

**DOCUMENTS DE TREBALL  
DE LA FACULTAT DE CIÈNCIES  
ECONÒMIQUES I EMPRESARIALS**

*Col·lecció d'Economia*

*E08/197*

**Re-visiting the Health Care Luxury Good Hypothesis:  
Aggregation, Precision, and Publication Biases?**

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**Abstract:** While a growing literature examining the relationship between income and health expenditures suggests that health care is a luxury good, this conclusion is contentiously debated due to heterogeneity of the existing results. This paper tests the luxury good hypothesis using meta-regression analysis, taking into consideration publication selection, precision, and aggregation bias. The findings suggest that publication bias exists, a result that is robust irrespectively of the tests employed. Precision and aggregation bias also appear to play a role in the generation of estimates. The corrected income elasticity estimates range from 0.26 to 0.84, although we cannot reject the luxury good hypothesis for some of the performed corrections.

**JEL Classification:** I1, I10, I11, I18

**Keywords:** meta-regression analysis, health care, luxury good, income elasticity, aggregate health expenditure, regional health expenditure

**Resum:** Mentre que una creixent literatura que ha examinat la relació entre la renda i la despesa sanitària suggereix que els serveis sanitaris són un be de luxe (elasticitat renda superior a la unitat), aquesta conclusió es contínuament debatuda atesa l'heterogeneïtat dels resultats. Aquest article testa la hipòtesis dels serveis sanitaris com bens de luxe fent server anàlisi de meta- regressió, particularment analitzant l'existència de biaixos de selecció de publicació, precisió així com biaixos d'agregació. Els resultats apunten l'existència d'un biaix de publicació, robust independentment dels controls analitzats. Els biaixos de precisió i agregació semblen tenir un paper en la generació de les estimacions de l'elasticitat renda. Els nostres resultat suggereixen que l'elasticitat renda dels serveis sanitaris un cop corregir pels biaixos esmentat varien entre 0.26 i 0.84, però no podem rebutjar que la elasticitat renda es igual a la unitat en algunes estimacions de l'elasticitat corregides.

**Paraules clau:** meta- regressió , serveis sanitaris , be de luxe, elasticitat renda, despesa sanitària agregada i despesa sanitària regional.

## 1. Introduction

Beginning with the seminal paper by Newhouse (1977), a contentious debate has raged over the income elasticity of demand<sup>1</sup>, the central question being whether health expenditures increase faster than per capita income. The general finding has been that income elasticity estimates exceed unity, implying that health care is a luxury good (Newhouse, 1987, Gerdtham and Johnson, 2000). Given the marked implications for the allocation of health care resources, the debate has often centered on the methodological robustness of elasticity estimates. The argument reads that if health care is a “necessity”, this necessitates more redistribution of health care resources and arguably greater public involvement in health care. That is, the value of income elasticity provides insight into the optimal level of health expenditures in the economy and the efficient proportion of public and private health spending<sup>2</sup>.

Some researchers suggest caution in interpreting the early results that health care is a luxury good as misspecification may be a possibility (Culyer, 1987). As a result, the methodological debate has focused on the existence of specific controls, such as health system controls (Gerdtham and Johnson, 2000), and the methods used, primarily the statistical properties of the data. Another source of variation is the heterogeneity of health care (Parkin, 1987; Gertham, 1992, Roberts, 2000), which depends on whether the data is measured at the national, regional, or individual level (Getzen, 2000; DiMatteo, 2003). The interdependence of several forms of health care implies that an

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<sup>1</sup> We define the income elasticity of demand as the percentage change in health expenditures that is associated with a one percent change in income. The formula is given by:  $e_I = (\partial HE / \partial I)(HE/I)$  where  $HE$  represents health expenditures and  $I$  represents income. If the income elasticity is less than one, then the health care expenditures are a necessity good. If the income elasticity is greater than one, then health care expenditures are a luxury good.

<sup>2</sup> If the income elasticity exceeds unity, some might argue that universal health coverage is unnecessary as the private market is more efficient in the provision of coverage. Alternatively, it may be that income inequalities are prevalent, although this interpretation rests on the assumptions that most health care consumption is necessary and that there is significant unmet demand among lower-income populations.

aggregate analysis is more intuitive, but there may be biases in employing aggregate estimates to infer individual behavior. Most research that utilizes aggregate data relies on country-level aggregation, mainly due to data availability, but interestingly, studies employing regional data do not necessarily find income elasticities below one. This evidence might imply that aggregation bias exists, which is consistent with findings from studies that relied on aggregate data to examine crime (Glaeser et al, 2003; Glaeser and Sacerdote, 2007). However, the increasing availability of data and statistical methods implies the need for a paper that aggregates the existing studies on the basis of such effects. To date no study has investigated empirical biases in the income elasticity literature.

