Estimating the price elasticity of beer: Meta-analysis of data with heterogeneity, dependence, and publication bias

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ABSTRACT

Precise estimates of price elasticities are important for alcohol tax policy. Using meta-analysis, this paper corrects average beer elasticities for heterogeneity, dependence, and publication selection bias. A sample of 191 estimates is obtained from 114 primary studies. Simple and weighted means are reported. Dependence is addressed by restricting the number of estimates per study, author-restricted samples, and author-specific variables. Publication bias is addressed using a funnel graph, trim-and-fill, and Egger's intercept model. Heterogeneity and selection bias are examined jointly in meta-regressions containing moderator variables for econometric methodology, primary data, and precision of estimates. Results for fixed- and random-effects regressions are reported. Country-specific effects and sample time period are unimportant, but several methodology variables help explain the dispersion of estimates. In models that correct for selection bias and heterogeneity, the average beer price elasticity is about -0.20, which is less elastic by 50% compared to values commonly used in alcohol tax policy simulations.

Keywords: alcohol prices, alcohol taxes, price elasticity, meta-analysis

JEL classification: I120, I100, C10, C590

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1. Introduction

Excessive consumption of beverage alcohol, especially by youth and young adults, can be addressed by a variety of policy tools, including demand-reducing policies in the form of higher alcohol taxes and prices (Phelps, 1988; Cook, 2007; Cawley and Ruhm, 2012). The economic justification for higher alcohol taxes is based on two broad principles. First, there are external costs associated with excessive consumption such as drink-driving, violence, property destruction, and drunken behavior. Higher alcohol taxes reflect social costs not contained in market prices, which restores economic efficiency. Other options include restrictions on time, place or manner of consumption, and severe penalties for some costly behaviors. Second, some individuals are poorly informed about addictive, intoxicating or adverse health effects of excessive alcohol use, and their current consumption may not be optimal due to information costs. Higher alcohol taxes also address this market failure, although better health information or restricted sales are policy alternatives. However, a broad, population-based tax policy is difficult to sustain if there are two types of consumers: heavy drinkers who account for the majority of social costs but whose demands are relatively price inelastic; and light/moderate drinkers with more elastic demands. In the worst case scenario, demands by heavy-drinking individuals are perfectly inelastic, so higher taxes or prices do little to reduce consumption and social costs, and end up imposing welfare losses on moderate drinkers.[1] The taxation paradox is often challenged by reviewers citing particular studies suggesting heavy drinkers are highly responsive to prices (e.g., Xu and Chaloupka, 2011), but this view has been criticized on empirical grounds by Ayyagari et al. (2011), Nelson (2013a, 2013b), and Ruhm et al. (2012). Clearly, efficacy of taxation policies is tied to the price elasticity (or inelasticity) of demand for alcohol, but elasticity estimates exhibit substantial dispersion across drinking patterns, beverages, countries and econometric models and methods, making precise summary estimates difficult to achieve.

The objective of this paper is to summarize and synthesize estimates of the price elasticity of beer, while accounting for dispersion and potential biases due to heterogeneity (factual and methodological); dependence of estimates due to methodology and sampling; and publication selection bias. Meta-analysis and meta-regression analysis are used to address these data issues, and I first discuss deficiencies in several previous meta-analyses that failed to analyze fully the dispersion of estimates for alcohol price elasticities. The importance of this investigation for public policy is illuminated by prior studies of drinking patterns for what is termed the “beer-drinking subculture.” First, Greenfield and Rogers (1999) and Rogers and Greenfield (1999) present survey evidence showing that the top 5% of US drinkers by volume account for 39-42% of alcohol consumption, men and young adults are overrepresented among heavy drinkers, and beer accounts for the bulk of alcohol consumed by heavy drinkers (80% compared to 4% for

[1] An economic model that addresses this paradox is due to Pogue and Sgontz (1989), who base optimal alcohol taxes on relative social gains and losses. Lacking empirical evidence, they assume heavy drinkers and moderate drinkers have identical price elasticities as their optimal tax simulation requires only the relative price elasticity of the two groups; for contrary evidence, see Nelson (2013a, 2013b).

