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Abstract

One of the most frequently examined relationships in education economics is the impact of tuition increases on the demand for higher education. We provide a quantitative synthesis of 443 estimates of this effect reported in 43 studies. While large negative estimates dominate the literature, we show that researchers report positive and insignificant estimates less often than they should. After correcting for this publication bias, we find that the literature is consistent with the mean tuition-enrollment elasticity being close to zero. Nevertheless, we identify substantial heterogeneity among the reported effects: for example, male students and students at private schools react strongly to changes in tuition. The results are robust to controlling for model uncertainty using both Bayesian and frequentist methods of model averaging.

Keywords: Enrollment, tuition, demand for higher education, meta-analysis, publication bias, model averaging

JEL Codes: I23, I28, C52

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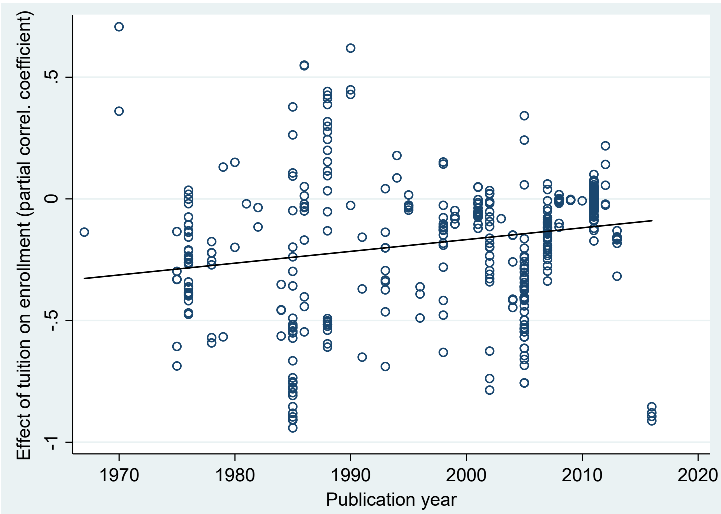
1 Introduction

The responsiveness of demand for higher education to changes in tuition fees constitutes a key parameter not only for deans but also for policymakers. It is therefore not surprising that dozens of researchers have attempted to estimate this relationship. While the relationship (often, but not always, presented in the form of an elasticity) can be expected to vary somewhat across different groups of students and types of schools, there has been no consensus even on the mean effect, as many literature surveys demonstrate (see, for example, Jackson & Weathersby, 1975; Chisholm & Cohen, 1982; Leslie & Brinkman, 1987; Heller, 1997): the estimates often differ by an order of magnitude, as we also show in Figure 1. Our goal in this paper is to exploit the voluminous work of previous researchers on this topic, assign a pattern to the differences in results, and derive a mean effect that could be used as "the best estimate for public policy purposes" that the literature has sought to identify (Leslie & Brinkman, 1987, p. 189).

Achieving our two goals involves collecting the reported estimates of the effect of tuition on enrollment and regressing them on the characteristics of students, schools, and other aspects of the data and methods employed in the original studies. Such a "meta-analysis" approach is complicated by two problems, which have yet to be addressed in the literature on tuition

Figure 1: No clear message in 50 years of research



Notes: The figure depicts a common metric (partial correlation coefficient) of the reported effect of tuition on enrollment in higher education institutions.

and enrollment:[1] publication selection and model uncertainty. Publication selection arises from the common preference of authors, editors, and referees for results that are intuitive and statistically significant. In the context of the tuition-enrollment nexus, one might well treat positive estimates with suspicion, as few economists consider education to be Giffen good. However, sufficient imprecision in estimation can easily yield a positive estimate, just as it can yield a very large negative estimate. The zero boundary provides a useful rule of thumb for model specification, but the lack of symmetry in the selection rule will typically lead to a an exaggeration of the mean reported effect (Doucouliagos & Stanley, 2013).

The second problem, model uncertainty, arises frequently in meta-analysis because many factors might influence the reported coefficients. Nevertheless, absent a theory that would specify which variables must be included in and which must be excluded from the model, researchers face a dilemma between model parsimony and potential omitted variable bias. The most common solution is to employ stepwise regression, but this approach is not appropriate because important variables can be excluded by accident in sequential t-tests. Instead, we employ model averaging techniques that are commonly used in growth regressions: Bayesian model averaging and frequentist model averaging, which are well described and compared by Amini & Parmeter (2012). The essence of model averaging is to estimate (nearly) all models with the possible combinations of explanatory variables and weight them by statistics related to goodness of fit and parsimony.

Our results suggest that the mean reported effect of tuition on enrollment is significantly downward biased because of publication selection (in other words, positive and insignificant estimates of the relationship are discriminated against). After correcting for publication selection, we find no evidence of a tuition-enrollment nexus on average. This result holds when we construct a synthetic study with ideal parameters (such as a large data set, control for endogeneity, etc.) and compute the implied "best-practice estimate": this estimate is also close to zero. Nevertheless, we find evidence of substantial and systematic heterogeneity in the reported estimates. Most prominently, our results suggest that male students and students at private schools display substantial responsiveness to changes in tuition.

The remainder of the paper is organized as follows. Section 2 describes our approach to data collection and the basic properties of the data set. Section 3 tests for the presence of publication

[1] To the best of our knowledge, the only existing meta-analysis on this topic is Gallet (2007).

selection bias. Section 4 explores the data, method, and publication heterogeneity in the estimated effects of tuition on enrollment and constructs a best practice estimate of the relationship. Section 5 concludes the paper. An online appendix, available at meta-analysis.cz/education, provides the data and code that will allow other researchers to replicate our results.

2 The Data Set

Researchers often, but not always, estimate the tuition-enrollment relationship in the form of the price elasticity of demand for higher education:

$$\ln Enrollment_{it} = \alpha + PED \cdot \ln Tuition_{it} + YED \cdot \ln Income_{it} + Controls_{ijt} + \epsilon_{it}, \qquad (1)$$

where the demand for education $Enrollment_{it}$ typically denotes the total number of students enrolled in higher education institution i in time period t, *Tuition* denotes the tuition payment for higher education, *Income* denotes the family income of a student, and its respective coefficient *YED* denotes the income elasticity of demand. ϵ is the error term. The vector $Controls_{ijt}$ represents a set of explanatory variables j, such as proxies for the quality of education (university ranking, percentage of full professors employed, student/faculty ratio, average score on assessment tests), funding opportunities (grants, external financial support, cost of loans), or labor market conditions (the level of unemployment or the wage gap between university-educated and high-school-educated workers).

From the empirical literature reporting the effect of tuition on the demand for higher education, we collect the coefficient *PED*. In (1), *PED* denotes the elasticity and captures the percent change in demand for higher education if tuition increases by one percent. The relationship between enrollment and tuition is, however, not always captured by an elasticity; the demand equation can take forms other than that with logarithms on both sides: the relationship can be linear or represented by the student price response coefficient (Jackson & Weathersby, 1975). Moreover, the definitions of the tuition and enrollment variables vary: while tuition can represent net financial aid or include other fees, enrollment can represent the total headcount of the enrolled, the number of applications, the percentage of enrolled students, or enrollment probability. Even the uncertainty measure surrounding the point estimates reported in the literature cannot always be converted into a standard error.

To be able to use elasticities and, simultaneously, make the sample fully comparable, we would need to eliminate a substantial part of the data (just as Gallet, 2007, did; moreover, our study faces an additional sample reduction since not all studies report an uncertainty measure, which we need to account for publication bias). Maximizing the number of observations and minimizing the mistakes made through conventional conversion calls for a different type of common metric. McPherson (1978, p. 180) supports the case of an ordinal measure: "*There is probably not a single number in the whole enrollment demand literature that should be taken seriously by itself. But a careful review of the literature will show that there are some important qualitative findings and order-of-magnitude estimates on which there is consensus, and which do deserve to be taken seriously.*" We follow Babecky & Havranek (2014) and Valickova *et al.* (2015), among others, and convert the collected estimates into partial correlation coefficients. Now, the PED coefficient translates to

$$PCC(PED)_{ij} = \frac{T(PED)_{ij}}{\sqrt{T(PED)_{ij}^2 + DF(PED)_{ij}}}, \tag{2}$$

where $PCC(PED)_{ij}$ represents the estimated partial correlation coefficient of the i-th estimate of the tuition elasticity PED, with $T(PED)_{ij}$ representing the corresponding t-statistics and $DF(PED)_{ij}$ representing the corresponding number of degrees of freedom reported in the j-th study. We take advantage of the previously published surveys on this topic, especially Leslie & Brinkman (1987), Heller (1997), and Gallet (2007), and extend the data sample by searching the Google Scholar database. The search query is available online at meta-analysis.cz/education. We added the last study on September 23, 2016.

The sample of studies we collect is subjected to three major selection criteria. First, the study must investigate the relationship between tuition and enrollment with enrollment as the dependent variable. This criterion eliminates multiple studies, including Mattila (1982), Galper & Dunn (1969), and Christofides *et al.* (2001), which only estimate income effects on enrollment. Second, the explanatory variable *Tuition* cannot be a dummy variable, which excludes studies such as Bruckmeier & Wigger (2014) or Dwenger *et al.* (2012) (Hübner, 2012, for example, uses dummy variable indicating residence in a fee state to investigate the effects of tuition on enrollment probabilities). Third, the study must report a measure of uncertainty around the estimate (Corman & Davidson, 1984, for example, report neither t-statistics nor standard errors). The final sample of studies used in our meta-analysis is listed in Table 1.

