003, p. 260)

Obstfeld and Taylor confirmed the results of the well-known study by Bordo and Rockoff (1996). Their key finding was that the market considered adherence to the gold standard as a sign of financial rectitude—a credible commitment to "good [financial] housekeeping"—and charged lower risk premia on the foreign loans of gold standard countries than on the loans of countries not on gold—a result that supported the interpretation of the gold standard as a successful policy rule (Bordo and Kydland 1996). Lower risk premia in turn went hand in hand with massive capital flows from the center to the periphery (Obstfeld and Taylor 2004; Schularick 2006).

However, until now the empirical justification for rule-based solutions to credibility problems comes mainly from economic history. Contemporary tests of the credibility effects of hard pegs have brought mixed results. While Carlson and Valev (2001) underline the positive effects, other authors have pointed out that the effects vary according to political and economic conditions (Guidotti and Vegh 1999; Mulino 2002; De la Torre et al. 2003; Blomberg et al. 2005).

Using a comprehensive data set for the 1880–1914 period, this paper tests the hypothesis that the gold standard conferred the benefits of a rule-bound monetary policy. Our emphasis is on developing countries, where credibility problems tend to be most acute and where policy advice is most problematic. The present paper hence relates closely to two strands of the recent literature in international economics. First, we aim to contribute to the debate on the credibility of hard currency pegs in developing countries (Carlson and Valev 2001; Blomberg et al. 2005; Feuerstein and Grimm 2006). Second, we reconsider the role of the gold standard and other factors as drivers of financial globalization and, in particular, the massive capital flows from rich to poor economies before 1914 (Ferguson 2003a,b; Clemens and Williamson 2004; Flandreau and Zumer 2004; Mitchener and Weidenmier 2005; Ferguson and Schularick 2006; Schularick and Steger 2010). Our central question is: Did the gold standard indeed work as a monetary policy rule with significant positive effects on policy credibility in the periphery?

The literature on country risk premia under the gold standard has flourished in recent years. The pioneering econometric study of bond spreads in the pre–World War I capital market comes from Bordo and Rockoff (1996). Their claim that gold standard adherence was equivalent to a "good housekeep seal of approval" was confirmed by Obstfeld and Taylor (2003). Yet these findings have not gone unchallenged. Sussman and Yafeh (2001), Ferguson (2003b), Flandreau and Zumer (2004), and Ferguson and Schularick (2006) have all raised doubts about the importance of gold adherence for country risk premia. Alquist and Chabot (2011) have used high-frequency data to test return differences between gold and non-gold bonds, but have found none. Mauro et al. (2006) and Mitchener and Weidenmier (2005, 2010) have underlined the importance of political

factors in the pricing of sovereign debt at the time. Other recent contributions show that the interest in the operations of the pre–World War I bond market is not subsiding.[1]

We follow previous studies by looking for credibility effects in country risk premia in the international market before and after adoption of the gold standard.[2] We regress country risk premia on a number of economic and political indicators for creditworthiness and look to a gold standard variable to determine whether gold adoption had statistically significant effects on the risk perception of international investors. The key contribution of the paper is to show that, whereas a small positive effect is visible in developed economies, poor countries' risk premia did not fall after they went on gold.

We have assembled a comprehensive database covering almost the entire universe of developing country borrowing in the London capital market before 1914. The data set, which was assembled by hand from contemporary publications, covers interest rates and economic control variables for close to sixty borrowers in the London capital market between 1880 and 1913. In contrast to previous data sets that focused on the narrower Atlantic economy, ours allows us to pay special attention to the experience of developing countries (as defined by their relative state of economic development). Using this data set, we reconsider, integrate and move beyond the methodology of previous studies. We are able to reproduce some earlier results, but also cover two neglected issues. We single out countries that moved from a paper standard to gold, as opposed to "switching" from silver (or bimetallism) to gold. While ensuring comparability of our econometric approach with previous studies, we also propose to move beyond standard static panel models and estimate a dynamic system generalized methods of moment (GMM) model, which allows us to model risk perception as a Bayesian updating of country risk while taking account of the potential endogeneity of some regressors.[3]

Our investigation proceeds in four steps. The first section reviews the original hypothesis that adherence to the gold standard worked as a credible commitment mechanism and describes the criticisms it has encountered. Section 2 presents our data set and introduces the estimation strategy. Section 3 contains the empirical heart of this paper. We first reproduce the findings of previous studies, before moving on to new estimations for our complete sample and for various subsamples of developed and developing countries. Our findings here suggest that the market's assessment of gold standard adherence varied between country groups. Most importantly, there is little evidence that the gold standard offered a short-cut to policy credibility for the poor periphery. While we confirm a modest gold effect in the core economies, in developing countries the market looked behind "the thin film of gold".[4] Section 4 asks why gold standard membership did not work as credible commitment mechanism in the periphery. We argue that in view of the economic and political instability of poor

[1] See Mitchener and Weidenmier (2009), Accominotti et al. (2010), Dilley (2010), Schiltz (2011), Tooze and Ivanov (2011).

[2] An analysis of the interest rate spread between foreign and local currency bonds can be found in Mitchener and Weidenmier (2006). Their results suggest that local and foreign rates did not converge after gold adoption, hence currency risk remained prominent. However, the assumption of identical default risk on local and international debt is debatable.

[3] An exception here are Obstfeld and Taylor (2003) who estimated a model using the Arellano–Bond difference estimator, whereas we propose the newer system estimator by Arellano and Bover (1995) who alleviates some of the shortcomings with regard to instrument validity using the difference estimator.

[4] The phrase is J.H. Clapham's, quoted in Sayers, *Bank of England*, vol. I, p. 9. Clapham was referring to the small size of the Bank of England's gold reserve, but the phrase is suggestive of a wider point, namely that the gold standard's credibility depended on much more than formal commitments by monetary authorities.

economies the market anticipated that the rule was not likely to be sustained in view of adverse economic and political circumstances. Section 5 concludes that earlier doubts on the credibility effect of the hard currency peg expressed, *inter alia*, by Eichengreen and Hausman (1999), Ferguson (2003b), Flandreau and Zumer (2004), Ferguson and Schularick (2006) and Mitchener and Weidenmier (2006, 2010) were partly justified. The explanation for the low risk premia charged to developing borrowers in the sovereign bond market before World War I must go beyond monetary commitments to include not just economic fundamentals but also the political determinants of creditworthiness.

1. The "good housekeeping" hypothesis and its critics

The degree of international financial integration reached before 1914 was truly impressive. In the decades before World War I, Britain exported on average between 4 and 5 percent of her gross domestic product (GDP) abroad, while capital-importing economies could run current account deficits of even higher magnitudes for many years, even decades. Foreign investments in relation to GDP in 1913 stood at about 200 percent in Argentina, Chile and South Africa, and at or above 100 percent in countries such as Brazil, Mexico, Egypt, and Malaysia—about twice as high as the corresponding figures today (Twomey 2000). About 40 percent of the total volume of British capital flows between 1880 and 1913 went to countries other than the comparatively rich settler economies. Today, in contrast, only 10–15 percent of global capital market flows reach countries classified as less developed by the World Bank (Schularick 2006). Moreover, evidence is accumulating that capital market integration at the time had positive real economic effects in the recipient countries (Schularick and Steger 2010).