wine and 16% for spirits). Second, beer is more often involved with drink-driving and other risky behaviors, regardless of confounding variables such as age, gender, and education (Berger and Snortum, 1985; Hennessy and Saltz, 1990). Third, beer accounts for two-thirds of all alcohol consumed by binge drinkers (Naimi et al., 2007), and generally is the preferred beverage among males, young adults, and college students (Dawson, 1993; Kerr et al., 2004; Snortum et al., 1987). As noted by Cook and Moore (1993, p. 53), there is no basis for claiming that beer is the "drink of moderation." A refined or targeted tax policy could levy higher taxes on beer compared to wine and spirits, but success of this policy depends on the price elasticity of beer (and cross-price elasticities with wine and spirits). Overall, tax policies that address social costs of excessive consumption must carefully consider price responsiveness of alcohol demands for beverages and consumers, especially the price elasticity of beer drinkers.[2]

Numerous empirical studies estimate demand functions for beer. The present study reviews, summarizes, and synthesizes 191 price elasticity estimates – the effect size – drawn from 114 primary studies. Criteria for inclusion and exclusion of primary studies and estimates are described below. Estimates are based on several types of data for 24 different countries. However, six English-speaking countries – Australia, Canada, Ireland, New Zealand, the UK, and US – account for 143 estimates or 75% of the total. Estimates exhibit substantial dispersion and contain potential biases, which reflect several factors. First, there is variation due to factual heterogeneity, such as sample time period and country. There also are numerous methodological factors that vary from study to study, such as level of aggregation, econometric models, estimation methods, and measurement of prices and income. Second, investigators may report more than one elasticity estimate from the same data or different investigators may use identical or similar data sets. This means that effect-size estimates are not independent, which biases summary estimates and their standard errors (Borenstein et al., 2009; Nelson and Kennedy, 2009). Third, studies and estimates may be subject to publication selection bias, which distorts the magnitude and precision of reported effect sizes (Borenstein et al., 2009; Stanley and Doucouliagos, 2012). A complete analysis must consider all three sources of dispersion. Previous analyses have addressed heterogeneity and dependence, but this is the first meta-analysis to report summary estimates of beer price elasticities that correct for publication bias.

The remainder of the paper is divided into four sections. Section 2 contains a brief review of three previous meta-analyses of alcohol price elasticities, and comments on methods used here compared to earlier analyses. I also describe basic steps in the analysis, including selection of primary studies and estimates. Several weighted-average estimates for price and income elasticities are reported, where weights are based on inverse variances. Both fixed-effect and random-effects models are applied. Section 3 contains an investigation of publication bias using

[2] Most studies of drinking patterns for beer are based on US survey data, and may not apply to all countries in my sample. A few cross-country comparisons exist, such as Bloomfield et al. (2002), Kuntsche et al. (2009), Simpura and Karlsson (2001), and Srivastava and Zhao (2010). For discussion of global convergence in beverage preferences, see Aizenman and Brooks (2008), Colen and Swinnen (2011), Selvanathan and Selvanathan (2006), and Smith and Solgaard (2000).

several methods, including a funnel graph, trim-and-fill analysis, Egger's intercept or funnel-asymmetry test, and a cumulative meta-analysis. Corrected weighted-averages are reported. Section 4 examines the effects of heterogeneity within the set of 191 estimates, and presents meta-regressions that test for effects of independent variables describing factual and methodological heterogeneity and publication bias. Section 5 contains a summary of results and discusses implications for alcohol tax policy. Overall, the demand for beer is less elastic than reported in previous meta-analyses or contained in many policy discussions of alcohol problems.

2. Meta-analysis: Procedures in previous analyses and the present study

Meta-analyses in economics must cope with several problems that arise due to the observational nature of data used in econometric studies, reporting methods favored by academic researchers and journals, and potential biases arising from selective reporting and publication of empirical results in primary studies (Nelson and Kennedy, 2009; Stanley and Doucouliagos, 2012). These problems include heteroskedasticity, heterogeneity, outliers, dependence of estimates, and publication bias. Three previous meta-analyses examine price and income elasticities for alcohol beverages: Fogarty (2009); Gallet (2007); and Wagenaar et al. (2009). This section first summarizes approaches taken by prior analyses, with particular attention to primary data selection and estimation methodology.

