"Aggregation Bias" **DOES** Explain the PPP Puzzle*

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Abstract

This paper addresses the criticisms of Imbs, Mumtaz, Ravn and Rey (2002) put forward by Chen and Engel (2004). We show that their contentions are based on: (i) analytical counter-examples which are not empirically relevant; (ii) simulation results minimizing the extent of "aggregation bias"; (iii) unfounded claims on the impact of measurement errors on our results; and (iv) problematic implementation of small-sample bias corrections. We conclude, as in our original paper, that "aggregation bias" goes a long way towards explaining the PPP puzzle.

*This note is an addendum to our main paper "PPP Strikes Back: Aggregation and the Real Exchange Rate" (first draft December 2002, revised version April 2004). We thank Manuel Arellano, Lutz Kilian, Hashem Pesaran and Ron Smith for helpful discussions. This paper does not represent the views of the Bank of England or of Monetary Policy Committee members, and was written while Mumtaz was at London Business School.

1 Introduction

In "PPP Strikes Back: Aggregation and the Real Exchange Rate", we show that a dynamic aggregation bias can explain the PPP puzzle. Aggregate real exchange rates are persistent because their components have heterogeneous dynamics, for which established time series and panel methods fail to control. When we use estimators allowing properly for heterogeneity, the persistence of the real exchange rate falls dramatically. Its half-life, for instance, falls to around one year. We show that the corrected estimates are consistent with dynamic general stochastic equilibrium models and plausible degrees of nominal rigidity. Thus, arguably, we solve the PPP puzzle.

Chen and Engel (2004) [henceforth CE] criticize our paper on four grounds: 1) they question the applicability of the "aggregation bias" to the PPP puzzle; 2) they claim the size of the bias is "shown to be much smaller than the simulations in Imbs, Mumtaz, Ravn and Rey (2002) suggest"; 3) they contend that measurement errors contaminate our results and 4) that small sample bias is the main reason behind them. They also find that country-by-country estimates contradict our results. In this note, we refute each of the CE criticisms and cover in detail what is often left as footnotes in our revised main paper, Imbs, Mumtaz, Ravn and Rey (2004) [henceforth IMRR (2004)]. Thus, this paper is reserved for the reader who is interested in digging deeper into the detailed reasons why our results prevail despite the claims made by CE.

The next four sections all point toward the same direction: the claims made by CE are unfounded. In section 2, we discuss the general analytical proof presented in IMRR (2004) and explain why heterogeneous dynamics translate into an "aggregation bias" in our price data, thus answering CE's concern regarding the applicability of the bias to the PPP puzzle. The proof allows for sectoral correlations and expenditure weights. We show in particular that the counter-examples presented in CE are not empirically relevant, so that even though it is theoretically possible that the bias be non-positive, it is certainly not the case in our data, nor in theirs. Section 3 makes clear the simulations in CE do not yield a large aggregation bias because of their choice of parameters. The extent of heterogeneity they use in their simulations is smaller than in the data, and they choose the one set of initial conditions that acts to minimize the bias.

In section 4, we deal with claims in CE that we use a dataset plagued with measurement errors, supposedly at the source of our results. It is doubtful right at the outset that short-lived measurement error could explain our conclusions. CE's argument is that measurement error acts to lower persistence estimates based on sectoral data, while estimates based on aggregate data are immune to the problem as it tends to be averaged away. But for this to be convincing, one would need to observe persistence estimates at the disaggregated level systematically lower than in the aggregate. We do not: there are more than a few sector-level persistence estimates in excess of the aggregate. Nevertheless, we perform extensive robustness checks to show our results are not sensitive to this argument. First, we use exactly the same dataset that Charles Engel advocates on his website. When we apply the appropriate estimator to his data, we find a half-life of 13 months (with a confidence interval ranging from 9 to 24 months), hardly different from our results of 14 months in IMRR (2002). CE find a different result simply because they use an estimator which is rejected by their data: they implemented on their data sample the same estimator we used in our original dataset. They should not have done this, as their data call for an alternative specification. Second, we improved on the IMRR (2002) dataset by systematically checking Eurostat time series against national sources. We would therefore maintain that the final dataset used in IMRR (2004) is of better quality than Engel's. It yields a half-life around 11 months. Finally we perform formal tests for errors-in-variables and remove any suspicious series. Again, our results stand.[1]

In section 5, we discuss the importance of the claim that our results are not robust when our panel estimates is decomposed country-by-country. Criticizing our results on the ground that they do not hold within countries is akin to criticizing results based on panel unit-root tests on the ground that unit roots cannot be rejected on a country-by-country basis. The Section next details the small-sample bias corrections we implement, and shows that they still yield half-life estimates well below the "consensus view". CE find otherwise because they implement inappropriate estimators which, together with their bias correction technique, induce a *positive* bias in their corrected half-life estimates. Section 6 concludes.

2 Applicability of "Aggregation Bias" to the PPP puzzle

2.1 Some Theory

CE claim the "aggregation bias" is not really applicable to the PPP puzzle since the bias is not necessarily positive. They argue cross-sectional correlation of the errors may give rise to a negative bias. Further, the sign and magnitude of the bias may depend on expenditure weights. To bolster their claim, they present counter-examples for which the bias is either zero or negative.

We focus here on the case where the panel consists of the relative prices of goods for a single country pair. IMRR (2004) generalizes to panels of exchange rates. Consider an economy with N sectors, indexed by i. For simplicity,

[1] We note that IMRR (2002) had already a full section devoted to measurement error. This section is curiously not even mentioned in CE. We had also sent to Charles Engel estimates based on his own dataset as early as in January 2003, as an answer to one of his emails.

suppose that (the log of) relative prices in each sector follows an autoregressive process of order one, defined by

$$q_{it} = c_i + \rho_i \; q_{it-1} + \varepsilon_{it}, \; i = 1, .., N$$

with $c_i = c + \eta_i^c$ and $\rho_i = \rho + \eta_i^\rho$. We assume that η_i^c and η_i^ρ have zero mean and constant covariance, and that the set of random coefficients ρ_i has support within the interval $]-1, 1[$.[2] We also suppose that ε_{it} is independently distributed with mean 0 and variance σ_i^2 with $E(\varepsilon_{it}^2) = \sigma_i^2$.[3] We allow for non-zero cross-sectoral covariances of ε_{it}, with $E\left(\varepsilon_{it} \; \varepsilon_{jt}\right) = \sigma_{ij}$ for $i \neq j$. These correlations could arise, for example, from common shocks across goods or from omitted (unobservable) global influences. Without loss of generality, we order the N sectors so that $0 < \rho_1 \leq \rho_2 \leq ... \leq \rho_N < 1$.

