Additional Material to "Do Consumers Really Follow a Rule of Thumb? Three Thousand Estimates from 144 Studies Say 'Probably Not' "*

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Abstract

This online appendix provides a short section on how excess sensitivity is estimated, motivation for the inclusion of control variables, list of studies used in the meta-analysis, and three sets of additional results: Hedges's model of publication bias, BMA weighted by precision and the inverse of the number of estimates reported in each study, and detailed diagnostics of the BMA exercises.

1 Estimating Excess Sensitivity

Here we describe the most common strategies for measuring excess sensitivity, since they have implications for the design of the meta-analysis: they determine which estimates are comparable enough to be collected and what aspects of methodology influence the estimates. Readers interested in more details on these approaches can refer to the surveys by Attanasio & Weber (2010) and Jappelli & Pistaferri (2010). The starting point for the analysis of excess sensitivity is the consumption Euler equation under the assumption of quadratic utility, which leads to the following specification:

$$\Delta C_{t+1} = \alpha_0 + \lambda E_t \Delta Y_{t+1} + \epsilon_{t+1}. \quad (1)$$

where C is the level of consumption, Y is the level of disposable income, ϵ is white noise, and λ is the magnitude of excess sensitivity, which should be zero under the permanent income hypothesis. The estimate of λ provides a metric that allows us to compare the size of the departure from the hypothesis across studies. Furthermore, λ can be matched to the parameters of theoretical models and thus has an economic interpretation. For these reasons we focus on estimates that are quantitatively comparable to λ from (1).

*The full paper, data, and code is available at `meta-analysis.cz/excess_sensitivity`.

A statistically significant λ may imply that some of the agents in the economy are not fully rational, and Campbell & Mankiw (1989) discuss an alternative theoretical model that allows for non-optimizing households. In the model there are two groups of consumers: rational consumers who behave according to the permanent income hypothesis and rule-of-thumb consumers who simply consume their current income. Campbell & Mankiw (1989) show that λ from (1) then corresponds to the fraction of income accruing to the rule-of-thumb consumers. The authors estimate this fraction on aggregate US data and find it to be around 0.5, which has become the rule of thumb on the share of rule-of-thumb consumers.

Alternatively, the empirical failure of the permanent income hypothesis may stem from a misspecification of the estimation equation. For consumption growth in (1) to be a martingale in the absence of rule-of-thumb behavior the utility function needs to be quadratic and separable between consumption and leisure, public and private goods, and time periods; the interest rate needs to be constant; and households must have the opportunity to borrow freely to be able to smooth consumption. A vast amount of empirical work has been devoted to testing the standard model when these assumptions are relaxed. A common approach is to add extra explanatory variables to the right-hand side of (1):

$$\Delta C_{t+1} = \alpha_0 + \lambda E_t \Delta Y_{t+1} + \sum_i \alpha_i X^i_{t+1} + \epsilon_{t+1}, \tag{2}$$

where X^i can stand for hours of work (to control for the non-separability between consumption and leisure; for example, Attanasio & Weber, 1995), public goods (for the non-separability between public and private consumption, Aschauer, 1993), lagged change in consumption (for habit formation, Sommer, 2007), the time-varying interest rate (Campbell & Mankiw, 1989), or some controls that capture the severity of liquidity constraints (Bacchetta & Gerlach, 1997).

The issue of liquidity constraints in particular has received a lot of attention. Among the pioneers are Hayashi (1982) and Flavin (1985), who discuss models that relate the magnitude of excess sensitivity to the share of consumers that face liquidity constraints. Similarly to the specification with rule-of-thumb consumers, these models predict a correlation between changes in consumption and predictable income changes: liquidity-constrained consumers cannot smooth consumption. Unlike the model with rule-of-thumb consumers, though, these models imply an asymmetric response of consumption to increases and declines in income. Additionally, they predict that wealthy households do not exhibit excess sensitivity, since they are not liquidity-constrained. Many empirical studies test these predictions on household-level data by comparing estimates of excess sensitivity for households that are likely to face liquidity constraints and those that are not (e.g., Zeldes, 1989). Other researchers compare the estimates of excess sensitivity for income increases and declines (Shea, 1995).

Because specification (2) holds only under quadratic utility, most researchers follow Campbell & Mankiw (1989) and estimate the model in the logarithmic form, which is the first-order log-linear approximation of the Euler equation under power utility (lowercase letters denote variables in logs):

$$\Delta c_{t+1} = \alpha_0 + \overline{\lambda} E_t \Delta y_{t+1} + \sum_i \alpha_i x_{t+1}^i + \epsilon_{t+1}. \tag{3}$$