Among the potential biases, two are primarily important: publication selection (Stanley, 2008) and precision effects (Stanley, 2005). The existence of publication selection points toward a preference – typically among journal referees, editors, editorial boards, the disciplinary scientific community as a whole, and even authors themselves - for statistically significant results that confirm the prevailing theoretical paradigm. In terms of precision effects, there may be heterogeneity in the estimates given that all authors inevitably select different samples and employ different controls. Due to the inevitable heterogeneity of methods, samples, and classifications across different studies, meta-analysis or meta-regression analysis (MRA) are two available techniques for scrutinizing the existing studies and determining whether health care or its components are luxury goods. Meta analysis integrates the existing estimates of a defined outcome variable (Farly, 1982) and assumes that the individual studies can be homogenized through a standard measure of empirical effect or effect size (Glass *et al* 1981), which is held constant across all studies. The effect size is the difference in the outcome [e.g. elasticity] of the study and control groups, adjusted by the standard deviation of the control group.

Given the subjectivity of meta analysis, meta-regression analysis is an alternative tool that harmonizes the heterogeneity of studies to obtain a flexible estimate that can be adapted to context-specific circumstances (Stanley and Jarrell, 2005). MRA entails a regression analysis of existing studies with controls for the study type, the sample characteristics, and the scope and precision of the elasticity estimate, allowing us to test the sensitivity of the parameter of interest to certain objective characteristics. There have been some applications of MRA in the economics literature (Roberts, 2005), but there are only a few known applications to health care (Asensio-Boadi et al., 2007; Gemmill et al, 2007; Doucouliagos and Stanley, 2008).

To address the debate regarding whether health care is a luxury or necessity good, this paper pools the existing aggregate income elasticity estimates from social science journals. We then apply MRA to obtain a corrected income elasticity estimate, accounting for the precision, publication selection, and aggregation nature of the included papers. The analysis is restricted to total health expenditures given that studies which consider specific expenditure types (e.g., pharmaceutical or inpatient) or employ individual-level data might not produce comparable estimates. Once we control for the relevant study-specific factors, it becomes clear that income elasticity estimates suffer from publication bias. After removing the publication bias, we can no longer conclude that health care is luxury good.

The paper is organised as follows: Section 2 provides an overview of the existing studies, distinguishing between those that employed national-level data and those that used regional-level data. Section 3 describes the methods employed in the analysis and

offers more detail on the use of meta-regression analysis. Section 4 details the results, and Section 5 concludes with a discussion.

## **2. Brief overview of the literature**

This section of the paper covers the literature that has used national- and regional-level aggregate data to consider the relationship between income and overall health expenditure. Because studies that have used individual-level data or have distinguished between health expenditure categories may not be comparable, these types of papers were not included in the literature review or the analysis.

### **2.1. Studies using country-level data**

Two literature reviews that focused on country-level analyses of the relationship between income and health expenditures (Getzen, 2000; Gerdtham and Johnson, 2000) found that most papers reported income elasticity coefficients greater than one. Getzen (2000) argued that while evidence indicates that health care is necessity good at the individual level, it is a luxury good at the aggregate level, although Hansen and King (1996) suggest that this relationship could be spurious.

In dealing with international health care expenditure functions, the availability of data has fostered a significant amount of empirical work. However, health care systems are heterogeneously managed, regulated, and financed, and accordingly, there are sizeable differences in the health care packages among OECD countries. As a result, it is doubtful that data from different countries is measuring the same outcome. Another issue is that there might be a ‘stability problem’ when examining data over a large period of time (Jewell et al, 2003; Clemente *et al*, 2004).

