Do Consumers Really Follow a Rule of Thumb? Three Thousand Estimates from 130 Studies Say "Probably Not"*

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Abstract

We show that three factors combine to explain the mean excess sensitivity reported in studies estimating consumption Euler equations: the use of macro data, publication bias, and liquidity constraints. When micro data are used, publication bias is corrected for, and the households under examination do not face liquidity constraints, the literature implies no evidence for the excess sensitivity of consumption to income. Hence little remains for pure rule-of-thumb behavior. The results hold when we control for 45 additional variables reflecting the methods employed by researchers and use Bayesian model averaging to account for model uncertainty. The estimates of excess sensitivity are also systematically affected by the order of approximation of the Euler equation, the treatment of non-separability between consumption and leisure, and the choice of proxy for consumption.

Keywords: Excess sensitivity, rule-of-thumb consumers, liquidity constraints, publication bias, Bayesian model averaging

JEL Codes: C83, D12, E21

1 Introduction

A burgeoning literature investigates the effects of monetary and fiscal policy in a framework where a fraction of households neither save nor borrow, but follow the rule of thumb to consume their current income. Galí *et al.* (2004) show that the existence of such consumers affects the effectiveness of standard monetary policy rules, while Galí *et al.* (2007) document how rule-of-thumb behavior can help reconcile model predictions and empirical evidence concerning the effects of government spending on private consumption. Models with a sufficiently high share of rule-of-thumb consumers produce large fiscal multipliers, as illustrated by Leeper *et al.* (2017). The calibrated or prior value used for this share varies, but is usually substantial: for example,

*An online appendix with data and code is available at `meta-analysis.cz/excess_sensitivity`. Corresponding author: Tomas Havranek, `tomas.havranek@ies-prague.org`

Drautzburg & Uhlig (2015) use 0.25, Leeper *et al.* (2017) use 0.3, Bilbiie (2008) and Kriwoluzky (2012) use 0.4, Erceg *et al.* (2006), Galí *et al.* (2007), Forni *et al.* (2009), Cogan *et al.* (2010), Colciago (2011), and Furlanetto & Seneca (2012) use 0.5, while Andres *et al.* (2008) use 0.65. Models used by policymaking institutions to analyze fiscal stimulus typically assume 0.2–0.5 (Coenen *et al.*, 2012). We find that the literature on the excess sensitivity of consumption to anticipated income growth, often cited as the motivation for the calibrations, is inconsistent with such values. When corrected for the bias due to the omission of demographic controls and the bias due to publication selection, the literature yields a mean excess sensitivity of merely 0.13. That is outside the 90% probability interval even for the conservative prior used by Leeper *et al.* (2017). The remaining excess sensitivity, moreover, can be explained by liquidity constraints.

We thus argue that the empirical research on excess sensitivity is consistent with the standard representative-agent model in which the consumer behaves, to a first approximation, according to the permanent income hypothesis. To obtain this result, we collect 2,788 estimates of excess sensitivity reported in 133 published studies relying on consumption Euler equations and investigate why the estimates vary. In doing so, we take on the challenge put forward by the first survey of the micro literature estimating excess sensitivity, Browning & Lusardi (1996, p. 1833): "It is frustrating in the extreme that we have very little idea of what gives rise to the different findings. (. . .) We still await a study which traces all of the sources of differences in conclusions to sample period; sample selection; functional form; variable definition; demographic controls; econometric technique; stochastic specification; instrument definition; etc." To this end we use the methodology of meta-analysis, which has been employed in economics, for example, by Chetty *et al.* (2013) on the Frisch elasticity of labor supply, Havranek *et al.* (2015) on the elasticity of intertemporal substitution in consumption, and Card *et al.* (2018) on the effects on active labor market policy. We collect 48 variables that reflect the context in which researchers obtain their estimates, and use Bayesian model averaging to evaluate the variables' impact while accounting for model uncertainty.

Our results suggest that three factors contribute equally to the mean reported excess sensitivity, 0.4: methodology issues (especially the use of macro data), selective reporting of estimates (publication bias), and structural reasons for excess sensitivity (liquidity constraints). The mean coefficient corrected for the three factors mentioned above is zero, which implies no evidence for pure rule-of-thumb behavior related to the Keynesian consumption function. Aside from the difference between micro and macro studies, other aspects of data and methods systematically affect the reported estimates of excess sensitivity—but in different directions, so that their effects cancel out when we focus on the estimate conditional on best practice. We find that it is important to account for the non-separability between consumption and leisure and for intertemporal substitution in consumption. The measure of consumption matters for the estimated excess sensitivity, while the measure of income and the set of instruments in the model do not have a systematic impact on the results. The order of approximation of the Euler equation influences the results significantly, as does the treatment of time aggregation. In contrast, the choice of estimation technique does not affect the results in a systematic way.

We find that publication bias impacts micro studies, but not macro studies: because the underlying excess sensitivity is much smaller for micro data, researchers who strive to avoid negative (thus unintuitive) results have to engage in more specification searching with micro data than with macro data. We also find indications that researchers prefer to publish statistically significant results, which is consistent with Brodeur *et al.* (2016), who collect 50,000 p-values from various fields of economics and show that insignificant estimates are systematically underreported. In a similar vein, Ioannidis *et al.* (2017) survey evidence from 6,700 econometric studies and conclude that nearly 80% of the reported effects are exaggerated because of publication bias. In the context of excess sensitivity and rule-of-thumb consumption, however, the bias has received little attention, and we have not found any study that mentions this problem while building a calibration on previous estimates. As Glaeser (2006) remarks, economists tend to assume that the representative agent in their models maximizes her utility, but typically do not extend that assumption to the behavior of the average economist trying to publish her work. As we show in the remainder of the paper, the consequences of publication bias are at least as serious as the effects of the widely discussed misspecifications in the estimation of preference parameters using Euler equations.

2 Estimating Excess Sensitivity

In this section 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 the approaches to examining the response of consumption to income changes 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:

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

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).

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, as done by, 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, 1995a).

Because specification (2) only holds 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):

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

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 major distinction between studies is whether they employ aggregate or micro-level data. Studies that use 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). The majority of studies, however, use aggregate data.

Examining the relation between changes in consumption and predictable income changes is not the only way to test for 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 (e.g., Runkle, 1991; Jappelli *et al.*, 1998). Zeldes (1989), for instance, estimates the following specification:

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

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 having a share of income accruing to rule-of-thumb consumers, but Hall only estimates 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. In Section 5 we discuss in more detail the various contexts in which researchers obtain estimates of excess sensitivity.

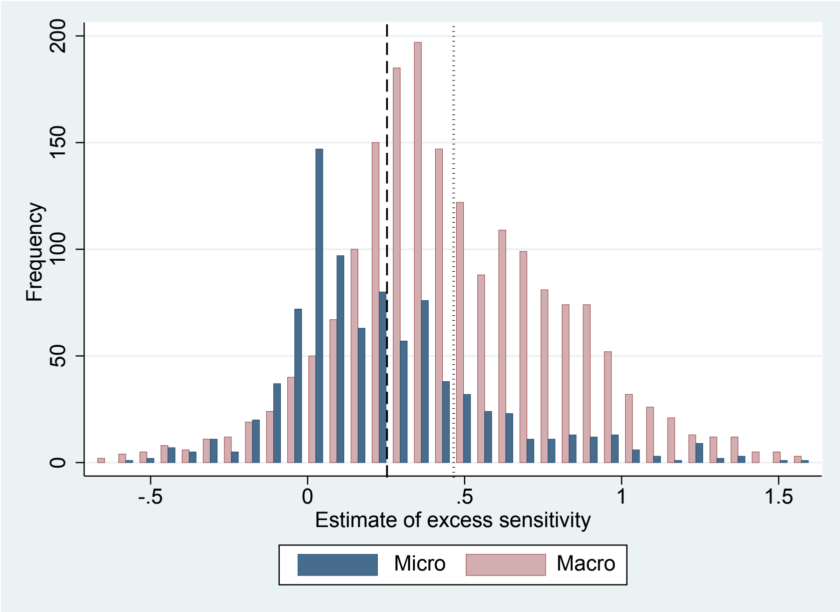
3 Data

To search for empirical studies on excess sensitivity we use Google Scholar, because unlike other commonly employed databases it goes through the full text of studies in addition to the title, abstract, and keywords. We design our search query so that it shows the best known empirical studies (surveyed in the previous section) among the first hits, and then read the abstracts of the first 400 studies returned by the search. The list of these studies, along with the search query, is available in the online appendix. When it is clear from the abstract that the study does not contain empirical estimates of excess sensitivity (for example, when the study is apparently theoretical), we move to the next abstract; otherwise we download the study and read it. Additionally we inspect the references and citations of the included studies to make sure we do not miss those that are not shown by our baseline search but could still be used. We add the last study on February 1, 2016, and terminate the search.

We apply three inclusion criteria. First, the study must present an empirical estimate of excess sensitivity quantitatively comparable with λ from (1). As we discuss in the previous section, estimates in some studies do not even display the same sign when the permanent income hypothesis is rejected (for instance, Zeldes, 1989). The incomparable estimates only form a small portion of the literature, since a vast majority of studies specify both consumption and income in differences. Second, the study must report standard errors for its estimates or other statistics from which standard errors can be computed.[1] In the next section we show that standard errors are necessary to allow testing for publication bias. Third, primarily due to feasibility considerations, we only collect published studies. Other things being equal, published studies are likely to be of higher quality than unpublished manuscripts because they are typically peer-reviewed. Published studies also tend to be better typeset, which reduces the danger of mistakes in data collection.

[1]Sometimes we have to employ the delta method to compute the standard error, typically when the authors include dummy variables multiplied by the expected change in income, thus replacing $\overline{\lambda} E_t \Delta y_{t+1}$ in (3) with $(\overline{\lambda}_1 + \overline{\lambda}_2 \cdot dummy) E_t \Delta y_{t+1}$. For example, Souleles (2002) adds dummies for consumers with low liquid wealth and low income, and for old consumers. Jappelli & Pistaferri (2011) add a dummy for post-1999 observations to test whether excess sensitivity in Italy is affected by the introduction of the euro. In such cases we collect the estimates of $\overline{\lambda}_1$ and $\overline{\lambda}_1 + \overline{\lambda}_2$ and approximate the standard errors with the estimated $se(\overline{\lambda}_1)$ and $\left[se(\overline{\lambda}_1)^2 + se(\overline{\lambda}_2)^2\right]^{1/2}$, because the authors typically do not report covariances between the estimates of regression parameters.