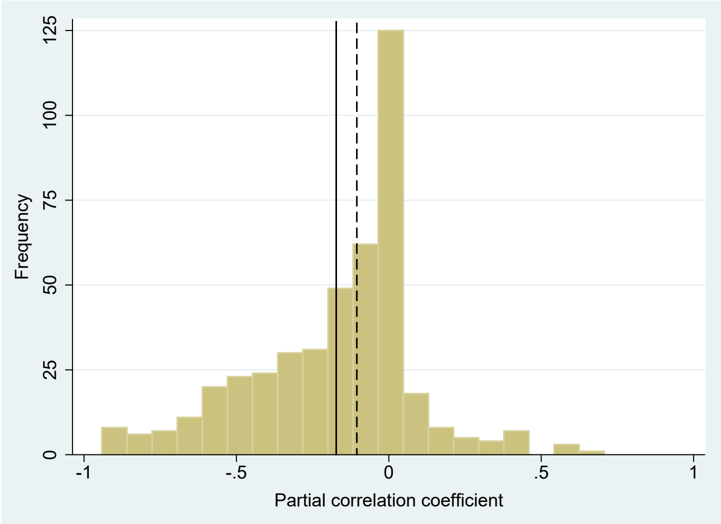
Table 1: Studies used in the meta-analysis

Agarwal & Winkler (1985)	Doyle & Cicarelli (1980)	Murphy & Trandel (1994)
Alexander & Frey (1984)	Elliott & Soo (2013)	Noorbakhsh & Culp (2002)
Allen & Shen (1999)	Grubb (1988)	Ordovensky (1995)
Berger & Kostal (2002)	Hemelt & Marcotte (2011)	Parker & Summers (1993)
Bezmen & Depken (1998)	Hight (1975)	Paulsen & Pogue (1988)
Bruckmeier *et al.* (2013)	Hoenack & Pierro (1990)	Quigley & Rubinfeld (1993)
Buss *et al.* (2004)	Hoenack & Weiler (1975)	Quinn & Price (1998)
Campbell (1967)	Hsing & Chang (1996)	Savoca (1990)
Canton & de Jong (2002)	Huijsman *et al.* (1986)	Shin & Milton (2008)
Chen (2016)	Kane (2007)	Suloc (1982)
Cheslock (2001)	King (1993)	Tannen (1978)
Chressanthis (1986)	Knudsen & Servelle (1978)	Toutkoushian & Hollis (1998)
Coelli (2009)	Koshal *et al.* (1976)	Tuckman (1970)
Craft *et al.* (2012)	McPherson & Schapiro (1991)	
Dearden *et al.* (2011)	Mueller & Rockerbie (2005)	

Previous literature surveys argue for a relatively modest magnitude of the relationship between tuition and enrollment (generally in terms of the mean student price response coefficient): Jackson & Weathersby (1975), a survey of 7 studies published between 1967 and 1973, places the enrollment change in the range of $(-0.05, -1.46)$ percentage points per \$100 tuition increase in 1974 dollars; McPherson (1978) updates the range to $(-0.05, -1.53)$. Leslie & Brinkman (1987), a survey of 25 studies published between 1967 and 1982, places the mean student price response coefficient at -0.7 per \$100 in 1982 dollars; Heller (1997), a survey of 8 studies published between 1990 and 1996, reports a range of $(-0.5, -1.0)$. The first literature survey to quantitatively examine the heterogeneity in the estimates appears much later: Gallet (2007), a meta-analysis of 295 observations from 53 studies published between 1953 and 2004, reports a mean tuition elasticity of demand for higher education of -0.6.

Our final data set covers 43 studies comprising 442 estimates of the relationship between the enrollment in a higher education institution and tuition, recalculated to partial correlation coefficients. The oldest study was published in 1967 and the newest in 2016, representing half a century of research in the area. The (left-skewed) distribution of the reported coefficients is shown in Figure 2; the coefficients range from $-.941$ to $.707$ and are characterized by a mean of -0.171 and a median of -0.103. Approximately 25% of the estimates are larger than 0.33 in the absolute value, which, according to Doucouliagos (2011), can be classified as a "large" partial correlation coefficient, while the mean coefficient is classified as a "medium" effect. Although the histogram only has one peak, Figure 5 and Figure 6 (presented in the Appendix) suggest substantial study- and country-level heterogeneity. Consequently, we collect 17 explanatory

Figure 2: Histogram of the partial correlation coefficients



Notes: The figure depicts a histogram of the partial correlation coefficients of the enrollment-tuition nexus estimates reported by individual studies. The dashed vertical line denotes the sample median; the solid vertical line denotes the sample mean.

variables that describe the data and model characteristics and investigate the possible reasons for heterogeneity below in Section 4.

Table 2 provides us with some preliminary information on the heterogeneity in the estimates. We summarize the simple mean values for each category and mean values weighted by the inverse number of estimates reported per study (to assign each study the same weight), according to different data, method, and publication characteristics. Larger studies with many estimates largely drive the simple mean of the partial correlation coefficients, especially in samples that consider private schools, female students, and countries outside the US. Thus, it seems reasonable to focus on the weighted statistics in the following discussion. We observe differences between the short- and long-term effects, which appear to be in line with intuition: a more negative long-term coefficient would suggest that, in the long run, students have more time to search for other competing providers of education. A substantial difference also appears when researchers do not account for the presence of endogeneity in the demand equation: controlling for endogeneity diminishes the partial correlation coefficient by 0.15; the effect itself is on the boundary between a small and medium effect according to Doucouliagos's guidelines.

The evidence on one of the most widely studied topics in the literature, the difference in the elasticity between public and private institutions, changes when weighting is applied: Hopkins

(1974), for example, finds that students in private institutions have a higher responsiveness than those in public schools, which is consistent with the weighted average from Table 2. The simple average is to some extent skewed by the considerable number of positive estimates in larger studies (Grubb, 1988; Hemelt & Marcotte, 2011), which would correspond to a situation in which private schools use tuition as a signal of the quality of the school. Male candidates seem to be more responsive to changes in tuition fees than female candidates, and the difference increases when weighting is applied (the result is well in line with Huijsman *et al.*, 1986, but contradicts Bruckmeier *et al.*, 2013, who do not find any differences). Spatial differences do not seem to be extensive; however, we observe that published studies report larger estimates of the effect of tuition on demand for higher education. The differences in publication status might indicate, although not necessarily, the presence of publication bias.

Table 2: Partial correlation coefficients for different subsets of data

		Unweighted			Weighted		
	No. of observations	Mean	95% conf. int.		Mean	95% conf. int.	
Temporal dynamics							
Short-run effect	209	-0.106	-0.515	0.199	-0.135	-0.567	0.360
Long-run effect	233	-0.229	-0.797	0.143	-0.233	-0.854	0.429
Estimation technique							
Control for endogeneity	31	-0.034	-0.517	0.448	-0.043	-0.328	0.448
No control for endogeneity	411	-0.181	-0.735	0.130	-0.219	-0.738	0.218
Data characteristics							
Private schools	115	-0.086	-0.540	0.298	-0.236	-0.687	0.151
Public schools	160	-0.198	-0.714	0.180	-0.154	-0.854	0.448
Male candidates	49	-0.330	-0.684	0.019	-0.329	-0.592	-0.022
Female candidates	46	-0.252	-0.614	0.050	-0.165	-0.535	0.050
Spatial variation							
USA	355	-0.150	-0.738	0.218	-0.196	-0.738	0.429
Countries outside USA	87	-0.256	-0.644	0.034	-0.136	-0.498	0.020
Publication status							
Published study	262	-0.249	-0.781	0.298	-0.209	-0.738	0.429
Unpublished study	180	-0.056	-0.365	0.056	-0.076	-0.465	0.360
All estimates	442	-0.171	-0.687	0.152	-0.186	-0.689	0.360

Notes: The table reports mean values of the partial correlation coefficients for different subsets of data. The exact definitions of the variables are available in Table 4. Weighted = estimates that are weighted by the inverse of the number of estimates per study.

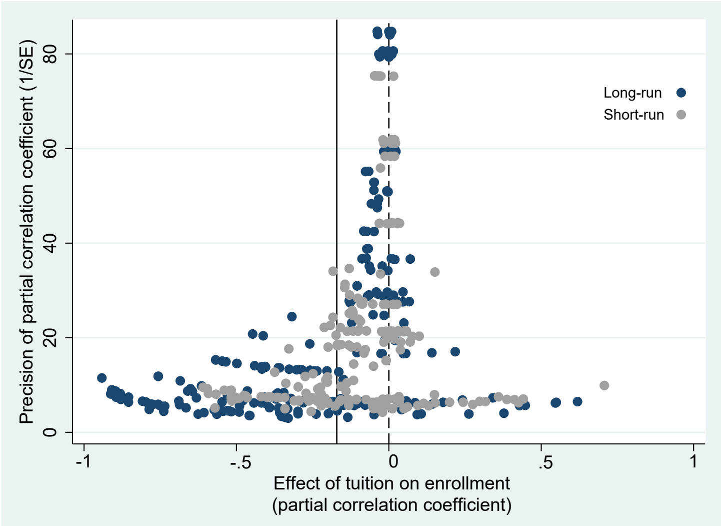
3 Publication Bias

Publication selection bias is especially likely to occur when a literature has a strong preference for a certain type of results. Both editors and researchers often yearn for significant estimates of a magnitude consistent with the commonly accepted theory. The law of demand, which implies a negative relationship between the price and demanded quantity of a good, is taken to be one of the most intuitive economic relationships; education is unlikely to be perceived as a Giffen good (Doyle & Cicarelli, 1980). Therefore, researchers treat positive estimates of the tuition-enrollment nexus with suspicion and often reveal their preference for the desired sign. (Canton & de Jong, 2002, p. 657), for example, comment on their results as follows: *"We find that the short-run coefficients all have the 'right' sign, except for the positive but insignificant coefficient on tuition fees . . . "*

Indeed, the unintuitive sign of an estimate might indicate identification problems; the probability of obtaining the 'wrong' sign increases with small samples, noisy data, or misspecification of the demand function (Stanley, 2005). We should, however, obtain the unintuitive sign of an estimate from time to time just by chance. Systematic under-reporting of estimates with the 'wrong' sign drives the global mean in the opposite direction. This distortion of reported results is a widely known phenomenon in economic research (Doucouliagos & Stanley, 2013). Studies addressing the law of demand are frequently affected by publication selection (Havranek *et al.*, 2012), but other areas also suffer from bias, with the economics of education being no exception: Fleury & Gilles (2015) report publication bias in the literature on the inter-generational transmission of education, Ashenfelter *et al.* (1999) find bias in the estimates of the rate of return to education, and Benos & Zotou (2014) report bias toward a positive impact of education on growth.