Some authors use the second-order approximation, which attempts to avoid the omitted variable problem inherent in the first-order approximation. Nevertheless, the drawback of both approximations is that $\overline{\lambda}$ can no longer be interpreted directly as the share of income allocated to rule-of-thumb consumers or the fraction of liquidity-constrained households, as already pointed out by Campbell & Mankiw (1989). With power utility the Euler equation reads

$$E_t \left[\beta \left(\frac{C_{t+1}^{PIH}}{C_t^{PIH}} \right)^{\sigma-1} R_t \right] = 1, \tag{4}$$

where C_t^{PIH} is consumption of permanent income consumers (β is the subjective discount rate, σ measures risk aversion, and R is the rate of return on assets). When rule-of-thumb consumers exist, the aggregate consumption can be written as $C_t = C_t^{PIH} + \lambda Y_t$. Substituting this into (4) yields

$$E_t \left[\beta \left(\frac{C_{t+1} - \lambda Y_{t+1}}{C_t - \lambda Y_t} \right)^{\sigma-1} R_t \right] = 1. \tag{5}$$

Weber (2000) shows that λ from equation (5) does not precisely correspond to $\overline{\lambda}$ from (3). Therefore, some researchers estimate (5) directly using non-linear GMM (Weber, 2000, 2002), in which the interpretation of the parameter is the same as in specification (2). We collect estimates from both approximated and exact Euler equations and evaluate whether the two yield systematically different results.

Another distinction between studies is whether they employ aggregate or micro-level data. Studies with micro data typically estimate models similar to (2), such as Garcia *et al.* (1997), or (3), such as Lusardi (1996) and Jappelli & Pistaferri (2000). Apart from control variables that account for non-separabilities, a variable interest rate, and liquidity constraints, the x_{t+1}^i's in micro studies usually include taste shifters (for example, the age of the head of household or the number of children) and time fixed effects. Alternatively, some studies employ synthetic cohort data, grouping households by age and estimating the model parameters using cohort averages, such as in Attanasio & Weber (1993), Blundell *et al.* (1994), and Attanasio & Browning (1995).

An important critique of the micro literature testing Euler equations is raised by Carroll *et al.* (2018), who argue that many micro studies use instruments that identify permanent rather than transitory components of predictable income growth, therefore obtaining exaggerated estimates of the excess sensitivity parameter. There is, however, a subset of more recent micro studies that are not prone to this criticism. These studies use data that allow predictable transitory changes in income to be observed directly: for example, economic stimulus payments of 2008 (Parker *et al.*, 2013), pre-announced growth dividends in Singapore (Agarwal & Qian, 2014), or furloughs of government employees (Baker & Yannelis, 2017). Researchers in this stream of literature estimate marginal propensities to consume out of the observable predictable payments and do not rely on an Euler equation.

To evaluate excess sensitivity, some researchers test the orthogonality of innovations in consumption to lagged variables (primarily the lagged level of income), since under the permanent income hypothesis no lagged information helps predict consumption (for example, Runkle, 1991; Jappelli *et al.*, 1998). Zeldes (1989), for instance, estimates the following specification:

$$\Delta c_{t+1} = \alpha_0 + \lambda' y_t + \sum_i \alpha_i x^i_{t+1} + \epsilon_{t+1}. \tag{6}$$

Similarly to (2), a statistically significant estimate of λ' indicates a departure from the permanent income hypothesis. Nevertheless, λ' is incomparable with $\overline{\lambda}$ from (3), and there is no straightforward way to match it to model parameters such as the share of rule-of-thumb consumers or liquidity-constrained households. What is more, if the departure from the permanent income hypothesis is due to liquidity constraints, the theory predicts that λ' will be *negative*: consumers with a high level of past income are less likely to be liquidity-constrained, and therefore the degree of predictability in consumption growth should be smaller. For these reasons we do not collect estimates based on specification (6).

The seminal paper by Hall (1978) also discusses the implications of rule-of-thumb consumers, but Hall estimates only a reduced form of the consumption function, from which we cannot infer a metric that would capture structural model parameters. Therefore we cannot include this study in the data set. Several papers follow a similar strategy but estimate a system of equations that includes a process for income (Flavin, 1981; Hall & Mishkin, 1982) or households' budget constraint (Hayashi, 1982). Such specifications allow us to recover structural estimates of excess sensitivity that can be interpreted along the lines of Campbell and Mankiw's model, so we include them in the data set. We also include new studies that use data from online personal financial software and can directly observe spending patterns (among others, Gelman *et al.*, 2014; Baker & Yannelis, 2017; Gelman *et al.*, 2019; Kueng, 2018; Olafsson & Pagel, 2018).

2 Reasons for Including Individual Control Variables

Here we introduce variables that may influence the reported estimates of excess sensitivity (and that complement the analysis presented in Section 4 of the main body of the manuscript). We divide them into eight categories: data characteristics, measures of liquidity constraints, definitions of the utility function, consumption measures, income measures, specification characteristics, estimation techniques, and publication characteristics. We briefly outline our reasoning for including each variable.