As these methodological issues have led many to question the validity of the elasticity results (Clemente *et al*, 2004; McCoskey and Selden, 1998; Hansen and King 1996; Blomqvist and Carter, 1997; Karatzas, 2000; Roberts, 2000), some researchers have addressed specific methodological issues underlying the determination of the health care expenditure function. In particular, these studies account for the potential non-stationary of the data, although there is no agreement on whether the data is co-integrated (Gerdtham and Lothgren, 2000; Clemente *et al*, 2004, Herwaetz and Theilen, 2003). Others have used panel data methods to account for potential differences in tastes and preferences in the health care expenditure function (Hitris and Possnett, 1992; Di Matteo and Di Matteo, 1998), but none of these analyses have considered spatial interactions, the existence of which might invalidate some of the existing conclusions. Some of the literature has focused on causality problems that occur when examining health expenditure and GDP, and this has been examined in the Spanish health care system (Devlin and Hansen, 2001). Okunade and Suraratdecha (2000) use a dynamic Engel specification of a Box–Cox expenditure model to account for the existence of inertia, especially in publicly financed health systems. It is important to note that they find that per capita real GDP elasticity's show the tendency for medical care to behave like a necessity in 20 of the 21 OECD countries.

In a further attempt to overcome some of the institutional heterogeneity issues, some studies have controlled for health system characteristics. Gerdtham et al. (1998) is one of the few studies that examines the influence of a set of institutional reforms. The authors find that systems with physicians as gatekeepers are consistently statistically significant and associated with lower health expenditures. Gerdtham et al. (1998),

Hansen et al. (1996), and Roberts (1998) control for the percentage of public to total health expenditures, but there are mixed results for this control.

## **2.2 Studies using regional-level data**

Importantly, the controls for institutional context may be insufficient to overcome institutional heterogeneity (Di Matteo and Di Matteo, 1998), and thus some studies have used sub-national data to overcome this bias. There have been regional studies conducted in five countries (Canada, Italy, Spain, Switzerland, and the United States), and all of these studies find an income elasticity below one (Cantanero, 2005; Costa-Font and Pons, 2006; Crivelli et al, 2007; Di Matteo, 2003; Gionannoni and Hittris, 2002; Vater and Rüefli, 2003). As data at the regional level has only become available relatively recently, most of the studies examining health expenditures at the regional level are from the last ten years.

## **2.3 Aggregation effects**

The bulk of evidence supporting the luxury good theory has been drawn from aggregate datasets, and there may be difficulties in drawing inferences about individual behaviour from aggregate data (Glaeser et al, 2002). Most studies using regional-level data have found elasticity values below one, while studies using national-level data find elasticity values above one. The difference in results could be due to the aggregation effect (Glaeser et al, 2002). In particular, the association between a country's income level and health care expenditures can be affected by strategic complementarities; such and preference or information spillovers due to information asymmetries. Furthermore, individual-level income does not adequately capture the effect of technology, while at



the national level, income includes the technology effect. In practice, measuring the technology effect is difficult because there is no accepted measure of technology change. Another reason for casting doubts on behavioural inferences resulting from aggregate data is that individual-level budget constraints differ from those at the regional or national level, particularly in the presence of universal or extended insurance coverage. The implication of this discussion is that aggregation effects may exist with national-level data, and it is important to test for this possibility.

### **3. Data and Methods**

#### **3.1. Methodology**

The intent of this paper is to determine the corrected magnitude of the income elasticity estimate derived from meta-regression analysis and to examine the extent to which the predicted elasticity differs from one ( $\beta_i \neq 1$ ). Specifically, the goal is to establish whether the elasticity is greater than one (a luxury good) or less than one (a necessity good) after controlling for study-specific characteristics.

Meta-regression analysis involves collecting the outcome variable and relevant study-specific information from the existing literature in a systematic manner to determine which factors influence the variability of the treatment variable (Stanley and Jarrell, 1989). These factors are then recorded as covariates, creating the meta-regression dataset. The assumption is that each observation is drawn from an overall statistical population. Based on this compiled dataset, we can test our main hypothesis that the income elasticity of demand is greater than one and determine the important factors that influence this treatment variable.

This technique has the distinct advantage of being less subjective than literature reviews where the researcher is interested in the average effect of a particular outcome variable. A literature review is subjective in that the researcher determines the inclusion criteria for the literature, the method of interpreting the results, and the potential reasons for varying results. Systematic literature reviews offer a methodological improvement and provide techniques for reducing the subjectivity, but researchers still have considerable leeway when deciphering the results and crediting various factors to variation in the outcome variable. As a result, the ultimate aim of MRA is to overcome some of the pitfalls of literature reviews, allowing us to obtain an “estimate of estimates” with some acceptable precision.