The so-called funnel plot commonly serves as a visual test for publication bias (see, for example, Irsova *et al.*, 2016, and the studies cited therein). It is a scatter plot with the effect's magnitude on the horizontal axis and its precision (the inverse of the standard error) on the vertical axis (Stanley, 2005). In the absence of publication bias, the graph resembles an inverted funnel, with the most precise estimates close to the underlying effect; with decreasing precision, the estimated coefficients become more dispersed and diverge from the underlying effect. Moreover, if the coefficients truly estimate the underlying effect while including some random

Figure 3: The funnel plot suggests publication selection bias



Notes: The dashed vertical line indicates a zero partial correlation coefficient of the elasticity of demand for higher education; the solid vertical line indicates the mean partial correlation coefficient. When there is no publication selection bias, the estimates should be symmetrically distributed around the mean effect.

error, the inverted funnel should be symmetrical. The asymmetry in Figure 3 indicates the presence of publication bias related to the sign of the effect; if the bias is related to statistical significance, the funnel becomes hollow and wide. The literature exhibits a very similar pattern of bias for the short- and long-term elasticity estimates; thus, in the calculations that follow, we do not further divide the sample based on these two characteristics, but we control for the differences in the next section.

Following Stanley (2005), we examine the correlation between the partial correlation coefficients PCCs and their standard errors in a more formal, quantitative way:

$$PCC_{ij} = PCC_0 + \beta \cdot SE(PCC_{ij}) + \mu_{ij}, \tag{3}$$

where PCC_{ij} denotes i-th effect with the standard error $SE(PCC_{ij})$ estimated in the j-th study, and μ_{ij} is the error term. The intercept of the equation, PCC_0, is the 'true' underlying effect absent publication bias; the coefficient of the standard error, β, represents publication bias. In the case of zero publication bias ($\beta = 0$), the estimated effects should represent an underlying effect that includes random error. Otherwise ($\beta \neq 0$), we should observe correlation between the PCCs and their standard error, either because researchers discard positive estimates of PCCs ($\beta < 0$) or because researchers compensate large standard errors with large estimates of PCCs.

Table 3: Funnel asymmetry tests detect publication selection bias

Panel A: Unweighted sample	OLS	IV	Proxy	Median
SE (publication bias)	-1.142***	-1.915***	-1.523***	-1.318**
	(0.36)	(0.43)	(0.27)	(0.52)
Constant (effect absent bias)	-0.059	0.016	-0.004	-0.035
	(0.06)	(0.04)	(0.04)	(0.07)
Observations	442	442	442	442
Panel B: Weighted sample	Precision		Study	
	OLS	IV	OLS	IV
SE (publication bias)	-1.757***	-2.305***	-1.023***	-1.753***
	(0.36)	(0.49)	(0.29)	(0.52)
Constant (effect absent bias)	0.001	0.026	-0.069**	0.015
	(0.02)	(0.02)	(0.03)	(0.05)
Observations	442	442	442	442

Notes: The table reports the results of the regression $PCC_{ij} = PCC_0 + \beta \cdot SE(PCC_{ij}) + \mu_{ij}$, where PCC_{ij} denotes i-th tuition elasticity of demand for higher education estimated in the j-th study, and $SE(PCC_{ij})$ denotes its standard error. Panel A reports results for the whole sample of estimates, and Panel B reports the results for the whole sample of estimates weighted by precision or study. OLS = ordinary least squares. IV = the inverse of the square root of the number of observations is used as an instrument for the standard error. Proxy = the inverse of the square root of the number of observations is used as a proxy for the standard error. Median = only median estimates of the tuition elasticities reported in the studies are included. Unweighted = model is not weighted. Study = model is weighted by the inverse of the number of estimates per study. Precision = model is weighted by the inverse of the standard error of an estimate. Standard errors in parentheses are clustered at the study and country level (two-way clustering follows Cameron *et al.*, 2011). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

In other words, the properties of the standard techniques used to estimate the tuition-enrollment nexus yield a t-distribution of the ratio of point estimates to their standard errors, which means that the estimates and standard errors should be statistically independent quantities.

Table 3 reports the results of (3). In Panel A we present four different specifications applied to the unweighted sample: simple OLS, an instrumental variable specification in which the instrument for the standard error is the inverse of the square root of the number of observations (as in, for example, Havranek *et al.*, 2017), OLS in which the standard error is replaced by the aforementioned instrument (as in Havranek, 2015), and study-level between-effect estimation.[2] In Panel B, we weight all estimates by their precision, which assigns greater importance to more precise results, and we weight all estimates by the inverse of the number of observations per study, which treats small and large studies equally. In accordance with the mean statistics from Table 2, the mean effect marginally increases (the mean response of enrollment to tuition changes becomes less sensitive) and even becomes significant but is still close to zero.

[2]It is worth noting that while we also intended to use study-level fixed effects (a common robustness check accounting for unobserved study-level characteristics, see the online appendix of Havranek & Irsova, 2017), the high unbalancedness of our panel data set and the fact that a number of studies report only one observation make this specification infeasible.

Two important findings can be distilled from Table 3. First, publication bias is indeed present in our sample, and according to the classification of Doucouliagos & Stanley (2013), the magnitude of selectivity ranges from substantial ($-2 > \beta > -1$) to severe ($\beta < -2$). Second, we cannot reject the hypothesis that the 'true' underlying correlation coefficient of the tuition-enrollment effect corrected for publication bias is zero. The estimated coefficient β suggests that the true effect is very small or indeed zero. Table 3, however, does not tell us whether data and method choices are correlated with the magnitude of publication bias or the underlying effect. We address these issues in the next section.

4 Heterogeneity

4.1 Variables and Estimation

Thirty years ago, Leslie & Brinkman (1987) concluded their review of the tuition-enrollment literature with disappointment regarding study heterogeneity: *"Weinschrott (1977) was correct when he warned about the difficulties in achieving consistency among such disparate studies."* Data heterogeneity in our own sample is obvious from Figure 5 and Figure 6 (presented in the Appendix) and the substantial standard deviations of the mean statistics we report in Table 2. Therefore, we codify 17 characteristics of study design into explanatory variables that capture additional variation in the data. The explanatory variables are listed in Table 4 and divided into four groups: variables capturing methodological differences, differences in the design of the demand function, differences in the data set, and publication characteristics. Table 4 also includes the definition of each variable, its simple mean, standard deviation, and the mean weighted by the inverse of the number of observations extracted from a study.

Table 4: Description and summary statistics of regression variables

Variable	Description	Mean	SD	WM
Partial correlation coef.	Partial correlation coefficient derived from the estimate of the tuition-enrollment relationship.	-0.171	0.271	-0.186
Standard error	The estimated standard error of the tuition-enrollment estimate.	0.097	0.070	0.115
Estimation characteristics				
Short-run effect	= 1 if the estimated tuition-enrollment effect is short-term (in differences) instead of long-term (in levels).	0.473	0.500	0.480

Continued on next page

Table 4: Description and summary statistics of regression variables (continued)

Variable	Description	Mean	SD	WM
OLS	= 1 if OLS is used for the estimation of the tuition-enrollment relationship.	0.446	0.498	0.687
Control for endogeneity	= 1 if the study controls for price endogeneity.	0.070	0.256	0.187
Design of the demand function				
Linear function	= 1 if the functional form of the demand equation is linear.	0.296	0.457	0.301
Double-log function	= 1 if the functional form of the demand equation is log-log.	0.507	0.501	0.501
Unemployment control	= 1 if the demand equation controls for the unemployment level.	0.495	0.501	0.350
Income control	= 1 if the demand equation controls for income differences.	0.643	0.480	0.653
Data specifications				
Cross-sectional data	= 1 if cross-sectional data are used for estimation instead of time-series or panel data.	0.204	0.403	0.303
Panel data	= 1 if panel data are used for estimation instead of cross-sectional or time-series data.	0.557	0.497	0.347
Male candidates	= 1 if the study estimates the tuition-enrollment relationship for male applicants only.	0.111	0.314	0.075
Female candidates	= 1 if the study estimates the tuition-enrollment relationship for female applicants only.	0.104	0.306	0.051
Private schools	= 1 if the study estimates the tuition-enrollment relationship for private schools only.	0.260	0.439	0.233
Public schools	= 1 if the study estimates the tuition-enrollment relationship for public schools only.	0.362	0.481	0.454
USA	= 1 if the tuition-enrollment relationship is estimated for the United States only.	0.803	0.398	0.839
Publication characteristics				
Publication year	Logarithm of the publication year of the study.	7.601	0.006	7.598
Citations	Logarithm of the number of citations the study received in Google Scholar.	3.529	1.079	3.227
Published study	= 1 if the study is published in a peer-reviewed journal.	0.593	0.492	0.828

Notes: SD = standard deviation, SE = standard error, WM = mean weighted by the inverse of the number of estimates reported per study.