It seems likely that the spread of globalization and the deepening of capital markets in this period were at least partly due to perception of falling country risks by European financial investors.[5] Bordo and Rockoff (1996) argued that the credibility (and hence risk reducing) effects of hard-currency pegs were the principal reason for this. Based on the experience of nine non-Western European countries and colonies, Bordo and Rockoff showed that "all other things equal, the rate on a gold bond would be 40 basis points lower if the country were on the gold standard" (Bordo and Rockoff 1996, p. 413). The market's preference for the gold standard thus provided an incentive to join the gold standard and stick to it, thereby contributing to the dynamic extension of the gold standard (Meissner 2005). Another test of the Bordo–Rockoff hypothesis was carried out by Maurice Obstfeld and Alan Taylor (Obstfeld and Taylor 2003). Their findings rely on a larger sample than that of Bordo and Rockoff. In their empirical analysis of yield spreads, they find gold standard adherence to have cut spreads by up to 30 basis points before the war. Moreover, Obstfeld and Taylor find that the credibility effect of gold adherence was strong enough to overrule even the most important solvency indicator—the relative burden of public debt:

[5] A more general "push-side" argument stresses the positive effects of the international gold standard on capital market integration. By decreasing exchange rate volatility in the core, the gold standard reduced uncertainty and transaction costs and led to deeper financial markets, see Bordo and Rockoff (1996) and Ferguson (2003b). The gold standard also reduced inflation expectations and thus led to very low nominal long-term interest rate levels in the core. The focus here is on the gold standard as a commitment mechanism in the recipient countries, hence as a "pull" factor.

> In the sovereign bond market before 1914, the gold standard did indeed confer a "seal of approval", whereas two key macro fundamentals, the public debt and the terms of trade, seem to have mattered little, if at all. (Obstfeld and Taylor 2003, p.275)

Other authors, however, have arrived at different conclusions. C.K. Hobson already argued in 1914 that improved debt fundamentals were the main cause for the reduction in emerging market spreads before World War I (Hobson 1914, p.158). A reduction of the debt burdens could indeed be observed in many peripheral countries, mainly as a consequence of robust growth and rising public revenues. Flandreau and Zumer (2004) took up the Hobson thesis and focused their analysis of country risk assessment before 1914 on reductions of debt service levels. Using a data set of 17 mostly European countries, they "rejected the conventional view that the exchange rate regime (participation to the gold standard) mattered in facilitating the global circulation of capital in the late 19th century". (Flandreau and Zumer 2004, p. 56) Their gold dummy was either statistically insignificant or had the "wrong" sign, suggesting that the enlargement of the gold club played little, if any, part in the interest rate convergence of the pre-1914 period.[6] But what mattered to investors if not gold? Flandreau and Zumer find evidence that corroborates Hobson's thesis. Spreads reacted to economic "fundamentals"—to be precise, public debt service as a ratio of tax revenues, economic growth and inflation (in sum, the real debt burden).[7] In their study on the impact of colonial affiliation on risk perception, Ferguson and Schularick (2006) also found no significant risk reduction attributable to gold standard adherence and demonstrated the importance of colonial status for country risk premia. Mitchener and Weidenmier (2006), too, found little evidence that gold standard adherence lowered the currency risk implied by differences between interest rates on local and foreign currency denominated debt. In an earlier case study, Sussman and Yafeh (2001) had also raised questions about the "good housekeeping" interpretation of the gold standard. Mauro et al. (2006) have argued that political news, not monetary arrangements were the key factor for investors' risk perception.

How can we account for such divergent empirical interpretations? Three possibilities come to mind. An important part of the problem could simply be the gold "coding" issue; quite apart from methodological differences, there are disagreements about when a particular country was actually "on gold". For example, it is far from clear even in the cases of well-researched economies such as Austria and Italy, both of which "shadowed" the gold standard without having fully convertible currencies. It is even harder to be sure for smaller economies for which there is less readily accessible evidence about convertibility clauses and exchange rates. There is therefore a subjective element to retrospective identifications of "on gold" and "off gold" countries, especially when these are inferred *ex post* from exchange rates.

The empirical specification could also be at the roots of disagreements. In the absence of a well-specified model including an appropriate set of control variables, the gold standard dummy may simply be a proxy for other omitted variables. The Japanese gold adoption in 1897 provides an illustration of this problem.[8] Conventional current-yield data show a

[6] However, Flandreau and Zumer also include a variable proxying exchange rate volatility. It is not implausible to assume that this variable was highly correlated with the gold standard variable.

[7] This interpretation is not wholly incompatible with the one put forward by Bordo and Rockoff. If gold standard adherence worked as an incentive mechanism for sound policy, it may also have contributed to improvements in fundamentals. However, doubts about the disciplining effect on policy are expressed in Mosley (2003).

[8] For a detailed discussion see Sussman and Yafeh (2001) and Mitchener et al. (2010).

reduction of more than 200 basis points between 1896 and 1897. As other fundamentals such as public debt, the budget deficit or the level of development remained by and large the same, a regression will give the full credit for that reduction to the gold standard variable. However, the year of the adoption of the gold standard was also the culmination of a long process of political and economic reform in the Meiji era, the success of which was demonstrated by Japan's military victory over China in 1895.[9] The same year saw a successful debt conversion. Arguably, these factors could have mattered more than the switch to gold convertibility in driving down Japanese yields.

Previous studies have included quite different sets of control variables. Some authors have opted for a "historical" approach when analyzing risk premia, relying only on data available to contemporaries (Ferguson 2001; Flandreau and Zumer 2004). Others have preferred a "modern" approach incorporating later data reconstructions such as GDP and ratios of public debt to GDP. The underlying methodological question is whether market risk perception should best be modeled inductively on the basis of indicators that were available to contemporaries, or deductively according to the predictions of today's economic models—on an "as if" basis, so to speak—at the risk of anachronism.

Finally, and perhaps most importantly, there is the question of sampling. Previous studies relied on data for a relatively small number of countries, whereas more than sixty independent states, dominions and colonies had government bonds listed at the London Stock Exchange between 1880 and 1914. At the same time, the samples in previous studies were geographically diverse, being either predominantly "Atlantic" (Bordo and Rockoff) or skewed towards the European periphery (Flandreau and Zumer). The inclusion of colonies alongside independent countries is another important issue. It is, for example, not obvious why gold standard adoption should be assumed to have had the same impact on a British colony—where it often came as by-product of a de facto currency union with the UK—as on an independent Latin American state.

2. Data and estimation strategy

To solve these empirical puzzles, it is necessary to have an encompassing data set with a broad range of control variables, including those of previous studies. With spreads of gold- or sterling-denominated sovereign bonds for 34 independent countries and 23 British colonies at annual frequency as well as almost all the economic controls used in previous studies, our bond data set is among the most comprehensive to have been constructed.[10] The yield data for the period 1880–1913 were collected by hand from *The Investor's Monthly Manual*

[9] A common problem underlying previous studies as well as our own is that of regime selection. The decision to introduce a monetary regime like the gold standard may have been endogenously determined, that is, dependent on certain fundamentals that needed to be in place before a country could adopt the gold standard. The impact of gold adoption should thus be interpreted cautiously. It is not independent of other factors, "but merely a partially unconditional average benefit accruing to countries in a position to adopt the gold standard." (Obstfeld and Taylor 2003).