Figure 1: Micro data yield smaller estimates of excess sensitivity

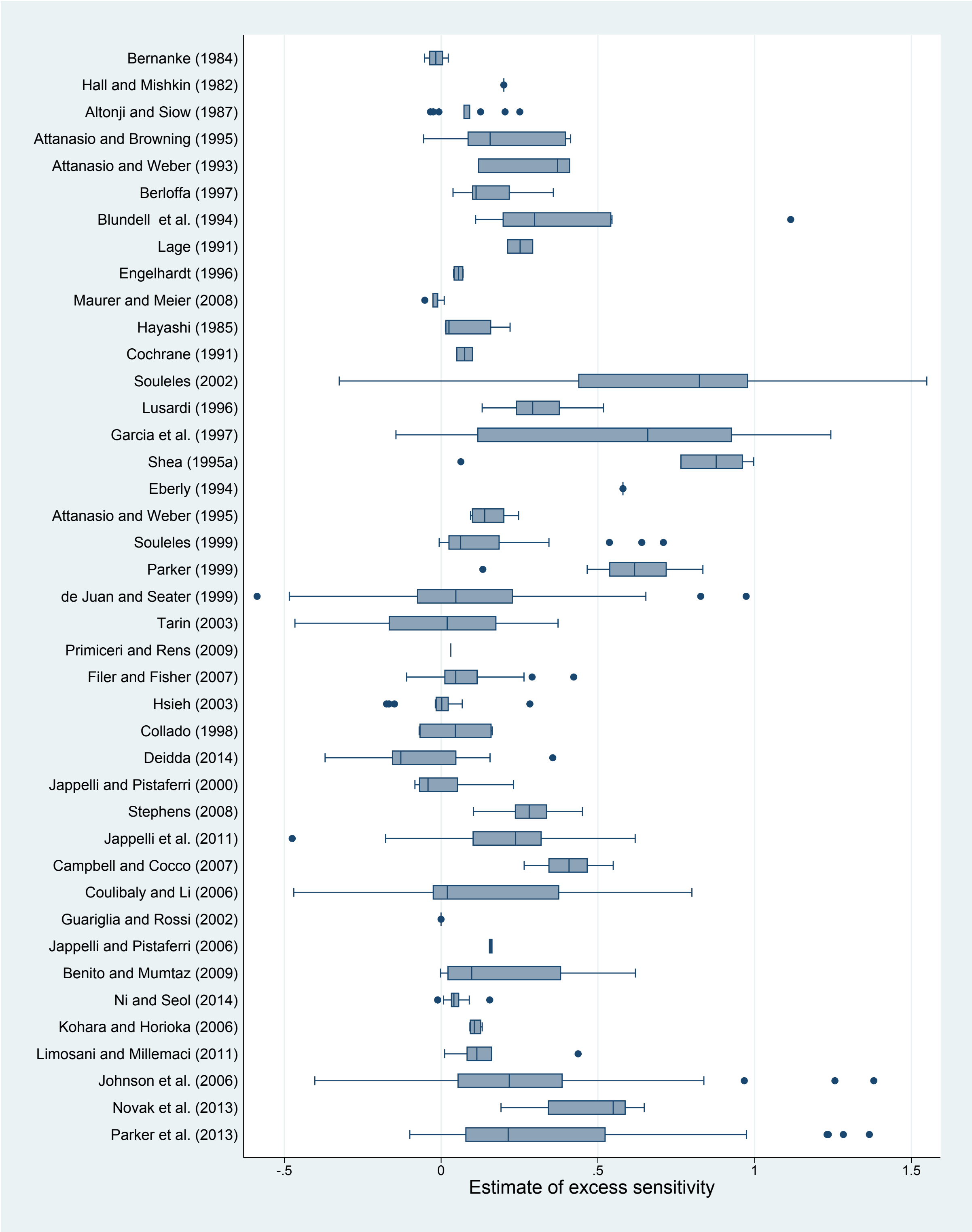


Notes: The figure shows histograms of the estimates of excess sensitivity reported in studies using micro and macro data. The dashed line denotes the mean of micro estimates; the dotted line denotes the mean of macro estimates.

Even with the restricted focus on published studies, our data set is to our knowledge the largest one ever used in an economic meta-analysis. We find 133 studies that conform to our inclusion criteria (the studies are listed in Appendix E), and together the studies provide 2,788 estimates of excess sensitivity. To put these numbers into perspective, we refer to the survey by Doucouliagos & Stanley (2013), who review 87 earlier meta-analyses and find that the largest one includes 1,460 estimates from 124 studies. The oldest study in our data set was published in 1981 and the newest one in 2015, so our data set spans three and half decades of research. We collect all estimates reported in the studies: it is often impossible to determine which estimate the authors prefer, and including all estimates provides us with more variation to examine the sources of heterogeneity in the results. For this reason we also keep results from less prestigious journals, but in addition to data and methodology differences control for journal impact factor and the number of citations of each study. Twenty-six of the studies in our sample are published in the top five general interest journals in economics (they provide 382 estimates). The 133 studies combined have received more than 22,000 citations in Google Scholar, which testifies to the popularity of excess sensitivity exercises.

Apart from the estimates and their standard errors, we also collect 47 other variables that capture the context in which researchers obtain their estimates. Such a number of explanatory variables is unusual for a meta-analysis (Nelson & Kennedy, 2009, review 140 previous meta-analyses and report that the largest number of collected explanatory variables is 41), but that is due to the complexity of the literature on excess sensitivity. The description of all the variables is available in Appendix A, and we discuss them in detail in Section 5. It follows that we have to collect almost 140,000 data points (the product of the number of estimates and the number

Figure 2: Micro estimates of excess sensitivity vary widely



Notes: The figure shows a box plot of the estimates of excess sensitivity reported in micro studies. Following Tukey (1977), the box shows the interquartile range (P25–P75) with the median highlighted. Whiskers cover the interval from (P25 $-$ 1.5 $\cdot$ interquartile range) to (P75 + 1.5 $\cdot$ interquartile range) if such estimates exist. The dots show the remaining (outlying) estimates reported in each study. Studies are sorted by mid-year of the sample in ascending order.

of variables), which is a laborious but complex exercise that cannot be delegated to research assistants. To minimize the danger of mistakes in data coding, we collect the data ourselves and both independently double-check random portions of the resulting data set. The process of data collection including re-checking and correcting of some entries took six months. The final data set is available in the online appendix.

Out of the 2,788 estimates of excess sensitivity that we collect, 885 are computed using micro data and 1,903 are computed using macro data. The overall mean of all the estimates is 0.4, but the statistic differs greatly between micro and macro estimates: the mean of the macro estimates is 0.48, remarkably close to the original estimate of the share of rule-of-thumb consumers by Campbell & Mankiw (1989), but the mean of the micro estimates is half that value, 0.24. Figure 1 shows that while micro estimates account for less than a third of the data set, they dominate the distribution of the estimates below 0.2. In contrast, few micro estimates are larger than 0.5. The economics profession favors micro studies, which follows from the observation that they comprise more than three quarters of the empirical papers on excess sensitivity published in the top five journals. The mean coefficient reported in the top journals, therefore, is very close to the mean of the micro studies, which would lead us to the conclusion that the best available estimate of the proportion of rule-of-thumb consumers is around one quarter. Nevertheless, Figure 1 also shows that an unexpectedly large portion of the micro estimates lie just above zero, which could be due to censoring of negative results.

The micro estimates of excess sensitivity are far from homogeneous and differ both across and within studies, as the box plot in Figure 2 documents. The studies in the figure are sorted in ascending order by the age of the data they employ; nevertheless, we do not detect any obvious trend in the results. Almost all studies report some estimates close to 0.2, and most studies report some estimates that are either negative or positive but very close to zero, especially the half of the studies that use newer data. Figure 2 testifies to the importance of controlling for the exact methodology employed in the studies. A part of the between-study variation, however, may also be due to publication bias, as the authors may treat negative and insignificant results differently.

4 Publication Bias

Negative estimates of excess sensitivity are inconsistent with the theory: an anticipated increase in income growth either should have no effect on consumption growth (according to the permanent income hypothesis) or should stimulate consumption (according to the Keynesian consumption function). Although theoretically implausible, negative estimates will appear from time to time given sufficient noise in the data and imprecision in the estimation methodology. For the same reason, researchers will sometimes obtain estimates that are large but also far away from the true value, so the mean estimate will be unbiased if researchers report all estimates. The zero lower bound, however, is a psychological barrier, breaching of which tells the authors that something may be wrong with their model. Even the first survey of the micro literature on excess sensitivity (Browning & Lusardi, 1996, pp. 1833–1834) mentions the problem: "Almost all studies find that the expected income growth (or lagged income) variable has the predicted sign (...). Note, however, that this could be due to publication censoring: investigators who find the 'wrong' sign may continue with specification searches until they have the 'right' sign." In this section we test the above conjecture.[2]

We exploit a property of the techniques used to estimate excess sensitivity: the ratio of the estimated coefficient to its standard error has a t-distribution. It follows that the numerator and denominator of this ratio should be statistically independent quantities. Put differently, the coefficient γ in the following regression should be zero (to our knowledge, this relation was first explicitly mentioned by Card & Krueger, 1995, in the context of the literature on the effects of the minimum wage on employment):

$$\hat{\lambda}_{ij} = \lambda_0 + \gamma \cdot SE(\hat{\lambda}_{ij}) + u_{ij}, \tag{7}$$

where $\hat{\lambda}_{ij}$ and $SE(\hat{\lambda}_{ij})$ are the i-th estimates of excess sensitivity and the corresponding standard error reported in the j-th studies; u_{ij} is a disturbance term. If researchers discard negative estimates, however, a positive relationship arises between estimates and their standard errors. The positive relationship is due to the heteroskedasticity of (7): estimates with small standard errors are close to the underlying excess sensitivity, but as precision decreases, the dispersion of estimates increases; some get large, some get negative. When negative estimates are underreported, a positive γ follows. In addition, if the authors prefer statistically significant results, they will continue with specification searches until they find $\hat{\lambda}$ large enough to offset the standard error and produce a sufficiently large t-statistic. The estimate of γ thus measures the strength of publication bias, which might have two sources—selection for positive sign or selection for statistical significance. The estimate of λ_0 captures the mean excess sensitivity coefficient corrected for publication bias.

Table 1 presents the results of the tests for publication bias. We estimate the model separately for micro and macro estimates, because the previous section (and especially Figure 1) shows that censoring is probably a more serious issue for micro studies than for macro studies. In all estimations we cluster standard errors at the study level, because estimates reported in the same study are unlikely to be independent. Moreover, some studies use the same or very similar data sets, which also results in dependence among the estimates. To mitigate this problem, we additionally cluster standard errors at the level of similar data sets. We define data sets as similar if they comprise the same country or countries and start with the same year (many studies just add a couple of years to a data set used elsewhere). Our implementation of two-way clustering follows Cameron *et al.* (2011).

The first column of Table 1 shows the results of an OLS regression. For micro studies we obtain a positive and statistically significant estimate of publication bias and also a significant estimate of the underlying excess sensitivity corrected for the bias. The corrected coefficient,

[2] To keep consistency with previous studies on the topic (DeLong & Lang, 1992; Card & Krueger, 1995; Görg & Strobl, 2001; Stanley, 2001; Ashenfelter & Greenstone, 2004), we use the common term "publication bias." A more precise label is "selective reporting," because the problem concerns both published and unpublished studies and is not necessarily connected to the publication process.

Table 1: Publication bias only affects micro studies

Panel A: micro estimates	OLS	FE	BE	Precision	Study	IV
SE (publication bias)	0.448^{***}	0.328^{**}	0.602^{***}	0.841^{***}	0.479^{**}	0.970^{*}
	(0.0786)	(0.157)	(0.163)	(0.225)	(0.200)	(0.550)
Constant (effect beyond bias)	0.128^{***}	0.157^{***}	0.116^{***}	0.0318^{**}	0.137^{***}	0.0000550
	(0.0381)	(0.0384)	(0.0418)	(0.0146)	(0.0306)	(0.129)
Studies	41	41	41	41	41	41
Observations	885	885	885	885	885	885
Panel B: macro estimates	OLS	FE	BE	Precision	Study	IV
SE (publication bias)	0.0204	-0.0350	0.147	-0.106	0.135	-0.133
	(0.102)	(0.172)	(0.108)	(0.419)	(0.137)	(0.612)
Constant (effect beyond bias)	0.475^{***}	0.490^{***}	0.394^{***}	0.510^{***}	0.401^{***}	0.517^{***}
	(0.0460)	(0.0480)	(0.0451)	(0.117)	(0.0297)	(0.162)
Studies	94	94	94	94	94	94
Observations	1903	1903	1903	1903	1903	1903

Notes: The table presents the results of regression $\hat{\lambda}_{ij} = \lambda_0 + \gamma \cdot SE(\hat{\lambda}_{ij}) + u_{ij}$. $\hat{\lambda}_{ij}$ and $SE(\hat{\lambda}_{ij})$ are the i-th estimates of excess sensitivity and their standard errors reported in the j-th studies. The standard errors of the regression parameters are clustered at both the study and data set level and shown in parentheses (the implementation of two-way clustering follows Cameron *et al.*, 2011). OLS = ordinary least squares. FE = study-level fixed effects. BE = study-level between effects. Precision = the inverse of the reported estimate's standard error is used as the weight. Study = the inverse of the number of estimates reported per study is used as the weight. Instrument = we use the number of observations reported by researchers as an instrument for the standard error. The number of micro and macro studies does not add up to 133 because some studies report both micro and macro estimates. ***, **, and * denote statistical significance at the 1%, 5%, and 10% level.

however, is about a half of the simple mean of the reported micro estimates: 0.128. Such a difference indicates strong publication bias and is consistent with the rule of thumb suggested by Ioannidis *et al.* (2017), which says that in economics, on average, publication selection exaggerates the mean reported coefficients twofold. In contrast, we find no publication bias for macro studies, and here the underlying excess sensitivity is therefore very close to the mean of the reported effects. In the second column of the table we add study-level fixed effects in order to control for unobserved study-specific characteristics (such as quality). The estimates are similar to OLS. Note that the inclusion of study dummies also effectively controls for potential differences in excess sensitivity across countries, because most studies present estimates for just one country (adding a set of country dummies does not change the results up to the second decimal point). The third column of the table shows that using between-study instead of within-study variance for identification does not affect our conclusions.