The bilateral real exchange rate q_t can be approximated by a linear aggregation of the different sectors with weights ω_j associated with the j^{th} good.[4]

$$q_t = \sum_{j=1}^{N} \omega_j q_{jt}, \; \sum_{j=1}^{N} \omega_j = 1$$

PPP studies estimate the persistence of the real exchange assuming that its dynamics are best described by an AR(p) process. Many use an AR(1) as their standard specification. So will we to simplify the derivations. An aggregate estimation ignoring the heterogeneity in the dynamics of the subcomponents would write

$$q_t = c + \rho q_{t-1} + \varepsilon_t$$

with

$$c = \sum_{j=1}^{N} \omega_j c_j, \; \varepsilon_t = \sum_{j=1}^{N} \omega_j \varepsilon_{jt} + \sum_{j=1}^{N} \omega_j \eta_j^\rho q_{jt-1}$$

It is immediately apparent that since lagged dependent variables are present in the error term, aggregation across sectors also leads to a biased estimate of the mean persistence. In IMRR (2004), we show the conditions for this "aggregation bias" to be positive and increasing in the degree of heterogeneity. In particular, we show that the bias Δ writes

$$\Delta = \sum_{i=1}^{N} \left(\rho_i - \rho\right) \delta_i \tag{1}$$

with $\delta_i = \dfrac{\left[\frac{\omega_i^2 \sigma_i^2}{1-\rho_i^2} + \sum_{i \neq j}^{N} \frac{\omega_i \omega_j \sigma_{ij}}{1-\rho_i \rho_j}\right]}{\sum_{i=1}^{N}\left(\frac{\omega_i^2}{1-\rho_i^2}\sigma_i^2 + \sum_{i \neq j}^{N} \frac{\omega_i \omega_j \sigma_{ij}}{1-\rho_i \rho_j}\right)}$. Hence the bias depends on cross sectoral correlations, persistence of the various sectors and expenditure weights. We

[2] We consider drawing from a discrete set of H values in the interval $]-1; 1[$.

[3] See IMRR(2004) for other technical assumptions.

[4] See IMRR (2004).

prove that the aggregation bias is positive ($\Delta > 0$) whenever the coefficients δ_i are positively correlated with the persistence parameters ρ_i. This turns out to be unambiguously the case in our data, as well as in Engel's. Furthermore, in IMRR (2004), we show that the bias is not only positive but also quantitatively important.

Could the bias be negative or negligible *in theory*? It certainly could, as is obvious from the inspection of the above expression. And it is easy to come up with simple analytical examples in which the bias is either zero or negative. This is what CE do. On page 6, CE develop an example where two price series are perfectly negatively correlated (and thus exactly cancel out). In that case, if $N = 3$, the aggregate persistence is that of the third, uncorrelated series. But none of these rather extreme assumptions hold in price data. Next CE choose to linearize the expression of the bias around a *perfectly homogeneous* case to argue the bias is small or inexistent whenever $\omega_i = \omega$ and $\sigma_{ij} = c$, or $\sigma_i = \sigma$ and $\sigma_{ij} = c$. We note the Taylor expansion is computed around the homogeneous case. This is important, as we showed in IMRR (2002) that the magnitude of the bias increases with heterogeneity, and indeed is zero under homogeneity. Since CE focus on almost homogeneous processes, and use an approximation imposing linear effects of parameter heterogeneity, it is to be expected the heterogeneity bias will be small. Irrespective of the expansion point chosen to perform the approximation, however, what matters is whether the restrictions imposed in all these experiments are plausible empirically or not. In our data, or in Engel's, they are not.

CE made some other related points:

1) CE claim on page 4 that "we can unambiguously state there is aggregation bias only when the weights in the price index are equal for all i, the innovation variance σ_i^2 are the same for all i and the cross-correlations of all series are equal". Inspecting expression (1) shows immediately that this is false. There are many different cases, with different weights, different variances and covariances, which give a positive bias Δ. Even the condition that we show in IMRR (2004) to be strongly sufficient for the positivity of the bias (*i.e.* $0 \leq \delta_i \leq \delta_{i+1}$ for all i) can be fulfilled under many possible data configurations, again with different weights, variance and covariances. And, in our data as well as in Engel's, the bias is unambiguously positive, even though weights and innovation variances are *not* the same.

2) CE mention evidence in Rogers and Jenkins (1995) that unit roots can be rejected only for a few perishable items, which tend to have a low weight in the CPI. They infer that the bias should be small. Things actually go the exact opposite way. CE probably confuse the δ_i's (estimation weights) with the expenditure weights ω_i's. In reality, the inspection of the expression of Δ shows that for the bias to be negative, one would need *highly* persistent relative prices

to have (ceteris paribus) *low* CPI weights ω_i.[5] The fact that low persistence items tend to have low CPI weights would if anything reinforce the positivity of Δ, since it suggests ω_i (and therefore δ_i, ceteris paribus) tend to be low for low ρ_i. Of course, the only sure answer to this question is to compute δ_i and compare it with estimates of ρ_i. In our data, the verdict is unambiguous, and the bias is positive.[6]

3) CE criticize us on the ground that it is "well-known" that summing AR processes yields an ARMA process. Well-known or not, this *only* happens under heterogeneity. In the absence of any heterogeneity, the roots cancel out and the aggregate real exchange rate is an AR process, akin to the one driving sectoral prices. In other words, estimates of the persistence in real exchange rate that only include autoregressive terms (no matter how many) ignore heterogeneity. And since nearly all the papers of the PPP literature estimate AR(p) -and often AR(1)- they *de facto* ignore heterogeneity. Furthermore, pursuing the route of taking heterogeneity into account by allowing for ARMA terms in the real exchange rate may not even be feasible in practice, since with sufficient heterogeneity, one would quickly run out of degrees of freedom unless the sample period is long enough. Heterogeneous estimators are better-suited to tackling the issue than estimating processes with infinite (or even high order) ARMA terms.

We have now demonstrated the claim of CE that the "aggregation bias" is not applicable to the PPP puzzle because the bias could be negative or zero in theory, is irrelevant empirically. But it should also be clear from the expression of δ_i that covariances in prices across sectors will affect the magnitude of the bias.[7] In IMRR (2004) we introduce heterogeneous estimators that do account for correlated residuals. By contrast, CE only point to the possible importance of non-zero σ_{ij}, and give empirically implausible analytical counter-examples. The estimators they use, however, do not investigate which way non-zero σ_{ij} affects the aggregation bias, *in the data*. Our estimators do.

Accounting for cross-sectional dependence in as large a panel as ours, while continuing to allow for heterogeneity in the slope coefficients is by no means straightforward. We implement two estimators that can handle both issues, and show correlated residuals to be an important chracteristic of the data. We find that (i) half-lives are even lower once correlated residuals are accounted for. Thus, $\sigma_{ij} \neq 0$ is an important element of Δ, but *not* because it tends to decrease the magnitude of our bias, as CE claim. (ii) Simulations suggest estimated half-lives are biased (upwards) if correlated residuals are not accounted for. The issue is important empirically and allowing for correlated residuals actually strengthens our conclusions.

[5] As well as low innovation variance σ_i^2, and/or low covariances with other components of the real exchange rate. We note that in our price data covariances are systematically positive.

[6] See IMRR (2004).

[7] These correlations are uniformly non-negative in our data.

2.2 Some Intuition

It is important to understand aggregation is a problem in panels *because* of ignored heterogeneity across the components of the real exchange rate. In other words, estimates based on international panels of real exchange rates will generate high persistence because each real exchange rate is composed of many sectoral relative prices whose dynamic properties are heterogeneous. Separating "aggregation bias" and dynamic heterogeneity bias, as CE seem to suggest on page 3 of their paper, is impossible. They are one and the same issue. In particular, our main argument is that wrongly imposing an identical speed of adjustment for the *components* of the real exchange rate can lead to a bias in persistence estimates, both in aggregate time series and in a panel setting. More precisely, if the speed of relative price adjustment differs across goods, the speed of adjustment of the real exchange rate will not be an unbiased estimate of the average speed of adjustment of relative prices. Thus, our main argument is *not* that "impos[ing] an identical speed of adjustment across all real exchange rates [...] can bias estimates of the half-life of real exchange rates" (CE, p. 3-4). While such a bias is possible, our concern is about imposing identical speeds of adjustment across *different types of goods* and how this may affect estimates of relative price persistence.