[10] The absence of gold or sterling-denominated bonds for France, Germany, Holland, and Switzerland forced us to eliminate these four countries in order to avoid the inclusion of currency risk premia. In all, fewer than ten countries were left out because of absent control variables. These included small Caribbean borrowers and a few colonial issuers such as Barbados and Trinidad. The group of British colonies includes the individual Australian and South African provinces before unionization. Details regarding the bonds used can be found in the appendix.

and *The London Stock Exchange Weekly Intelligence*, and refer to long-term (typically over ten years) bonds that were actively traded in the secondary market and had quotations for at least 3 years in a row.[11] The bulk of the historical economic control variables was also collected by hand from contemporary publications such as the Statesman's Yearbook (1880–1916), *Fenn's Compendium* (Fenn, 1893; Fenn and Nash, 1883; Nash, 1889; Van Oss, 1898), and the Annual Reports of the Corporation of Foreign Bondholders (1880–1914).[12] We used some data from modern statistical compilations (such as Mitchell's volumes; Mitchell (1992, 1993, 1995)), but only when those were also available to nineteenth-century investors. However, since we also wanted to test whether the incompatible findings of previous studies were due to the choice of "historical" versus "modern" indicators, our database also includes modern GDP estimates and related ratios.[13] Despite this effort, data are not available for all years in our panel. Nevertheless, we have nearly three times as many observations and countries as the widest-ranging previous study.

Table 1 summarizes our data set. What can be seen at a glance is that the choice of the economic control variables has a strong impact on the number of observations and on the number of countries in the sample. The main reason is that GDP reconstructions are only available for a limited number of countries.

Other than quantitative economic control data, we constructed a number of dichotomous dummy variables. As is conventional, we included a dummy variable for countries that were not honoring their repayment obligations, in other words defaulters. The information was taken from the Annual Reports of the Corporation of Foreign Bondholders, which contain detailed information on countries that did not pay the amounts due to bondholders.[14] Since one could expect that the market punished previous defaulters, a "memory" variable was given the value of one for ten years after a default occurred.[15] Two political variables capture the potential effects of international war and civil unrest on market risk perception.

There are two different approaches when it comes to coding dummies for gold standard adherence: *de iure* and *de facto* membership. In the first instance, we regarded countries as being "on gold" only if convertibility was formally legislated as well as maintained in practice. But we also tested the sensitivity of our results to two alternative codings. First, following Flandreau and Zumer, we counted the *de facto* adherents as being "on gold". Second, we double-checked the sensitivity of our results with the gold matrix from Meissner (2005), which gives slightly different dates for gold adoption.

A significant challenge concerned the appropriate way to control for asset market shifts that might affect spreads over time. Two options are at hand: first, simple time dummies that capture such movements in global risk appetite as are not accounted for by the variation in country fundamentals; second, a specification inspired by the international capital asset pricing model (CAPM), namely the correlation of individual assets with the market-wide risk (with country-specific slopes or "betas"). With the latter approach, there is again a

[11] In line with previous studies, we excluded all observations with spreads of more than 1000 basis points, since all these referred to bonds that were in default for many years, full repayment of which was considered unlikely.

[12] We also rely on material collected and kindly shared by other authors, in particular on the data sets of Obstfeld and Taylor (2003) and Clemens and Williamson (2004). For a detailed discussion of contemporary country risk indicators, see Ferguson and Schularick (2006).

[13] A detailed description of the data can be found in the data appendix.

[14] Unlike Obstfeld and Taylor (2003) we do not distinguish between partial and full defaulters, since we saw no objective method to classify systematically the individual cases. We assume that the bond market would have reacted to any payment problem.

[15] See Flandreau and Zumer (2004).

Table 1. *Summary statistics*

	N	Mean	SD	Minimum	Maximum
Spread over consols	1382	185.86	170.44	−8.49	993.11
"historical"					
Debt/revenue	1346	4.99	3.39	0.05	23.70
Debt/exports	1288	3.98	4.44	0.00	38.74
Debt service/revenue	783	0.24	0.17	0.01	2.92
Deficit/revenue	1318	0.13	0.38	−0.59	9.60
Trade balance/exports	1322	−0.26	2.43	−67.06	0.79
Exports per capita (log)	1322	0.87	1.21	−3.00	4.20
"modern"					
Debt/GDP	677	0.69	0.59	0.01	4.26
Exports/GDP	677	0.20	0.16	0.03	0.93
Deficit/GDP	642	−0.01	0.03	−0.13	0.18
Trade balance over GDP	674	−0.01	0.06	−0.30	0.32
GDP per capita (log)	1024	7.29	0.63	5.70	8.63

Source: see data appendix.

problem of anachronistic modeling, as the CAPM had not been invented at the time. In addition, the empirical support for CAPM remains rather weak. On the other hand, one can argue that there is no reason to believe that nineteenth-century investors were indifferent to the systematic risk of their investments. In the interest of comparability with recent studies, we report our regressions in the CAPM specification.[16] For this purpose, we constructed a global spread as the debt-weighted average of country spreads over the risk-free British benchmark bond known as the "consol".[17]

Finally, we moved beyond previous analyses in two ways. First, we focused specifically on countries that went on the gold standard from a previous paper standard. Following the time-inconsistency literature, one could expect a larger credibility gain arising from a switch from paper to gold than one from silver to gold. Second, we introduced a "probation variable" in order to see if the market rewarded gold adherence only in the case of faithful compliance over time. We experimented both with 3- and 5-year periods, but obtained similar results and therefore present only the results for the 5-year probation period. We also went beyond simple dummies and tested a spline variable, but obtained comparable results.

In our econometric approach, our overarching goal was to ensure comparability with previous studies. To control for heterogeneity in our panel, we opted for a standard fixed effects model, where individual country dummies capture the effects of time invariant but unmeasured economic characteristics such as geography or culture. Like previous authors, we found evidence of serial correlation and heteroskedasticity in our large panel, which makes ordinary least squares (OLS) problematic. Both feasible generalized least squares (FGLS) and panel-corrected standard errors (PCSE) are alternatives. In both variants, serial correlation can be accounted for via an AR(1) term.[18] Both estimators generally yield similar

[16] However, simple time-dummies lead to similar results. The distinction is more problematic with regard to colonies which had structurally "low betas", see Ferguson and Schularick (2006).

[17] We also experimented with an unweighted average without finding any significant differences from the results stated below. The same is true for a GDP-weighted world return, which comes at the cost of a smaller sample.

[18] We also tried a common AR(1) for all panels, but obtained similar results.

results, but we chose to present the potentially more robust PCSE estimates, as the number of groups is large relative to the number of years in our full sample. In such cases, FGLS produces overconfident test statistics (Beck and Katz 1995a, b).[19] However, the key findings of this paper are independent of the FGLS/PCSE estimation methods. As part of our sensitivity checks, we also considered a logistic default probability—an assumption not often seen in historical research so far, but suggested by contemporary research on spread determinants (Eichengreen and Mody 1998; Kamin and von Kleist 1999).