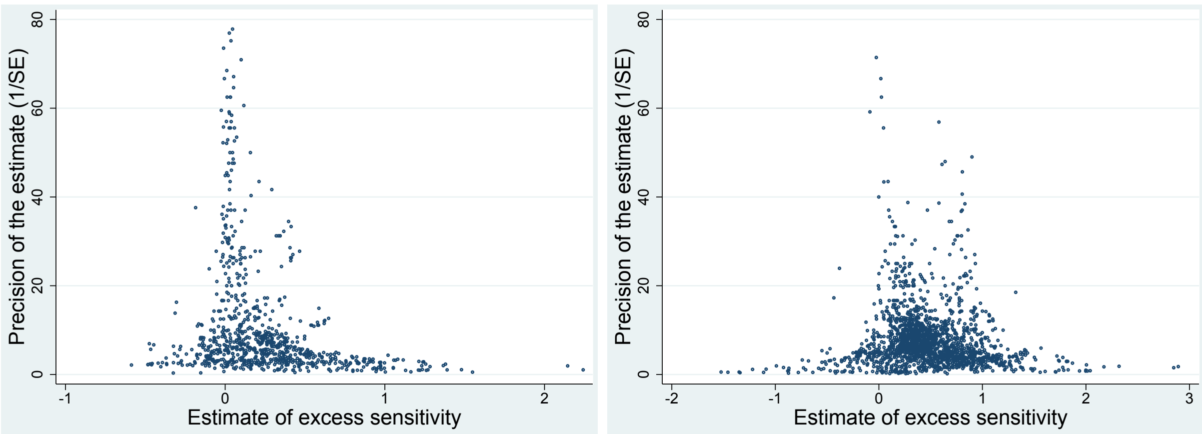
Several weighting schemes can be used to estimate the meta-analysis model. Because the response variable in (7) is itself an estimate, it has been suggested to use the inverse of its variance as the weight (Stanley & Doucouliagos, 2015), which effectively means multiplying (7) by precision and therefore adjusting for the apparent heteroskedasticity. This approach has the additional intuitive allure of giving more weight to more precise results. The problem with precision weights in economics, unlike medical research, is that the estimation of standard errors is an important feature of the model, and if the study underestimates the standard error, weighting by precision can create a bias by itself. Moreover, Lewis & Linzer (2005) show that when the response variable is estimated, the weighted-least-squares approach often leads to inefficient estimates and underestimated standard errors, and that OLS with robust standard errors typically performs better. The fourth column of Table 1 shows that the application of precision weights results in a much stronger estimated publication bias and a negligible estimate of the underlying excess sensitivity for micro studies, implying an exaggeration by a factor of 8 due to the bias. In the fifth column we use the inverse of the number of estimates reported per study as the weight, which effectively gives each study the same impact on the results. These alternative weights yield results that are close to those of OLS.

An important caveat is the potential joint determination of estimates and their standard errors. If some techniques affect both estimates and their standard errors in the same direction, the finding of a positive γ in (7) can be spurious. To account for such endogeneity we need an instrument correlated with the standard error but not with estimation techniques. We use the number of observations employed by researchers to compute each excess sensitivity coefficient, because data size is related to the standard error by definition, but is unlikely to be much related to the technique used in the paper. The results are shown in the last column of Table 1. As can be expected, the use of the instrumental-variable approach results in a substantial drop in the precision of our estimates. For macro studies the results are very close to the baseline case, but for micro studies we obtain evidence of an even stronger publication bias and statistically insignificant excess sensitivity beyond the bias.

Regression (7) can be thought of as a reduced-form specification for measuring the magnitude of publication bias; it tells us little about the sources of publication selection. In Figure 3 we investigate the incidence of the first potential source: selection of estimates for the "right" sign. The figure is a scatter plot showing the estimates of excess sensitivity on the horizontal axis and their precision on the vertical axis. The most precise estimates should be close to the true underlying value, and the dispersion should increase with decreasing precision, yielding an

Figure 3: Negative micro estimates are underreported

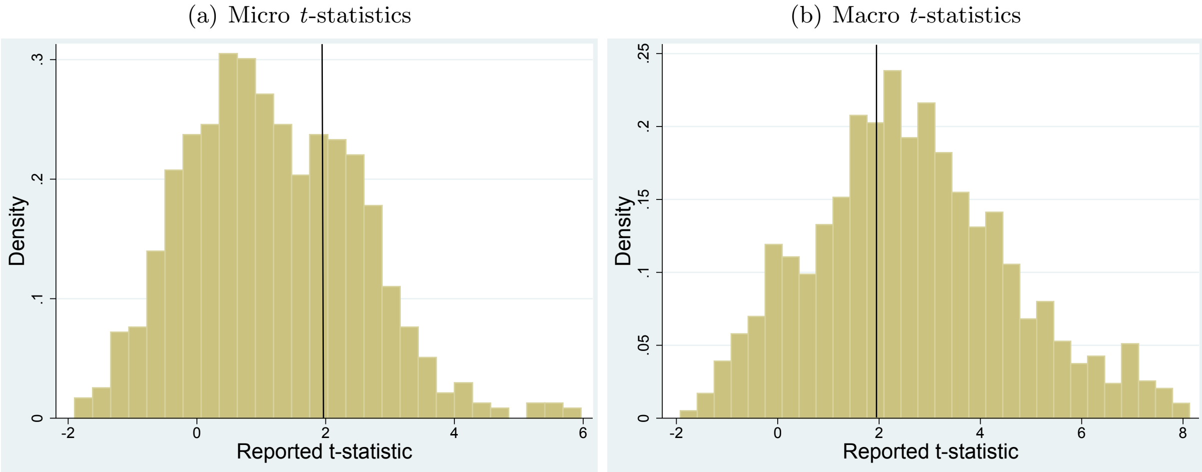
(a) Micro estimates (b) Macro estimates



Notes: In the absence of publication bias the funnel should be symmetrical around the most precise estimates. We exclude estimates with extreme magnitude or precision from the figure but include all in the regressions.

inverse-funnel shape (Egger *et al.*, 1997). In the absence of publication selection all imprecise estimates, both positive and negative, have the same chance of being reported. While in our case the funnel is relatively symmetrical for macro estimates (the two distinct peaks of the funnel suggest heterogeneity, which we focus on in the next section), for micro estimates it is not: a large fraction of negative estimates are missing from the funnel. We conclude that selection for positive sign contributes to the observed publication bias among micro studies.

Figure 4: Marginally insignificant micro estimates are underreported



Notes: In the absence of publication bias the distribution of the t-statistics should be approximately normal. The vertical line denotes the critical value associated with 5% statistical significance. We exclude estimates with large t-statistics from the figure but include all in the regressions.

Figure 4 provides evidence on the incidence of the second source of publication bias, selection for statistical significance. Brodeur *et al.* (2016) show that a stylized fact of empirical economics is the underreporting of estimates that are just insignificant: researchers prefer to report significant estimates. A similar pattern is observed by Havranek (2015) in the literature on the elasticity of intertemporal substitution in consumption, which is often estimated in the same regression with excess sensitivity. Both studies point to a two-humped distribution of the reported t-statistics. In the case of excess sensitivity we do not observe such a shape for macro studies, but the distribution of micro t-statistics is consistent with a mild preference against estimates that are just insignificant at the 5% level. To examine this source of publication bias among micro studies more formally, we estimate the model put forward by Hedges (1992), who links the probability of an estimate being reported to the level of statistical significance (1%, 5%, 10%, or none). The results, presented in Appendix B, suggest that the probability of publication indeed depends on statistical significance.

Why do micro studies display publication bias, whereas macro studies do not? We argue that because the underlying excess sensitivity for macro data is about 0.5, it is easy for macro studies to obtain positive and statistically significant estimates without getting involved in much specification searching. In contrast, the underlying value for micro data is small, about 0.13, which means that due to sampling error micro estimates often turn out to be insignificant or even negative. Since negative estimates are difficult to interpret, they raise doubts about the specification of the model (and about the feasibility of publication of such results). The selection process may be almost entirely unintentional. Few researchers want to explicitly inflate their estimates; after all, the true excess sensitivity is not negative, so it makes little sense to build a paper on negative results. Yet, in consequence, micro studies are likely to conduct more specification searches than macro studies, which on average strengthens publication bias.

5 Heterogeneity

The difference between micro and macro studies in excess sensitivity and publication bias can also be shown by using all 2,788 estimates and regressing the value of the estimate on *i*) a dummy variable that equals one for micro studies and *ii*) an interaction of the dummy with the estimate's standard error.[3] We report the result in the first column of Table 2. The constant in the regression is 0.48, which corresponds to the mean reported macro estimate of excess sensitivity. The coefficient on the interaction captures the strength of publication bias in micro studies. The coefficient on the dummy variable *Micro* measures the difference between micro and macro estimates when we account for publication bias: in comparison with the discussion in Section 3 the difference increases approximately by the amount of exaggeration among micro estimates due to the bias and reaches 0.35. The implied excess sensitivity, conditional on the use of micro data and corrected for publication bias, is therefore $0.48 - 0.35 = 0.13$ (reported as the implied share of rule-of-thumb consumers at the bottom of the table). While the coefficient is statistically significant at the 1% level, it is too small to be of practical significance for structural models. For example, Galí *et al.* (2007) show that, even assuming imperfectly competitive labor markets, with the share of rule-of-thumb consumers below 0.25 the consumption multiplier in the standard new Keynesian model is still negative.

The second column of Table 2 documents that the remaining excess sensitivity can be explained by liquidity constraints. We have noted that there are many ways to control for liquidity constraints in consumption Euler equations, and our approach to capturing these different ways is described in detail in Table A2 in Appendix A. In short, the variable *Liquidity unconstr.* equals one when the authors estimate excess sensitivity for a subset of households that are unlikely to be liquidity constrained (such as stockholders or rich households) or when the authors add a control variable that captures the severity of liquidity constraints (for example, the ratio of housing equity to annual income). Ours is a crude definition of liquidity constraints, yet suffices to explain away the excess sensitivity altogether: the mean estimate conditional on the use of micro data, correction for publication bias, and limited or no liquidity constraints is 0.01. Households that do not face liquidity constraints display no excess sensitivity; no support in the data remains for pure rule-of-thumb, or non-Ricardian, consumption behavior.

In the third column of the table we show the consequences of ignoring publication bias. The estimate of the difference between micro and macro studies decreases from 0.35 to 0.23,

[3] Since most studies published in the top five journals use micro data, replacing the *Micro* dummy with a *Top journal* dummy would yield similar results.

Table 2: Excess sensitivity explained by macro data, publication bias, and liquidity constraints

	Bias only	Baseline	Bias ignored	Precision	Study
Micro	-0.352***	-0.337***	-0.227***	-0.461***	-0.285***
	(0.0555)	(0.0527)	(0.0606)	(0.0806)	(0.0516)
Micro x SE (bias)	0.448***	0.454***		0.853***	0.481**
	(0.0786)	(0.0795)		(0.223)	(0.198)
Liquidity unconstr.		-0.138**	-0.131**	-0.0677	-0.0803*
		(0.0548)	(0.0555)	(0.0531)	(0.0484)
Constant	0.480***	0.489***	0.489***	0.503***	0.436***
	(0.0408)	(0.0414)	(0.0414)	(0.0814)	(0.0436)
Implied RoT share	0.13***	0.01	0.13*	-0.03	0.07
Studies	133	133	133	133	133
Observations	2,788	2,788	2,788	2,788	2,788

Notes: The response variable is the estimated excess sensitivity. The standard errors of the regression parameters are clustered at both the study and data set level and shown in parentheses (the implementation of two-way clustering follows Cameron *et al.*, 2011). RoT = rule of thumb. The implied share of rule-of-thumb consumers is computed as the sum of constant, micro, and liquidity unconstr., and it therefore corresponds to the mean reported excess sensitivity conditional on the use of micro data, correction for publication bias, and computation for liquidity-unconstrained households. Precision = the inverse of the reported estimate's standard error is used as the weight. Study = the inverse of the number of estimates reported per study is used as the weight. ***, **, and * denote statistical significance at the 1%, 5%, and 10% level.

because now we compare the unbiased mean estimate from macro studies with the mean estimate from micro studies, which is exaggerated by 0.12 due to publication bias. The coefficient on the variable *Liquidity unconstr.* remains close to -0.13 and is still statistically significant at the 5% level. The implied share of rule-of-thumb consumers conditional on limited liquidity constraints is 0.13, and the difference of this estimate from the previous one ($0.13 - 0.01 = 0.12$) fully reflects the upward bias that arises because of publication selection. Using the information from Section 3 and the first three specifications of Table 2 we can decompose the mean overall coefficient reported for excess sensitivity, 0.4. We find that three factors contribute approximately equally to the positive and apparently large reported excess sensitivity: First, the use of macro data in some studies increases the overall mean from 0.24 to 0.4 and is thus responsible for a difference of 0.16 in the excess sensitivity coefficient. Second, publication bias exaggerates the mean micro estimate twofold, from about 0.12 to 0.24. Third, the residual excess sensitivity coefficient of approximately 0.12 is due to liquidity constraints.