2.1 Previous meta-analyses: procedures and methods

Gallet (2007) was the first to conduct a meta-analysis of alcohol elasticities. He examines 132 primary studies that yield 1172 price elasticities and 1014 income elasticities. Multiple estimates are obtained from many studies, so dependencies must be addressed. Unweighted median values are reported for beer, wine, spirits, and total alcohol, which ignore precision. Meta-regressions are estimated, which pool elasticities for three beverages and total alcohol. These regressions control for heterogeneity, but pooling of beverages makes coefficient estimates difficult to interpret. There is no reason to suppose that the marginal effect of, say, panel data is the same for beer elasticities as it is for wine, spirits, or total alcohol. Heteroskedasticity is addressed using White's robust procedure for standard errors, but a preferred method is to employ weighted least-squares with inverse variances as weights (Hedges and Olkin, 1985; Rhodes, 2012). Gallet uses author-specific dummy variables for studies reporting multiple estimates or authors with multiple studies based on the same or similar data. Alternative methods include use of restricted samples, cluster robust standard errors, and hierarchal/multi-level models. It is unclear how Gallet's study handles outliers and he does not discuss publication bias as a data problem.

The meta-analysis by Fogarty (2009) departs in several important ways from procedures used by Gallet. Fogarty employs full and restricted samples, with weights based on inverse variances. This procedure corrects for heteroskedasticity due to different sample sizes and other features of the data. Heterogeneity is addressed by a variety of covariates, with special attention paid to theoretical concepts being analyzed (conditional vs. unconditional elasticities, Hicksian vs. Marshallian price elasticities). Both fixed- and random-effects models are estimated. Outliers are selectively deleted (Fogarty, 2009, p. 32), and dependence is addressed by restricting the number of observations to one per study or a few observations that differ in important ways. Fogarty pools observations for three beverages, so his meta-regressions suffer from the same shortcoming as Gallet's study. Fogarty does not include dummy variables for beverage-type. Finally, Fogarty presents several funnel graphs, which appear to show asymmetries in the dispersion of price elasticities for each beverage. He reports "moderate publication bias only for the beer own-price elasticity" (Fogarty, 2009, p. 32). This conclusion is reexamined here using a greater variety of statistical methods.

Wagenaar et al. (2009) fail to improve on the above studies. First the study does not distinguish between price and tax elasticities, which is misleading.[3] In some cases, aggregate and survey-based results are combined, without attention to basic differences in results. For example, it is unclear how their analysis handles drinking-participation elasticities in survey studies that use self-reported alcohol consumption. Procedures for estimating primary standard errors, if any, are not reported. Second, the meta-analysis is conducted using partial correlation coefficients: the change in standard deviation units of alcohol consumption for a one standard deviation change in price/tax. While this procedure allows comparisons of studies employing different theoretical concepts, it fails to address an important policy issue – magnitude of price elasticities by beverage. Third, dependence is handled by restricting observations to one per study, but exclusion criteria applied are uncertain; see Wagenaar et al. (2009, p. 182). The study begins with 1003 estimates, but appears to end up analyzing only 190 estimates. Separate analyses are conducted for beer, wine, spirits, and total alcohol, but there are no covariates for observable sources of heterogeneity in a random-effects model. Fourth, publication bias is not addressed. Fifth, Wagenaar et al. (2009) omit a number of econometric studies that report price elasticities for alcohol beverages and averages reported are unweighted. The study concludes there is an inverse relationship between price and alcohol consumption, but magnitude is an important issue for tax policy.

2.2 Present meta-analyses: procedures and methods

I begin by describing the selection of studies and effect-size estimates, and then provide a summary of average unweighted and weighted price and income elasticities for beer.

[3] The authors pool price and tax elasticities, and report unweighted averages as if these are comparable concepts. In their Table 5 for heavy drinking, studies use both alcohol prices and taxes, and most studies report multiple insignificant coefficients. It is possible to derive price elasticities from tax elasticities (see Cook, 2007, p. 72), but Wagenaar et al. are not transparent about how their estimates are derived or which results are obtained from each primary study. For example, Cook and Moore (1994) reported eleven coefficients for the tax elasticity for the probability of binge drinking by youth, with different magnitudes and statistical significance. Overall, it is unclear how Wagenaar et al. obtained summary estimates for price elasticities and standard errors.