The analysis begins with the collation of information from relevant studies, where we have  $N$  estimates of  $\eta_i$  (the dependent variable) and  $i=1, \dots, N$ . We identify the  $k$  characteristics of the diverse studies and integrate the findings as follows:

$$\eta_i = \beta + \beta_0 S_{\eta} + \sum_{k=1}^K \beta_k X_{ik} + \varepsilon_i \quad (1).$$

The reported income elasticity estimate of each  $i$  study ( $\eta_i$ ) equals the real income elasticity estimate ( $\beta$ ) adjusted for the standard error of  $\eta_i$  ( $S_{\eta}$ ) and the  $k$  characteristics ( $X_{ik}$ ) of each published study. The  $X_{ik}$  are the independent variables and account for the processes which explain the production of empirical results, while the parameters ( $\beta_0, \beta_k$ ) represents the biases associated with specific characteristics that lead to misspecifications (Stanley and Jarrell, 2005). The covariates might be variables

measuring the quality of the study (e.g., the impact factor of the journal), numerical continuous variables accounting for the study size, and any other relevant characteristics of the study (e.g., outlier estimates). Given that estimates are obtained by varying degrees of precision, it is possible to control for publication bias by including the standard error of the estimate in the regression. The standard error might be indicative of precision or publication bias, perhaps because journals are more likely to publish significant estimates (i.e. standard errors might be biased downwards).

Given that the model is based on estimates from previous regressions, it is important to examine the distributional properties of the data. In the absence of publication selection, estimates will vary randomly, hence *symmetrically*, around the “true” effect (Stanley, 2008).

### **3.2 The Funnel Asymmetry (FAT) and the Precision Effects Test (PET)**

Because the model is based on estimates from previous research, it is important to examine the distributional properties of the data. While MRA coefficients should be unbiased and consistent (Stanley and Jarrell, 1989), the fact that the revised studies are drawn from different datasets leading to heterogeneity, have differing sample sizes, and utilize different controls and methods generally leads to heteroskedasticity in the error term. As a result, we first need to test for the existence of heteroskedasticity, and if the bias is present, correct for this effect using a weighted least squares (WLS) regression. This estimator divides equation (1) by the standard error of  $\eta_i$  ( $S_\eta$ ). The dependent variable in the WLS model becomes the t-statistic as follows:

$$t_i = \frac{\eta_i}{S_\eta} = \beta_o + \beta_1 \frac{1}{S_\eta} + \sum_{k=1}^K \frac{\beta_k X_{ik}}{S_\eta} + \mu_i \quad (2),$$

where  $t_i$  is the t-value associated with the  $i^{th}$  reported estimate in the MRA dataset. The  $X_{ik}$  are the study specific controls including the impact factor of the journal (quality), institutional features of the data (health system type), type of data (public or private), and the level of aggregation (regional or national). This regression facilitates the funnel asymmetry test (FAT), which is related to funnel plots and tells us about the existence of funnel asymmetry (hence the name (Egger *et al.* 1997, Sutton *et al.* 2000 and Stanley 2005)). FAT is an initial method of identifying potential biases in the elasticity estimates, and it is common practice in performing MRA. The null hypothesis is:  $\beta_0 = 0$ , the rejection of which is taken to indicate the presence of publication selection.

Equation (2) also facilitates the Precision Effect Test, which allows us to identify an empirical effect even if publication bias exists. The null hypothesis of PET normally would read as follows:  $\beta_1 = 0$  (in our case, the null hypothesis is  $\beta_1 = 1$  as we are testing health is a luxury good). It is important to note that FAT is considered to have low value and is sensitive to the introduction of controls (Stanley, 2007). On the other hand, PET suffers from inflated Type I errors if the existing heterogeneity is larger than the sampling error (Stanley, 2005). Therefore, it is important to perform further confirmatory tests.

### **3.3 The Precision Effect Estimate with Standard Error**

As an extension of model (2), a Heckman-like correction, the Precision Effect Estimate with Standard Error (PEESE) model, can be used to obtain an estimate that is robust to

standard error. For a complete derivation of this model, please see Stanley and Doucouliagos (2007). The PEESE equation starts from the premise that there is a nonlinear relationship between the observed outcome and its standard error, yielding the equation:

$$\eta_i = \alpha + \alpha_0 S_\eta^2 + \sum_{k=1}^K \alpha_k X_{it} + \varepsilon_i \quad (4),$$

Assuming heteroskedasticity in the error term, we again apply the WLS correction to yield:

$$t_i = \alpha_0 S_\eta + \frac{\alpha}{S_\eta} + \frac{\sum_{k=1}^K \alpha_k X_{it}}{S_\eta} + \delta_i \quad (5),$$

so that  $\alpha$  estimates the magnitude of the empirical effect corrected for publication selection. The advantage of using the t-value rather than the elasticity as the dependent variable is that t-values have a specific unit and dimensionality, providing a standardized measure of interest. As with any other empirical specification, as long as the model is not misspecified, it measures the specific meta-effects. One method of gauging the sensitivity of the model to misspecifications is to vary the independent variables and measure the effects.