Our paper explains how heterogeneity in the dynamics at the good level translates into persistent aggregate real exchange rates. *Our result does not require nor imply that persistence be systematically smaller at the disaggregated level.* Hence results in Crucini and Shintani (2002) and Engel (2000) that CE invoke in their conclusion do not contradict ours in any way. That Crucini and Shintani should find *homogeneously* fast mean reversion in a wide range of good prices immediately suggests they will not find much evidence in support of "aggregation bias". in their data. There is little heterogeneity in their estimates which all point to strikingly low half lives. If we are right, there should not be much of an aggregation bias in a dataset of goods which unanimously tend to revert to parity quickly. But surely, there *is* a PPP puzzle. Most aggregate relative prices do tend to revert to parity slowly. That they do not in Crucini and Shintani (2002) does not invalidate our contention. Indeed, our argument explains why they do not find the PPP puzzle in an aggregated version of their data.

It is also peculiar that CE assert that "there is already a large literature devoted to [...] biases in panel estimates" due to heterogeneity, with application to the real exchange rate (page 4). An EconLit search yielded one single paper dealing with dynamic heterogeneity in the real exchange rate, and the point there was heterogeneity across countries, not across sectors.[8] To our knowledge, the insights of Pesaran and Smith (1995) about dynamic heterogeneity

[8] See Boyd and Smith (1999), who conclude there is very little heterogeneity in real exchange rates dynamics between countries.

biases were never applied to studies of the real exchange rate, nor the point made that this very heterogeneity actually obscures aggregate estimates in an international panel. Given the difficulties we have encountered in providing an intelligible intuition for our estimation strategy in accounting for heterogeneity, it seems strange to contend the issue we discuss is "well-known" in this literature. In fact, in their footnote 1, CE describe the Random Coefficient Model (RCM) erroneously. RCM does not compute an arithmetic mean of sectoral persistence estimates to obtain aggregate half-lives. There are weights, and they are optimally inferred from the observed heterogeneity in the data, using a Generalized Least Squares procedure akin to that implemented in Random Effects estimators.

3 Simulations

The second main criticism of CE concerns the Monte-Carlo simulations in IMRR (2002). The reason why we performed Monte-Carlo simulations in the original version of our paper was not to ensure there was a bias. Comparing estimates where heterogeneity is allowed for to ones where it is not is indeed sufficient to prove the presence of a bias. Our Monte-Carlo simulations were meant to quantify how the bias responds to variations in the extent of heterogeneity and/or of persistence (and how various estimators perform at capturing it). CE propose to use Monte-Carlo simulations to prove there is no bias, but both our data and theirs (even cleansed of measurement error and small-sample bias) show it is there. The proof is in the pudding.

CE question the validity of the bias derived in our simulations on grounds that we allow the possibility for explosive roots in our simulated sectoral prices. In communications we had with CE in 2003, we had changed the distribution of sectoral persistence coefficients in our simulations so that they did not include any value (weakly) above unity, and showed the simulated bias remained substantial, almost identical.[9] Indeed, the Monte-Carlo simulations in IMRR (2004) *exclude* de facto any explosive roots. Thus, the discrepancy has to come from somewhere else.

There are first some obvious reasons. In IMRR (2002), we show the bias increases with the extent of heterogeneity, so part of the reason for the differences in simulation results stems from the fact that CE use a range for the heterogeneity in sectoral persistence parameters that is smaller than ours (and indeed smaller than what our -cleansed- data imply). By the same token, allowing for

[9] CE do not mention this fact even though we posted our correspondence with Charles Engel on the web in September 2003. See Imbs, Mumtaz, Ravn and Rey (2003): The PPP Controversy - A Summary of the Debate surrounding "PPP Strikes Back" at faculty.london.edu/jimbs, faculty.london.edu/mravn or www.princeton.edu/~hrey.

cross-sectoral correlations present in the data increases the magnitude of the bias - CE never allow for them in their simulations. However, there is another, more subtle yet important reason.

In their Monte Carlo experiments CE allow for fixed effects. The initial conditions in their simulations are such that each cross-sectional unit starts at its asymptotic mean. This is at best a special case, which has important consequences for the results. Instead, Hamilton (1994, Chapter 11) suggests drawing the initial conditions from the asymptotic distribution of the variables. Alternatively, Arellano and Bond (1991) impose zeros for all initial conditions. In IMRR (2004), we use the actually observed initial conditions. It turns out that the heterogeneity bias is important under *any* of these three standard alternatives.

The intuition is as follows. As soon as initial conditions differ from the asymptotic mean of the cross-sectional units, there is an initial period of convergence towards the asymptotic mean even in the absence of any shocks. The MG and the RCM estimators allow for heterogeneous dynamics over this adjustment period, but FE and indeed any other aggregate estimators do not. Monte-Carlo simulations that ignore this initial adjustment period will tend to minimize the discrepancy between heterogeneous estimators and standard ones. Indeed, simulations that assume initial conditions that are specifically equal to their long run values are the *only ones* that will tend to minimize the superiority of heterogeneous estimators in the presence of heterogeneity. It seems inappropriate to reject the possibility that there could be an aggregation bias, on grounds of the one simulation setup that minimizes its impact.

In IMRR (2004) we confirm that the bias is large, using actually observed initial conditions.[10] We stress that this is *not* meant to establish the existence of the bias in the data (since the formal tests and the results show this), but rather to investigate its robustness across heterogeneity and persistence parameters. We provide analytical and direct empirical support for the presence of a bias in our data. Given the importance of initial conditions, Monte-Carlo simulations should be used neither to establish nor to disprove the existence of a bias. We do not propose to do the former. CE try to do the latter, thus hijacking the original purpose of our simulation exercises.

4 Data

The third claim in CE concerns the impact of measurement error on our estimates. First, if short-lived measurement error were to explain our low estimates

[10] In our simulations, we actually truncate the first 50 observations off our Monte-Carlo simulations, in order that the importance of initial conditions be minimized. Even so, we continue to find a large bias.

for persistence, it should do so on the basis of systematically lower persistence at the sectoral level, which would naturally aggregate into low half-life for the real exchange rate. As we show in IMRR (2004), there are no few instances of sectoral persistence exceeding the aggregate measure, which would be impossible if measurement error were generating our results. Even so, CE argue otherwise. In this Section, we detail the reasons for this discrepancy.

On his website, Charles Engel lists revisions to the Eurostat dataset we use that he deems appropriate. We present below estimates based on these exact data. They confirm the half-life drops dramatically when heterogeneous dynamics are taken into account.[11] We also explain in detail why CE find otherwise: they implement an estimator which is rejected by their data. In IMRR (2002), we used the official Eurostat data corrected for some obvious typos (and other mistakes) to obtain our initial estimates. Our section 6.1 was entirely devoted to tests of errors-in-variables, and re-ran all our estimations with suspicious observations replaced by (sufficiently) lagged values, as is customary. Our results were confirmed.[12] Finally, in IMRR (2004) we go one step further, and use sources from national statistical agencies to verify the consistency of the Eurostat data, as well as the corrections suggested on Charles Engel's website. Our revised dataset is therefore arguably of better quality than Engel's. Again, our results stand.