Estimation characteristics The exact distinction between short- and long-run effects is disputable in most economic literatures (see, for example Espey, 1998). If the author does not clearly designate her estimate, we follow the basic intuition and classify the growth estimates as short term and the level estimates as long term. Static models, however, introduce ambiguity. If the data set only covers a short period of time, the estimate might not reflect the full long-term response; thus, we label such estimates as *short-run effects*. Hoenack (1971) notes the importance of temporal dynamics: lowering costs in the long run encourages students to apply for higher education; in the short run, however, the change can only influence the current applicants. The long-run effects are, therefore, likely to be larger. We do not divide the sample between short- and long-run elasticities, which conforms to our previous discussion and the practice applied by the previous meta-analysis on this topic (Gallet, 2007).

Researchers use various techniques to estimate the tuition-enrollment relationship. Fixed effects, in particular, dominate the panel-data literature. More than one-third of the estimates are a product of simple *OLS*, and surprisingly few studies control for endogeneity: as Coelli (2009) emphasizes, an increase in tuition fees could be a response to an increase in the demand for higher education. Therefore, the estimated impact may also include a positive price response to the supply of student vacancies and thus underestimate the effect of tuition on the demand for higher education (Savoca, 1990). For this reason, we could expect the estimates that do not account for the endogeneity of tuition fees, such as those derived using OLS, to indicate a smaller impact on enrollment than those derived using, say, instrumental variables (as in Neill, 2009, for example).[3] To address endogeneity bias, we include a dummy variable indicating methods that do *control for endogeneity.*

Design of the demand function The relationship between tuition fees and the demand for higher education can be captured in multiple ways. We present in (1) the *double-log functional form* of the demand function, which produces the elasticity measure and accounts for half of the estimates in our sample (Allen & Shen, 1999; Noorbakhsh & Culp, 2002; Buss *et al.*, 2004). Some authors, including McPherson & Schapiro (1991) and Bruckmeier *et al.* (2013), capture the simple linear relationship between the variables using a *linear demand function.* Semi-elasticities are also sometimes estimated and can be captured by the semi-log functional form (Shin & Milton, 2008); few authors use non-linear Box-Cox transformations (such as Hsing & Chang, 1996, who test whether the estimated elasticity is indeed constant). We suspect that despite the transformation of all the estimates into a universal measure of partial correlation coefficients, some systematic deviations in the estimates might remain based on the form of the demand function.

Researchers also specify demand equations to reflect various social and economic conditions of the applicants. We account for whether researchers control for the two most important of these conditions: the *income* level and *unemployment* rate. Lower-income students should be

[3]Note that some studies, such as Coelli (2009), use OLS while simultaneously attempting to minimize endogeneity bias using other than methodological treatments: mostly by including a detailed set of individual youth and parental characteristics.

more responsive to changes in tuition than higher-income students (McPherson & Schapiro, 1991); we expect systematic differences between results that do and do not account for income differences. The effect of controlling for the unemployment level is not as straightforward. Some authors (such as Berger & Kostal, 2002) hypothesize that the unemployment rate might be positively associated with enrollment, as attending a higher education institution can represent a substitute for being employed. An unfavorable employment rate, by contrast, reduces the possibilities of financing higher education. Labor market conditions can also be captured by other variables, for example, real wages, as the opportunity costs of attending school (Mueller & Rockerbie, 2005) or a wage gap (Bruckmeier *et al.*, 2013) reflecting the differences in earnings between those who did and did not participate in higher education.

Data specifications Leslie & Brinkman (1987) note that while cross-sectional studies reflect the impact of explicit prices charged in the sample, panel studies reflect that each educational institution implicitly accounts for the price changes of other institutions. Different projection mechanisms could introduce heterogeneity in the estimates. Thus, we include a dummy variable for studies that rely on *cross-sectional* variation and for studies that rely on *panel data* (the reference category being time-series data). Since approximately 80% of our data are estimates for the *USA*, we plan to examine whether geography induces systematic differences in the estimated partial correlation coefficients. Elliott & Soo (2013) conduct a study of 26 different countries including the US: the global demand for higher education seems to be more price sensitive than US demand, although this conclusion is not completely robust.

The issue of *male* and *female* participants and their responsiveness to price changes has also been discussed in previous studies. Savoca (1990) claims that females could face lower earnings upon graduation; therefore, they may see higher education as a worse investment and be less likely to apply. Bruckmeier *et al.* (2013) shows that gender matters when technical universities are considered, while Mueller & Rockerbie (2005) find that male Canadian students are more price sensitive than their female counterparts. McPherson & Schapiro (1991), however, argue that the gender effect is in general constant across income groups, and Gallet (2007) does not find significant gender-related differences in reported estimates. The differences between *public* and *private* educational institutions are also frequently discussed, and researchers agree that these institutions face considerably different demand, unless student aid is provided. The results of Funk (1972) suggest the student price response to be consistently lower for private schools. Hight (1975) supports these conclusions and argues that the demand for community or public colleges tends to be more elastic than the demand for private colleges. In a similar vein, Leslie & Brinkman (1987) note that the average student at a private school has a higher family income base; furthermore, a lower-income student, who is also more likely to enroll in a public school, typically demonstrates higher responsiveness to increases in tuition. However, Bezmen & Depken (1998) find those who apply to private schools to be more price sensitive.

Publication characteristics While we do our best to control for the relevant data and method characteristics, it is unfeasible to codify every single difference between individual estimates. There might be unobserved aspects of data and methodology (or, more generally, quality) that drive the results. For this reason, a number of modern meta-analyses (such as Havranek *et al.*, 2015) employ a variable representing the *publication year* of the study: new studies are more likely to present methodological innovations that we might have missed in our previous discussion. Furthermore, we exploit the *number of citations* in Google Scholar to reflect how heavily the study is used as a reference in the literature and information on *publication status* since the peer-review process can be thought of as an indication of study quality.

The purpose of this section is to investigate which of the method choices systematically influence the estimated partial correlation coefficients and whether the estimated coefficient of publication bias from Section 3 survives the addition of these variables. Ideally, we would like to regress the partial correlation coefficient on all 17 characteristics listed above, plus the standard error. Since we have a relatively large number of explanatory variables, however, it is highly probable that some of the variables will prove redundant. The traditional use of model selection methods (such as eliminating insignificant variables one by one or choosing the final model specification in advance) often leads to overly optimistic confidence intervals. In this paper, we opt for model averaging techniques, which can address the model uncertainty inherent in meta-analysis.

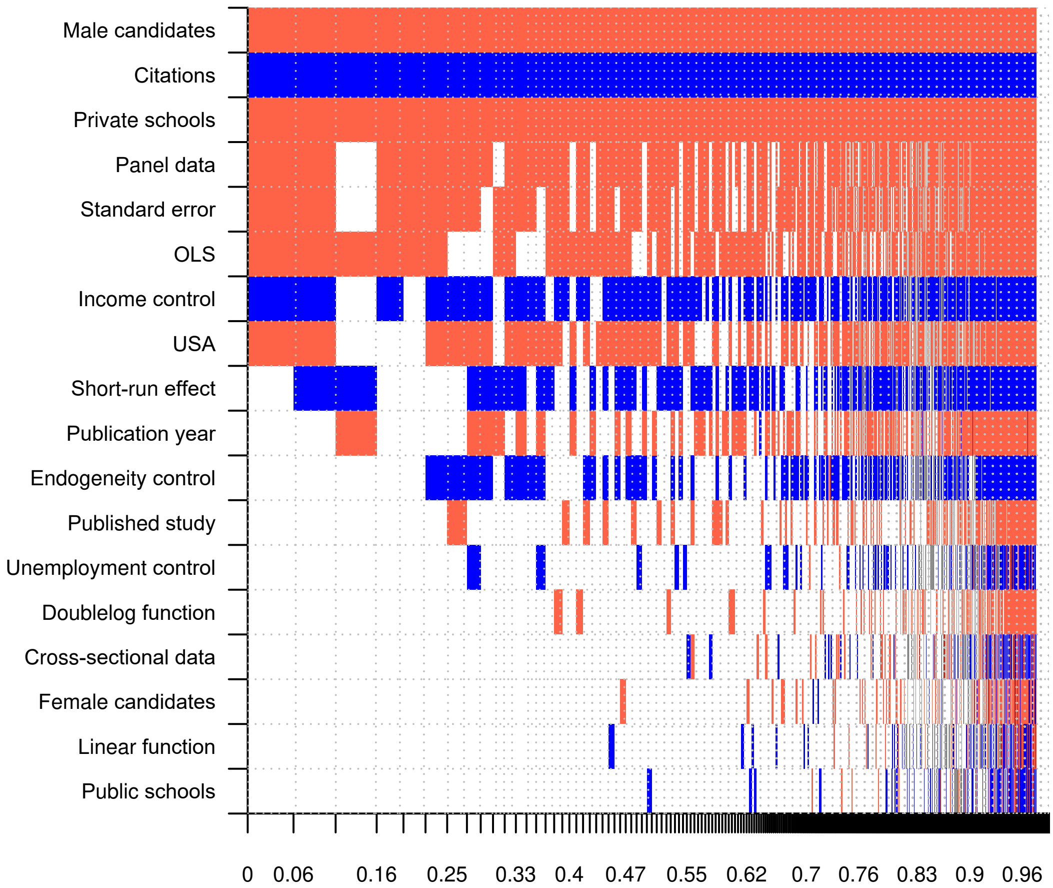
Bayesian model averaging (BMA) is our preferred choice of estimation technique to analyze heterogeneity. BMA processes hundreds of thousands of regressions consisting of different subsets of the 18 explanatory variables. With such a large model space (2^{18} models to be estimated), we decide to follow some of the previous meta-analyses (such as Havranek & Rusnak, 2013, who also use the `bms` R package by Feldkircher & Zeugner, 2009) and apply the Markov chain Monte Carlo algorithm that considers only the most important models. Bayesian averaging computes weighted averages of the estimated coefficients (posterior means) across all the models using posterior model probabilities (analogous to information criteria in frequentist econometrics) as weights. Thus, all the coefficients have an approximately symmetrical distribution with a posterior standard deviation (analogous to the standard error). Each coefficient is also assigned a posterior inclusion probability (analogous to statistical significance), which is a sum of posterior model probabilities for the models in which the variable is included. Further details on BMA can be found, for example, in Eicher *et al.* (2011).