### 3.4 Meta-significance testing

A further constancy test is meta significance testing (MST), which relies on the assumption that if there is a genuine underlying effect, there will also be a logarithmic

relationship between a study's  $t$ -statistic and its degrees of freedom. Statistical theory predicts that the  $t$ -ratio will be related to the square root of the degrees of freedom, or :

$$E(\log|t_i|) = \gamma_0 + \gamma_1 \log(df_i) \quad (6),$$

where  $\gamma_1 = 0$  would confirm the null hypothesis of no effect and the remainder would suggest evidence of selection bias (Stanley, 2005). A meta significance test is developed by simply regressing the logarithm of elasticity estimates against the degrees of freedom, and as Stanley (2005, 2007) demonstrates, the slope should be precisely 0.5. Another possible test is one developed by Mookerjee (2006) that involves a regression between the  $t$ -value of the outcome variable and the square root of the degrees of freedom. The significance of such a relationship reflects the existence of publication bias.

### 3.5 Homogeneity

Homogeneity, i.e. whether there is a common mean, is another aspect of the dataset that needs to be considered. We can test for homogeneity using the

$Q = \sum (\eta_i - \bar{\eta}_{\text{var}(\eta)})^2 / \text{var}(\eta_i)$  statistic, where  $\eta_i$  is each elasticity estimate,  $\bar{\eta}_{\text{var}(\eta)}$  is a weighted average of each elasticity estimate corrected by its variance, and  $\text{var}(\eta_i)$  is the variance of each estimate. Under the null hypothesis of homogeneity,  $Q$  is distributed as  $\chi^2_{N-1}$  where  $N$  is the number of studies. If the null hypothesis of homogeneity is rejected, this suggests that regression analysis is needed.

### **3.6. Data selection**

The likely predictors of the outcome variable are a number of theoretical controls and study-specific covariates. All of these potential independent variables can be identified from the specific papers collected for the analysis and are classified as follows:

- (a) measurement and methods or study-specific characteristics (e.g. the number of observations),
- (b) institutional setting (e.g. the type of insurance coverage, the type of health system)
- (c) publication or dissemination effects (e.g. whether published in a social science journal, quality or impact factor of the journal), and
- (d) method or data specific controls (e.g. the presence of outliers).

Each of these predictors is intended to capture specific biases that influence the outcome variable. One of the most important considerations in the regression may be the coefficient on the standard error variable as a positive coefficient may be indicative of a publication or dissemination effect. That is, some social science journals might be more interested in publishing studies with income elasticity estimates greater than one as this confirms the luxury good hypothesis. A further example is that estimates can vary significantly across study characteristics, such as the number of observations and the journal where the estimate was published. The institutional setting, such as whether the estimate was generated from a tax-based or social insurance-based country, may also be a key factor in determining the income elasticity as it reflects the distribution of income across the population and possible cultural factors. Finally, the presence of outliers related to specific studies that are of varied quality is another important effect.

The search for available evidence involved prescreening, selecting, and then classifying the income elasticity values and the associated study characteristics to create the MRA database. In developing the database, we identified and cross-referenced published studies using Econlit, Medline, and Sociofile up until 2006. An important point is that we restricted our sample to income elasticity estimates derived from aggregate datasets and published in social science journals. The intuition regarding aggregate estimates was explained previously, and the use of estimates published in social science journals has two primary reasons. One argument is that social science journals are more likely to provide elasticity estimates and the associated standard errors. Additionally, an interesting sub-question to consider in the analysis is whether publication bias exists in this particular area of the literature.

In most cases both the income elasticity and the associated standard error were available in the paper. In a few cases, the standard error was not provided, but where possible we calculated this either from the given t-value or from the mean square error (MSE). In the case where the study reported the t-value associated with  $\eta_i$  rather than the standard error, we used the formula for the t-value:

$$t - value = \frac{\eta_i - 0}{s.e.(\eta_i)}, \quad (7).$$

Thus, we substituted in the known values of  $\eta_i$  and the t-value to solve for  $s.e.(\eta_i)$ .