In our baseline model, we regressed the annual risk premium (**P**), i.e. the interest rate differential between the yield of a gold (or sterling) bond of an issuer and the yield on the risk-free British consols, in a fixed-effects framework, on a vector of economic controls (**X**), a gold standard dummy (**G**), and the world spread (**S**):

$$\mathbf{P}_{i,t} = \alpha_i + \beta_i \mathbf{S}_t + \gamma \mathbf{X}_{i,t} + \delta \mathbf{G}_{i,t}, + u_{i,t}. \quad (1)$$

To test the robustness of our analysis, we also estimated a dynamic panel model. Integrating the lag of the dependent variable (s) allowed us to model country risk perception as a Bayesian updating process. Deviations from steady-state country risk can persist longer than in classical AR(1) models. However, because fixed effects models with lagged-dependent variables bias OLS estimates, we opted to use a GMM framework. We examined the validity of the internal instruments and tested for serial correlation of the error term. The dynamic panel regression model of the first order takes the following form:

$$\mathbf{P}_{i,t} = \alpha \mathbf{P}_{i,t-1} + \beta_i \mathbf{S}_t + \gamma \mathbf{X}_{i,t} + \delta \mathbf{G}_{i,t} + u_{i,t}. \quad (2)$$

The GMM estimator allowed us to address the potential endogeneity of some regressors by using internal instruments. The estimator, introduced by Arellano and Bover (1995), combines the standard set of equations in first differences with suitably lagged levels as instruments, with an additional set of equations in levels with suitably lagged first differences as instruments.[20]

3. The gold standard hypothesis re-estimated

Our empirical analysis proceeds in three steps. First, we aim to reproduce the results of previous studies. The second step is to enlarge the sample to cover all 57 countries. Finally, we look more closely at subsamples of developed and developing economies.

[19] Obstfeld and Taylor (2003) as well as Flandreau and Zumer (2004) employed a fixed-effects FGLS model, while Bordo and Rockoff (1996) chose a SUR (seemingly unrelated regression) approach. A possible caveat is that FGLS needs two crucial data transformations in order to produce an estimate of the unknown variance–covariance matrix of the disturbances. It is certainly superior in "asymptopia", but was found to perform poorly when applied to finite real-world samples, especially if the number of countries grew large relative to the time-periods (Beck and Katz 1995a, b). This would seem to call for the less demanding PCSE method which was found to perform well in comparable research situations and has emerged as a quasi-standard in "large N, smaller T" cross-country studies in comparative political economy; see Beck and Katz (1995a). We thoroughly tested both variants but the core findings do not materially change. In order to save space, we chose to present only the PCSE estimates. In addition, neither the country-specific betas and rhos nor the up to 57 unit effects are shown. All additional results are available from the authors on request.

[20] We use the "xtabond2" routine in Stata written by Roodman (2005). The one-step robust estimator is applied, two-step estimation yielded analogous results.

3.1 Reproduction of the findings of previous studies

As our data were collected from a number of different sources, a natural starting point is to see if we can replicate the findings of Bordo and Rockoff as well as Obstfeld and Taylor (table 2). Both studies found evidence of a significant bonus for gold standard countries of 20–40 basis points. Regressions (1–2) restrict our data to the Bordo and Rockoff and Obstfeld and Taylor samples. Table 2 demonstrates that we are able to confirm their findings. Controlling only for gold standard membership and correlation with market risk, our data show a spread reduction of 25–40 basis points, similar to the benchmark figure Bordo and Rockoff arrived at earlier. These results are also robust the inclusion of time dummies (instead of betas) in regression (3).

However, these regressions omit a number of important risk determinants. As discussed above, there are two different ways to model nineteenth-century risk perception: a modern but anachronistic version, and one relying only on historical data. We first took the modern path and denominated the debt burden, exports, the public deficit and the trade balance by GDP and included real GDP per capita (in logs) to control for the income level. Then we took the historical route, scaling the debt burden by revenues and denominating the budget deficit by total revenues, indicating how much more a country's government spent than collected. We applied the same logic to the trade balance. To control for openness and the income level, we used exports per capita, an indicator that contemporaries are known to have relied on (though we calculated exports per capita in logs).

Starting with the "modern" specification (4), the regressions again neatly reproduce the findings of Obstfeld and Taylor. Gold cuts off about 25 basis points in spreads, but is only slightly above conventional significance thresholds. Using the identical sample of 20 countries, we then looked at the "historical" specification as described above. This was to see whether or not the difference between the "modern" and "historical" approach actually matters. Interestingly, regression (5) yields a similar result to the "modern" specification used before: gold standard membership remains worth about 25–30 basis points. The other coefficients also resemble their "modern" counterparts. We interpret this as an indication that the preference of historical over modern specifications may in fact be less important than has sometimes been suggested. Both sets of indicators seem to capture the same reality behind the numbers and approximate the risk perceptions of nineteenth-century investors reasonably well.

3.2 Full sample regressions

Regression (6) in table 3 profits from the full wealth of our data set, which (for the reasons given above) can be estimated only with the "historical" risk model (table 3). In regression (6) gold adherence is worth about 10 basis points economically, but is statistically not significant. Regression (7) amply documents the importance of economic fundamentals for spreads. The debt-to-revenue ratio is highly significant, both statistically and economically. The same is true of exports per capita. High exporters, it seems, enjoyed much lower spreads. Defaulters, in contrast, were heavily punished, and previous defaulters had to pay a significant premium. The deficit to revenue ratio and the trade balance seem to have played a less important role. Finally, political instability was clearly a point of concern for investors as internal crises raised country risk by about half a percentage point.

Table 2. *Reproduction of results of previous studies*

Regression	(1)	(2)	(3)	(4)	(5)
Sample	BR (1996)	OT (2003)	OT (2003)	"modern"	"historical"
GS × non-default	−24.00*** (6.30)	−39.83** (17.20)	−26.23* (14.93)	−24.25* (13.76)	−26.03* (14.18)
Default	90.60*** (11.26)	304.00*** (34.86)	273.70*** (30.32)	327.40*** (34.88)	310.80*** (35.37)
Previous default				27.71 (23.81)	34.24 (23.10)
Debt/GDP				39.77 (29.48)	
Exports/GDP				193.70 (163.80)	
Deficit/GDP				−67.75 (113.60)	
Trade balance over GDP				70.97 (124.80)	
GDP per capita (log)				−15.68 (33.36)	
International conflict				5.00 (7.15)	6.69 (7.03)
Civil conflict				8.40 (28.53)	14.01 (28.89)
Debt/revenues					7.53** (2.94)
Exports per capita (log)					12.97 (17.01)
Deficit/revenues					−8.89 (13.05)
Trade balance					9.98 (18.62)
Country effects	Y	Y	Y	Y	Y
Time dummies	N	N	Y	N	N
Observations	217	637	637	571	571
R^2	0.86	0.89	0.80	0.92	0.92
Number of countries	7	20	20	20	20

Standard errors in parentheses, *significance at 10 percent, **at 5 percent, ***at 1 percent levels.