The remaining two columns of Table 2 show the results of applying alternative weighting schemes. In the fourth column we use precision weights; similarly to the previous section, we find more evidence for publication bias and get an insignificant estimate of the underlying excess sensitivity. Also the coefficient on *Liquidity unconstr.* becomes statistically insignificant, because in this specification there is no excess sensitivity beyond publication bias left for explanation by liquidity constraints. When we use weights that correspond to the inverse of the number of observations reported by each study, we obtain results closer to the baseline specification. In this case the effect of liquidity constraints is smaller in absolute value, but the residual excess sensitivity, which we interpret as reflecting the share of pure rule-of-thumb consumers, is again not statistically different from zero.

5.1 Control Variables

In the remainder of this section we test the robustness of our findings from Table 2 concerning the magnitude of publication bias, the difference between micro and macro studies, the impact of liquidity constraints, and the share of pure rule-of-thumb consumers. To this end we control for 45 additional variables that may influence the reported estimates of excess sensitivity (we originally collected more variables but were forced to exclude some of them due to collinearity concerns or insufficient variation). The definitions and summary statistics of these variables are available in Table A1 in Appendix A, and 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. In this subsection 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 to estimate excess sensitivity. 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 aggregated data necessarily omit demographic variables that affect tastes, and Attanasio & Weber (1993) show how such an omission can generate spurious excess sensitivity. About a third of the estimates come from micro studies.

Next, we retain the variable included in Table 2 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 only used by 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 Table A2 in Appendix A: the dummy equals one, for example, when the author only uses 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 only increases in income; 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, 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. Seven 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 6% of the studies follow this advice. 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 almost a half of the studies in our data set.

Consumption measure Researchers often only use consumption of 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 44% 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 7% 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 16% of the studies use data that allow predictable changes in income to be observed directly: for example, data on reported subjective income expectations (Jappelli & Pistaferri, 2000), labor contracts (Shea, 1995b), or economic stimulus payments of 2008 (Parker *et al.*, 2013). Next, 7% 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 15% 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 52% 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. Ninety per cent 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 still collect such estimates of excess sensitivity (19% of our data set), but 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- and the long-run 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- and long-run estimates, but add a control for this method.

Another aspect of methodology is the assumption of a time shift: effectively an interaction of the excess sensitivity coefficient and 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 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; 25% of the estimates) or TSLS (45% of the estimates); the latter assumes homoskedastic errors. Techniques based on maximum likelihood are used by 10% of the estimates, while OLS is employed in 17% 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 Section 4. 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.

5.2 Estimation and Results

To address the challenge put forward by Browning & Lusardi (1996) and investigate why different researchers produce such different estimates of excess sensitivity, we intend to regress the reported estimates on the variables introduced in the previous subsection. Such a regression, however, would have 48 explanatory variables. If we estimate the model using OLS, the standard errors of many regression coefficients will be exaggerated because some variables will prove redundant for the explanation of excess sensitivity. Thus we face substantial model uncertainty, since there is no theory to help us slash the number of explanatory variables. A common solution is stepwise regression, but in employing that we might accidentally eliminate some of the important variables. Instead we opt for Bayesian model averaging (BMA), which was designed specifically to tackle model uncertainty (Raftery *et al.*, 1997). BMA has recently been used in economics and finance, for example, to estimate the key determinants of economic growth (Moral-Benito, 2012), to forecast real-time measures of economic activity (Faust *et al.*, 2013), and to investigate the predictability of stock returns (Turner, 2015).

BMA runs many regression models in which different subsets of the explanatory variables are used. Each model gets assigned a statistic called the posterior model probability, which is analogous to adjusted R^2 in frequentist econometrics: it measures how well the model fits the data conditional on model size. The result is a weighted average of all the regressions, the weights being the posterior model probabilities. Instead of statistical significance, for each variable we obtain the posterior inclusion probability (PIP), which is the sum of the posterior model probabilities for the models in which the variable is included. With 48 variables, however, we cannot estimate all the 2^{48} possible models, because it would take many months using a standard personal computer. We use the Model Composition Markov Chain Monte Carlo algorithm (Madigan & York, 1995), which walks through the models with the highest posterior model probabilities. To ensure convergence we employ 100 million iterations and 50 million burn-ins. The R package that we use was developed by Zeugner & Feldkircher (2015).

Figure 5 presents the results concerning the importance of each variable; every column corresponds to an individual regression model. The variables are depicted on the vertical axis and sorted by posterior inclusion probability in descending order. Blue color (darker in greyscale) means that the variable is included and the estimated sign is positive. Red color (lighter in greyscale) means that the estimated sign is negative. The horizontal axis measures cumulative posterior model probability, so that the best models are shown on the left. The very best model, according to BMA, includes 17 explanatory variables, but only accounts for 2% of the cumulative posterior model probability—for this reason we focus on the more robust overall weighted average, not just the best specification. The figure makes it clear that about a third of all the variables are useful in explaining the differences among the estimates of excess sensitivity.

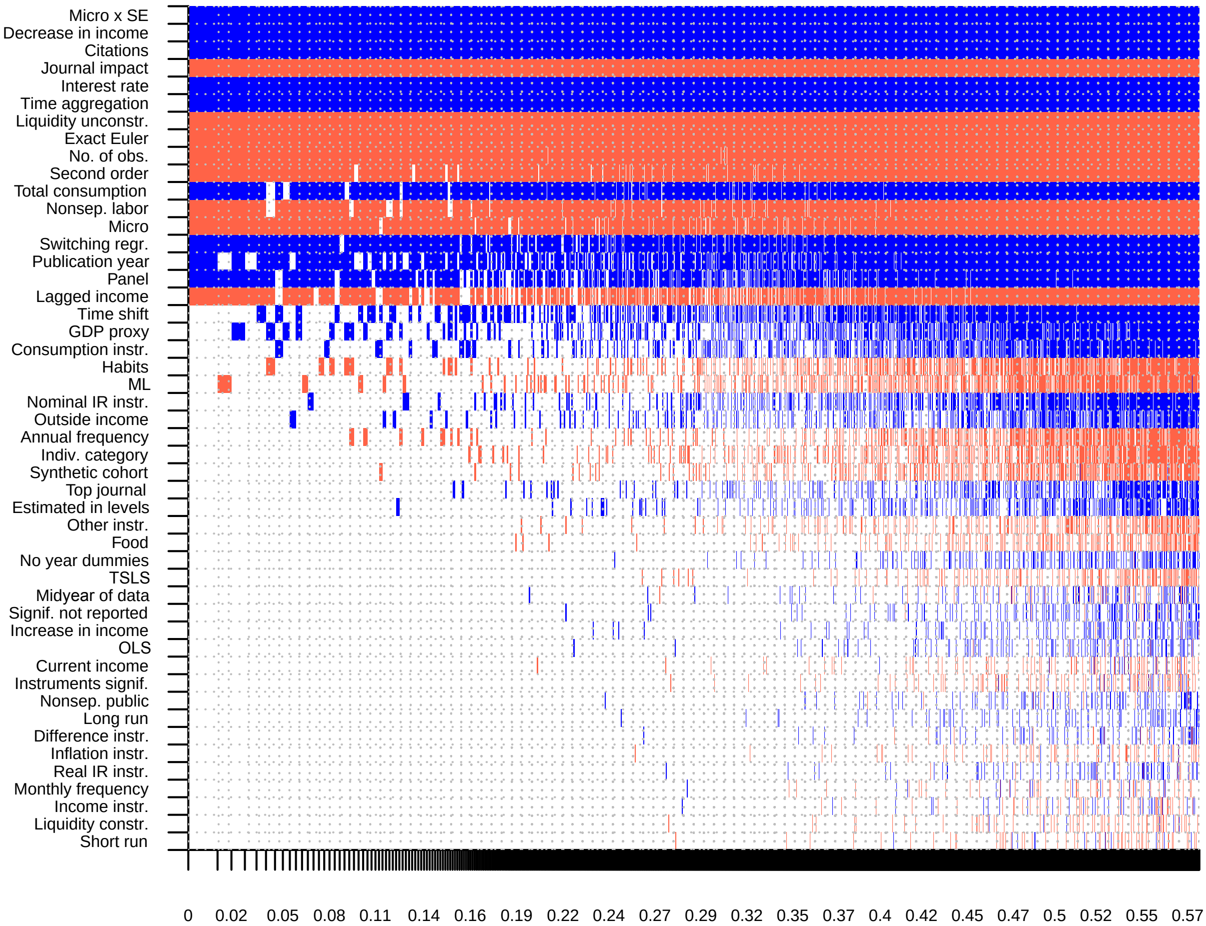
The numerical results of BMA are reported in the left-hand panel of Table 3. (More details on the BMA estimation are available in Appendix D.) In the right-hand panel of the table we estimate OLS as a robustness check, but only include variables that have a posterior inclusion probability of at least 0.5 in BMA and thus have a non-negligible impact on the response vari-

Table 3: Why do estimates of excess sensitivity differ?

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.	-0.035	0.011	0.986	-0.038	0.015	0.010
Midyear of data	0.000	0.000	0.016			
Micro	-0.166	0.076	0.907	-0.191	0.078	0.014
Micro x SE (bias)	0.428	0.058	1.000	0.442	0.076	0.000
Panel	0.074	0.055	0.705	0.103	0.053	0.054
Synthetic cohort	-0.011	0.045	0.070			
Annual frequency	-0.005	0.016	0.100			
Monthly frequency	0.000	0.004	0.007			
Liquidity constraints						
Liquidity unconstr.	-0.118	0.028	0.999	-0.125	0.043	0.004
Decrease in income	0.214	0.037	1.000	0.212	0.145	0.144
Liquidity constr.	0.000	0.003	0.006			
Increase in income	0.000	0.005	0.013			
Utility function						
Habits	-0.015	0.037	0.175			
Nonsep. public	0.000	0.005	0.010			
Nonsep. labor	-0.138	0.057	0.916	-0.159	0.029	0.000
Interest rate	0.109	0.024	1.000	0.118	0.030	0.000
Consumption measure						
Total consumption	0.099	0.039	0.931	0.092	0.032	0.004
Food	-0.002	0.012	0.026			
Indiv. category	-0.008	0.027	0.093			
Income measure						
Outside income	0.008	0.026	0.106			
Current income	0.000	0.005	0.011			
Lagged income	-0.142	0.110	0.690	-0.209	0.043	0.000
GDP proxy	0.023	0.042	0.264			
Instruments signif.	0.000	0.003	0.010			
Signif. not reported	0.000	0.005	0.014			
Consumption instr.	0.011	0.024	0.193			
Income instr.	0.000	0.002	0.007			
Difference instr.	0.000	0.002	0.007			
Nominal IR instr.	0.009	0.024	0.148			
Inflation instr.	0.000	0.003	0.007			
Real IR instr.	0.000	0.002	0.007			
Other instr.	-0.001	0.007	0.034			
Specification						
Exact Euler	-0.205	0.051	0.998	-0.214	0.066	0.001
Estimated in levels	0.003	0.013	0.054			
Second order	-0.137	0.053	0.943	-0.128	0.046	0.006
Short run	0.000	0.003	0.006			
Long run	0.000	0.005	0.008			
Time shift	0.055	0.074	0.407			
No year dummies	0.001	0.012	0.020			
Time aggregation	0.129	0.028	1.000	0.137	0.042	0.001
Technique						
ML	-0.016	0.039	0.164			
TSLS	0.000	0.005	0.019			
OLS	0.000	0.005	0.012			
Switching regr.	0.154	0.076	0.882	0.164	0.037	0.000
Publication						
Publication year	0.004	0.003	0.730	0.004	0.002	0.060
Citations	0.076	0.013	1.000	0.079	0.018	0.000
Top journal	0.004	0.018	0.062			
Journal impact	-0.117	0.022	1.000	-0.117	0.026	0.000
Constant	0.305	NA	1.000	0.306	0.085	0.000
Observations	2,788			2,788		

Notes: ES = excess sensitivity. PIP = posterior inclusion probability. SD = standard deviation. The table shows unconditional moments for BMA. In the frequentist check we only include explanatory variables with PIP > 0.5. The standard errors in the frequentist check are clustered at both the study and data set level. A detailed description of all variables is available in Table A1.