Alternatively, if the authors only reported the mean square error, we took the square root of the MSE to obtain  $s.e.(\eta_i)$ .



#### 4. Results

After the data collection process, our final sample consisted of 167 comparable elasticity estimates from a set of 48 published studies. Before proceeding to the regression, we first considered the possibility of homogeneity in the sample by calculating the  $Q$ -statistic. At a value of 30,641 ( $p=0.000$ ), this was high enough to indicate that significant heterogeneity existed in the sample. The implication was that regression analysis was needed.

The next step was to visually examine the data to get a feel for any publication bias. Figure 1 is a funnel plot, which plots the elasticity estimates against a measure of precision ( $1/s.e$ ). With the exception of a few outlier estimates, most of the income elasticity coefficients range from 0 to 2. The funnel plot also suggests that the value of one is likely at the centre of the distribution, and with the exception of few outliers, the distribution appears to be symmetrical around that value. Interestingly, if we look at the descriptive statistics for the income elasticity value (Table 1), there is significant variability as indicated by the values at the 10<sup>th</sup> and 90<sup>th</sup> percentiles.

**[Insert Figure 1 and Table 1 about here]**

Following the methodology put forward in Section 3, we then ran a set of separate regressions. We first examined the FAT and PET tests using equation (2). Table 2 shows that the constant ( $\beta_0$ ) is significant and positive, meaning that we can reject the null hypothesis of no selection bias (according to FAT). In line with the interpretation of the funnel plot, the direction of the bias is initially positive. However, when more

controls are introduced, part of the selection or publication bias appears to be picked up by the controls, and the test becomes insignificant.

**[Insert Table 2 about here]**

Next is the coefficient on  $\beta_{1/s.e}$ , which is an unbiased estimate of the income elasticity of demand after correcting for selection bias (according to PET). The coefficient is positive statistically significant, with values ranging 0.26 to 0.71. Wald tests only rejected the hypothesis that  $\beta_{1/s.e}$  equals one for the first simple specification at the conventional 5% significance level, while for the rest of the specifications, we can only reject the hypothesis at a 10% level.

In addition to the corrected elasticity estimate, the interpretation the controls are also important. Two controls, the use of regional data and the impact factor of the journal, were consistently significant. It appears that studies using regional data yield lower income elasticity values, with coefficients being negative and ranging from 0.664 to 0.51. This is consistent with the aggregation bias hypothesis, which remains irrespective of the introduction of additional controls. As for the second effect, there is a positive relationship between the impact factor and the income elasticity, suggesting that high impact factor journals have a preference for significant and higher elasticity estimates.

Table 3 provides the estimates of the PEESE model (from equation (5)) where  $\hat{\alpha}_{1/s.e}$  is the effect corrected for publication selection following Stanley and Doucouliagos (2007). The precision-corrected elasticity estimate lies between 0.38 and 0.84, depending on the specific study controls introduced. These results are in line with

previous results using the PET, overall indicating that health is not a luxury good. The coefficients on the study controls not surprisingly appear to previous similar to the coefficients for the same controls reported in Table 2 .

**[Insert Table 3 about here]**

Finally, Table 4 provides the results of the meta-regression test (MRT), which tests the existence of a logarithmic relationship between the degrees of freedom and the t-value. Consistent with the previous results, we find a significant and robust effect that confirms the existence of selection bias. Post-estimation tests reject the null hypothesis of the coefficient being 0.5 ( $F(1, 39)=7.73$ ). We also ran the regression proposed by Mookerjee (2006) between the t-value and the square root of the degrees of freedom. The significance of the coefficient on the degrees of freedom variable can be interpreted as additional evidence confirming the intuition of publication bias.

**[Insert Table 4 about here]**

## **5. Conclusion**

This paper has examined the existence of publication bias along with aggregation and precision effects to revisit the hypothesis of health care being a luxury good. Drawing from a battery of existing methodologies (FAT, PET, PEESE and MRT), our results suggest the publication bias does exist. Interestingly, we find that the income elasticity of demand for health care lies between 0.26 and 0.8, which negates the hypothesis that health care is a luxury good. This result is consistent with the proposal that health care is an individual necessity and an aggregate luxury (Getzen, 2000).