4.1 Data Corrections, Part 1: On the Importance of Testing for the Appropriate Estimator.

Engel's data have 127 cross sections for the period 1981:01-1996:12. There are 9 countries and a maximum of 16 goods. The coverage is considerably lower than the data used in IMRR (2002). In particular, Greece, Finland and Ireland are not included, as are goods such as Rents and Tobacco. In fact, the number of cross sections in their data set is about half of those used in the original version of our paper.[13] There are two standard estimators which control for heterogeneous dynamics: the Random Coefficient Model (RCM) and the Mean Group (MG). One should perform an appropriate test to ascertain which one should be used in a given data sample. With a smaller cross-sectional dimension and a shorter sample, it is to be expected the RCM estimator will perform less well, but the MG estimator still has good small sample properties. We now discuss this in detail.

Table 1 confirms that we closely reproduce CE's RCM and FE estimates when using their data. However, the MG estimator produces a much shorter

[11] Between January and June 2003, we circulated and sent to Charles Engel revised estimates based on various versions of Engel's dataset. The Figures in CE ignore this fact. See IMRR (2003).

[12] Oddly enough, there is no mention of this section of the paper in CE.

[13] Our dataset had 221 cross sections.

half-life (25 months), with a precise confidence interval (9 to 31 months).[14] In their Table 5, CE find an upper bound of 142 months when using the MG estimator on their data. Since we find low upper bounds when we use their exact data, the discrepancy must stem from their bootstrapping technique, which they do not explain. We follow Ron Smith's suggestion to bootstrap, and use the mean coefficients to draw the residuals, before performing sampling from the residuals themselves.

Figure 1 shows that our findings remain true for all possible lag lengths. The implication is clear. *Allowing for heterogeneity makes a significant difference in the CE sample as well. The Hausman test strongly rejects parameter homogeneity.* CE do not find a strong effect of heterogeneity on the basis of the RCM estimator, but in this much smaller sample the RCM estimator is rejected, and they should use the MG estimator instead. Why do RCM and MG not perform equally well? Both estimators allow for slope heterogeneity, but only the former imposes distributional assumptions on heterogeneity. Using CE's data a Hausman test for heterogeneity strongly rejects homogeneous slopes. However, the alternative hypothesis could be either heterogeneous and deterministic, or heterogeneous and random. In other words, the alternative hypothesis is consistent with both MG and RCM. In order to distinguish between the two we use a test devised by Pudney (1978). This is a test for the assumptions underpinning random coefficients. A rejection of the null hypothesis implies rejection of the random coefficient assumption. In an AR(5) model we obtain a test statistic of 126.72 (0.00). This implies that the Mean Group model is more appropriate in these data.[15] When implemented on exactly Engel's data, the MG estimator yields a half life of 25 months, as shown in Table 4. But none of these estimates allow for cross-sectoral correlations. And indeed, the half-life drops further to 21 and 13 months when we allow for correlations across sectoral prices via a MG SURE or a MG Common Correlated Effect estimator (henceforth MG CCE[16]), respectively.[17] We show in IMRR (2004) that the common effects are an important characteristic of price data.

The fact that CE's panel is narrower than ours could also explain their problems with the RCM model, especially at high lags. This is particularly important as the heterogeneous estimators we propose are essentially averages, and their consistency and efficiency depends on the cross-section of the panel. We conducted a simple experiment. We assumed a heterogeneous data generating process, with 220 cross sections. Then we estimated RCM models only on the first 100 cross sections. Figure 2 plots the distribution of the resulting estimates and contrasts it with estimates from the whole panel. It is clear that

[14] All half-lives in our paper are defined as the number of periods it takes for the impulse response to cross 0.5 *permanently*, as is customary.

[15] In our original data the statistic was 10.29 (0.90).

[16] See Pesaran (2002).

[17] Table 4 also reports all the alternative measures of persistence we use to bolster our argument in IMRR (2004). They all lead to the same conclusion.

the estimator using fewer cross sections has a much more dispersed distribution, i.e. the estimates are less precise. This problem is likely to be more severe as the number of parameters increases.

Direct evidence on the importance of this point can be seen in Table 2, where we list estimates obtained when CE's dataset is expanded. We add the following: 1) Data for Greece, Finland and Ireland. 2) Data for Tobacco and Rents. In each case, the data for all countries were checked and any outliers were removed, in a way similar to the selection method described on Charles Engel's website. This gives us a panel with 191 cross sections, a number closer to our original data. The fixed effects half-life is close to CE's estimate. The heterogeneous estimators, however, now produce shorter half-lives. In particular the MG model gives a half-life of 20 months. Figure 3 plots the half-lives obtained from these estimators against the lag-length. The MG half-lives are consistently less than two years. The RCM model produces half-lives close to two years, whereas the Fixed Effects estimator is biased upwards. Note that RCM and MG converge at higher lag lengths, as they should. Note also that we do *not* observe the kind of impulse responses found by CE.[18] In IMRR (2004) we provide confidence intervals for these results based on corrected data. Thus, CE find different results -even though we use almost identical data- because they implement the very same estimator we used on our original sample. But this estimator is rejected in their data. Had they used the proper estimator, they would have obtained our results.

4.2 Data Corrections, Part 2: Formal Tests for Errors-in-Variables, and Even Better Data.

CE correct the data by removing outliers and parts of the series that appear inconsistent. We do a similar exercise when we expand their data. However, removing "suspicious" data may also be problematic since it introduces a degree of subjectivity. In other words, there is a chance that it remove shocks that are actually informative. In fact, it is possible that such a procedure may produce persistence. We can infer the impact of this from the following experiment: 10,000 AR(1) processes with an autoregressive coefficient of 0.95 were generated. Estimation was carried out on (i) the generated series without any changes (ii) on series where "outliers" where replaced by an average over $t+1$ and $t-1$. The mean estimated half-life in case (i) is 13.52 months, which is very close to the true half-life of 13.51. In case (ii) this goes up to 22.5 months. Figure 4 plots the distribution of the estimates. It is easy to see that in case (ii) the distribution is much more dispersed around a higher mean.

[18] We tried various other combinations of the data. For example, adding data for Greece and Ireland only, produces very similar results. In addition we considered removing every series that has repeated observations (as noted by Charles Engel on his web page). Again the MG estimator gives a half-life of 23 for an AR(5) model.

This is perhaps not very surprising because in this case outliers are erroneously removed. In reality, many of the "corrected" data may of course be true measurement errors. Correcting for measurement errors on the basis of removing replacing outliers is problematic, however, since it would for instance not remove "small" measurement errors. For that reason, a more objective approach to the measurement error problem might be desirable.

In the original version of our paper, we reported RCM estimates based on GMM estimators with instruments chosen to account for errors in variables. We showed this did not affect the results. We now examine how this estimator performs. We generate data for AR(1) models using a coefficient of 0.95. Then we add a random error ν to these data where $\nu\tilde{\ }N(0, 0.3)$. A typical sample is shown in Figure 5. It can be seen that the series inclusive of the error has many possible outliers. Next we estimate models using OLS, which is expected to be biased, and GMM, which is consistent. The distribution of the resulting estimates is shown in Figure 6. There is a downward bias in OLS, but GMM performs much better and its mean is close to the true estimate. This does indicate that if errors in variables were a substantial problem we should have observed a large difference between RCM estimates based on OLS and GMM. In IMRR (2002), we found very similar results from using either estimator, indicating that measurement error did not account for our results.

The data we use in IMRR (2004), is the results of thorough checks of the Eurostat data against series published by national statistical agencies.[19] Our results, all presented in our main paper, are almost unchanged. Our best estimate for the half life is 11 months with a confidence interval ranging from 7 to 12 months. Hence, once more, our results are confirmed.

5 Country-by-Country Evidence and Small-Sample Bias

The last section of CE develops two points. First, it is argued our results do not hold on a country-by-country basis, and there is little evidence of cross-sectoral heterogeneity within countries. Second, it is claimed that our persistence estimates suffer from a small sample bias, affecting least squares estimators when the data are persistent.