Data characteristics To account for potential small-sample bias, we control for the number of observations used by the researchers. For example, Attanasio & Low (2004) note that log-linearized Euler equations may provide biased estimates of the underlying parameters if the time series used for the estimation is not long enough. We also include the average year of the data period to see whether there is a trend in the reported results. Of major importance is the dummy variable *Micro*, which equals one when the study uses micro-level data. Studies that use macro data rely on aggregated time series, and Attanasio & Weber (1995) show how such an aggregation can generate spurious excess sensitivity. About 40% of the estimates come from micro studies.

Next, we retain the variable needed to control for publication bias, the interaction between *Micro* and the reported standard error of the estimate. We also use a dummy variable reflecting the use of panel data, which allow the authors to control for unobservable household- or country-level factors. We distinguish between two groups of micro studies in our data set: the first group uses household-level data, while the second group constructs panels of birth cohorts (corresponding to the dummy variable *Synthetic cohort*). The synthetic cohort method, however, is used by only a small fraction of the studies. Concerning the frequency of the data used in the estimations, Bansal *et al.* (2012) argue that in consumption Euler equations the wrong choice of data frequency (that is, one not corresponding to consumers' decision frequency) can lead to biased results. We include dummy variables for monthly and annual frequencies, with quarterly data representing the baseline case.

Liquidity constraints While most studies that explore liquidity constraints are interested in identifying the excess sensitivity coefficient when the constraints are not binding (which we capture by the dummy *Liquidity unconstr.* explained above), some also estimate excess sensitivity under fully binding liquidity constraints. For this case we construct a dummy variable *Liquidity constr.* and explain it in detail in the main body of the manuscript: the dummy equals one, for example, when the author uses only data for poor households. Another aspect of study design is also connected to the issue of liquidity constraints: if liquidity-constrained households expect a drop in their income, the constraints to borrow are not binding because the optimal response in order to smooth consumption is to save (Altonji & Siow, 1987). We use the corresponding dummy variable, *Decrease in income*, separately from *Liquidity unconstr.*, because occurrences of decreases in expected income are scarce and the estimates are typically imprecise. For completeness we also include a control for the case where the estimate is computed using increases in income only; in this situation liquidity constraints are binding.

Utility function Predictable movements in consumption growth can also be generated by habit formation. Sommer (2007) argues that habit formation explains the observed response of consumption growth to income changes entirely, a result corroborated by Carroll *et al.* (2018), and we include a dummy variable that equals one when the study assumes habit formation while estimating excess sensitivity. Ten per cent of the studies in our sample do so. Next, Aschauer (1985) provides evidence suggesting that households' utility is non-separable between the consumption of private and public goods, which would mean that the assumption of separability results in a misspecification of the consumption Euler equation. Six per cent of the studies in our data set allow for this non-separability, and we examine whether such an approach has systematic effects on the results.

In a similar vein, several authors argue that disregarding the potential non-separability between consumption and leisure can lead to spurious estimates of excess sensitivity (for example, Basu & Kimball, 2002), and 7% of the studies follow this advice. On the other hand, in micro studies that use demographics to predict changes in income, controlling for leisure family structure may result in lower estimate of excess sensitivity, as this control is highly correlated with predictable income growth. Another potential source of bias in estimating excess sensitivity is ignoring the variation in the interest rate, so we include a dummy variable that equals one when the interest rate is included in the regression with expected income change and therefore the study also estimates the elasticity of intertemporal substitution. This is the case for about 40% of the studies in our data set.

Consumption measure Researchers often use consumption of only non-durable goods to estimate excess sensitivity; durable goods are excluded because of the volatility of spending on durables and the problems with imputing a service flow to the stock of durables. When durables are included, consumption growth also ceases to be white noise and becomes a moving-average process (Mankiw, 1982). Yet 41% of the studies also use durable consumption, and we control for this aspect of methodology. Many micro studies have to use food as a proxy for consumption due to data limitations, but Attanasio & Weber (1995) show that utility can in fact be non-separable between food and other categories of non-durable consumption, which may also result in a bias. About 14% of the studies use other subcategories of consumption, for example apparel. Again, such an approach can only be expected to yield unbiased results if utility is separable between the particular subcategory and other consumption goods.

Income measure An important feature of the studies estimating excess sensitivity is the definition of expected income. About a quarter of the studies use data that allow predictable changes in income to be observed directly: for example, economic stimulus payments of 2008 (Parker *et al.*, 2013), pre-announced growth dividends in Singapore (Agarwal & Qian, 2014), or furloughs of government employees (Baker & Yannelis, 2017). Next, 6% of the studies use current income changes and 2% use lagged income changes as a proxy for expected income growth, and we include the corresponding dummy variables. The baseline approach, employed by most studies in the literature, involves estimating expected income using instrumental variables. When data on disposable income are not directly available for the period and country under investigation, GDP is used instead; this is the case for 14% of the studies.