We also find that two study controls are consistently important predictors of the elasticity value. Studies using regional data yielded lower elasticity values, providing evidence for the existence of aggregation effects. Journal quality is also an important predictor, and it seems that journals with a better impact factor, namely more established journals, exhibit a systematic tendency to report higher elasticity effects. Other controls such as institutional and methodological factors did not appear to influence the elasticity estimates.

It is important to bear in mind the potential limitations of this analysis. Over time, more studies appear to be showing that after introducing appropriate controls, income elasticity estimates decline markedly (Sen, 2006). Hence, future analyses that include more estimates might find less effect of publication bias or even publication bias in the opposite direction. In addition, there may be other important characteristics, such as indicators of health expenditure types (e.g. pharmaceutical, inpatient), that explain heterogeneity in elasticity estimates. However, given that expenditures are not independent and instead reflect an underlying demand for health channelled through agency relationships, an overall income elasticity measure would be more reliable than estimates for specific types of health expenditure. Future research could account for other sources of heterogeneity using individual-level data.

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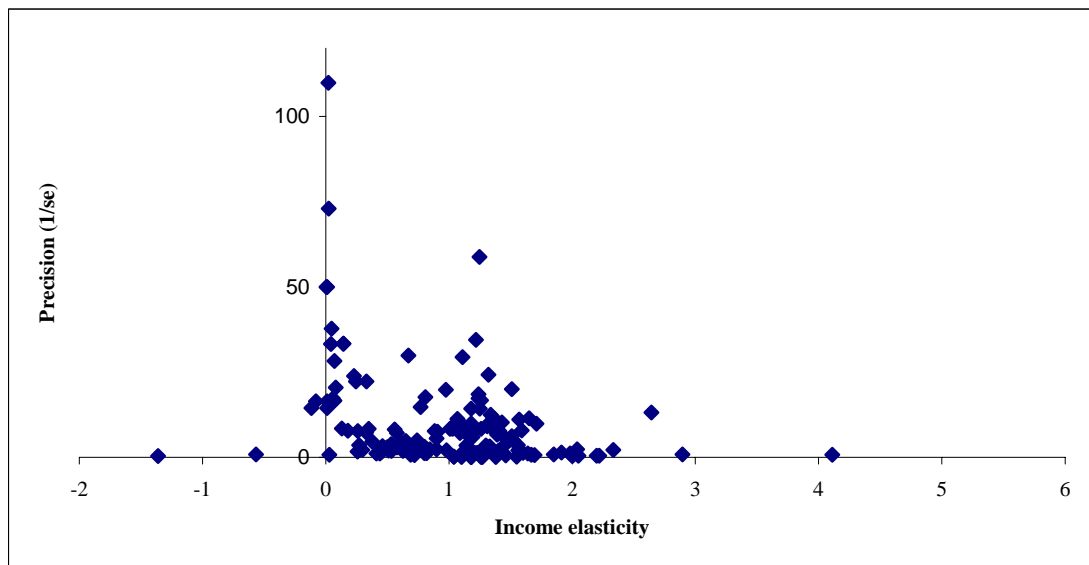
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**Figure 1. Funnel Plot**



**Table 1. Definitions of the variables and summary statistics (N=167)**

<b>Variable</b>	<b>Definition</b>	<b>Mean<sup>a</sup></b>	<b>Median</b>	<b>10<sup>th</sup> percentile</b>	<b>90<sup>th</sup> percentile</b>
inc_elasticity	Income elasticity of demand	0.999 (0.073)	0.908	0.0793	1.654
std_error	Standard error of the elasticity	1.215 (0.397)	0.290	0.0450	1.471
region	Indicates whether the data was regional (vs. national)	0.246 (0.033)	0.000	0.0000	1.000
df	Degrees of Freedom of each database	421.20 (36.11)	24.2	17.1	671.3
nhs	Dummy for the percentage of NHS observations in the study	0.532 (0.031)	0.500	0.0000	1.000
public	Dummy for public health expenditure	0.090 (0.022)	0.000	0.0000	0.000
impact	The impact factor of the medium where the paper was published	0.907 (0.075)	0.300	0.0000	2.500
panel	Indicates whether the study used panel data techniques	0.174 (0.029)	0.000	0.0000	1.000