That our evidence should weaken on a country-by-country basis is unsurprising. We have made up for the lack of detailed disaggregated data on relative prices by using the country dimension in our panel. Criticizing our results on grounds that they do not hold within countries is akin to criticizing results

[19] Our data are posted at faculty.london.edu/jimbs, faculty.london.edu/mravn or princeton.edu/~hrey

based on panel unit-root tests, on ground that unit roots cannot be rejected on a country-by-country basis. For instance in the related literature on real exchange rates persistence, should one dismiss the results in Frankel and Rose (1996) or Murray and Papell (2003), which rest on the improved performance of unit-root tests when a panel dimension is brought to the task? Should one counter their argument with claims that it does not hold on a country-by-country basis?

We think one should not. That is not to say we have not tried to increase the sectoral (or temporal) dimension of our panel as well, but for all its faults Eurostat provides to our knowledge the best coverage of disaggregated relative prices there is. As we underline in our paper, the number of (monthly) observations in our data is large relative to the literature, and enough to alleviate somewhat the weakness of (panel) unit root tests. If the purpose were to address the question of real exchange rate persistence within countries, one would need much more disaggregated sectoral data than those we have.

Second, CE claim that a small sample bias pervades our estimates. At first, CE's results may appear consistent with the evidence presented in an interesting paper by Choi, Mark and Sul (2003) [CMS henceforth]. CMS examine the relative importance of three influences on real exchange rate persistence estimates: temporal aggregation, small samples, and heterogeneity. Their empirical analysis centers on the relative magnitude of the first two biases, because they fail to reject slope homogeneity in their data (they use a panel of aggregate real exchange rates, not sectoral data). Indeed we report similar results when we test for slope heterogeneity in a panel of aggregate real exchange rates. We found heterogeneity to be key at the sectoral level not at the country level.

The bias correction procedure used by CMS - suggested by So and Shin (1999) and extended by Sul, Phillips and Choi (2002) to account for common effects in residuals - is only meant to assess the relative importance of the small sample and the temporal aggregation biases. In their panel of aggregate real exchange rates, CMS find the small sample bias dominates. Their quantification of the heterogeneity bias, on the other hand, is based on simple Monte Carlo experiments, in which the data generating process has slope heterogeneity. In their simulations, they examine whether the total bias of a simple OLS estimator is positive (in which case the heterogeneity bias dominates) or negative (in which case the small sample bias dominates). For artificial data with fewer than 200 observations, they find that the total bias under OLS is negative, but somewhat sensitive to the calibration of heterogeneity. For 200 observations, the bias of the OLS estimator can turn positive. Importantly however, their data generating process does not allow for the common components that we find are important. Their simulations, therefore cannot be used to dismiss the heterogeneity bias *in our data*, where common components are crucial. Above all, CMS never propose to implement their bias correction procedures to data with slope heterogeneity. They never claim, with reason, that the methods they apply have desirable properties when applied to data with characteristics akin to ours.

By contrast, CE correct for the small sample least squares bias by implementing the standard So and Shin (1999) procedure, which relies on recursive demeaning of the data, and a bootstrap-after-bootstrap procedure suggested by Kilian (1998). They find that either method gives rise to a substantial increase in the corrected half-life of the data. The Kilian (1998) procedure implies an increase in the mean bias corrected MG estimate to 44 months as against least squares estimates of 26 months. The 95 percent confidence interval goes from 13 months to infinity. Application of the So and Shin correction instead raises the point estimate to 161 months with a 95 percent confidence interval spanning 112 months to infinity. Thus, their results imply a worsening of the PPP puzzle, and bring our results into doubt.

There are several problems with CE's procedure. First, and this is a major point, relatively little is known about the properties of the bias correction methods that CE use, when applied to heterogeneous panels with common correlated effects. We note again that CMS carefully tests for homogeneity (and fail to reject) before applying these corrections on their data. Second, CE consider estimators that do not allow for common effects while we show common effects to be an important characteristic of our price data. Third, there are reasons to question CE's application of the Kilian (1998) procedure. In particular, whenever the statistic of interest is a non-linear function of the estimated autoregressive parameters, Pesaran and Zhao (1999) have shown that in heterogeneous panels, bias corrections should be performed *directly* on the statistic of interest, and *not indirectly* on estimated parameters. Using corrected autoregressive coefficients to calculate a half-life, as CE do, may result in asymptotically biased corrections.[20]

We therefore first provide some Monte Carlo evidence on the properties of the bias reduction methods.[21] Unlike CE, our correction procedures do account for correlated residuals. Following Pesaran and Zhao (1999), we perform our correction directly on the half-life, which is the variable of interest.[22] In Appendix B, we describe the steps in our *direct* bootstrapping approach. It stands in stark contrast with the indirect bias correction implemented in CE. We report the outcome of a simple Monte Carlo experiment meant to compare the

[20] Pesaran and Zhao (1999) discuss this issue in an application to a panel setting where the object of interest is a long-run multiplier of a change in an exogenous variable. But the same problem arises when estimating the half-life. Indeed, the issue will arise whenever one attempts to correct short-term point estimates in order to obtain an unbiased assessment of long-run phenomena. In general, the expression for long-run properties is highly non-linear in short-run estimates.

[21] Phillips and Sul (2003) suggest a Panel Feasible Generalized Median Unbiased estimator that can be applied in heterogeneous dynamic panels with common effects. Their method generalizes the median unbiased correction to a SUR estimator. As noted in IMRR (2004) the cross-sectional dimension of our data is large and the MG CCE estimator may outperform the SUR estimator. For this reason we consider alternative bias correction methods.

[22] We also use a balanced version of our dataset in order to diminish the (considerable) computational burden of the experiments. If anything, truncation works against us, since it makes it harder to obtain precise half-life estimates.

direct and indirect approaches in the presence of correlated residuals. We assume a data generating process in which the panel units are generated by AR(1) processes with slope heterogeneity and common correlated effects. We assume that the time series dimension, T, is 200 and that the cross sectional dimension, N, is 180 so that the panel is close to ours. By assuming AR(1) processes for the panel units we minimize the defects inherent in the indirect approach because the non-linearity here is less severe than for higher order autoregressive processes. We then apply the direct and indirect versions of the Kilian (1998) procedure to the MG and MG CCE estimators and the So and Shin (1999) procedure to the MG estimator.[23]

Table 3 presents the results. We report least-squares results as well as bias-corrected estimates, using both the direct and indirect approaches to the bootstrap procedure and, for the simple MG estimation the So and Shin procedure. Several results are worth mentioning. First, the least squares MG estimates display a negative bias when the common correlated effect is relatively unimportant. However, as common component rise in importance, simple MG becomes increasingly inaccurate, as the bias due to the neglected common effects starts dominating the least squares bias. Finally, a *positive* bias arises. This suggests the simulations in CMS, which do not allow for common effects, are not well-tailored to *our data,* where common effects are important. The least squares MG CCE estimator has a negative - but very small - bias regardless of the importance of the common effect.

Second, using the indirect approach, or the So and Shin procedure to correct the MG-based estimates is only accurate when the common effect is relatively small. As common effects rise in importance, both induce a *positive* bias, which can be large. Our suggested direct approach works better but still implies a systematic positive bias. In contrast both bootstrap procedures (direct or indirect) are accurate for the MG-CCE estimator, and give relatively precise estimates that appear immune to the properties of the common effect.