Table 3. *Full sample results*

Regression	(6)	(7)	(8)	(9)	(10)	(11)
Estimation	PCSE	PCSE	PCSE	PCSE	PCSE	SGMM
Spread (t-1)						0.59*** (0.05)
GS × non-default	−11.27 (10.73)	−11.05 (10.15)	2.980 (11.55)	−11.35 (10.41)		11.74 (21.12)
GS × default	−19.57 (66.93)	37.23 (55.94)	1.041 (57.04)	36.81 (55.97)		−45.52 (44.84)
GS-paper × non-default					−4.87 (12.66)	
GS-paper × default					36.89 (56.11)	
GS adherence for 5 years				2.80 (9.82)	1.26 (9.69)	
Default	239.00*** (24.14)	259.50*** (25.38)	256.20*** (31.66)	259.90** (25.36)	259.60*** (25.30)	190.30*** (50.09)
Previous default		95.55*** (16.95)	111.80*** (23.60)	96.04*** (16.91)	95.60*** (16.91)	74.27*** (24.67)
Debt/revenue		7.75*** (1.86)		7.76*** (1.86)	7.69*** (1.86)	7.55** (2.82)
Exports per capita (log)		−11.40 (8.91)	−21.60* (11.58)	−11.58 (8.97)	−13.12 (8.76)	−17.85*** (6.78)
Deficit/revenues		−0.64 (4.25)	−20.40** (8.67)	−0.64 (4.26)	−0.65 (4.31)	8.94** (4.50)
Trade balance		−2.06*** (0.57)	−1.88*** (0.54)	−2.050*** (0.57)	−1.97*** (0.57)	1.46* (0.75)
International conflict		4.52 (10.38)	8.20 (10.09)	4.63 (10.36)	4.98 (10.44)	19.75* (11.67)
Civil conflict		91.59*** (15.88)	113.00*** (11.83)	91.61*** (15.93)	90.26*** (15.86)	50.56** (20.65)
Debt service/revenues			92.50*** (32.56)			
Constant						26.78 (16.56)
Observations	1,382	1,241	746	1,241	1,241	1,200
R^2	0.92	0.94	0.94	0.94	0.94	
Number of countries	57	55	42	55	55	55

Note: Dependent variable is the spread over consols. Standard errors in parentheses, *denotes significance at 10 percent, **at 5 percent, ***at 1 percent levels. Prais–Winsten regression with correlated panels corrected standard errors (PCSE) in a static model. Unit-effects, "betas" and country-specific rhos are not reported, but available on request from the authors. Robust one-step Arellano–Bond system GMM dynamic panel estimation in dynamic specification.

Regression (8) employs a different denominator for the debt burden, namely the debt service-to-revenue ratio, as advocated by Flandreau and Zumer (2004). With this modification, the gold effect is even turned upside down as the coefficient estimates turn positive, indicating that countries that joined gold paid higher spreads. However, the effect might be due to a smaller sample and the endogeneity concerns could also figure more prominently when looking at debt service ratios. Yet so far it seems obvious that the good housekeeping effects tend to become economically much smaller and statistically insignificant when the sample gets bigger.

The following regressions look at two potentially important omissions in the previous literature. First, regression (9) adds a "probation dummy", to see whether the market rewarded gold standard adherence only after a 5-year period of faithful compliance with the rules. The result is unconvincing, both statistically and economically: Even after 5 years of rule-bound monetary policy, the credibility effects as measured by country risk spreads were tiny and statistically not robust. Regression (10) focuses on countries that joined the gold standard from a paper standard and excludes those that simply "switched" from silver or bimetallism to gold. The idea is that the credibility effects associated with a "hard peg" might already have been reaped with the adoption of silver convertibility (Mexico and India being the most prominent examples) so that the gold effect might be more obvious in countries that made the transition directly from paper to gold. However, the results documented in table 3 do not support this idea. We find a statistically insignificant 5 basis points reduction in country risk for economies that adopted gold coming from a paper standard.

Regression (11) in table 3 presents a different model, namely a dynamic panel model that includes the lagged-dependent variable as a regressor. By using internal instruments, we are also able to control for the potential endogeneity of some of the regressors such as the debt ratio. Despite the different estimation strategy the results are consistent with our previous results. According to the GMM estimation (11), the effect of joining gold becomes negative, adding even more doubts to the robustness of the "good housekeeping" argument for our large sample.

In sum, while we were able to reproduce earlier findings of a gold effect using previous smaller samples, the gold effect tended to become less visible in our much larger country sample. Though still often "correctly" signed, the gold dummy was no longer significant, even when we varied the gold coding criteria or looked only at countries that made the paper–gold transition. As this seems to underline the importance of sample selection, the logical next step is to look more closely at subsamples.

3.3 Individual subsamples and interactions

An important feature of our full sample may be the presence of twenty-three British colonies (table 4). Colonial bonds were treated as a different asset both on account of their lower spreads and their much lower correlation with market risk (Ferguson and Schularick 2006). Some colonies were effectively in a currency union with the UK. Moreover, colonies tended to have above-average trade openness, as well as British-style fiscal and legal institutions. As a first step, we used a Chow-test to find out, whether there were significant structural differences, i.e. unequal coefficients, between independent countries and British

Table 4. *Effects in rich and poor countries*

Regression	(12)	(13)	(14)	(15)	(16)	(17)	(18)
Sample	British colonies	Independent countries	Rich independent	Poor independent	Poor independent	All independent	All independent
Spread ($t-1$)							
GS × non-default	−2.65 (11.84)	−10.47 (13.39)	−36.39*** (10.76)	−7.17 (19.44)	−4.46 (16.52)		
GS × default		29.27 (56.79)		−28.36 (68.48)	−9.33 (60.79)		
GS × non-default (LDC)						−10.20 (18.99)	−8.87 (16.30)
GS × default (LDC)						25.33 (61.55)	30.21 (56.91)
GS × non-default (DC)						−29.46** (13.92)	−19.48 (18.56)
Default		255.70*** (25.56)	94.55*** (30.35)	252.60*** (26.56)	287.00*** (27.65)	291.30*** (24.52)	256.20*** (25.62)
Previous default		93.82*** (17.11)	39.01* (22.52)		99.86*** (18.16)	99.95*** (18.43)	93.58*** (17.05)
Debt/revenue	0.36 (0.81)	9.35*** (2.57)	3.10 (2.58)		14.05*** (3.07)		9.33*** (2.58)
Exports per capita (log)	9.23 (5.90)	−33.47** (16.69)	−46.36*** (11.95)		−61.75** (23.98)		−33.30** (16.71)
Deficit/revenues	7.45*** (2.84)	−0.89 (4.74)	1.59 (5.27)		−1.63 (4.89)		−0.82 (4.73)
Trade balance	−2.20*** (0.44)	1.41 (18.64)	7.26 (11.14)		20.79 (24.34)		1.59 (18.70)
International conflict	132.00*** (17.11)	−3.74 (11.03)	1.24 (6.09)		−3.07 (13.72)		−3.87 (10.97)
Civil conflict		91.26*** (15.56)	−40.90 (42.82)		89.36*** (16.00)		91.12*** (15.58)
Observations	519	722	337	462	385	832	722
R^2	0.83	0.94	0.95	0.92	0.95	0.93	0.94
Number of countries	24	34	12	22	19	34	31