Concerning the instruments used to estimate expected income, the approach of the studies in our sample varies widely. The problem of weak instruments in particular has been a recurrent theme in the literature estimating the parameters of the consumption Euler equation (see, for example, Yogo, 2004; Kiley, 2010). Therefore we collect information on whether the authors report statistics on instrument strength and, if they do, whether the instruments are jointly significant at the 5% level. We find that 47% of the studies do not report these statistics, and most of the remaining studies report that the instruments are statistically insignificant. Hence we corroborate the 20-year-old observation by Browning & Lusardi (1996, pp. 1834) in their survey of the micro literature on excess sensitivity: "Very few studies present measures of fit for the auxiliary equation used to predict income growth but those that do (...) report very low R^2's." Next, to see whether the definition of the instrument set affects the reported results in a systematic way, we create dummy variables that reflect the inclusion of some of the typically used instruments: lags of consumption, lags of income, lags of the growth rates of those values, the nominal interest rate, inflation, the real interest rate, and other variables.

Specification Weber (2000) shows that the log-linear approximation of the consumption Euler equation does not yield estimates of excess sensitivity that can be directly attributed to the share of income accruing to rule-of-thumb consumers. Instead, he advocates estimating the exact Euler equation. In a more general setting, Carroll (2001) criticizes the first- and second-order approximations of the consumption Euler equation and shows that they can produce a bias in the estimated parameters. By contrast, Attanasio & Low (2004) argue that with sufficiently long panels the first-order approximation yields consistent estimates of the parameters in question. Moreover, Browning & Lusardi (1996) note that when estimating the exact, non-linear Euler equation it is difficult to address the problem of measurement error in consumption (which is likely substantial; Runkle, 1991). The advantage of the second-order approximation over the first-order approximation is the control for expected consumption risk (Jappelli & Pistaferri, 2000). We include two dummy variables, *Exact Euler* and *Second order*, to see whether the choice of the approximation of the Euler equation matters for the estimation of excess sensitivity. Two thirds of the studies, though, use the first-order approximation.

Several studies estimate the relationship between consumption and income in levels rather than in logs, which arises naturally with the assumption of the quadratic utility function, for which marginal utility is linear. As Campbell & Mankiw (1989) note, however, with power utility the estimation in levels becomes incorrectly specified. We collect such estimates of excess sensitivity (26% of our data set) and include a corresponding control variable to examine whether they differ systematically from the rest of the estimates. Next, a small fraction of the studies use both expected income changes and lagged expected income changes in their specification, which makes it possible to identify both the short-run and cumulative excess sensitivity (Wirjanto, 1996). The motivation for this approach is that rule-of-thumb consumers may react to changes in income with a lag. Once again we rather err on the side of inclusion and collect both short-run and cumulative estimates, but add a control for this method.

A number of studies assume a time shift and include interaction of the excess sensitivity coefficient with a dummy variable that equals one starting with a particular year. Such a specification yields two estimates of excess sensitivity corresponding to two different time periods. Next, for studies using household-level data with long time series it is important to include time fixed effects, because household consumption may be affected by aggregate shocks, which render forecast errors correlated across individual households. We find that 4% of studies using household data omit to include these controls. Finally, an old issue in consumption Euler equations is the control for time aggregation (Hall, 1988). One approach to this problem is to omit the first lags of variables from the instrument set (Campbell & Mankiw, 1989). Alternatively, researchers may account for serial correlation in the error term by directly estimating the moving average parameter with nonlinear instrumental variables methods (Cushing, 1992; Carroll *et al.*, 1994).

Technique We also control for the econometric technique used in the estimation, which, however, overlaps with and is often dictated by the definition of the measure of income described above. The studies in our data set typically use either GMM (the reference category for our set of dummies; 24% of the estimates) or TSLS (41% of the estimates); the latter assumes homoskedastic errors. Techniques based on maximum likelihood are used by 9% of the estimates, while OLS is employed in 23% of cases. An additional disadvantage of OLS with respect to approaches based on instrumental variables is the limited possibility to control for measurement error. Finally, a small number of estimates are constructed using a switching regression, which is sometimes employed to isolate consumers that face liquidity constraints.

Publication While we attempt to control for relevant aspects of data and methodology that influence the reported estimates of excess sensitivity, it is impossible to account for all the differences that we observe in the literature. Study quality, in particular, is hard to codify. One solution is to introduce study fixed effects, which we use in the section on publication bias. Nevertheless, many of the data and method variables discussed in this section display very limited within-study variation (for example, the use of micro data), so that we cannot use these variables and study-level fixed effects in the same specification. What we can do is include variables that proxy for study quality. The first such variable is publication year, which reflects implicit advances in data and methodology not captured by the variables introduced earlier. To account for different publication lags at different journals, we collect the year when the study first appears in Google Scholar as a working paper, which is typically 3 years prior to final publication. We control for the number of citations normalized by study age. Moreover, we include a dummy variable that equals one if the study is published in one of the top five general interest journals in economics and also use the recursive discounted RePEc impact factor of the journal.