Selection of primary studies and estimates. A search of the substantial literature on alcohol demand was conducted during the months of August-October 2012, with reference lists from recent meta-analyses providing a useful starting point. Articles, chapters, books, reports, dissertations, and working papers were examined on alcohol demand and alcohol-related outcomes, such as traffic fatalities, crime, labor productivity and wages, and other adverse outcomes. Some econometric studies on alcohol harms include first-stage or structural demand estimates, which are easily overlooked. Searching related literatures also is one way to reduce publication bias. Among search terms used were combinations of “alcohol” AND “tax” OR “price,” OR “elasticity.” Searches also were conducted using “beer,” “wine” OR “spirits.” Among databases searched were AgEcon Search, EconLit, JSTOR, RePEc, SSRN, and the on-line retrieval engines for EBSCO Host, ProQuest, ScienceDirect Journals, and Wiley Online Library. The search was restricted to materials in the English language, but not limited to articles in peer-reviewed journals. A total of 578 studies were recovered and examined. Abstracts, tables, and other summaries were screened by the author to select alcohol-consumption studies with price elasticity estimates. This procedure narrowed the search to 232 studies of alcohol demand, which was further reduced to 178 studies that contained sufficient information on price elasticities and standard errors. There are 60 primary studies not included in Fogarty (2009) or Gallet (2007), and 54 studies included in their analyses that are excluded here.[4] There are 135 studies that are not included in Wagenaar et al. (2009). A bibliography of 578 studies is available at the author’s web page: http://econ.la.psu.edu/people/biographies/nelson_bio.shtml.

Data coding and selection of moderator variables. Data were collected and recorded on coding sheets by the author, including page or table numbers for primary estimates. Data were entered in an Excel file by a research assistant, and then rechecked by the author against page-table information in the primary source publication. Elasticity and standard error data were collected for beer, wine, spirits, and total alcohol. This paper uses data on beer price and income elasticities from 114 of the 178 studies. Following previous meta-analyses, data also were collected on a variety of potential moderator variables that are summarized in Table 1. In general, moderator variables are conditioned so there are sufficient positive observations in each of several possible categories. I did not try to collect data on every possible estimation method or model (2SLS, 3SLS, MLE, SUR, etc.) as some methods are employed by too few studies. For example, only four estimates are based on the rational addiction model. Meta-regressions should properly pool these observations with other similar categorical variables, such as lagged dependent variable models.

[4] I excluded primary studies in Fogarty (2009) or Gallet (2007) if based on older data (8 studies); standard errors could not be calculated (13); only income elasticities are reported (9); relative prices of beverages are used (3); wine varietals are used (3); duplicate studies (2); firm/brand data are used (4); and tax rates are employed rather than prices (12). Two primary studies employ two data sets each, and each data set is coded as a separate study. Eight outlier observations were deleted selectively based on preliminary analysis, either extreme effect sizes or extreme standard errors.

Addressing dependence and precision. Empirical studies in economics frequently report multiple estimates based on the same data or major portions of the same data set. Using all estimates creates within-study dependence or correlation, which will bias summary statistics. More weight is assigned to primary studies with multiple estimates than to studies with one or two estimates. More importantly, using multiple estimates will bias standard errors of summary statistics, with understatement of errors being the generally expected outcome (Borenstein et al., 2009, p. 226). There are several possible solutions to dependence problems, including use of a single “best” estimate per study or model, cluster robust standard errors, and hierarchical/multi-level models (Nelson, 2013c). Between-study dependence also is encountered when primary investigators use the same data to estimate different models in separate publications or different investigators use similar data. Following Fogarty (2009, p. 10), I restricted the collection of effect sizes to one or two estimates per primary study or model. In general, the primary author’s preferred result was selected from each study and an important variation was used to capture different model specifications or estimation methods. Results of this selection procedure are summarized in Appendix Table A1. While this restriction reduces within-study dependence, it does not address between-study dependence. In particular, several authors have published multiple studies using similar data sets, such as repeated country-level models using time-series data for increasingly longer time periods (e.g., Duffy, 1990, 1991, 1995; Selvanathan and Selvanathan, 2005, 2006, 2007). How serious is this duplication is a matter of judgment, since investigators also tend to use different econometric methods, resulting in different “treatment effects.” A more complete analysis of between-study dependence is reported below, where I consider restricted data sets and meta-regression models with author-related variables. There is no evidence of systematic bias due to sampling methods employed.