Note: Dependent variable is the spread over consols. Standard errors in parentheses, *denotes significance at 10 percent, **at 5 percent, ***at 1 percent levels. Prais–Winsten regression with correlated panels corrected standard errors (PCSE) in a static model. Unit-effects, "betas" and country-specific rhos are not reported, but available on request from the authors. Robust one-step Arellano–Bond system GMM dynamic panel estimation in dynamic specification.

colonies. The resulting *F*-statistic is far above the critical value, so that we reject the idea that both groups had equal coefficients.[21]

Regression (12) in table 4 confirms that colonies were treated differently from independent borrowers when they entered the capital market. Debt and income levels did not matter for risk premia, while exports per capita have the wrong sign, implying that poorer colonies paid lower interest. The gold dummy is statistically and economically insignificant. In short, colonies could borrow cheaply because they were colonies. The monetary regime did not matter.[22]

What happened when we looked only at the determinants of bond spreads of independent borrowers? In contrast to colonies, fundamentals re-appear as important drivers of risk perception in regression (13). The effect of debt and the income level on risk premia is large, while the value of gold is very small and again—by a substantial margin—statistically insignificant. The question hence remains why the gold effect is much weaker in our sample compared with previous smaller samples. A brief look at the list of countries we added—such as Turkey, China, Persia, Siam, the Balkan states, and, besides Mexico, a number of smaller Latin American countries—suggests that the gold effect may lose significance as the number of capital-poor independent countries grows relative to more advanced "Atlantic" economies.

Were poor countries different in some way? We performed another Chow-test, splitting the sample into a poor country sample and a rich country sample to see if there are structural differences between the two. Countries with a GDP per capita of less than one-third of the UK were classified as poor developing countries.[23] In total, we count 22 independent developing countries.[24] Again, the Chow-test led us to reject the assumption that both groups have equal coefficients.

We were now left with two groups, consisting of 12 relatively developed independent countries (DCs), mainly belonging to the "Atlantic" economy, and a group of 22 independent, but less developed countries (LDCs) from Eastern Europe, Latin America, and Asia. Running a separate regression for the 12 DCs in the sample, we found a surprising result. For the developed (non-colonial) economies, the gold standard hypothesis seemed to hold: joining the gold club brought a statistically highly significant reduction of risk premia of up to 35 basis points, just as the early study by Bordo and Rockoff had found. Yet a separate regression for the 22 less DCs yielded an equally clear but different result as shown in regressions (15) and (16) in table 4. We started by regressing country risk premia on the gold standard variable and a default dummy, without further economic controls. As can been from regression (15), statistical and economic significance is minimal; only the sign

[21] This presence of parameter differences between sovereign states and colonies in the nineteenth century capital market was demonstrated by Ferguson and Schularick (2008). Using the same methodology, Accominotti et al. (2011) recently confirmed this finding.

[22] Most colonies in Asia and Africa switched to a gold-exchange standard shortly before or after 1900. The case of India is a well-known example. Yet some colonies like Hong Kong remained on silver throughout.

[23] The GDP per capita threshold is US$1,500 (1990 prices, PPP) in 1900 according to Maddison (1995) which is roughly equivalent to one-third of British GDP per capita at the time. The regressions yielded similar results when we split the sample at US$2,000 and 1,000, and also a geographic split (all countries outside Western Europe and North America being classified as developing countries) led to identical conclusions about the indifference of the market to monetary commitments in poor countries.

[24] In total, we count fifteen individual default episodes in eight of these countries between 1880 and 1913. On average, countries remained in default for a little less than 10 years. We count 136 default-years in our data set, corresponding to about 15 percent of the total country-year observations.

on the gold standard variable is still negative. Moreover, with the addition of the full set of controls in (16) the gold standard variable turns both economically and statistically insignificant. The 95 percent confidence interval spans from +30 basis points to −35 basis points. There is thus no evidence that gold standard adherence brought tangible credibility gains for developing economies. It also did not make a difference whether we used CAPM betas or time dummies, *de iure* or *de facto* coding—we could not find robust evidence for a good housekeeping effect in developing countries.

Two last regressions sum up this key finding of our paper. Instead of looking at individual subsamples, we interacted the gold standard variable with a dummy for LDCs and DCs, using the same definitions as above. We hence ask whether the effects of going on gold varied according to whether a country was rich or poor. In the interest of transparency, we start again with a simple regression without additional controls and then move on to include additional economic control variables. Regressions (17) and (18) demonstrate the different dynamics in developing and developed countries. In the case of the rich countries in our sample, the gold standard effect is economically meaningful (albeit with 20 basis points somewhat lower than assumed before) and, in regression (17) also statistically significant. However, when poor countries adopted the gold standard, confidence intervals explode and the economic size of the effect becomes negligible (18). All in all, the regressions demonstrate the dichotomy that we noted before: gold standard adherence seems to have brought some credibility gains for developed, but not for developing countries.

The market, we infer, did not confer a "good housekeeping seal of approval" on poor peripheral countries merely because they adopted the gold standard. Many peripheral countries tried but few, if any, reaped the benefit of enhanced credibility supposedly associated with gold standard membership. This, then, explains why previous studies could not agree on the importance of the gold effect. In those studies where country risk perception was modeled on the basis of GDP reconstructions, the data availability led to the selection of a relatively wealthy country sample. But the gold standard hypothesis vanishes if the whole population of foreign borrowers in London is taken into account. The market, it seems, did not reward gold adherence in poor countries and rich countries equally. Credibility gains associated with gold convertibility were limited to countries above a certain state of economic development.

3.4 Developing country subsample

Finally, we looked more closely at the determinants of risk pricing in the poor periphery, including some potential risk factors that we had not considered before (table 4).

The first test is again a dynamic specification that addresses the potential endogeneity of some of the regressors through internal instruments. Regression (19) has no further controls, regression (20) contains the full set of control variables. The result is virtually the same. Both dynamic models show no risk-reducing effects of joining the gold standard in poor countries. The gold standard dummy is not only insignificant, but takes on a positive sign. In regression (21), we test whether countries that transitioned directly from paper to gold saw greater improvements in spreads. The result is again negative. Regression (21) in table 5 gives the baseline for all poor economies, while regression (22) adds the share of primary products in exports, the average tariff rate and the terms of trade. The key finding remains unchanged: gold adoption did not lead to credibility gains for poor countries. Finally, we obtained the