Although BMA is the most frequently used tool to address model uncertainty, recently proposed statistical routines for frequentist model averaging (FMA) make the latter a competitive alternative. Frequentist averaging, unlike the Bayesian version, does not require the use of explicit prior information. We follow Rusnak *et al.* (2017), the first study to apply FMA in the meta-analysis framework, who use the approach of Amini & Parmeter (2012), which is based on on the works of Hansen (2007) and Magnus *et al.* (2010). As in the case of BMA, we attempt to restrict our model space from the original 2^{18} models and use Mallow's model averaging estimator (Hansen, 2007) with an orthogonalization of the covariate space according to Amini & Parmeter (2012) to narrow the number of estimated models. Mallow's criterion helps to select asymptotically optimal weights for model averaging. Further details on this method can be found in Amini & Parmeter (2012).

4.2 Results

The results of the BMA estimation are visualized in Figure 4. The rows in the figure represent individual explanatory variables and are sorted according to the posterior inclusion probability from top to bottom in descending order. The columns represent individual models and are sorted according to the model inclusion probability from left to right in descending order. Each cell in the figure thus represents a specific variable in a specific model; a blue cell (darker in grayscale) indicates that the estimated coefficient of a variable is positive, a red cell (lighter in grayscale) indicates that the estimated coefficient of a variable is negative, and a blank cell

Figure 4: Model inclusion in Bayesian model averaging



Notes: The figure depicts the results of BMA. On the vertical axis, the explanatory variables are ranked according to their posterior inclusion probabilities from the highest at the top to the lowest at the bottom. The horizontal axis shows the values of the cumulative posterior model probability. Blue color (darker in grayscale) = the estimated parameter of a corresponding explanatory variable is positive. Red color (lighter in grayscale) = the estimated parameter of a corresponding explanatory variable is negative. No color = the corresponding explanatory variable is not included in the model. Numerical results are reported in Table 5. All variables are described in Table 4. The results are based on the specification weighted by the number of estimates per study.

indicates that the variable is not included in the model. Figure 4 also shows that nearly half of the variables are included in the best model and that their signs are robustly consistent across different models.

A numerical representation of the BMA results can be found in Table 5 (our preferred specification is BMA estimated with the uniform model prior and unit information prior following Eicher *et al.*, 2011). Additionally, we provide two alternative specifications: first, a frequentist check estimated by simple OLS with robust standard errors clustered at the study and country level in which we only include variables from BMA with posterior inclusion probability higher than 0.5. Second, we provide a robustness check based on FMA, which includes all explanatory variables. All estimations are weighted using the inverse of the number of estimates reported per study. In Appendix A, we also provide the robustness checks of BMA with different priors (following Fernandez *et al.*, 2001; Ley & Steel, 2009) and different weighting (by precision). Complete diagnostics of the BMA exercises can be found in Appendix B.

In interpreting the posterior inclusion probability, we follow Jeffreys (1961). The author categorizes values between 0.5 and 0.75 as weak, values between 0.75 and 0.95 as positive, values between 0.95 and 0.99 as strong, and values above 0.99 as decisive evidence for an effect. Table 5 thus testifies to decisive evidence of an effect in the cases of *Male candidates*, *Private schools*, and *Citations*, to positive evidence of an effect in the case of *Panel data*, and to weak evidence of an effect in the cases of the *Short-run effect*, *OLS*, *Income control*, and *USA* variables. While our robustness checks seem to support the conclusions from BMA, the evidence for an effect of the variables *OLS* and *Control for endogeneity* changes when FMA is employed.

Publication bias and estimation characteristics Although diminished to almost a half of its original value (Table 3), the evidence for publication bias represented by the coefficient on the *Standard error* variable survives the inclusion of controls for data and method heterogeneity. The result supports our original conclusion that publication bias indeed plagues the literature estimating the relationship between tuition fees and the demand for higher education. The evidence on the *short-run effect* is in line with expectation from Table 2 (and the conclusions of Gallet, 2007): its positive coefficient suggests a lower sensitivity to price changes in the short-run than in the long-run, when the enrollees have more time to adapt to a new pricing scheme and search for adequate substitutes.

Table 5 reports that the evidence on the importance of the *OLS* and *Control for endogeneity* variables is mixed across different model averaging approaches. The instability of the two coefficients is somewhat intuitive: studies using simple OLS rarely control for endogeneity; the correlation coefficient of these variables is -0.45. The direction of the effect of controlling for endogeneity that we identify is, however, not consistent with what is often found in the literature (Savoca, 1990; Neill, 2009): estimates that do not account for endogeneity are expected to show smaller effects since these estimates may capture the positive effects of price on the supply

Table 5: Explaining heterogeneity in the estimates of the tuition-enrollment nexus

Response variable:	Bayesian model averaging			Frequentist check (OLS)			Frequentist model averaging		
Tuition elasticity	Post. mean	Post. SD	PIP	Coef.	SE	p-value	Coef.	SE	p-value
Constant	0.002	NA	1.000	-0.149	0.051	0.004	0.005	0.003	0.086
Standard error	-0.650	0.439	0.758	-0.673	0.099	0.000	-0.712	0.252	0.005
Estimation characteristics									
Short-run effect	0.052	0.053	0.575	0.093	0.007	0.000	0.138	0.038	0.000
OLS	-0.097	0.067	0.742	-0.105	0.044	0.018	-0.016	0.052	0.766
Control for endogeneity	0.052	0.073	0.414				0.165	0.052	0.002
Design of the demand function									
Linear function	0.000	0.011	0.064				-0.072	0.052	0.172
Double-log function	-0.003	0.015	0.095				-0.068	0.046	0.134
Unemployment control	0.008	0.026	0.132				0.073	0.045	0.107
Income control	0.083	0.065	0.718	0.054	0.021	0.009	0.191	0.040	0.000
Data specifications									
Cross-sectional data	0.000	0.022	0.093				-0.085	0.050	0.091
Panel data	-0.112	0.073	0.784	-0.018	0.013	0.168	-0.238	0.055	0.000
Male candidates	-0.351	0.062	1.000	-0.227	0.073	0.002	-0.381	0.065	0.000
Female candidates	-0.007	0.040	0.072				-0.145	0.110	0.189
Private schools	-0.169	0.039	0.996	-0.077	0.003	0.000	-0.173	0.048	0.000
Public schools	0.001	0.012	0.060				0.011	0.038	0.767
USA	-0.095	0.086	0.643	-0.038	0.036	0.283	-0.196	0.060	0.001
Publication characteristics									
Publication year	-0.013	0.018	0.421				-0.016	0.013	0.212
Citations	0.043	0.010	1.000	0.033	0.003	0.000	0.053	0.010	0.000
Published study	-0.015	0.037	0.183				-0.054	0.052	0.301
Studies	43			43			43		
Observations	442			442			442		

Notes: SD = standard deviation. SE = standard error. PIP = posterior inclusion probability. Bayesian model averaging (BMA) employs priors suggested by Eicher *et al.* (2011). The frequentist check (OLS) includes the variables recognized by BMA to comprise the best model and is estimated using standard errors clustered at the study and country level. Frequentist model averaging (FMA) follows Mallow's averaging using the orthogonalization of covariate space suggested by Amini & Parmeter (2012). All variables are described in Table 4. Additional details on the BMA exercise can be found in the Appendix B in Table 8 and Figure 8.

of education. Our results suggest that controlling for endogeneity understates the reported effects. This finding is consistent with Gallet (2007), who report that methods that control for endogeneity generate more positive estimates than OLS.

Design of the demand function According to our results, the functional form of the demand function does not systematically affect the reported coefficients. This conclusion differs from the findings of Gallet (2007), who argue that the outputs of semi-log, linear, and Box-cox functional forms are significantly different from the results produced by directly estimating the double-log demand function. Furthermore, the inclusion of the control variable for *unemployment* also does not seem to drive the estimated sensitivity of enrollment to tuition changes; the control for an individual's *income* group, however, significantly decreases the estimated sensitivity.