Standard meta-analysis methods require estimates of effect size precision in the form of standard errors (Stanley and Doucouliagos, 2012, p. 27). For half of the effect sizes, standard errors could be obtained directly, i.e., the primary study reports an estimate for the standard error (or t-statistic). For the other half, standard errors had to be obtained from estimates reported in other portions of the study. If the primary study reported a regression coefficient for price and its standard error or t-statistic, then I used this information to derive standard errors for reported price elasticities. Thus, if the investigator reports a price coefficient and t-statistic, a standard error was derived by dividing the elasticity value by the t-statistic on price. (No elasticity values were calculated.) This procedure introduces bias in the standard errors, with expectation of an upward bias as covariance terms are ignored. As a check, I regressed standard errors on effect sizes and a dummy indicator variable for estimated errors. For price, the dummy coefficient was 0.053 (se = .025). The coefficient is just significant, and indicates a modest upward bias (mean elasticity standard error is 0.207). For income, the dummy coefficient was 0.080 (.03). The estimate is statistically significant and indicates an upward bias (mean elasticity standard error is 0.205). As an additional check, means and regressions reported below use restricted samples or include a dummy variable that identifies observations with estimated errors. There is no evidence of systematic bias due to estimation procedures for effect-size standard errors.

2.3 Summary of average beer elasticities in three meta-analyses

Table 2 summarizes average beer price and income elasticities using analyses reported in Gallet (2007), Fogarty (2009), and the present study. Unweighted average price elasticities are about -0.30 to -0.50. Unweighted income elasticities are about 0.50 to 0.70 in Fogarty and the present study, but smaller in Gallet. Possible reasons are inclusion in the two former analyses of more diverse studies by countries or outliers in Gallet's income data (see below).

In a meta-analysis, fixed-effect models use inverse variances for weights and treat dispersion in estimates as due solely to stochastic sampling error in each primary study (Borenstein et al., 2010). That is, all studies are viewed as estimating a *common*, or fixed, population effect size. Estimates with smaller standard errors provide more precise information about the population value and are given greater weight. The *plural* or random-effects model also accounts for variation in population values by estimating a common between-study variance based on observed dispersion, which is added to the study-level variance. Inverses of combined variances provide the weight for each effect size. Thus, in a random-effects model, true effect sizes are similar or comparable, but not identical across studies. This is the simplest procedure for addressing heterogeneity. Random-effect models give greater weight to less precise studies, which is an advantage given estimation procedures described above. As pointed out by Fogarty (2009, p. 28), random-effects weights also reduce the importance of outliers in estimated standard errors when study-level variances are small. Using random-effects, Table 2 indicates a weighted-mean price elasticity of -0.35 and a weighted-mean income elasticity of 0.57. Fixed-effect means are noticeably less elastic: price elasticity is -0.23 and income elasticity is 0.39.

Table 2 also presents summary values for two individual countries (Canada, USA) and groups of countries: Australia-New Zealand; Nordic countries (Denmark, Finland, Norway, and Sweden); United Kingdom-Ireland; and all other countries. Medians for price range from -0.22 to -0.39. Random-effect price means range from -0.25 for Australia-NZ to -0.45 for Nordic countries. For income, means range from 0.35 for the US, 0.70 for Australia-NZ, and 0.93 for all other counties. While average beer price elasticities vary across countries, the extent of variation is not substantial. This outcome is further confirmed by meta-regression results reported below. Greater country-to-country variation is observed for average beer income elasticities. The bottom rows in Table 2 show, first, results for a restricted sample that deletes nine studies using similar data, which eliminates 30 observations (see Appendix). This reduces the sample size to 161 observations for price elasticities and 139 observations for income elasticities. The results of this restriction are a slightly more elastic price mean (-0.38 compared to -0.35) and less elastic income mean (0.54 compared to 0.57). These are not substantial changes. The second restricted sample excludes observations where standard errors are estimated. The results for this restriction are a slightly more elastic price mean (-0.41 compared to -0.35), and more elastic income mean (0.60 compared to 0.57). Again, these are not substantial changes.