Table 5. *Developing country subsample*

Regression	(19)	(20)	(21)	(22)	(23)
Sample	LDC	LDC	LDC	LDC	LDC
Model	SGMM	SGMM	PCSE	PCSE	PCSE
Spread ($t-1$)	0.43*** (0.08)	0.59*** (0.04)			
GS × non-default	17.68 (33.84)	17.81 (20.39)			−0.04 (0.04)
GS × default	−33.38 (62.48)	−68.48 (41.87)			
GS-paper × non-default			10.61 (18.01)	2.14 (24.09)	
GS-paper × default			−7.65 (61.24)		
Default	229.90*** (74.69)	191.90*** (46.99)	286.80*** (27.60)	334.80*** (34.10)	0.54*** (0.06)
Previous default		62.90*** (22.88)	98.89*** (18.12)	101.80*** (21.27)	0.18*** (0.04)
Debt/revenue		5.79** (2.84)	14.01*** (3.07)	14.65*** (3.35)	0.03*** (0.01)
Exports per capita (log)		0.08 (15.64)	−64.76*** (23.31)	−78.36** (31.97)	−0.14** (0.06)
Deficit/revenues		2.22 (5.024)	−1.62 (4.93)	−2.32 (4.43)	−0.00 (0.01)
Trade balance		42.14 (29.50)	23.05 (24.37)	17.31 (28.27)	0.03 (0.05)
Primary product exports (percent)		5.60 (11.25)		−544.40 (472.60)	−0.55 (0.85)
Tariff rate (log)		35.57 (24.56)		−254.70** (129.50)	−0.28 (0.27)
Log-change in terms of trade				−12.39 (36.48)	−0.00 (0.07)
International conflict			−2.56 (13.70)	−1.23 (13.55)	0.02 (0.03)
Civil conflict			87.58*** (15.96)	90.25*** (17.28)	0.14*** (0.04)
Observations	444	372	385	282	282
R^2			0.95	0.93	0.98
Number of countries	22	19	19	12	12

Note: Dependent variable is the spread over consols. Standard errors in parentheses, *denotes significance at 10 percent, **at 5 percent, ***at 1 percent levels. Prais–Winsten regression with correlated panels corrected standard errors (PCSE) in a static model. Unit-effects, "betas" and country-specific rhos are not reported, but available on request from the authors. Robust one-step Arellano–Bond system GMM dynamic panel estimation in dynamic specification.

same result when we took the dependent variable in logs (23). In sum, gold standard adherence did not convey tangible credibility benefits on poor countries.

4. Policy credibility in the poor periphery

Whatever its significance for relatively rich independent countries, gold adoption made little, if any, difference to the perceived country risk of two important sub-groups within our global sample: British colonies and poor independent countries. It is questionable whether the positive effects that are evident for the top third of countries on the pre-1913 income ladder should therefore be interpreted as evidence of a rule of the sort proposed by Bordo et al., or as exceptions to a more general rule that monetary regime changes by themselves do little to enhance credibility. Below a certain income threshold, policy credibility remained by and large unaffected by changes in the monetary regime. For a poor country seeking to borrow in London at sustainable rates, we are tempted to suggest, it made more sense to become a British colony than to join the gold standard.

Why did bond market investors reward gold standard adherence in more developed countries, but disbelieve promises of "good housekeeping" in less developed ones? We propose two explanations that are not necessarily mutually exclusive. Both focus on characteristics of developing countries that reduce the probability that a commitment to a currency peg will have a durable disciplining effect on policy-making. First, as Drazen and Masson (1994) have pointed out, the credibility of policies and the credibility of policy-making are two different things. The market is unlikely to find the promise of "tough" policies equally credible in all circumstances. Like Drazen and Masson, we are uncomfortable with the dogma that "tying one's hands" is automatically rewarded by the market, because it implies—wrongly in our view—that investors do not think about the likely sustainability of the "promise of self-restraint", which is highly contingent on a country's economic and political situation and prospects. Even if economic policy-makers before 1914 were more insulated from popular political pressures than would be the case after 1918, other factors remained that affected the probability of their sticking to their gold-standard commitments in the face of adverse conditions. Poor countries, because of their backward economic structures, were more exposed than most rich countries to shocks—to the vagaries of world agricultural markets, sudden changes in terms of trade and growth trajectories. Agrarian lobbies, with their fondness for currency devaluations and low interest rates, were even more powerful in poor countries than in rich precisely because the interest groups supportive of gold commitments (notably bankers and bourgeois rentiers) were much smaller and weaker. An investor had good reasons to believe that Sweden would be less likely to suspend convertibility than Siam or Venezuela.

Table 6 compares a number of plausible factors that contributed to the market's assessment of the "promise of self-restraint". It shows that the more advanced countries, on which gold adherence seems to have conferred a credibility bonus, were also special in other respects: they were about twice as open, they traded about twice as much with the UK, their exports were much less dominated by primary products, and they were better integrated into world markets as measured by their considerably smaller shipping distances from London. Their income levels, in other words, can be seen as a proxy for a number of other characteristics that were likely to bolster market confidence in their long-run commitments to gold. For the great majority of developing countries, however, the gold commitment was a

Table 6. *Economic fundamentals periphery versus core, 1880–1913*

	Poor countries	Rich countries
GDP per capita (USD 1990)	1025	2419
Average annual growth rate	0.01	0.02
Trade with the UK	21.53	35.94
Exports/GDP	0.11	0.20
Primary product share in exports	0.93	0.72
Terms of trade[a]	0.10	0.08
Average tariff rate	21.05	13.89
Distance from London	3.12	1.63
Years of internal or external conflict	0.08	0.01

Source: see text and data appendix.
[a]Standard deviation of annual changes.

rule that could be overthrown at relatively low cost and one that was therefore quite likely to be challenged in a crisis. It would be surprising if it had been very credible.

Our second explanation is political. In the eyes of the market, the credibility gains through gold standard adoption may have been low in poor countries simply because political instability was high. This meshes with the findings in Mauro et al. (2006). In other words, where the political and social fabric of a country is still crisis prone, its monetary regime is likely to be a second-order concern for the market. As political conflict is typically more heated, the rules of the political game are rewritten much more often in poor countries than in developed ones. Yet if constitutions change frequently, investors have good reason not to put too much faith in the durability of one particular law that requires monetary policy to follow a strict rule. Investors in Colombian, Greek, or Persian bonds were most of the time concerned with permanent threats to internal or external security that could have ruined the credit of the country. Monetary clauses mattered much less in such cases. That would seem to be confirmed by the fact that the contemporary British press dwelt extensively on the political developments in these countries, but rarely (if ever) referred to convertibility arrangements. We cannot help feeling that, if the City had been as interested in currency clauses as some have claimed, financial journalists would have written a good deal more about them (Sussman and Yafeh 2001).

Our results suggest that the potential time-inconsistency of monetary policy was not the dominant concern of investors in developing countries before 1914. Their vulnerability to economic and political shocks was far more important. The same may apply today, in the most recent era of globalization. Feuerstein and Grimm (2006) have shown that a hard exchange rate peg is not the optimal monetary solution if vulnerability to shocks, not time-inconsistency, is the dominant problem. As policy can react to shocks only after a delay, even the threat of a shock can make the abandonment of the peg more likely *ex ante*. In a similar vein, Guidotti and Végh (1999) have argued that the credibility of hard pegs falls quickly after an initial stabilization period as the underlying economic weaknesses come to the fore again. Our empirical results show that, while theoretically appealing at first glance, pre-committing policy to a binding rule was no more effective remedy for the economic ills of developing countries in the first era of globalization.

5. Conclusions

The hypothesis that gold standard membership conferred a "good housekeeping seal of approval" on international borrowers before 1914 is not wholly without empirical foundation. Statistically, there was some kind of benefit in the form of reduced risk premia—but only for certain countries that went onto gold. Yet even this limited vindication of the good housekeeping hypothesis may require qualification. In those relatively advanced countries for which the hypothesis seems to hold, the gold dummy may merely be a proxy for improvements not properly reflected by other covariates; or it may merely capture the effect of low transaction costs. Unilateral promises of exchange rate stability and of complementary economic policies may have provided additional credibility, but only in special circumstances.