<sup>a</sup>standard errors in parentheses

**Table 2. Funnel Asymmetry Test (FAT) and Precision Effect Test (PET)**

	coefficient (s.e.)	coefficient (s.e.)	coefficient (s.e.)	coefficient (s.e.)	coefficient (s.e.)	coefficient (s.e.)
$\beta_{1/s.e}$	0.265 <sup>b</sup> (0.148)	0.712 <sup>a</sup> (0.321)	0.644 <sup>a</sup> (0.320)	0.645 <sup>a</sup> (0.321)	0.665 <sup>b</sup> (0.412)	0.662 <sup>a</sup> (0.331)
$\beta_{region}$		-0.634 <sup>a</sup> (0.303)	-0.588 <sup>a</sup> (0.293)	-0.613 <sup>a</sup> (0.299)	-0.605 <sup>b</sup> (0.324)	-0.515 <sup>b</sup> (0.291)
$\beta_{impact}$			0.220 <sup>a</sup> (0.099)	0.222 <sup>a</sup> (0.100)	0.223 <sup>a</sup> (0.094)	0.229 <sup>a</sup> (0.087)
$\beta_{public}$				0.085 (0.094)	0.086 (0.089)	0.103 (0.069)
$\beta_{NHS}$					-0.030 (0.360)	
$\beta_{panel}$						-0.157 (0.208)
$\beta_0$	3.673 <sup>a</sup> (1.259)	2.305 <sup>b</sup> (1.402)	1.409 (1.051)	1.351 (1.054)	1.308 (1.076)	1.181 (1.159)
$R^2$	0.16	0.452	50.13	50.505	50.505	50.516
$F_M^{167}$	3.21	20.49	24.96	20.31	37.5	35.94

<sup>a</sup>significant at the 5% level, <sup>b</sup>significant at the 10% level

**Table 3. Precision Effect Estimate with Standard Error (PEESE)**

	coefficient (s.e.)	coefficient (s.e.)	coefficient (s.e.)	coefficient (s.e.)	coefficient (s.e.)
$\alpha_0$	0.022 (0.020)	0.013 (0.013)	0.008 (0.008)	0.008 (0.008)	0.008 (0.008)
$\alpha_{1/s.e}$	0.387 <sup>a</sup> (0.139)	0.824 <sup>a</sup> (0.278)	0.691 <sup>a</sup> (0.296)	0.691 <sup>a</sup> (0.297)	0.742 <sup>a</sup> (0.371)
$\alpha_{region}$		-0.690 <sup>a</sup> (0.290)	-0.610 <sup>a</sup> (0.284)	-0.638 <sup>a</sup> (0.287)	-0.613 <sup>a</sup> (0.322)
$\alpha_{impact}$			0.262 <sup>a</sup> (0.119)	0.261 <sup>a</sup> (0.119)	0.261 <sup>a</sup> (0.121)
$\alpha_{public}$				0.100 (0.100)	
$\alpha_{NHS}$					0.103 (0.091)
$\alpha_{panel}$					-0.086 (0.345)
$R^2$	0.340	0.591	0.645	0.648	0.680
F-test	5.340	6.530	85.93	72.69	121.0

<sup>a</sup>significant at the 5% level, <sup>b</sup>significant at the 10% level

**Table 4. Meta-significance Tests**

	MST					Mookerjee (2006)
	coefficient (s.e.)	coefficient (s.e.)	coefficient (s.e.)	coefficient (s.e.)	coefficient (s.e.)	coefficient (s.e.)
$\gamma_0$	-0.03 (0.403)	0.006 (0.007)	0.002 (0.008)	-0.001 (0.008)	0.000 (0.022)	2.50 (1.855)
$\gamma_1$	0.380 <sup>a</sup> (0.08)	0.285 (0.077)	0.225 <sup>a</sup> (0.052)	0.223 <sup>a</sup> (0.052)	0.224 <sup>a</sup> (0.052)	0.196 <sup>a</sup> (0.100)
$\gamma_{region}$		-0.077 <sup>a</sup> (0.037)	0.048 <sup>a</sup> (0.007)	0.049 <sup>a</sup> (0.007)	0.049 <sup>a</sup> (0.007)	
$\gamma_{impact}$			-0.030 (0.210)	0.013 (0.008)	0.013 (0.007)	
$\gamma_{public}$				-0.030 (0.205)	-0.002 (0.022)	
$\gamma_{NHS}$					-0.033 (0.211)	
$R^2$	0.16	0.32	0.40	0.41	0.41	0.05
F-test	11.46	20.56	25.67	21.3	44.7	3.8

<sup>a</sup>significant at the 5% level, <sup>b</sup>significant at the 10% level