Data specifications Leslie & Brinkman (1987) report that estimates produced from cross-sectional data sets and time-series data sets do not vary substantially, and our results support this conclusion. *Panel data*, which combine both cross-sectional and time information, however, lead to partial correlation coefficients that are 0.11 smaller, other things being equal. We also argue that *male students* exhibit a systematically larger (by 0.35) sensitivity to changes in tuition in comparison with the general population, which is in contrast to the results of Gallet (2007), who finds that gender-related characteristics fail to significantly affect the reported tuition elasticity. The results are, however, in line with those of Mueller & Rockerbie (2005), who find males to be more price sensitive than females. As an explanation, Mueller & Rockerbie (2005) argue that since the rate of return to a university degree might be higher for a female than for a male, females are willing to spend more on tuition fees.

Some studies estimate the effect of tuition in public schools, while others consider private schools. We find that candidates applying to *private schools* are more responsive to changes in tuition. One interpretation of the different magnitudes of the price sensitivity is that the more or the better the substitutes are for a particular commodity, the higher the price sensitivity. In our case, students should be able to more easily switch to a substitute institution when private school tuition rises than when public school tuition rises, as the pool of substitutes for private institutions should be larger and also includes public schools (where costs are lower). These results, however, contradict those of Leslie & Brinkman (1987) and Hight (1975), who note that the average enrollee at a private school is rich and, thus, less price-elastic. Estimates for the US seem to be less negative than those for other countries. We would argue that given the extent of the US system of higher education, the pool of close substitutes might be larger in the US than in the rest of the world, where a single country hosts a smaller number of universities.

Publication characteristics There are two results on publication characteristics that are consistent with the meta-analysis of Gallet (2007): the insignificance of publication year and publication status. *Publication year* may capture changes in methodological approaches; nevertheless, Table 5 indicates the newer studies do not report systematically different results. Further, we show in Table 2 that the partial correlation coefficients reported in *Published studies* are arguably smaller than those in unpublished or unrefereed studies. The impact of other explanatory variables, however, erases this link; in fact, Table 5 suggests the publication status of a study does not matter for the magnitude of the estimates. More important is how much attention the paper attracts from readers, which is captured by the number of *citations*. Highly cited articles report less-sensitive estimates of the tuition-enrollment relationship.

Thus far, we have argued that the mean reported value of the tuition-enrollment partial correlation coefficient, -0.19 (shown in Table 2), is significantly exaggerated by the presence of publication bias. The effect absent publication bias, shown in Table 3, is close to zero. We have also seen that the effect is substantially influenced by data, method, and publication characteristics. To provide the reader with a 'rule-of-thumb' mean effect that controls for all these influences and potential biases, we construct a synthetic 'best-practice' study that employs our preferred choices with respect to all the sources of heterogeneity in the literature. The definition of best practice is subjective, but it is a useful check of the combined effect of various misspecifications and publication bias. Essentially, we create a weighted average of all estimates by estimating fitted values from the BMA and FMA specifications.

The ideal study that we imagine would be published in a refereed journal, highly cited, and recent; thus, we set all publication characteristics at the sample maxima (we censor, however, the number of citations at the 99% level due to the presence of outliers—although using the sample maximum would provide us with an even stronger result). We remove any sources of publication and endogeneity bias; thus, we set the *standard error* and *OLS* at the sample minima and the *control for endogeneity* at the sample maximum. We prefer the usage of broader data sets and favor the inclusion of controls for the economic environment, and thus, we set the panel data set and controls for income and unemployment at the sample maxima. Moreover, we prefer the double-log functional form since it directly produces an elasticity, a measure with a clear interpretation, independent of the current price level. We leave the remaining variables at their sample means.

Table 6: Best practice estimation yields a tuition-enrollment effect that is close to zero

	Bayesian model averaging			Frequentist model averaging		
	Mean	95% conf. int.		Mean	95% conf. int.	
Short-run effect	-0.010	-0.032	0.012	0.069	0.047	0.090
Long-run effect	-0.062	-0.081	-0.042	-0.070	-0.089	-0.050
Private schools	-0.167	-0.190	-0.144	-0.141	-0.164	-0.117
Public schools	0.003	-0.033	0.039	0.043	0.007	0.079
Male candidates	-0.361	-0.529	-0.192	-0.348	-0.517	-0.180
Female candidates	-0.017	-0.120	0.087	-0.112	-0.216	-0.008
All estimates	-0.037	-0.055	-0.019	-0.003	-0.021	0.015

Notes: The table presents mean estimates of the partial correlation coefficients implied by the Bayesian/frequentist model averaging and our definition of 'best practice.' The confidence intervals for BMA are approximate and constructed using the standard errors estimated by simple OLS with robust standard errors clustered at the study and country level.

The 'best-practice' estimation in Table 6 yields a partial coefficient of -0.037 with a 95% confidence interval of $(-0.055;\ -0.019)$. The estimated standard errors are relatively small, and even with plausible changes to the best practice approach (such as changing the design of the demand function), the results reported in Table 6 only change at the third decimal place. The best-practice estimation thus corroborates our previous assertions regarding the demand sensitivity to tuition changes: in general, we observe higher responsiveness to price changes in the long-run, higher responsiveness among individuals enrolled in private schools, and higher responsiveness among male students.

5 Concluding Remarks

In this paper we conduct a quantitative synthesis of 442 estimates of the relationship between tuition and the demand for higher education reported in 43 studies. Our extension of the previous meta-analysis by Gallet (2007) is twofold: first, we include a formal treatment of publication bias, and second, we include a treatment of model uncertainty using model averaging methods when searching for the determinants of the underlying effect. The literature shows signs of substantial publication selection against positive estimates, suggesting that many researchers use the sign of the estimated effect as a specification test (education is unlikely to be a Giffen good). The mean effect beyond publication bias is close to zero. When we attribute greater weight to the more reliable estimates (published in respected journals and derived using appropriate methodology), we obtain a similarly small mean estimate of the tuition-enrollment nexus.

We also find evidence for systematic dependencies between the estimated effects and data, method, and publication characteristics. Male students are more sensitive to changes in tuition, as are students at private schools. Previous research has yielded mixed results on both of these relationships. Our findings concerning male students are consistent with those of Mueller & Rockerbie (2005), who argue that because female students tend to have a higher rate of return from university education, they are willing to spend more on tuition fees. Concerning private schools, it might be easier for their students to find substitutes in the event of an increase in tuition; for public school students, a large portion of the market (most private schools) is already unaffordable. Next, we find that highly cited studies tend to report little sensitivity of enrollment to tuition, although the direction of causality is unclear. Our results also suggest that the reported responsiveness is higher for US students and when panel data are used, while it is lower when income is controlled for and in the short run.

Two qualifications of our analysis are in order. First, while we would prefer to work with elasticities, many studies estimate the relationship between tuition and enrollment using approaches other than the log-log specification. We already have to exclude a significant portion of studies because they do not report standard errors, t-statistics, or confidence intervals for their results, thus making it impossible for us to test the presence of publication bias. Restricting our data set to log-log specifications would drastically reduce the number of degrees of freedom available for our analysis. While it is possible to recompute some of the other coefficients to elasticities evaluated at the sample mean, many studies do not report the statistics necessary for this computation. Therefore, we choose to work with partial correlation coefficients, which can be computed easily from all the studies. Since our main result indicates negligible partial correlation absent publication bias, it also directly translates into a finding of a zero mean elasticity of demand for higher education to tuition fees. Second, the results of a meta-analysis are obviously conditional on the quality of the previous studies included in the sample. For instance, if all studies in the literature share a common misspecification that biases their results toward zero, we are unable to control for such a misspecification, and our results are thus also biased. Therefore, the correct interpretation of our analysis is that, judging from the available empirical research, our best guess concerning the effect of tuition on enrollment is close to zero.