3 Hedges's Model of Publication Bias

Hedges (1992) introduces a model which assumes that the probability of publication of estimates is determined by their statistical significance. The probability of publication changes only when a psychologically important p-value is reached: in economics these threshold values are 0.01, 0.05, and 0.1. When no publication bias is present, all estimates, significant and insignificant at the conventional levels, should have the same probability of being published. We estimate both the original model of Hedges (1992) and the augmented model of Ashenfelter *et al.* (1999), which allows for heterogeneity related to publication bias in the estimates of the underlying

Table 1: Hedges's test of publication bias

	Unrestricted model		Restricted ($\omega_j = 1$)	
	Coefficient	Standard error	Coefficient	Standard error
ω_2	-1.087	0.250		
ω_3	-0.426	0.213		
ω_4	0.377	0.087		
Constant	0.083	0.008	0.12	0.005
σ	0.129	0.005	0.13	0.005
Log likelihood	1259.0		1190.0	
Observations	1,224		1,224	
χ^2 (H_0: all estimates have the same probability of publication): 137.8, p-value < 0.001.				

Notes: In the absence of publication bias estimates with different statistical significance should have the same probability of being reported. ω_1, the weight associated with the probability of publication for estimates significant at the 1% level, is set to 1. ω_2, ω_3, and ω_4 show the relative probabilities for estimates significant only at the 5% level, estimates significant only at the 10% level, and insignificant estimates. σ is the estimated measure of heterogeneity (standard deviation) of the estimates of excess sensitivity.

Table 2: Hedges's test of publication bias, controlling for publication characteristics

	Unrestricted model		Restricted ($\omega_j = 1$)	
	Coefficient	Standard error	Coefficient	Standard error
ω_2	-0.814	0.217		
ω_3	-0.264	0.188		
ω_4	0.418	0.079		
Publication year	-0.0001	0.0007	-0.001	0.001
Citations	0.062	0.007	0.060	0.007
Top journal	-0.037	0.014	-0.037	0.013
Journal impact	-0.078	0.007	-0.078	0.021
Constant	0.087	0.021	0.142	0.004
σ	0.116	0.005	0.118	0.004
Log likelihood	1344.8		1285.6	
Observations	1,224		1,224	
χ^2 (H_0: all estimates have the same probability of publication): 118.5, p-value < 0.001.				

Notes: In the absence of publication bias estimates with different statistical significance should have the same probability of being reported. ω_1, the weight associated with the probability of publication for estimates significant at the 1% level, is set to 1. ω_2, ω_3, and ω_4 show the relative probabilities for estimates significant only at the 5% level, estimates significant only at the 10% level, and insignificant estimates. σ is the estimated measure of heterogeneity (standard deviation) of the estimates of excess sensitivity.

effect. The augmented log-likelihood function is (Ashenfelter *et al.*, 1999, p. 468)

$$L = c + \sum_{i=1}^{n} \log w_i(X_i, \omega) - \frac{1}{2} \sum_{i=1}^{n} \Big(\frac{X_i - \mathbf{Z}_i \Delta}{\eta_i}\Big)^2 - \sum_{i=1}^{n} \log(\eta_i) - \sum_{i=1}^{n} \log \Big[\sum_{j=1}^{4} \omega_j B_{ij}(\mathbf{Z}_i \Delta, \sigma) \Big], \quad (7)$$

where $X_i \sim N(\Delta, \eta_i)$ are the estimates of excess sensitivity. The parameter Δ is the average underlying excess sensitivity, and $\eta_i = \sigma_i^2 + \sigma^2$, where σ_i are the reported standard errors of the estimates and σ measures heterogeneity in the estimates. The probability of publication is determined by the weight function $w(X_i)$. In this model $w(X_i)$ is a step function associated with the p-values of the estimates. We choose four steps reflecting different levels of conventional statistical significance of the estimates: p-value < 0.01, $0.01 <$ p-value < 0.05, $0.05 <$ p-value < 0.1, and p-value > 0.1. $B_{ij}(\Delta, \sigma)$ represents the probability that an estimate X_i will be assigned weight ω_i. For the first step, p-value < 0.01, we normalize ω to 1 and evaluate whether the remaining three weights differ from this value. Z_i is a vector of the characteristics of estimate X_i; here we opt to include publication characteristics of the estimate (publication year, number of citations, publication in a top journal, and impact factor of the journal where the study was published) which might potentially be related to publication bias. We include only micro estimates in the model.

Table 1 shows the estimation results of the model where Z includes only a constant (that is, no heterogeneity in the estimates of excess sensitivity is explicitly modeled). The table includes two models, an unrestricted model and a restricted model with restriction $\omega_2 = \omega_3 = \omega_4 = 1$. The unrestricted model assumes publication bias, while the restricted model assumes no bias (in other words, all coefficients have the same probability of being published, their different statistical significance notwithstanding). The restriction is rejected, which suggests publication bias: estimates significant at the 1% level are much more likely to get published than all other estimates (the differences among the three remaining groups are not statistically significant). The results are similar when we allow for heterogeneity in the estimates of excess sensitivity that might potentially be related to publication bias (Table 2).