In summary, our results in Table 3 suggest that, in the presence of correlated residuals, the preferred estimate should unsurprisingly be MG-CCE. Then, the Kilian (1998) bias correction procedure is accurate, especially if computed using the direct approach. In contrast, applying bias reduction techniques to the MG estimator not allowing for common effects results in corrected estimates that are themselves biased *upwards*, indeed overestimating the true degree of persistence. This is especially true when the indirect approach is used.[24]. This may well

[23] In the Monte Carlo experiments we perform only the first bootstrap step of the Kilian procedure. It would be computationally infeasible to implement bootstrap-after-bootstrap in the panel setting because of time constraints and cycling. Cycling would imply that the simulated distribution would not emulate the asymptotic distribution. The empirical estimates, however, *do* apply bootstrap-after-bootstrap.

[24] The RMSE of the indirect approach to the bias correction for the MG estimator is large, while both corrections have quite small RMSE when MG CCE estimator is implemented.

explain the results in CE, who apply blindly the So and Shin (1999) method, and the indirect version of the Kilian (1998) procedure.

In IMRR (2004) we apply the bootstrap-after-bootstrap procedure to our panel of relative prices. We find a very modest increase in the estimated half-life. In particular, the corrected half-life estimate increases to eighteen months only, as opposed to our uncorrected estimate of eleven months. Our confidence interval is narrow and excludes the "consensus view". The indirect approach yields slightly higher but very comparable results, with a corrected half-life of twenty months. As suggested in our simulations, choosing the direct or indirect approach makes little difference when common effects are accounted for. It does however change the results dramatically (and push corrected estimates upwards) when the estimation does not allow for common effects, as in CE. We therefore confirm the robustness of our results, and account for the discrepancy with CE's. They simply use estimators and bias correction procedures that are inappropriate in price data.

6 Conclusion

Chen and Engel (2004) put together a series of criticisms of IMRR (2002). We are grateful to the authors for inducing us to refine our argument in response to their efforts to overturn our results over the course of fifteen months. It came as a surprise that almost none of our extensive exchanges with Charles Engel were included, nor discussed nor even mentioned in CE.[25] But on the other hand it opened the possibility for this detailed addendum to our main paper, IMRR (2004). Here, as there, the verdict is clear, and once again, PPP strikes back.

[25] See IMRR (2003).

Appendix A: Asymptotic equivalence of RCM and MG estimators

Pesaran (2003) demonstrates the asymptotic equivalence between the RCM estimator and the MG estimator and here we summarize the key parts of Pesaran's analysis.

Consider a panel with two cross sections and t_i observations. Let the OLS coefficients be $\hat{b}_1$ and $\hat{b}_2$ and covariance matrices V_1 and V_2 where:

$$V_i = E\left[(\hat{b}_i - b_i)(\hat{b}_i - b_i)'\right]$$

where b_i is the true value of the coefficient. Consider the MG estimator in this panel:

$$\beta_{mg} = \frac{1}{2}\left(b_1 + b_2\right)$$

The RCM estimator is:

$$\begin{aligned}\beta_{rcm} &= \left(\frac{1}{2}\frac{b_1^2 + b_2^2 - 2b_1b_2 + 2V_2}{b_1^2 + b_2^2 - 2b_1b_2 + V_2 + V_1}\right)b_1 + \left(\frac{1}{2}\frac{b_1^2 + b_2^2 - 2b_1b_2 + 2V_1}{b_1^2 + b_2^2 - 2b_1b_2 + V_2 + V_1}\right)b_2 = \\ &\Psi_1 b_1 + \Psi_2 b_2\end{aligned}$$

As $t_i \to \infty$, $\left(\hat{b}_i - b_i\right) \longrightarrow^p 0$ and $\Psi_i \longrightarrow \frac{1}{2}$ as V_i gets smaller. In other words for large T, the variance of the estimators gets very small and the RCM weights approach the MG weights.

Appendix B. Direct Bootstrapping Method

The bias correction procedure can be summarized as follows:

Step 1 Use the appropriate estimator to get group specific slopes, intercepts and error variances. Denote the estimated mean slope coefficients by $\widehat{\rho}^S$ where S denotes the relevant estimator. Compute the implied half-life and denote this by $\widehat{T}^S_{\frac{1}{2}}$.

Step 2 Generate bootstrap samples of the innovations $\left[\widehat{\varepsilon}^j_{it}\right]^T_{t=1}$ for $i = 1, ..N$ where j denotes replications. We generate these using a non-parametric bootstrap. When we allow for cross-sectional dependence, we first pre-whiten the residuals and then re-color them after the non-parametric bootstrap. Generate artificial samples of relative prices.

Step 3 Use the artifcial data to estimate the mean coefficients of the model: i.e. $\widetilde{\rho} = \frac{1}{N}\sum \widetilde{\rho}_i$. We use the method detailed in Kilian (1998) to generate the cross section specific coefficients $\widetilde{\rho}_i$. Use $\widetilde{\widetilde{\rho}}$ to calculate the half-life $\widetilde{T}^S_{\frac{1}{2}}$.

Step 4 Repeat steps 2 and 3 R times and obtain the bootstrap average $\frac{1}{R}\sum \widetilde{T}^S_{\frac{1}{2}}$.

Step 5 The bias corrected half life is given by $2\widehat{T}^S_{\frac{1}{2}} - \frac{1}{R}\sum \widetilde{T}^S_{\frac{1}{2}}$

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Table 1			
Method	p	$\sum \rho$	Half-Life
FE	*12*	*0.97767*	*35*
RCM	*5*	*0.97951*	*35*
MG	**5**	**0.97063**	**25**
Hausman Test	80.804 (0.000)		

Table 2			
Method	p	$\sum \rho$	Half-Life
FE	*12*	*0.97757*	*33*
RCM	*5*	0.97247	*26*
MG	**5**	37.068	**21**
Hausman Test	37.068 (0.000)		

Table 3. Monte Carlo Evidence on Bias Corrections

		Simple MG Estimator				MG CCE Estimator		
	True	Least Sq.	Indirect	Direct	So-Shin	Least Sq.	Indirect	Direct
$\lambda = 0$	14.54	10.78	14.96	13.06	14.81	10.11	16.45	12.82
$\lambda = 0.08$	14.54	11.14	15.74	13.57	15.45	10.16	16.58	12.88
$\lambda = 0.16$	14.53	11.47	16.43	14.04	16.25	10.23	16.76	12.97
$\lambda = 0.24$	14.53	12.02	17.73	14.85	17.17	10.32	16.83	13.08
$\lambda = 0.32$	14.54	12.69	19.45	15.81	18.70	10.41	16.93	13.17
$\lambda = 0.40$	14.52	13.34	21.24	16.80	20.24	10.54	17.06	13.33
$\lambda = 0.48$	14.54	14.35	24.23	18.32	22.89	10.77	17.14	13.56
$\lambda = 0.56$	14.53	16.08	31.24	21.01	26.60	11.16	17.26	13.93