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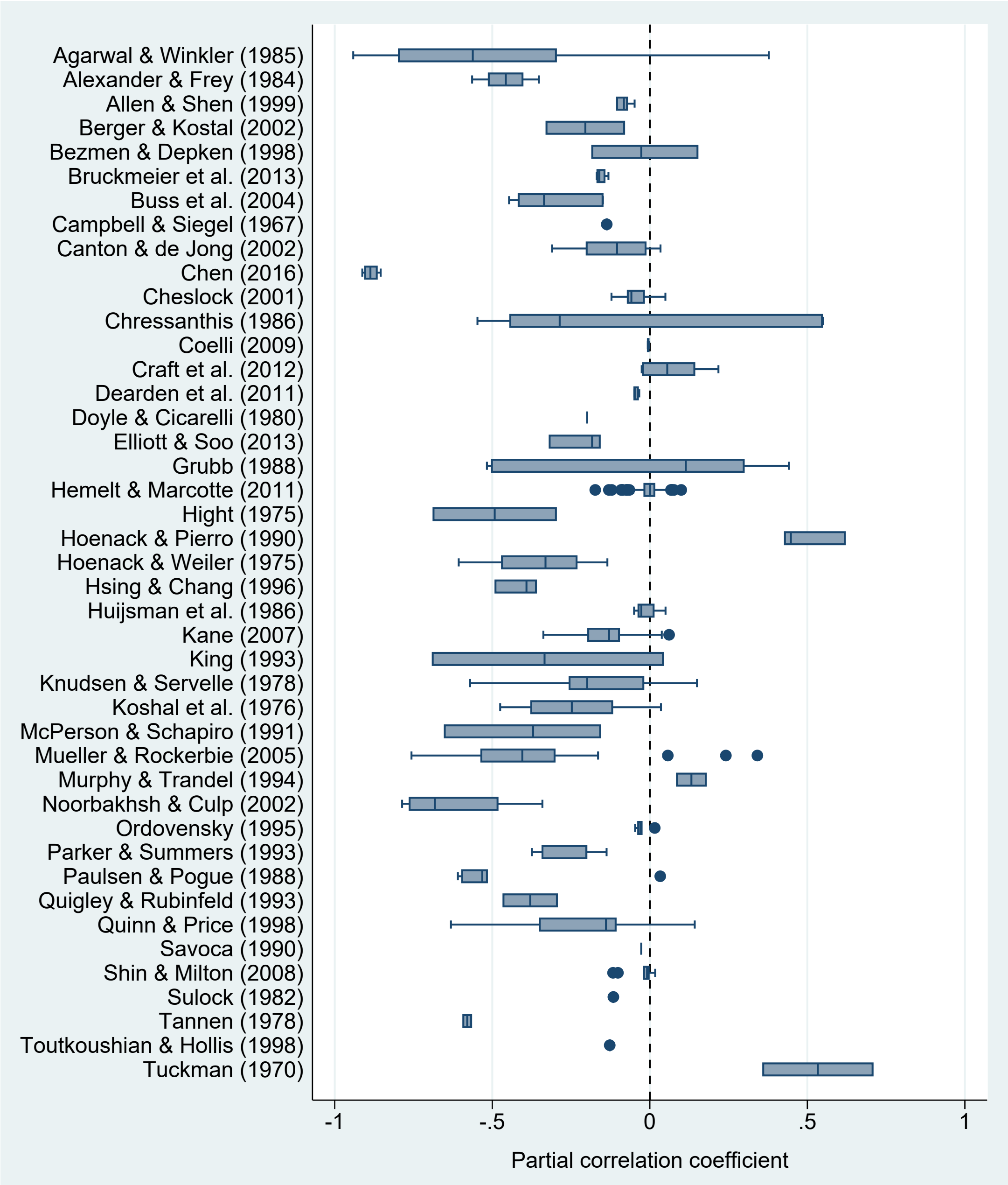
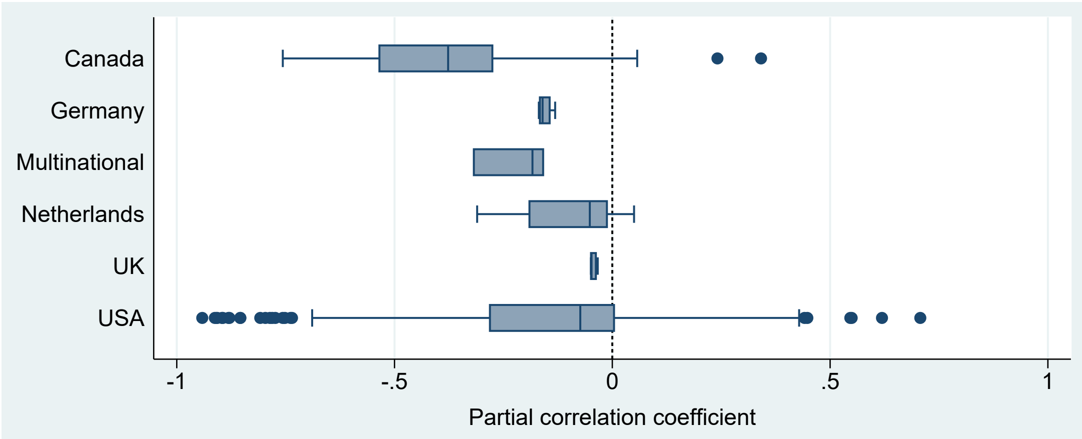
A Supplementary Statistics and Robustness Checks

Figure 5: Estimates of the tuition-enrollment nexus vary within and across studies



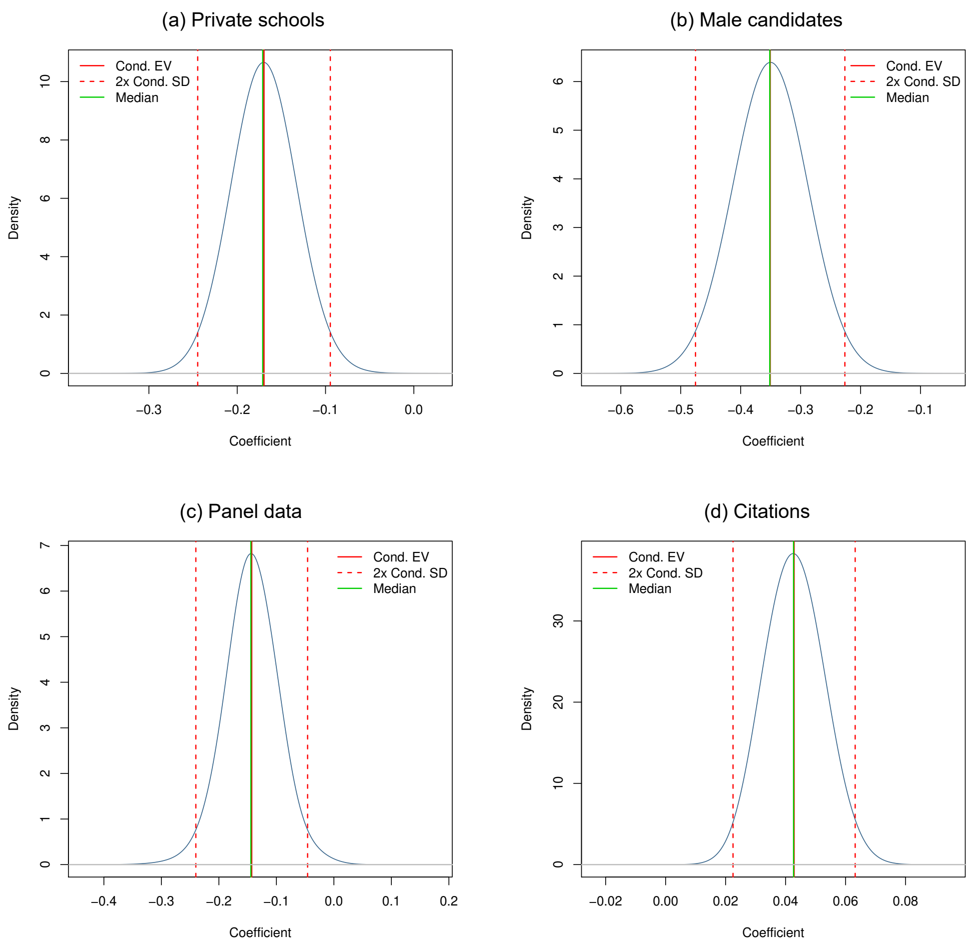
Notes: The figure shows a box plot of the partial correlation coefficients capturing the relationship between tuition and the demand for higher education reported in individual studies.

Figure 6: Estimates of the elasticity vary across different countries



Notes: The figure shows a box plot of the partial correlation coefficients capturing the relationship between tuition and the demand for higher education reported for individual countries.

Figure 7: Posterior coefficient distributions for the most important characteristics



Notes: The figure depicts the densities of the regression parameters from Table 5 with the highest posterior inclusion probabilities.

Table 7: Explaining heterogeneity in the estimates (robustness checks for Table 5)

Response variable:	A: Observations-weighted BMA			B: Precision-weighted BMA		
Tuition elasticity	Post. mean	Post. SD	PIP	Post. mean	Post. SD	PIP
Constant	0.002	NA	1.000	-0.017	NA	1.000
Standard error	-0.614	0.444	0.728	-0.277	3.915	0.513
Estimation characteristics						
Short-run effect	0.058	0.054	0.617	0.023	0.031	0.417
OLS	-0.096	0.067	0.742	-0.001	0.013	0.075
Control for endogeneity	0.054	0.073	0.417	0.012	0.024	0.262
Design of the demand function						
Linear function	-0.001	0.015	0.076	-0.105	0.038	1.000
Double-log function	-0.004	0.016	0.101	-0.054	0.054	0.639
Unemployment control	0.010	0.029	0.156	0.074	0.022	0.977
Income control	0.079	0.069	0.657	0.019	0.033	0.335
Data specifications						
Cross-sectional data	-0.003	0.028	0.116	0.198	0.029	1.000
Panel data	-0.109	0.077	0.746	0.009	0.027	0.157
Male candidates	-0.349	0.063	1.000	-0.213	0.055	0.996
Female candidates	-0.010	0.049	0.091	-0.030	0.066	0.219
Private schools	-0.169	0.039	0.996	0.173	0.032	1.000
Public schools	0.001	0.012	0.065	0.167	0.028	1.000
USA	-0.091	0.088	0.593	-0.183	0.031	1.000
Publication characteristics						
Publication year	-0.015	0.018	0.472	0.019	0.514	0.512
Citations	0.043	0.010	0.999	0.029	0.007	1.000
Published study	-0.015	0.038	0.190	-0.128	0.042	0.984
Studies	43			43		
Observations	442			442		

Notes: SD = standard deviation. PIP = posterior inclusion probability. Panel A represents BMA applied to data weighted by the number of observations per study using parameters and uses model priors according to Fernandez *et al.* (2001) and Ley & Steel (2009). The respective visualization is represented by Figure 9, and the respective diagnostics of the BMA can be found in Table 9. Panel B represents BMA applied to data weighted by the inverse of the standard error and uses model priors according to Eicher *et al.* (2011). The respective visualization is represented by Figure 10, and the respective diagnostics of the BMA can be found in Table 10. Different weighting schemes for meta-analysis are discussed in greater detail by Zigraiova & Havranek (2016, p. 28-30). All variables are described in Table 4.