4 Weighted BMA

The tables on the following two pages show the results of the baseline BMA exercise when observations are weighted, respectively, by precision and by the number of estimates reported per study.

Table 3: Why do estimates of excess sensitivity differ? (precision weights)

Response variable:	Bayesian model averaging			Frequentist check (OLS)		
Estimate of ES	Post. mean	Post. SD	PIP	Coef.	Std. er.	p-value
Data characteristics						
No. of obs.	-0.020	0.002	1.000	-0.019	0.007	0.008
Midyear of data	0.004	0.001	1.000	0.003	0.002	0.054
Micro	-0.337	0.024	1.000	-0.333	0.057	0.000
Micro x SE (bias)	0.703	0.077	1.000	0.891	0.163	0.000
Panel	0.119	0.019	1.000	0.110	0.055	0.047
Synthetic cohort	-0.008	0.021	0.151			
Annual frequency	0.015	0.020	0.400			
Monthly frequency	0.000	0.003	0.016			
Liquidity constraints						
Liquidity unconstr.	0.000	0.000	0.018			
Decrease in income	0.069	0.012	1.000	0.068	0.027	0.013
Liquidity constr.	0.010	0.007	0.768	0.015	0.002	0.000
Increase in income	0.000	0.001	0.007			
Utility function						
Habits	-0.083	0.011	1.000	-0.094	0.031	0.002
Nonsep. public	0.000	0.002	0.009			
Nonsep. labor	-0.069	0.011	1.000	-0.065	0.021	0.002
Interest rate	0.055	0.015	0.998	0.035	0.031	0.257
Consumption measure						
Total consumption	0.000	0.002	0.023			
Food	0.000	0.000	0.018			
Indiv. category	-0.017	0.005	0.986	-0.016	0.006	0.008
Income measure						
Outside income	-0.126	0.023	1.000	-0.123	0.035	0.000
Current income	-0.025	0.032	0.428			
Lagged income	-0.201	0.029	1.000	-0.200	0.039	0.000
GDP proxy	0.281	0.016	1.000	0.280	0.049	0.000
Instruments signif.	0.010	0.018	0.261			
Signif. not reported	-0.003	0.010	0.111			
Consumption instr.	0.002	0.008	0.066			
Income instr.	-0.017	0.023	0.392			
Difference instr.	0.000	0.002	0.010			
Nominal IR instr.	0.062	0.024	0.935	0.071	0.043	0.097
Inflation instr.	0.082	0.025	0.981	0.078	0.054	0.144
Real IR instr.	-0.021	0.024	0.477			
Other instr.	0.000	0.001	0.009			
Specification						
Exact Euler	-0.005	0.018	0.091			
Estimated in levels	0.000	0.001	0.008			
Second order	-0.094	0.019	1.000	-0.100	0.033	0.003
Short run	0.000	0.001	0.008			
Cumulative	0.016	0.013	0.676	0.017	0.010	0.093
Time shift	0.000	0.004	0.009			
No year dummies	0.031	0.033	0.529			
Time aggregation	0.046	0.014	0.977	0.044	0.035	0.213
Technique						
ML	0.000	0.004	0.019			
TSLS	-0.101	0.012	1.000	-0.097	0.025	0.000
OLS	-0.007	0.015	0.199			
Switching regr.	0.003	0.014	0.056			
Publication						
Publication year	0.006	0.001	1.000	0.006	0.002	0.001
Citations	0.017	0.006	0.956	0.014	0.010	0.173
Top journal	0.055	0.013	0.998	0.047	0.024	0.046
Journal impact	-0.035	0.005	1.000	-0.041	0.011	0.000
Constant	0.115	NA	1.000	0.128	0.081	0.116
Observations	3,127			3,127		

Notes: Weighted by precision. PIP = posterior inclusion probability. SD = standard deviation. In the frequentist check we include only explanatory variables with PIP > 0.5. The standard errors in the frequentist check are clustered at both the study and data set level.

Table 4: Why do estimates of excess sensitivity differ? (study weights)