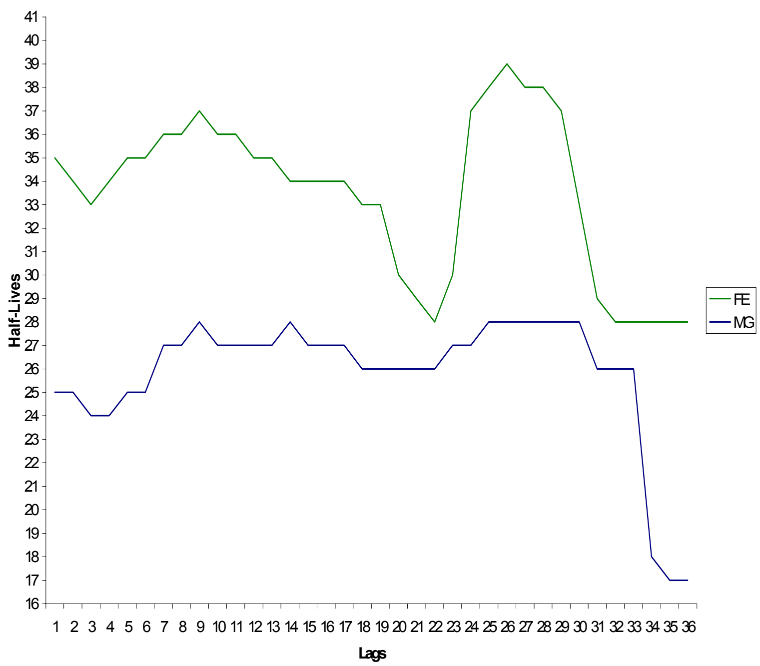
Notes: The table reports the mean MG estimates of the half-life of relative prices in a Monte Carlo experiment where the data is generated by the process: $q_{it} = \alpha_i + \beta_i q_{it-1} + x_t + \varepsilon_{it}$, $x_t = \lambda x_{t-1} + \xi_t$, $\alpha \sim N(0,1)$, $\beta \sim U[0.93, 0.99]$, $\varepsilon_i \sim i.i.d(0,1)$, $\xi \sim N(0,1)$. We assume that $T = 200$, and that $N = 180$. We initially draw 250 observations but then drop the first 50 to lower the impact of the initial condition. The column "True" reports the true half-life based on the impulse response function. The columns "least squares" report the least squares estimates of the half-life based on either the MG or the MG CCE estimators. The column denoted "So-Shin" reports the results of implementing the So and Shin (1999) bias correction to the MG estimator. The columns "indirect" and "direct" report the results of implementing the indirect and the direct versions of the bias reduction methods based on the Kilian (1998) bootstrap procedure. The number of replications is 1000.

Estimates using Charles Engel's Data

Table 4: Persistence Estimates using Disaggregated Data

$q_{ict} = \gamma_c + \sum_{k=1}^{K} \rho_{ik}\ q_{ict-k} + e_{ict}$					
Model	P	$\sum_{k=1}^{K} \rho_{ik}$	Half-Life	LAR	CIR
Fixed Effects	5	0.98	35 $(26, 43)$	0.97 $(0.962, 0.977)$	48.72
Fixed Effects (SURE)	5	0.98	35 $(28, 45)$	0.98 $(0.972, 0.983)$	49.53
Generalised Fixed Effects	5	0.99	50 $(24, 152)$	0.99 $(0.970, 0.995)$	71.35
Mean Group	5	0.97	25 $(9, 31)$	0.97 $(0.861, 0.974)$	35.09
Mean Group CCE	5	0.94	13 $(9, 24)$	0.93 $(0.827, 0.959)$	17.18
Mean Group (Sure)	5	0.97	21 $(17, 25)$	0.96 $(0.954, 0.973)$	29.50
$^{a}H0 : \rho_i = \rho$	80.804 (0.000)				

Figure 1



Half-Lives
Lags
FE
MG
41
40
39
38
37
36
35
34
33
32
31
30
29
28
27
26
25
24
23
22
21
20
19
18
17
16
1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20 21 22 23 24 25 26 27 28 29 30 31 32 33 34 35 36

Figure 2

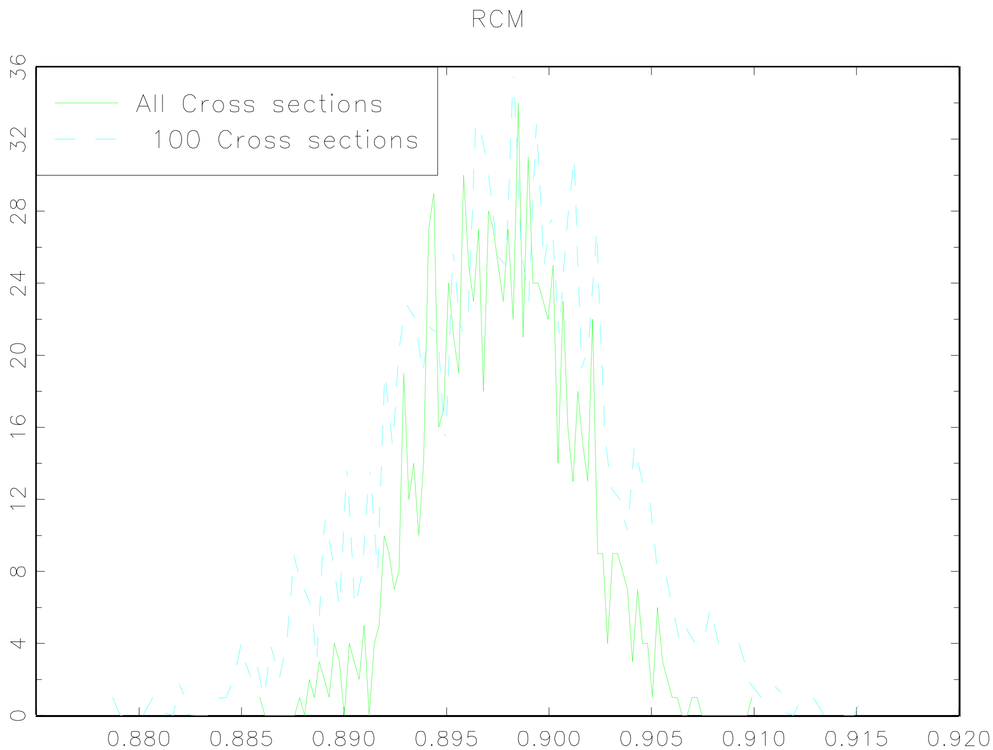

RCM
All Cross sections
100 Cross sections
0
4
8
12
16
20
24
28
32
36
0.880
0.885
0.890
0.895
0.900
0.905
0.910
0.915
0.920