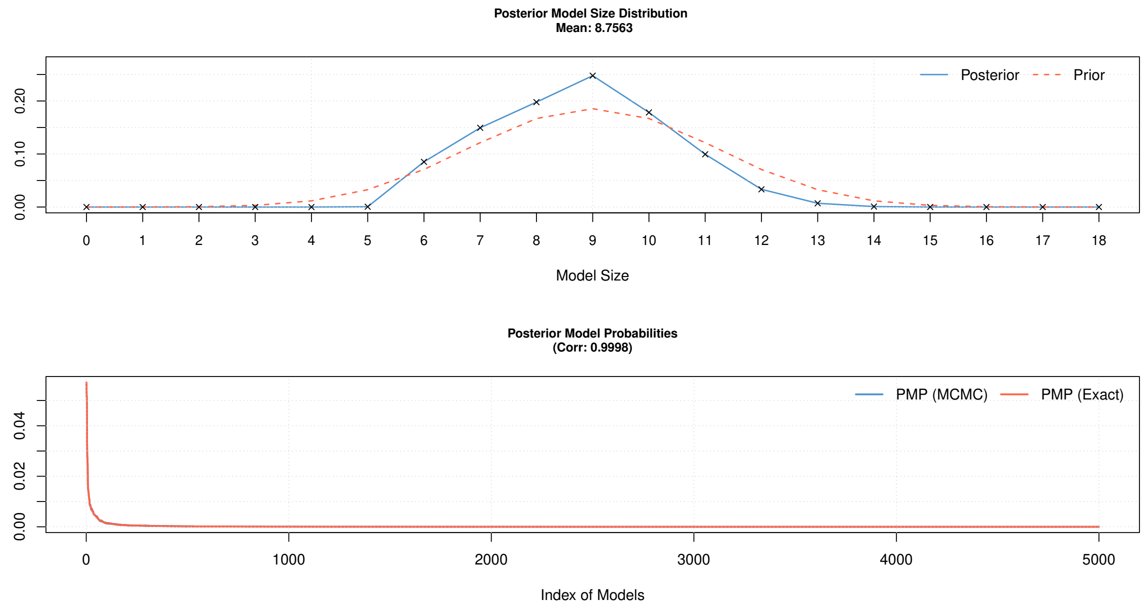
B Diagnostics of BMA

Table 8: Summary of main BMA estimation

Mean no. regressors	*Draws*	*Burn-ins*	*Time*	*No. models visited*
8.7563	$2 \cdot 10^6$	$1 \cdot 10^5$	5.495486 mins	578,591
Modelspace	*Visited*	*Topmodels*	*Corr PMP*	*No. obs.*
262,144	22.10%	100%	0.9988	442
Model prior	*g-prior*	*Shrinkage-stats*		
Uniform	UIP	Av = 0.9977		

Notes: We employ the priors suggested by Eicher *et al.* (2011), who recommend using 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 the data). Results of this BMA exercise are reported in Table 5.

Figure 8: Model size and convergence of main BMA estimation



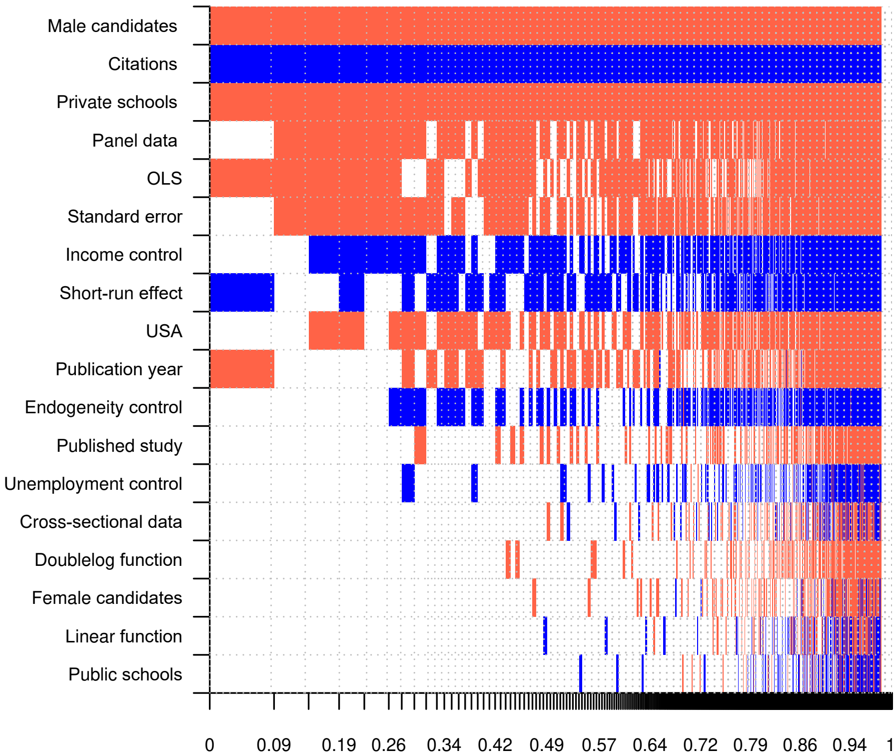
Notes: The figure depicts the posterior model size distribution and the posterior model probabilities of the BMA exercise reported in Table 5.

Table 9: Summary of BMA estimation—robustness check A

Mean no. regressors	*Draws*	*Burn-ins*	*Time*	*No. models visited*
8.7698	$2 \cdot 10^6$	$1 \cdot 10^5$	5.869949 mins	572,046
Modelspace	*Visited*	*Topmodels*	*Corr PMP*	*No. obs.*
262,144	21.80%	100%	0.9999	442
Model prior	*g-prior*	*Shrinkage-stats*		
Random	BRIC	Av = 0.9977		

Notes: We employ the "random" model prior, which refers to the beta-binomial prior advocated by Ley & Steel (2009); Zellner's g prior is set according to Fernandez *et al.* (2001). The results of this BMA exercise are reported in Table 7 (specification A).

Figure 9: Model inclusion in BMA—robustness check A



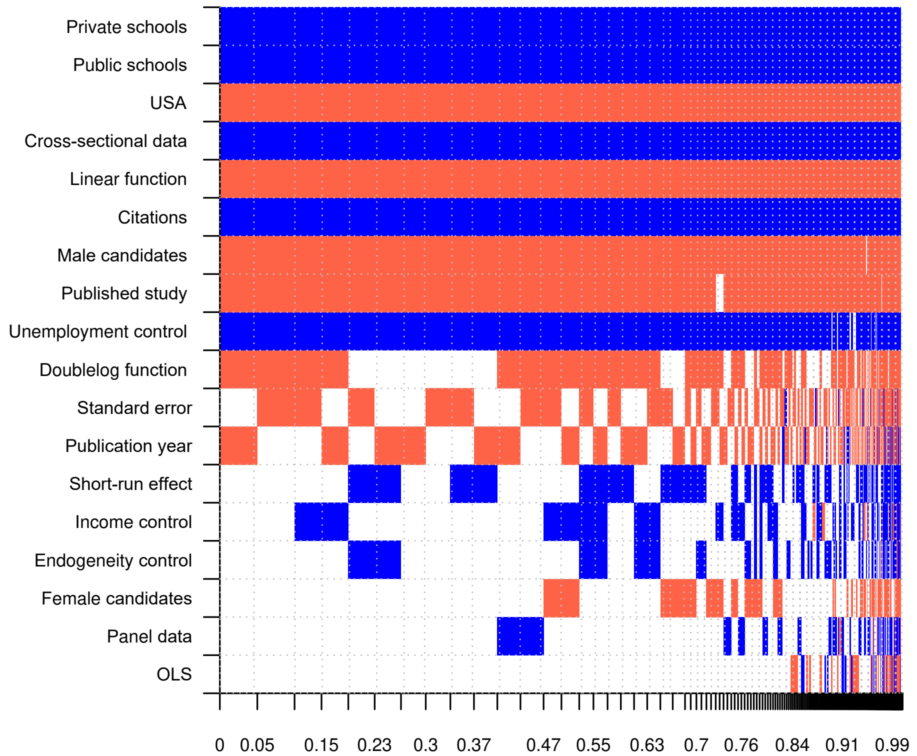
Notes: The figure depicts the results of the BMA related to specification A reported in Table 7.

Table 10: Summary of BMA estimation—robustness check B

Mean no. regressors	*Draws*	*Burn-ins*	*Time*	*No. models visited*
12.0906	$2 \cdot 10^6$	$1 \cdot 10^5$	6.009023 mins	465,235
Modelspace	*Visited*	*Topmodels*	*Corr PMP*	*No. obs.*
262,144	17.70%	100%	0.9995	442
Model prior	*g-prior*	*Shrinkage-stats*		
Uniform	UIP	Av = 0.9977		

Notes: We employ the priors suggested by Eicher *et al.* (2011), who recommend using 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 the data). The results of this BMA exercise are reported in Table 7 (specification B).

Figure 10: Model inclusion in BMA—robustness check B



Notes: The figure depicts the results of the BMA related to specification B reported in Table 7.