By applying the full range of available empirical techniques to our expanded sample of sovereign and colonial borrowers, we have shown that there were no benefits to going on gold for the majority of less developed economies before the First World War. If the international gold standard performed any service for such countries, it was by minimizing inflation expectations in rich countries, and thus contributing to the low and stable long-term interest rates in the core that were so crucial for encouraging capital flows to the periphery.

In the last era of globalization, as today, investors priced country risk on the basis of a complex mixture of economic fundamentals and political factors such as colonial status. In this sense, it may make more sense to think of the gold standard less as a "seal of approval" and more as a kind of "thin film", behind which investors looked. The key historical lesson from the gold standard era is that in poor countries—where policy credibility is a particularly acute problem—rule-bound monetary policy did not result in credibility gains. Vulnerability to economic and political shocks, not time-inconsistency, was and remains the overarching concern for international investors.

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Appendix A. Data appendix

The data appendix contains in tabular form the country sample, short descriptions of the selected bonds and the sources of the annual yield data. All data are taken from Ferguson and Schularick (2006), except for the following additional sources. GDP per capita and GDP growth: Maddison (2001); primary product exports, terms of trade, tariff rates, effective distance from Clemens and Williamson (2004); UK trade shares from Meissner (2005).

The country sample and sources of yield data are shown below.

	Source	Bond
Austria	IMM	4 percent gold rentes
Belgium	IMM	3 percent of 1874 (1880–1897)
Denmark	IMM	4 percent of 1861 (1880–1884), 3 percent gold loan of 1894 (1895–1913)
Hungary	IMM	4 percent gold rentes
Italy	IMM	5 percent Marem. railw. 1862
Norway	IMM	4 percent of 1880 (1880–89), 3 percent of 1888 (1890–1913)
Sweden	IMM	4 percent (1880–1889), 3 percent (1891–1899), 3.5 percent of 1879 (1900–1913)
Bulgaria	IMM	6 percent of 1880 (1890–1902), 6 percent gold loan (1902–1913)
Greece	IMM	5 percent independence (1880–1884), 5 percent of 1884 (1885–1899), 5 percent of 1881
Montenegro	LSE	5 percent loan (1910–1913)
Portugal	IMM	3 percent of 1853 (1880–1899), 3 percent ser. (1900–1913)
Rumania	IMM	8 percent loan (1880–1889)
Russia	IMM	4 percent Nicolas Railway 1867
Serbia	IMM	4 percent unified (1898–1913)
Spain	IMM	3 percent ext. (1880–1882), 4 percent ext. (1883–1913)
Ottoman Empire/ Turkey	IMM	4.25 percent of 1871 (1880–1884), 5 percent priority (1885–1889), 5 percent customs (1889–1899), 4 percent of 1891 (1900–1913)
Liberia	IMM	5 percent ext. (1902–1913)
Orange (b.1900)	IMM	6 percent bonds of 1884 (1885–1894)
Transvaal	IMM	5 percent scrip. (1892–1899), 3 percent loan (1903–1913)
Egypt (b. 1882)	IMM	4 percent unified debt
China	IMM	8 percent of 1874 (1880–1884), 6 percent (1885–1894), 6 percent gold loan (1895–1899), 4.5 percent gold bonds (1900–1913)
Japan	IMM	7 percent of 1873 (1880–1896), 4 percent Sterling loan (1899–1913); new loan data were kindly shared by Marc Weidenmier
Persia	IMM	5 percent loan (1911–1913)
Siam	LSE	4.5 percent Sterling loan (1905–1913)

(*Continued*)

Continued

	Source	Bond
United States		Source: Calomiris's gold equivalent yields from the data set of Obstfeld and Taylor (2003).
Argentina	IMM	6 percent of 1867 (1880–1888), 5 percent of 1886 (1889–1913)
Brazil	IMM	4.5 percent gold loan (1880–1909), 5 percent (1910–1913)
Chile	IMM	5 percent of 1873 (1880–1886), 4.5 percent of 1886 (1887–1913)
Colombia	IMM	4.75 percent of 1873 (1880–1896), 3 percent ext. (1897–1899), 3 percent ext. (1900–1913)
Guatemala	IMM	6 percent Sterling (1880–1888), 4 percent (1889–1913)
Mexico	IMM	3 percent of 1851 (1880–1887), 6 percent (1888–1899), 5 percent cons. Ext. (1900–1913)
Nicaragua	IMM	6 percent bonds (1887–1899), 4 percent (1900–1910), 6 percent Sterling loan (1911–13)
Peru	IMM	5 percent cons. (1880–1889), 5.5 percent loan (1911–1913)
Salvador	LSE	6 percent bonds (1891–1899), 6 percent Sterling (1908–1913)
Uruguay	IMM	6 percent 1871 (1880–1883), 5 percent unified 1883 (1884–1892), 3.5 percent (1893–1913)
Venezuela	IMM	3 percent new cons. (1882–1905), 3 percent diplomatic debt (1906–1913)
British colonies	Source	Bond
Ceylon	IMM	4 percent of 1880
Hong Kong	IMM	4 percent debentures (1890–1893), 3.5 percent (1894–1913)
India	IMM	4 percent (1880–1884), 3.5 percent (1885–1913)
Straits settlements	IMM	4.5 percent of 1877 (1880–1890), 3.5 percent (1907–1913)
Canada	IMM	5 percent (1880–1884), 3.5 percent (1884–1889), 3 percent loan 1938 (1890–1913)
British Guyana	IMM	4 percent (1889–1913)
Jamaica	IMM	4.5 percent of 1879 (1880–1889), 4 percent (1890–1913)
New South Wales	IMM	4 percent bonds
New Zealand	IMM	5 percent (1880–1885), 4 percent (1886–1894), 3 percent (1895–1913)
Queensland	IMM	4 percent
South Australia	IMM	4 percent 1974
Tasmania	IMM	4 percent of 1878–1883
Victoria	IMM	4.5 percent of 1879 (1880–1895), 4 percent (1896–1900)
Western Australia	IMM	4.5 percent of 1879 (1880–1890), W.Austr. 4 percent 1881 (1891–1900)
Australia (from 1900)	IMM	4 percent bonds (NSW)
Gold Coast/Ghana	IMM	3 percent (1902–1913)
Mauritius	IMM	4.5 percent of 1876 (1880–1889), 4 percent (1890–1813)
Nigeria (Lagos)	IMM	3.5 percent (1904–1913)
Sierra Leone	IMM	4 percent conv. bonds (1904–1913)
Cape Colony	IMM	4.5 percent of 1873 (1880–1889), 4 percent Cons. (1890–1913)
Natal	IMM	4.5 percent of 1876 (1880–1889), 4 percent (1890–1913)
Orange (from 1900)	IMM	6 percent bonds of 1884 (1885–1894)
Transvaal (from 1900)	IMM	5 percent scrip. (1892–1899), 3 percent loan (1903–1913)
Egypt (from 1882)	IMM	4 percent unified debt