Response variable:	Bayesian model averaging			Frequentist check (OLS)		
Estimate of ES	Post. mean	Post. SD	PIP	Coef.	Std. er.	*p*-value
Data characteristics						
No. of obs.	0.000	0.002	0.037			
Midyear of data	0.003	0.001	0.998	-0.002	0.003	0.426
Micro	-0.322	0.044	1.000	-0.291	0.048	0.000
Micro x SE (bias)	0.454	0.048	1.000	0.437	0.124	0.000
Panel	-0.025	0.039	0.341			
Synthetic cohort	0.000	0.005	0.012			
Annual frequency	0.145	0.018	1.000	0.108	0.042	0.011
Monthly frequency	-0.157	0.031	1.000	-0.157	0.064	0.014
Liquidity constraints						
Liquidity unconstr.	0.000	0.002	0.009			
Decrease in income	0.321	0.036	1.000	0.337	0.134	0.012
Liquidity constr.	0.000	0.003	0.009			
Increase in income	-0.005	0.018	0.090			
Utility function						
Habits	-0.162	0.023	1.000	-0.149	0.063	0.019
Nonsep. public	0.000	0.005	0.014			
Nonsep. labor	-0.123	0.026	1.000	-0.112	0.046	0.014
Interest rate	0.147	0.019	1.000	0.141	0.081	0.083
Consumption measure						
Total consumption	0.019	0.026	0.396			
Food	-0.004	0.016	0.070			
Indiv. category	0.000	0.004	0.013			
Income measure						
Outside income	-0.001	0.006	0.019			
Current income	-0.269	0.029	1.000	-0.274	0.124	0.027
Lagged income	-0.298	0.036	1.000	-0.316	0.077	0.000
GDP proxy	0.309	0.028	1.000	0.325	0.099	0.001
Instruments signif.	0.001	0.010	0.027			
Signif. not reported	0.010	0.023	0.220			
Consumption instr.	0.000	0.003	0.013			
Income instr.	-0.001	0.008	0.042			
Difference instr.	-0.001	0.007	0.032			
Nominal IR instr.	0.062	0.039	0.779	0.061	0.045	0.176
Inflation instr.	-0.001	0.007	0.025			
Real IR instr.	-0.112	0.021	1.000	-0.123	0.081	0.128
Other instr.	-0.003	0.011	0.070			
Specification						
Exact Euler	-0.310	0.039	1.000	-0.329	0.093	0.000
Estimated in levels	-0.020	0.029	0.381			
Second order	-0.153	0.027	1.000	-0.134	0.060	0.025
Short run	0.178	0.025	1.000	0.161	0.133	0.226
Cumulative	0.001	0.007	0.017			
Time shift	0.010	0.032	0.106			
No year dummies	0.026	0.043	0.310			
Time aggregation	0.013	0.024	0.258			
Technique						
ML	0.001	0.007	0.020			
TSLS	-0.126	0.016	1.000	-0.141	0.042	0.001
OLS	0.000	0.004	0.014			
Switching regr.	0.222	0.042	1.000	0.247	0.111	0.027
Publication						
Publication year	0.009	0.001	1.000	0.011	0.005	0.024
Citations	0.034	0.011	0.983	0.028	0.021	0.190
Top journal	0.093	0.030	0.978	0.096	0.042	0.022
Journal impact	-0.022	0.020	0.612	-0.038	0.025	0.132
Constant	-0.005	NA	1.000	0.351	0.150	0.019
Observations	3,127			3,127		

Notes: Weighted by the inverse of the number of estimates reported per study. PIP = posterior inclusion probability. SD = standard deviation. In the frequentist check we include only explanatory variables with PIP > 0.5. The standard errors in the frequentist check are clustered at both the study and data set level.

5 Diagnostics of BMA

Table 5: Summary of BMA estimation, baseline specification

Mean no. regressors 19.5920	*Draws* $1 \cdot 10^8$	*Burn-ins* $5 \cdot 10^7$	*Time* 3.70969 hours
No. models visited $16,157,481$	*Modelspace* $2.8 \cdot 10^{14}$	*Visited* $5.7 \cdot 10^{-7}\%$	*Topmodels* 81%
Corr PMP 1.0000	*No. Obs.* $3,127$	*Model Prior* uniform	*g-Prior* UIP
Shrinkage-Stats Av= 0.9997			

Notes: No weights are used. In this specification we employ the priors suggested by Eicher *et al.* (2011) based on predictive performance: the uniform model prior (each model has the same prior probability) and the unit information prior (the prior provides the same amount of information as one observation of data).

Figure 1: Model size and convergence, baseline specification

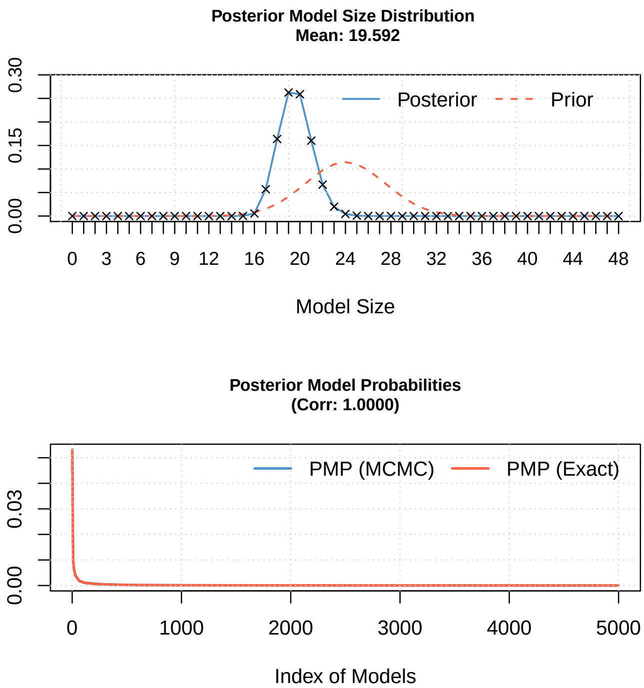


Table 6: Summary of BMA estimation, precision weights

Mean no. regressors	*Draws*	*Burn-ins*	*Time*
26.8321	$1 \cdot 10^8$	$5 \cdot 10^7$	2.73489 hours
No. models visited	*Modelspace*	*Visited*	*Topmodels*
19, 363, 590	$2.8 \cdot 10^{14}$	$6.9 \cdot 10^{-6}$%	75%
Corr PMP	*No. Obs.*	*Model Prior*	*g-Prior*
0.9999	3, 127	uniform	UIP
Shrinkage-Stats			
Av= 0.9997			