Figure 3

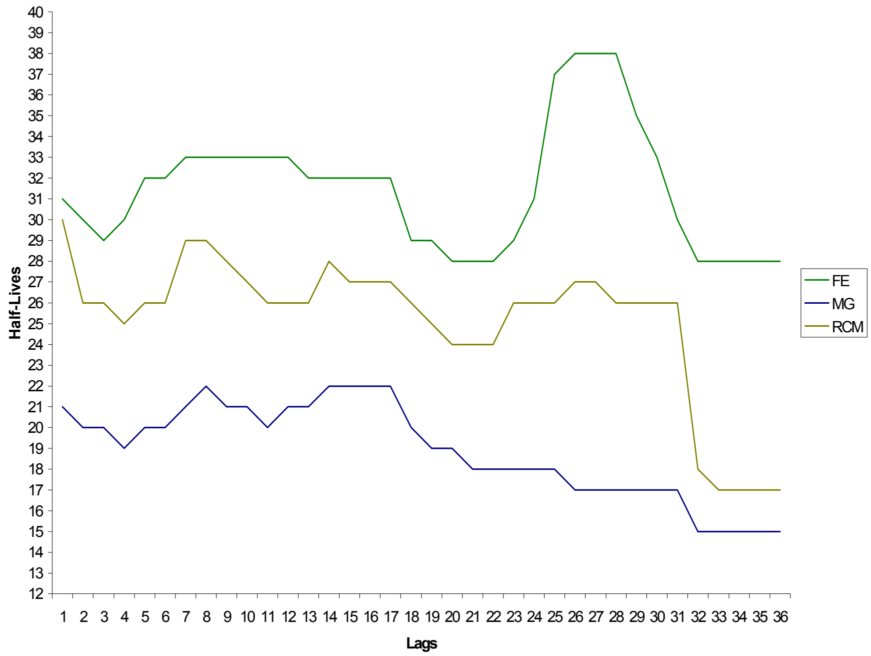
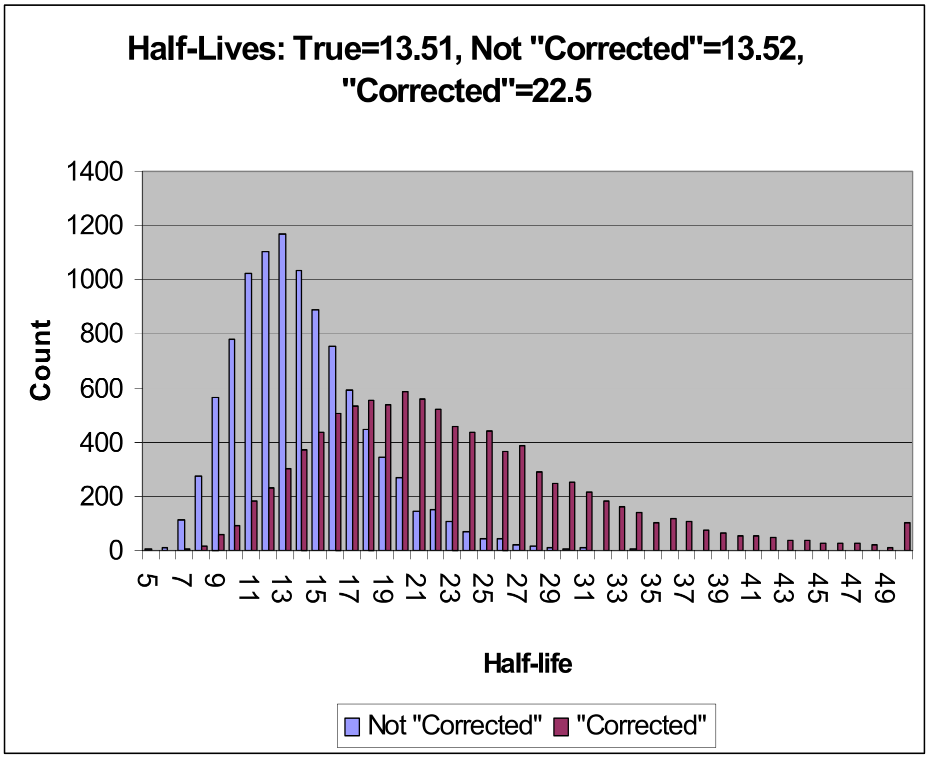


Figure 4



Half-Lives: True=13.51, Not "Corrected"=13.52, "Corrected"=22.5
1400
1200
1000
800
600
400
200
0
Count
5
7
9
11
13
15
17
19
21
23
25
27
29
31
33
35
37
39
41
43
45
47
49
Half-life
Not "Corrected"
"Corrected"

Figure 5

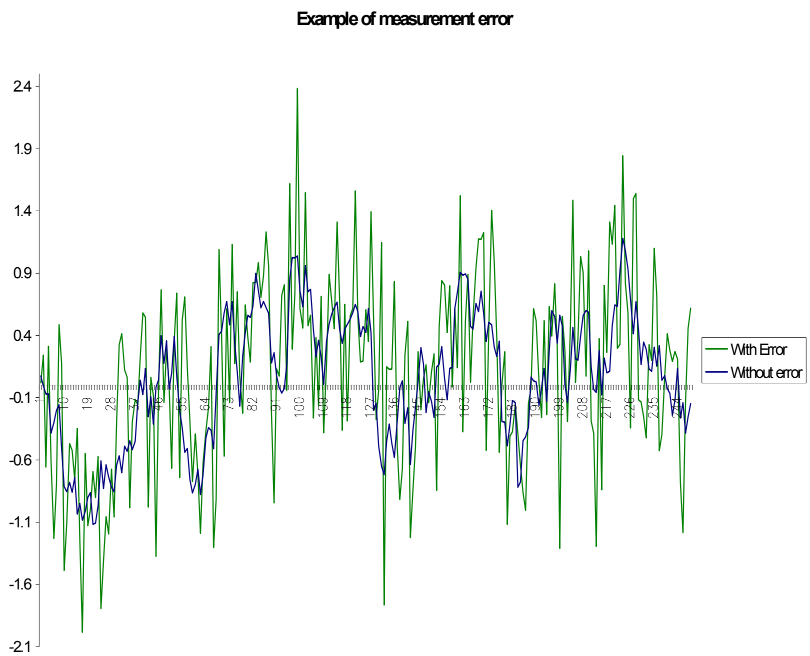
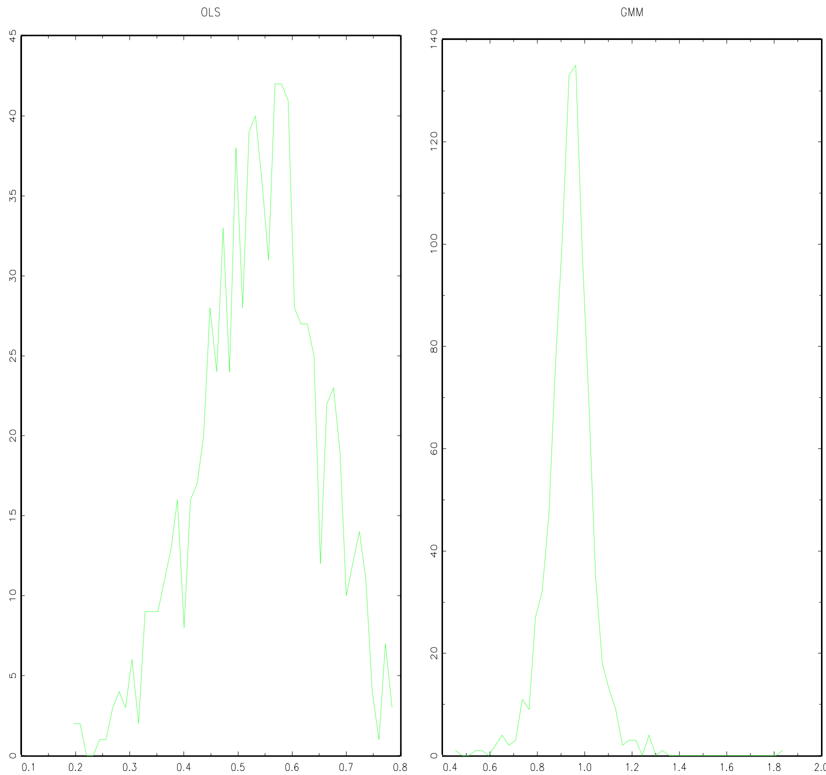

Example of measurement error
2.4
1.9
1.4
0.9
0.4
-0.1
-0.6
-1.1
-1.6
-2.1
With Error
Without error

Figure 6


OLS
GMM
0
5
10
15
20
25
30
35
40
45
0.1
0.2
0.3
0.4
0.5
0.6
0.7
0.8
0
20
40
60
80
100
120
140
0.4
0.6
0.8
1.0
1.2
1.4
1.6
1.8
2.0