Figure 5: Model inclusion in Bayesian model averaging

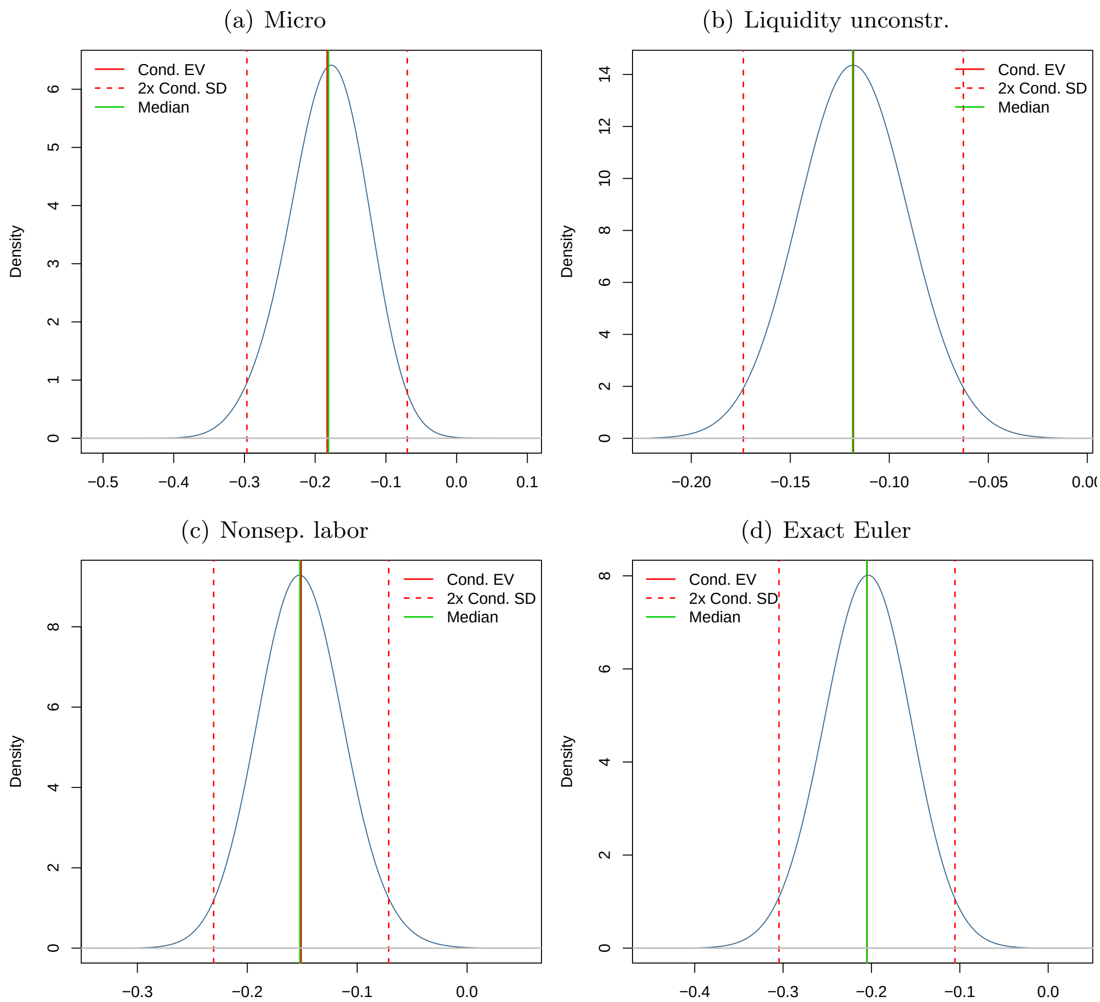


Notes: The response variable is the estimate of excess sensitivity. Columns denote individual models; variables are sorted by posterior inclusion probability in descending order. The horizontal axis denotes cumulative posterior model probabilities; only the 5,000 best models are shown. Blue color (darker in greyscale) = the variable is included and the estimated sign is positive. Red color (lighter in greyscale) = the variable is included and the estimated sign is negative. No color = the variable is not included in the model. Numerical results of the BMA exercise are reported in Table 3. A detailed description of all variables is available in Table A1.

able according to the classification by Kass & Raftery (1995). The right-hand part of the table, therefore, is a combination of BMA (reducing model uncertainty) and OLS (frequentist estimation). In Table 3 we show the conventional unconditional moments for BMA, which means that the reported posterior mean and posterior standard deviation for each variable are computed using even the models in which the variable is not included. For important variables the choice between conditional and unconditional moments does not matter, because with a large enough PIP the variable is included in virtually all regressions with high posterior model probabilities. In Figure 6 we depict conditional moments and only show the distribution of the actually estimated regression parameters. The figure also depicts "confidence" intervals (denoted by dashed lines) for each parameter constructed using the posterior standard deviations. The use of conditional moments does not alter our inference regarding the key variables.

Data characteristics Our results suggest that, other things being equal, studies with larger data sets tend to report smaller estimates of excess sensitivity. We interpret this finding as evidence for modest but systematic small-sample bias that exaggerates the estimates. The difference between micro and macro estimates remains large even when all the additional aspects of study design are controlled for, which is also apparent from the top-left panel of Figure 6. Hence the importance of controlling for demographic variables that affect households' tastes. The publication bias coefficient retains its sign, significance, and magnitude, and we conclude that the evidence for publication bias presented earlier was not due to omitted aspects of data and methodology. We also find that panel data tend to be associated with larger reported estimates of excess sensitivity, but the corresponding variable is not statistically significant at the 5% level in the frequentist check. The remaining data characteristics, the age of the data and data frequency, do not influence the reported excess sensitivity in a systematic way.

Figure 6: Posterior coefficient distributions for selected variables



Notes: The figure depicts the densities of the regression parameters encountered in different regressions in which the corresponding variable is included (that is, the depicted mean and standard deviation are conditional moments, in contrast to those shown in Table 3). For example, the regression coefficient for *Liquidity unconstr.* is negative in almost all models, irrespective of model specification. The most common value of the coefficient is approximately -0.12.

Liquidity constraints Both BMA and OLS confirm the importance of liquidity constraints for the estimation of excess sensitivity. When excess sensitivity is estimated using a sample of households for which the constraints are not binding, the reported coefficient is on average 0.12 smaller, which is close to the value reported in Table 2. The variable *Decrease in income* has a large PIP, but it is insignificant in the frequentist check. The estimates obtained using income decreases are usually imprecise, because data on expected decreases in income are scarce.

Utility function In contrast to Sommer (2007), we find that controlling for habit formation does not typically help explain excess sensitivity. In a similar vein, we find little evidence for the importance of non-separability between the consumption of private and public goods. The non-separability between consumption and leisure, by contrast, matters. When separability is assumed, excess sensitivity is overestimated on average by 0.14, which is consistent with Attanasio & Weber (1995) and Jappelli & Pistaferri (2000); the estimate is robust, as can be seen from Figure 6. Our results also suggest that when estimating excess sensitivity it is important to control for intertemporal substitution effects by including the interest rate.

Consumption measure The definition of the consumption variable affects the results in a systematic way: when the consumption of durable goods is included, researchers tend to report excess sensitivity larger by 0.1. The potential non-separabilities between various categories of non-durable consumption, on the other hand, seem to have no important impact.

Income measure It is surprising to find that the definition of income growth has little systematic effect on the reported excess sensitivity. The use of lagged income growth as a rough proxy for expected income growth is associated with a downward bias, but such a method choice is rarely made. The various approaches to defining the instrument set for the estimation of income growth appear to have no systematic effects on the results. (Changing the instrument set can change the results dramatically, as every applied researcher knows. Our point is that we find no *systematic* bias associated with a particular strategy of choosing instruments.)

Specification The order of approximation of the consumption Euler equation matters for the results: the second-order approximation typically yields estimates of excess sensitivity smaller by 0.14 when compared to the first-order approximation. The distance from the log-linear approximation increases to 0.21 when researchers estimate the exact Euler equation. These results are in line with Carroll (2001), who shows that first- and second-order approximations may create an upward bias in the estimates of excess sensitivity. Next, our results show that studies that account for time aggregation tend to report larger estimates of excess sensitivity and thus that ignoring time aggregation creates a downward bias.

Technique We find that the choice of econometric technique has a limited impact on the estimated excess sensitivity. Estimates from switching regressions tend to be larger than those obtained using other methods, but switching regressions have only been applied in this context by a couple of studies.

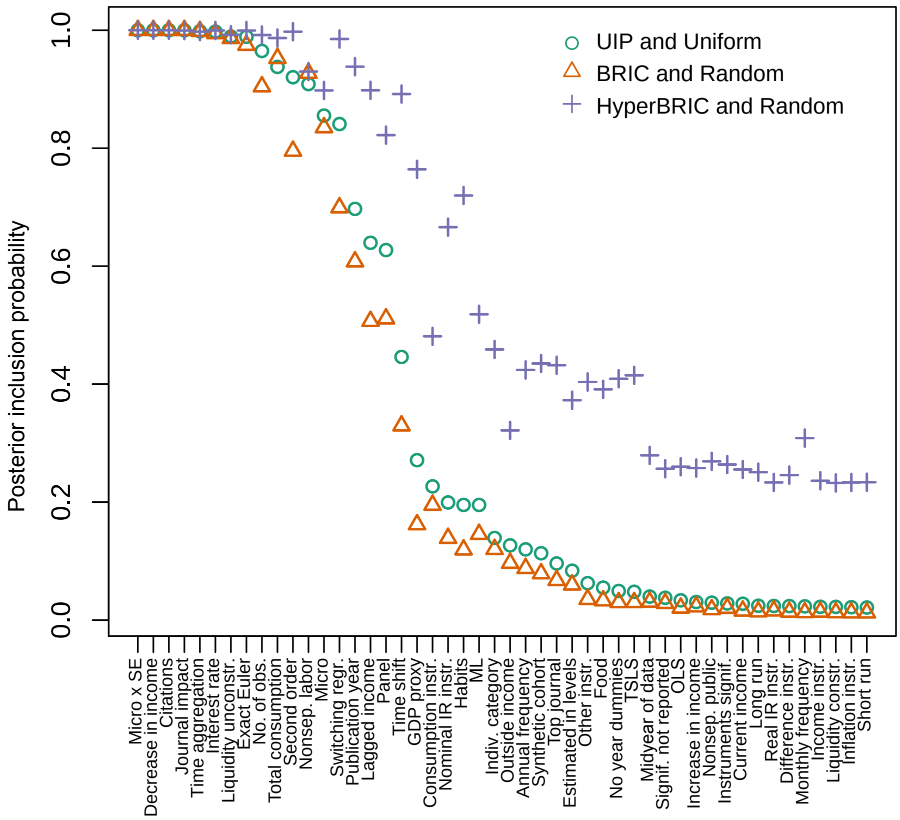
Publication Our results suggest that the reported estimates of excess sensitivity increase in the year of publication of the study, which might reflect additional unobservable effects of improving data and methods on the results. The estimate, however, is small: a mere 0.004 increase per year; the coefficient is also statistical insignificant at the 5% level in the frequentist check. The number of citations of the study and the impact factor of the journal where the study is published have opposite effects on the results. Frequently cited studies tend to report large estimates, but better journals tend to publish smaller estimates. While both variables may also capture quality aspects that are otherwise unobservable, the results concerning citations are influenced by several highly cited studies, such as Campbell & Mankiw (1989), which typically find large estimates of excess sensitivity and therefore of the share of rule-of-thumb consumers.