Notes: The inverse of the reported estimate's standard error is used as the weight. In this specification we employ the priors suggested by Eicher *et al.* (2011) based on predictive performance: the uniform model prior (each model has the same prior probability) and the unit information prior (the prior provides the same amount of information as one observation of data).

Figure 2: Model size and convergence, precision weights

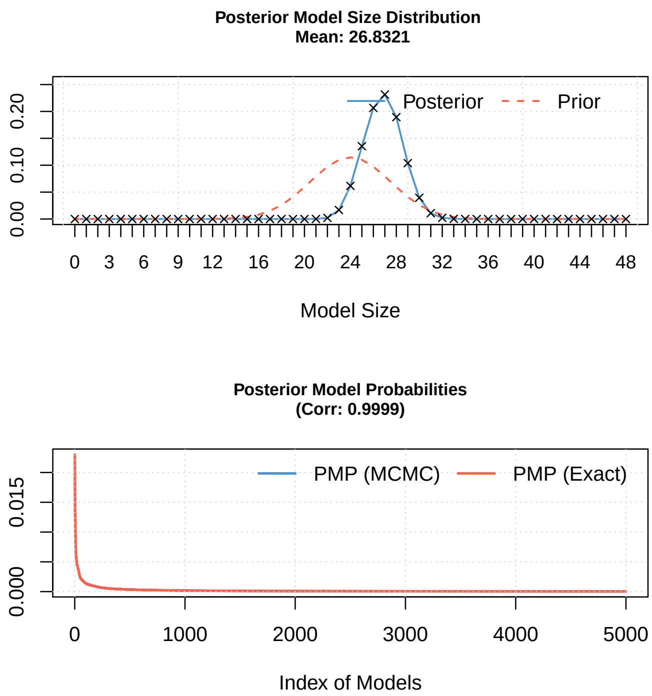
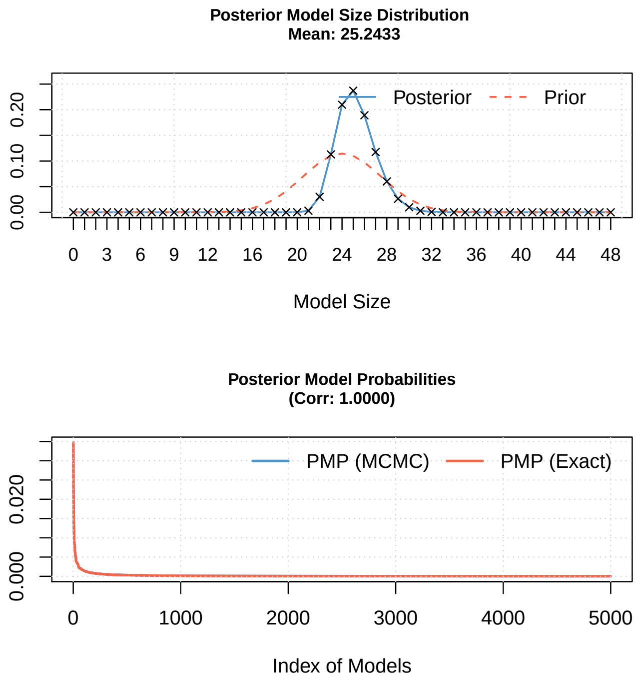


Table 7: Summary of BMA estimation, weights based on the number of estimates per study

Mean no. regressors	*Draws*	*Burn-ins*	*Time*
25.2433	$1 \cdot 10^8$	$5 \cdot 10^7$	2.790386 hours
No. models visited	*Modelspace*	*Visited*	*Topmodels*
18,909,766	$2.8 \cdot 10^{14}$	$6.7 \cdot 10^{-6}\%$	82%
Corr PMP	*No. Obs.*	*Model Prior*	*g-Prior*
1.0000	3,127	uniform	UIP
Shrinkage-Stats			
Av= 0.9997			

Notes: The inverse of the number of estimates reported per study is used as the weight. In this specification we employ the priors suggested by Eicher *et al.* (2011) based on predictive performance: the uniform model prior (each model has the same prior probability) and the unit information prior (the prior provides the same amount of information as one observation of data).

Figure 3: Model size and convergence, weights based on the number of estimates per study



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