An important aspect of Bayesian model averaging is the selection of priors for the regression parameters (Zellner's g-prior) and models. Because of the lack of ex ante information on the magnitude of the regression parameters we always employ the agnostic prior of zero for the regression coefficients. There are different approaches to determining the weight of this prior relative to the information value of the data, and in the baseline estimation we use the unit information prior, which assigns the prior the same weight as one data observation. We also use the uniform prior for models, which gives each model equal prior probability. This set of g- and model priors is recommended by Eicher *et al.* (2011), who find that it performs well in predictive exercises. As a robustness check, we use an alternative to the unit information prior, the BRIC prior suggested by Fernandez *et al.* (2001), which takes into account the number of explanatory variables for the determination of the weight of the zero prior for the regression parameters. In the new set of priors we also employ the random beta-binomial model prior (Ley & Steel, 2009), which implies that each *model size* has the same prior probability. (When, by contrast, each *model* has the same prior probability, the prior probability of the most common model sizes is large.) In the third and last set we keep the random beta-binomial model prior, but employ the data-dependent hyper-g prior suggested by Feldkircher (2012), which should be less sensitive to potential outliers.

Figure 7 depicts how the posterior inclusion probabilities change when we depart from the baseline set of priors. We can see that changing the g-prior from the unit information prior to BRIC and the model prior from the uniform to the random beta-binomial prior has little impact on the results, though it slightly reduces the PIP for most variables. The data-dependent hyper-g prior, on the other hand, yields substantially higher PIPs for almost all variables, but broadly preserves the ranking of the variables according to their PIP. All three approaches agree that the 13 most important variables have PIPs larger than 0.9. We conclude that the choice of priors does not affect our main findings.

The BMA exercise shows not only that the reported excess sensitivity depends on the use of micro data, the extent of publication selection, and the control for liquidity constraints, but that other data, method, and publication aspects matter as well. Similarly to the discussion of Table 2, we can evaluate the mean reported excess sensitivity coefficient due to pure rule-of-thumb behavior. To do this, we need to make the coefficient conditional on the value of each of

Figure 7: Posterior inclusion probabilities across different prior settings



Notes: UIP and Uniform = priors according to Eicher *et al.* (2011), who recommend using the unit information prior for the parameters and the uniform model prior for model size, since these priors perform well in predictive exercises. BRIC and Random = we use the benchmark g-prior for parameters suggested by Fernandez *et al.* (2001) with the beta-binomial model prior for the model space, which means that each model size has equal prior probability (Ley & Steel, 2009). HyperBRIC and Random = we use the data-dependent hyper-g prior suggested by Feldkircher (2012) and Feldkircher & Zeugner (2012), which should be less sensitive to the presence of noise in the data.

the 48 variables—to construct an estimate given by the "best practice" in the literature. That is, using the results presented in Table 3 we compute the fitted value of the excess sensitivity after plugging in sample maxima for the aspects of studies that we prefer (based on our understanding of the consensus in the literature), sample minima for the aspects that we do not prefer, and sample means for the aspects on which we have no strong opinion. While different researchers have different opinions on what constitutes best practice, most of the variables have a negligible impact on the estimated excess sensitivity, so that our preference regarding their values does not matter much for the resulting estimate. The most important study aspects, aside from the three factors mentioned above, are the treatment of non-separability between consumption and leisure, control for the interest rate, and the order of approximation of the Euler equation.

We use the following definition of best practice. We give more weight to large studies and also plug in the sample maximum for *Midyear of data*. We prefer micro data over macro data because micro data allow the researchers to control for demographic factors; among micro studies, we choose household-level studies rather than cohort-level studies. We plug in zero for the publication bias variable to remove the effects of publication selection. We prefer studies with panel data, because panel data make it possible to control for idiosyncratic aspects of households or countries. We plug in zero for *Annual frequency* and one for *Monthly frequency*, because Bansal *et al.* (2012) show that the household's decision frequency is approximately monthly. We require that the best-practice study controls for habits, non-separabilities between consumption and leisure, and intertemporal substitution. We choose exact estimation of the consumption Euler equation rather than first- and second-order approximation, because of the arguments by Carroll (2001) and the fact that approximated Euler equations do not yield estimates of excess sensitivity that correspond precisely to the share of income accruing to rule-of-thumb households (Weber, 2000).

Next, we require that household-level studies include time fixed effects, so that the identification of excess sensitivity comes from cross-sectional variation and not from time-series variation correlated with consumption. We plug in "1" for the variable that captures the control for time aggregation. We prefer the use of non-durable consumption over total consumption or individual consumption categories (food or apparel). We also prefer studies that either employ observed expected changes in income or estimate expected income growth using instrumental variables. We plug in "1" for the case where instruments are statistically significant at the 5% level and "0" for the case where instrument strength is not reported. Because we are interested in the share of pure rule-of-thumb consumers, we also plug in "1" for the dummy variable that corresponds to using only liquidity-unconstrained households or other correction for financial constraints. We put more weight on studies published in the top five journals. Finally, we prefer studies that have been published recently, have received many citations, and are published in high-impact journals.[4] For the remaining variables we plug in sample means.

The resulting "best-practice" estimate of the share of pure rule-of-thumb consumers is 0.02, with a standard error of approximately 0.1. The estimate can also be viewed as a weighted average of all the 2,788 estimates, with more weight given to estimations that exploit large and new data sets, address major methodological problems raised in the literature, and are published in the best journals. The result, based on the BMA estimation, is close to a similar but simpler exercise based on Table 2, even though the standard error of the estimate now increases by a factor of 3 because of the complexity of the exercise. Therefore, controlling for additional 45 variables does not alter our finding that the share of pure rule-of-thumb consumers is close to zero and that even liquidity constraints do not generate substantial excess sensitivity. When we run the BMA exercise using alternative weights (Appendix C), we obtain similar estimates for the best-practice share of rule-of-thumb consumers: -0.04 for precision weights and 0.02 for weights based on the inverse of the number of estimates reported per study.

[4]For variables *Journal impact* and *No. of obs.* we use the 90^{th} centiles instead of sample maxima, since outliers appear in the upper tail of the distribution for these variables. Because increases in both variables diminish the estimated excess sensitivity, using sample maxima would result in an even smaller best-practice estimate. The best-practice estimate would be further reduced if we did not prefer highly cited and recently published studies.

6 Conclusion

We examine 2,788 estimates reported in 133 published studies that use Euler equations to evaluate the excess sensitivity of consumption to anticipated income growth. We find that the mean reported excess sensitivity, 0.4, plunges to 0.13 when we correct for the bias created by the omission of demographic variables and the bias attributable to the preferential reporting of positive and statistically significant results. The remaining excess sensitivity of 0.13 can be completely explained by liquidity constraints, and therefore our results do not indicate any evidence of pure rule-of-thumb behavior consistent with the Keynesian consumption function. Overall, it seems, the permanent income hypothesis is a pretty good approximation of the actual behavior of the average consumer, at least based on the investigations of empirical economists using consumption Euler equations during the last three and half decades.

Three caveats of our results are in order. First, while we control for 48 variables that reflect the context in which researchers obtain their estimates, we cannot rule out the possibility that all studies share a common misspecification that prevents them from identifying the underlying positive excess sensitivity. Hence the word “probably” in the title of the paper: our results are conditional on the ability of the literature on consumption Euler equations as a whole to pin down the parameter in question. In other words, the estimate of excess sensitivity that we present is the best guess we can make based on the empirical literature published so far. Second, even though we try to collect all published estimates of excess sensitivity and produce what is to our knowledge the largest meta-analysis in economics, we might still have missed some studies. We argue that an accidental omission does not create a bias as long as the studies are not omitted systematically because of their results. Third, the estimates that we collect are not independent of each other, because many studies use similar data. We partially address this problem by clustering standard errors not only at the level of studies, but also at the level of individual data sets.

Publication bias emerges as a critical issue for micro studies on excess sensitivity. The exaggeration due to publication selection is of a factor of 2, which corroborates the rule of thumb mentioned by Ioannidis *et al.* (2017): in most fields of empirical economics, dividing the mean reported coefficient by 2 yields an estimate close to the underlying effect corrected for publication bias. In a large survey among the members of the European Economic Association, Necker (2014) finds that a third of economists admit they have engaged in presenting empirical results selectively so they support their priors and in searching for control variables until they obtain a desired coefficient. While journal editors cannot observe the amount of self-censoring in manuscripts prior to submission, they can encourage authors to provide some basic checks of publication bias in their studies. The simplest check is the funnel plot—the scatter plot of estimates and their precision that should be symmetrical in the absence of the bias. Because the average study in our data set reports 21 estimates, such funnel plots would be meaningful in many cases and could serve as an indicator of potential problems for researchers before they submit their papers to journals.

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Appendix A: Description of Variables

Table A1: Definitions and summary statistics of explanatory variables

Variable	Description	Mean	Std. dev.
Data characteristics			
No. of obs.	The logarithm of the number of observations.	5.53	2.27
Midyear of data	The logarithm of the average year of the data used.	76.7	11.44
Micro	=1 if the coefficient comes from a micro-level estimation.	0.32	0.47
Micro x SE	The reported standard error if the study uses micro data.	0.08	0.20
Panel	=1 if panel data are used.	0.29	0.46
Synthetic cohort	=1 if quasipanel (synthetic cohort) data are used.	0.05	0.22
Annual frequency	=1 if the data frequency is annual (reference category: quarterly frequency).	0.46	0.50
Monthly frequency	=1 if the data frequency is monthly (reference category: quarterly frequency).	0.05	0.21
Liquidity constraints			
Liquidity unconstr.	=1 if *either* the model is estimated on a subsample of households that should not face liquidity constraints (e.g., stockholders) *or* the estimated specification includes controls that capture liquidity constraints (see Table A2 for more details).	0.10	0.31
Decrease in income	=1 if the estimate corresponds to expected decreases in income only.	0.05	0.23
Liquidity constr.	=1 if the model is estimated on a subsample of households that should face liquidity constraints (e.g., non-stockholders). See Table A2 for more details.	0.06	0.24
Increase in income	=1 if the estimate corresponds to expected increases in income only.	0.18	0.39
Utility function			
Habits	=1 if the model allows for habit formation in consumption.	0.10	0.29
Nonsep. public	=1 if the model allows for non-separability between private and public consumption.	0.07	0.25
Nonsep. labor	=1 if the model allows for non-separability between consumption and leisure.	0.06	0.24
Interest rate	=1 if the estimated specification includes a variable interest rate.	0.45	0.50
Consumption measure			
Total consumption	=1 if a proxy for consumption includes consumption of durables (reference category: non-durable consumption).	0.44	0.50
Food	=1 if food is used as a proxy for consumption (reference category: non-durable consumption).	0.06	0.23
Indiv. category	=1 if an individual subcategory of consumption, such as apparel or alcohol, is used as a proxy for consumption (reference category: non-durable consumption).	0.07	0.26
Income measure			
Outside income	=1 if the authors use observed expected change in current income rather than estimated expected change in current income (reference category: instrumented income).	0.16	0.36
Current income	=1 if current change in income is used to proxy for expected change in current income (reference category: instrumented income).	0.07	0.25
Lagged income	=1 if lagged change in income is used to proxy for expected change in current income (reference category: instrumented income).	0.02	0.13
GDP proxy	=1 if GDP/GNP is used as a proxy for disposable income.	0.15	0.36

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Table A1: Definitions and summary statistics of explanatory variables (continued)

Variable	Description	Mean	Std. dev.
Instruments signif.	=1 if the instruments used to forecast income are jointly significant at 5% (reference category: instruments insignificant).	0.22	0.42
Signif. not reported	=1 if the significance of the instruments used to forecast income is not reported (reference category: instruments insignificant).	0.52	0.50
Consumption instr.	=1 if the instrument set used to forecast income includes lags of consumption.	0.43	0.50
Income instr.	=1 if the instrument set used to forecast income includes lags of income.	0.53	0.50
Difference instr.	=1 if the instrument set used to forecast income includes lagged differences between logs of consumption and income.	0.17	0.37
Nominal IR instr.	=1 if the instrument set used to forecast income includes lags of the nominal interest rate.	0.13	0.34
Inflation instr.	=1 if the instrument set used to forecast income includes lags of the inflation rate.	0.07	0.26
Real IR instr.	=1 if the instrument set used to forecast income includes lags of the real interest rate.	0.29	0.45
Other instr.	=1 if the instrument set used to forecast income includes instruments different from those listed above.	0.40	0.49
Specification			
Exact Euler	=1 if the exact Euler equation is estimated for non-quadratic utility (reference category: first-order approximation).	0.04	0.20
Estimated in levels	=1 if the estimated specification is in levels rather than logarithms *and* the authors do not estimate the exact Euler equation (reference category: first-order approximation).	0.19	0.40
Second order	=1 if second-order approximation is used (reference category: first-order approximation).	0.06	0.24
Short run	=1 if the estimated specification includes both current and lagged changes in income *and* the estimate refers to current change.	0.07	0.26
Long run	=1 if the estimated specification includes both current and lagged changes in income *and* the estimate refers to cumulative change.	0.03	0.17
Time shift	=1 if the estimated specification accounts for time shifts in the response of consumption to income.	0.04	0.18
No year dummies	=1 if micro data is used *and* year fixed effects are not included.	0.04	0.20
Time aggregation	=1 if first lags are excluded from the instrument set *or* if the estimation method accounts for serial correlation.	0.83	0.38
Technique			
ML	=1 if maximum likelihood methods are used for the estimation (reference category for technique characteristics: GMM).	0.10	0.30
TSLS	=1 if two-stage least squares are used for the estimation.	0.45	0.50
OLS	=1 if ordinary least squares are used for the estimation.	0.17	0.37
Switching regr.	=1 if switching regression methods are used for the estimation.	0.03	0.17
Publication			
Publication year	The year of publication of the study minus 1980, the year when the first study on excess sensitivity was written.	20.73	7.1
Citations	The logarithm of the number of per-year citations of the study in Google Scholar (data for February 2016).	1.47	1.08
Top journal	=1 if the study was published in one of the top five general interest journals in economics.	0.14	0.34
Journal impact	The recursive discounted RePEc impact factor of the outlet (data for February 2016).	0.71	0.73

Notes: Collected from published studies estimating the excess sensitivity of consumption growth to expected changes in income. When dummy variables form groups, we mention the reference category. The last study was added on February 1, 2016.

Table A2: Construction of *Liquidity unconstr.* and *Liquidity constr.*

Panel A: Added Regressors

Several studies account for liquidity constraints by including variables that are thought to capture the severity of the constraints in the estimated specification as additional regressors. A common strategy is to estimate the following regression:

$$\Delta c_{t+1} = \alpha_0 + \lambda E_t \Delta y_{t+1} + \alpha_1 x_{1t+1} + \epsilon_{t+1},$$

where x_1 is a variable that captures the severity of liquidity constraints. This strategy can be employed by both macro and micro studies. For such studies we collect λ and assign the value of 1 to the dummy variable *Liquidity unconstr.*

Alternatively, some macro studies estimate excess sensitivity for individual countries and then run a cross-sectional regression of the excess sensitivity estimates on country-specific indicators of liquidity constraints (e.g., Sarantis & Stewart, 2003):

$$\lambda_i = \tilde{\lambda} + \alpha_1 x_{1i} + \epsilon_i.$$

In such cases we collect both λ_i's for individual countries and $\tilde{\lambda}$ for the whole group. We set *Liquidity unconstr.*= 0 for λ_i's and *Liquidity unconstr.*= 1 for $\tilde{\lambda}$.

Below we provide a list of variables that the studies in our data set use to capture liquidity constraints in this fashion. Each study referenced below reports some excess sensitivity estimates to which we assigned *Liquidity unconstr.*= 1.

Liquidity indicators	Studies
MACRO studies	
Degree of financial deregulation	Pozzi *et al.* (2004)
Private sector debt to GDP ratio	Sarantis & Stewart (2003)
Wedge between borrowing and lending rates	Bacchetta & Gerlach (1997), Roche (1995), Wirjanto (1995)
Mortgage credit growth	Bacchetta & Gerlach (1997)
Consumer credit growth	Bacchetta & Gerlach (1997), Ludvigson (1999)
Proportion of the total population aged 15–34	Sarantis & Stewart (2003)
Population growth rates	Sarantis & Stewart (2003)
Savings rate	Evans & Karras (1998), Sarantis & Stewart (2003)
Standard deviation of Hodrick-Prescott transitory GDP	Evans & Karras (1998)
Country-average expected income growth	Sarantis & Stewart (2003)
Assets owned by monetary and financial institutions	de Castro (2006)
Ratio of household financial wealth to income	Carroll *et al.* (2011)
Growth in total household liabilities	Carroll & Dunn (1997)
Debt service burden	Carroll & Dunn (1997)
Ratio of total household liabilities to annuity income	Carroll & Dunn (1997)
Nominal interest rate	de Castro (2006)
Unemployment rate	de Castro (2006)
Percentage of respondents agreeing that "Interest rates are high; credit is tight."	Madsen & Mcaleer (2000)
Changes in house prices	Chen *et al.* (2010)
Interest rate spread	Jappelli & Pistaferri (2011)
MICRO (and synthetic cohort) studies	
Consumer is a homeowner with a mortgage (proportion of homeowners with a mortgage for cohorts)	Campbell & Cocco (2007), Berloffa (1997)
Consumer is a homeowner outright (proportion of home-owners outright for cohorts)	Campbell & Cocco (2007), Blundell *et al.* (1994), Berloffa (1997),
Consumer recently purchased a house	Engelhardt (1996)
Growth in house prices	Campbell & Cocco (2007)
Loan-to-value ratio	Benito & Mumtaz (2009)
Housing equity to annual income	Benito & Mumtaz (2009)
Mortgage debt-to income	Benito & Mumtaz (2009)
Indicator taking the value of 1 for positive asset income	Benito & Mumtaz (2009)

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Table A2: Construction of *Liquidity unconstr.* and *Liquidity constr.* (continued)

Panel B: Sample Splits

Many micro studies account for liquidity constraints by splitting the sample of households into subsamples based on an indicator that captures the likelihood of being liquidity-constrained. The authors then perform separate excess sensitivity tests on each subsample and compare the results. For example, a common strategy is to split the sample based on the amount of liquid assets the households hold, obtaining two excess sensitivity estimates: for households with high levels of assets and for those with low assets. In such cases we assign *Liquidity unconstr.*= 1 to the estimate corresponding to the households likely to be unconstrained, and *Liquidity constr.*= 1 to the estimate corresponding to the constrained households.

Furthermore, several studies estimate switching regression models, determining endogenously the probability of being constrained for each household and obtaining separate estimates of excess sensitivity for the constrained and unconstrained groups (e.g., Garcia *et al.*, 1997). For such estimates we assign *Liquidity unconstr.* and *Liquidity constr.* using the same strategy as with sample splits. Additionally, some studies estimate models such as:

$$\Delta c_{it+1} = \alpha_0 + (\lambda_1 + \lambda_2 \cdot I_{it+1}) E_t \Delta y_{it+1} + \epsilon_{it+1},$$

where I_{it+1} is an indicator signaling whether the household is likely to be constrained. For example, I_{it+1} may be based on the assets the household holds in relation to income: $I_{it+1} = 0$ for households with high assets, $I_{it+1} = 1$ for those with low assets (e.g., Souleles, 2002). In such cases we collect λ_1 and $\lambda_1 + \lambda_2$; in this particular example we assign *Liquidity unconstr.*= 1 to λ_1 and *Liquidity constr.*= 1 to $\lambda_1 + \lambda_2$.

Below we provide a list of indicators used by studies in our data set to split samples of households. All the studies referenced below report some excess sensitivity estimates to which we assigned either *Liquidity constr.*= 1 or *Liquidity unconstr.*= 1.

Liquidity indicators	Studies
High/low wealth(assets) divided by income	Souleles (2002), Garcia *et al.* (1997), Jappelli & Pistaferri (2000), Stephens (2008), de Juan & J. Seater (1999), Deidda (2014), Souleles (1999), Tarin (2003)
High/low level of assets divided by consumption	Parker (1999)
High/low ratio of financial liabilities divided by assets	Deidda (2014)
High/low financial assets	Bernanke (1984), Filer & Fisher (2007), Parker *et al.* (2013)
High/low income	Souleles (2002), Hsieh (2003), Parker *et al.* (2013), Johnson *et al.* (2006)
High/low level of consumption	Parker (1999)
High/low consumption divided by income	Kohara & Horioka (2006)
High/low level of indebtedness	Deidda (2014)
High/low within-household correlation between income and consumption growth	Ni & Seol (2014)
Household head is old/young	Souleles (2002), Parker (1999), Stephens (2008)
Indicator specifying whether the household is a renter/homeowner	de Juan & J. Seater (1999), Filer & Fisher (2007), Parker *et al.* (2013), Tarin (2003)
Indicator for whether vehicle loan maturity is short/long	Stephens (2008)
Indicator for whether the household filed for bankruptcy within the last 10 years	Filer & Fisher (2007)
Indicator for whether the household holds credit cards	Kohara & Horioka (2006)
Indicator for whether the household head has college education	Kohara & Horioka (2006)
Indicator for whether the household's request for a loan has been rejected in the past	Deidda (2014)
Indicator for when the consumer reports he "can save some money"	Limosani & Millemaci (2011)
Indicator for when the consumer reports to be in debt	Limosani & Millemaci (2011)
Indicator for when the consumer reports he can "just about manage"	Limosani & Millemaci (2011)

Appendix B: Hedges's Model of Publication Bias (For Online Publication)

Appendices B, C, D, and E are only presented here for the convenience of reviewers. If the manuscript is accepted for publication, this material will be relegated to an online appendix.

Hedges (1992) introduces a model which assumes that the probability of publication of estimates is determined by their statistical significance. The probability of publication only changes 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 effect. The augmented log-likelihood function is (Ashenfelter *et al.*, 1999, p. 468)

$$L = c + \sum_{i=1}^{n} \log w_i(X_i, \omega) - \frac{1}{2}\sum_{i=1}^{n}\Big(\frac{X_i - \mathbf{Z}_i\Delta}{\eta_i}\Big)^2 - \sum_{i=1}^{n}\log(\eta_i) - \sum_{i=1}^{n}\log\Big[\sum_{j=1}^{4}\omega_j B_{ij}(\mathbf{Z}_i\Delta, \sigma)\Big], \quad (8)$$

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 only include micro estimates in the model.

Table B1 shows the estimation results of the model where Z only includes 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 B2).

Table B1: Hedges's test of publication bias

	Unrestricted model		Restricted ($\omega_j = 1$)	
	Coefficient	Standard error	Coefficient	Standard error
ω_2	-1.718	0.413		
ω_3	-1.019	0.380		
ω_4	-0.700	0.303		
Constant	0.175	0.014	0.153	0.008
σ	0.162	0.008	0.152	0.007
Log likelihood	745.3		721.4	
Observations	885		885	
χ^2 (H_0: all estimates have the same probability of publication): 47.8, 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 only significant at the 5% level, estimates only significant at the 10% level, and insignificant estimates. σ is the estimated measure of heterogeneity (standard deviation) of the estimates of excess sensitivity.

Table B2: Hedges's test of publication bias, controlling for publication characteristics

	Unrestricted model		Restricted ($\omega_j = 1$)	
	Coefficient	Standard error	Coefficient	Standard error
ω_2	-1.536	0.392		
ω_3	-0.874	0.358		
ω_4	-0.526	0.275		
Publication year	-0.001	0.001	-0.001	0.001
Citations	0.098	0.010	0.093	0.009
Top journal	-0.144	0.022	-0.149	0.021
Journal impact	-0.059	0.014	-0.051	0.013
Constant	0.107	0.035	0.098	0.031
σ	0.149	0.007	0.140	0.006
Log likelihood	796.1		774.6	
Observations	885		885	
χ^2 (H_0: all estimates have the same probability of publication): 43.0, 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 only significant at the 5% level, estimates only significant at the 10% level, and insignificant estimates. σ is the estimated measure of heterogeneity (standard deviation) of the estimates of excess sensitivity.

Appendix C: Weighted BMA (For Online Publication)

Table C1: 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.	-0.006	0.008	0.395			
Midyear of data	0.004	0.001	1.000	0.004	0.002	0.098
Micro	-0.313	0.033	1.000	-0.309	0.032	0.000
Micro x SE	0.731	0.081	1.000	0.698	0.157	0.000
Panel	0.025	0.034	0.388			
Synthetic cohort	0.066	0.056	0.617	0.090	0.049	0.067
Annual frequency	0.021	0.024	0.499			
Monthly frequency	-0.001	0.007	0.021			
Liquidity constraints						
Liquidity unconstr.	-0.079	0.015	1.000	-0.076	0.025	0.003
Decrease in income	0.137	0.025	1.000	0.147	0.055	0.007
Liquidity constr.	0.000	0.002	0.006			
Increase in income	-0.014	0.024	0.308			
Utility function						
Habits	-0.082	0.013	1.000	-0.081	0.027	0.003
Nonsep. public	-0.001	0.006	0.021			
Nonsep. labor	-0.118	0.021	1.000	-0.115	0.034	0.001
Interest rate	0.078	0.015	1.000	0.065	0.033	0.052
Consumption measure						
Total consumption	0.063	0.015	0.999	0.064	0.032	0.044
Food	0.000	0.003	0.017			
Indiv. category	-0.152	0.019	1.000	-0.153	0.036	0.000
Income measure						
Outside income	-0.107	0.041	0.934	-0.143	0.050	0.004
Current income	-0.083	0.032	0.928	-0.088	0.056	0.115
Lagged income	-0.230	0.033	1.000	-0.246	0.051	0.000
GDP proxy	0.241	0.020	1.000	0.250	0.051	0.000
Instruments signif.	0.000	0.002	0.008			
Signif. not reported	0.000	0.001	0.008			
Consumption instr.	0.007	0.016	0.203			
Income instr.	-0.046	0.023	0.870	-0.051	0.039	0.184
Difference instr.	0.002	0.008	0.054			
Nominal IR instr.	0.018	0.026	0.372			
Inflation instr.	0.033	0.036	0.515	0.058	0.050	0.249
Real IR instr.	-0.012	0.020	0.279			
Other instr.	0.000	0.001	0.007			
Specification						
Exact Euler	-0.113	0.030	0.992	-0.117	0.079	0.141
Estimated in levels	0.000	0.002	0.008			
Second order	-0.124	0.019	1.000	-0.116	0.031	0.000
Short run	0.000	0.004	0.018			
Long run	0.038	0.044	0.479			
Time shift	0.000	0.005	0.012			
No year dummies	0.013	0.026	0.234			
Time aggregation	0.027	0.022	0.664	0.040	0.035	0.251
Technique						
ML	0.000	0.003	0.009			
TSLS	-0.082	0.013	1.000	-0.085	0.027	0.002
OLS	-0.004	0.016	0.079			
Switching regr.	0.000	0.003	0.007			
Publication						
Publication year	0.006	0.001	1.000	0.006	0.003	0.016
Citations	0.040	0.008	1.000	0.035	0.015	0.021
Top journal	0.046	0.036	0.685	0.074	0.049	0.133
Journal impact	-0.056	0.014	0.998	-0.063	0.022	0.004
Constant	-0.023	NA	1.000	0.007	0.138	0.961
Observations	2,788			2,788		

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

Table C2: 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	0.021			
Midyear of data	0.002	0.001	0.880	-0.002	0.003	0.367
Micro	-0.312	0.051	1.000	-0.306	0.053	0.000
Micro x SE	0.463	0.054	1.000	0.449	0.120	0.000
Panel	-0.024	0.040	0.309			
Synthetic cohort	0.001	0.009	0.020			
Annual frequency	0.161	0.017	1.000	0.124	0.042	0.003
Monthly frequency	-0.001	0.009	0.023			
Liquidity constraints						
Liquidity unconstr.	0.000	0.003	0.011			
Decrease in income	0.290	0.044	1.000	0.285	0.188	0.129
Liquidity constr.	0.000	0.004	0.010			
Increase in income	-0.002	0.010	0.034			
Utility function						
Habits	-0.200	0.025	1.000	-0.190	0.077	0.013
Nonsep. public	0.000	0.005	0.017			
Nonsep. labor	-0.127	0.029	0.999	-0.123	0.049	0.012
Interest rate	0.143	0.021	1.000	0.150	0.081	0.066
Consumption measure						
Total consumption	0.012	0.024	0.237			
Food	-0.003	0.015	0.056			
Indiv. category	0.055	0.062	0.489			
Income measure						
Outside income	-0.021	0.042	0.243			
Current income	-0.272	0.032	1.000	-0.273	0.125	0.030
Lagged income	-0.294	0.039	1.000	-0.299	0.078	0.000
GDP proxy	0.297	0.029	1.000	0.317	0.095	0.001
Instruments signif.	0.001	0.008	0.023			
Signif. not reported	0.006	0.019	0.115			
Consumption instr.	0.000	0.003	0.014			
Income instr.	-0.001	0.006	0.034			
Difference instr.	0.000	0.004	0.018			
Nominal IR instr.	0.088	0.032	0.946	0.078	0.044	0.075
Inflation instr.	0.000	0.005	0.018			
Real IR instr.	-0.102	0.021	1.000	-0.118	0.080	0.143
Other instr.	-0.024	0.030	0.462			
Specification						
Exact Euler	-0.311	0.041	1.000	-0.341	0.103	0.001
Estimated in levels	-0.001	0.005	0.021			
Second order	-0.190	0.030	1.000	-0.167	0.059	0.004
Short run	0.215	0.028	1.000	0.201	0.154	0.191
Long run	0.194	0.057	0.981	0.176	0.082	0.032
Time shift	0.016	0.042	0.150			
No year dummies	0.066	0.056	0.648	0.088	0.068	0.194
Time aggregation	0.022	0.032	0.387			
Technique						
ML	0.001	0.007	0.021			
TSLS	-0.125	0.019	1.000	-0.144	0.042	0.001
OLS	0.000	0.006	0.017			
Switching regr.	0.225	0.045	1.000	0.247	0.105	0.019
Publication						
Publication year	0.012	0.002	1.000	0.014	0.006	0.019
Citations	0.049	0.009	1.000	0.039	0.021	0.064
Top journal	0.131	0.030	0.998	0.122	0.044	0.005
Journal impact	-0.047	0.017	0.947	-0.047	0.028	0.097
Constant	-0.007	NA	1.000	0.304	0.165	0.066
Observations	2,788			2,788		

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 only include explanatory variables with PIP > 0.5. The standard errors in the frequentist check are clustered at both the study and data set level.

Appendix D: Diagnostics of BMA (For Online Publication)

Table D1: Summary of BMA estimation, baseline specification

Mean no. regressors 18.2016	*Draws* $1 \cdot 10^8$	*Burn-ins* $5 \cdot 10^7$	*Time* 3.233509 hours
No. models visited 21,118,466	*Modelspace* $2.8 \cdot 10^{14}$	*Visited* $7.5 \cdot 10^{-7}\%$	*Topmodels* 58%
Corr PMP 0.9999	*No. Obs.* 2,788	*Model Prior* uniform	*g-Prior* UIP
Shrinkage-Stats Av= 0.9996			

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 D1: Model size and convergence, baseline specification

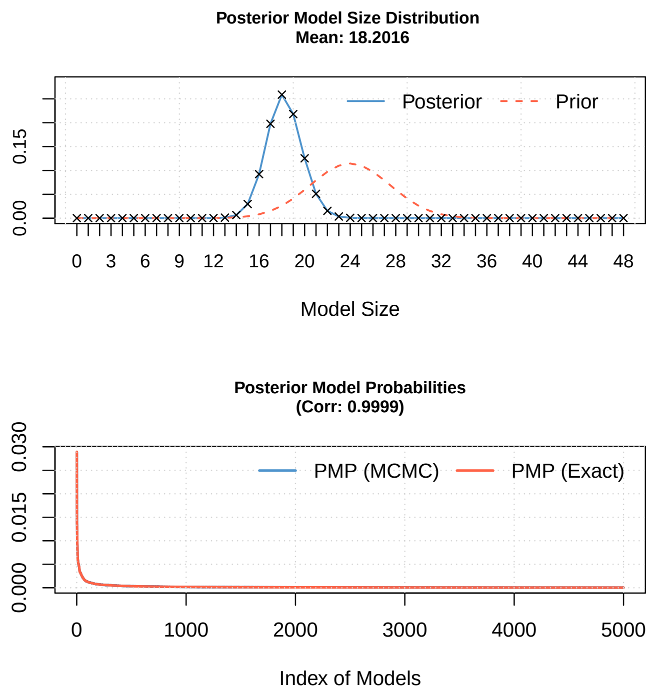
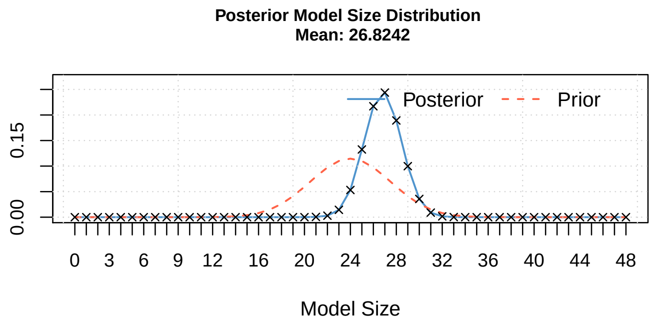


Table D2: Summary of BMA estimation, precision weights

Mean no. regressors 26.8242	*Draws* $1 \cdot 10^8$	*Burn-ins* $5 \cdot 10^7$	*Time* 3.392159 hours
No. models visited 20, 088, 509	*Modelspace* $2.8 \cdot 10^{14}$	*Visited* $7.1 \cdot 10^{-7}\%$	*Topmodels* 69%
Corr PMP 0.9999	*No. Obs.* 2, 788	*Model Prior* uniform	*g-Prior* UIP
Shrinkage-Stats Av= 0.9996			

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 D2: Model size and convergence, precision weights



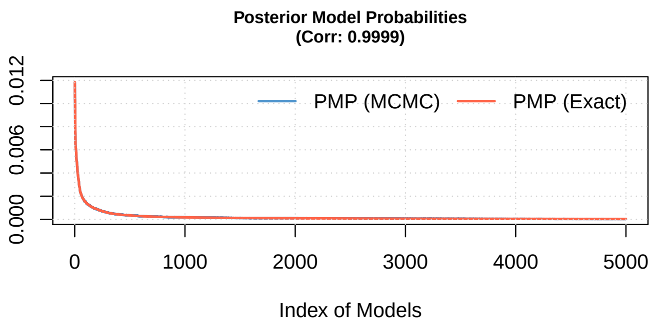
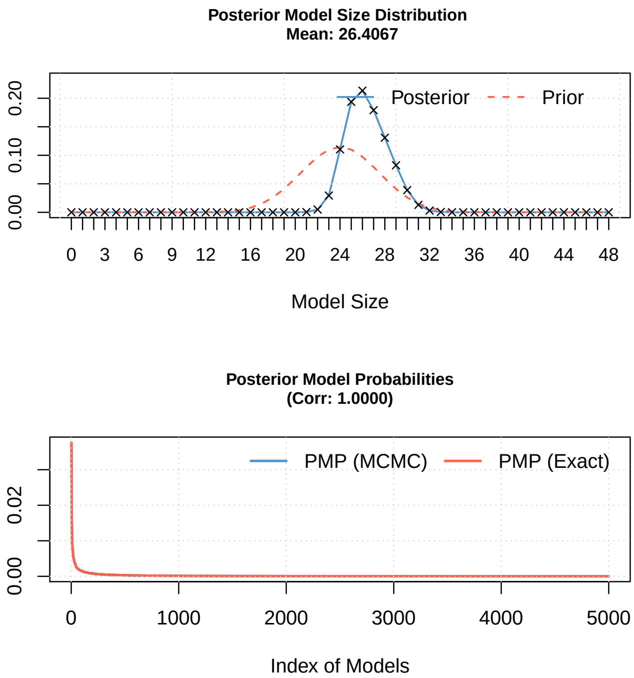


Table D3: Summary of BMA estimation, weights based on the number of estimates per study

Mean no. regressors 26.4067	*Draws* $1 \cdot 10^8$	*Burn-ins* $5 \cdot 10^7$	*Time* 3.372155 hours
No. models visited 18,182,073	*Modelspace* $2.8 \cdot 10^{14}$	*Visited* $6.5 \cdot 10^{-6}\%$	*Topmodels* 83%
Corr PMP 1.0000	*No. Obs.* 2,788	*Model Prior* uniform	*g-Prior* UIP
Shrinkage-Stats Av= 0.9996			

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 D3: Model size and convergence, weights based on the number of estimates per study



Appendix E: Studies Included in the Data Set (For Online Publication)

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