Overall, tentative results from the meta-analysis in this section are a beer price elasticity of about -0.35 and a beer income elasticity of 0.55. Cross-country comparisons indicate that average price elasticities are relatively uniform, but there is greater variation in average income elasticities. Overall, my average price elasticities are less elastic (smaller in absolute value) than those reported in Gallet and Fogarty, and smaller than many values used in the alcohol tax policy literature. For example, Cook (2007, p. 72) uses US excise tax data to derive a beer price elasticity of -0.74. He comments that his "estimated price elasticities are quite high for beer" (Cook, 2007, p. 73). An average US beer price elasticity of -0.29, derived from 42 estimates in 34 empirical studies, is less than half of Cook's reported value. The remainder of the study focuses on correcting average beer price elasticities for publication bias.

3. Publication bias: Detection and treatment

Publication bias occurs when primary researchers search among elasticity estimates and select those with statistically significant coefficients, "correct" signs, and more elastic values. The result is a biased set of published estimates, with general expectations of a positive association between reported effect sizes and their standard errors, i.e., less precise estimates are more likely to be published if they have larger effects.[5] The purpose of this section is to determine if publication bias exists for the sample of beer price elasticities, measure the extent of bias, and report average price elasticities corrected for bias. A standard set of procedures is used. It should be noted, however, that these procedures do not fully account for dispersion of values. The next section incorporates observed heterogeneity into the model.

3.1 Fixed-effect versus random-effects: which model?

A possible indicator of publication bias is a noticeable difference between fixed- and random-effects weighted means (see Table 2). This difference arises because less precise estimates are given greater weight in random- compared to fixed-effect models, reflecting unobservable heterogeneity that can be factual or methodological. As pointed out by Stanley and Doucouliagos (2012, p. 82), "random effects are then, in part, the result of these greater [methodological] efforts to select and report desired estimates." They recommend against use of random-effects models because random effects will be correlated with standard errors when there is publication selection. There are three possible reasons why this argument may be too strong. First, meta-data may contain heterogeneity that is unobservable. For example, Nelson and Kennedy (2009, p. 362) surveyed 140 meta-regression analyses and report a mean R-square of 0.48 (median = 0.44), while Fogarty (2009, p. 33) and Gallet (2007, p. 129) report R-squares of

[5] Card and Kruger (1995) attribute publication bias to three sources: (1) reviewers and journal editors may be predisposed to accept papers that support conventional views such as negative price elasticities; (2) reviewers and journals tend to favor papers with statistically significant results; and (3) primary researchers use t-statistics of two or more for the main covariates as a guide for model specification and selection. For economic models of publication bias, see Young et al. (2008) and Doucouliagos and Stanley (2012).

only 0.37 and 0.17 for alcohol price elasticities. The typical meta-analysis in economics includes numerous methodological explanatory variables, so R-squares suggest substantial unobserved heterogeneity. A random-effects model can be used to capture this source of heterogeneity. Second, given the "law of demand," there is less emphasis in alcohol demand studies on significant results, although primary researchers may choose to emphasize negative price elasticities. Publication bias is less likely if journals are open to empirical studies that do not fully satisfy the prevailing consensus or require significant results. For beer meta-data, there are 191 price elasticities: 121 are negative and statistically significant (t-statistic > 1.99); 62 are negative and insignificant (32.5%); and only eight are positive. Median t-statistics for the group of 121 significant values is 3.33 and only 1.04 for 62 insignificant observations. Third, bias is possible in both estimated effect sizes and estimated standard errors (Stanley and Doucouliagos, 2012, p. 112), so "more precise" estimates may simply reflect biased or inefficient estimates for standard errors. Using random-effects is a possible offset to this inefficiency. As a consequence of these concerns, results reported below use both fixed- and random-effects models.

3.2 Funnel graph and trim-and-fill mean estimates

Figure 1 displays funnel graph for beer price elasticities, where standard errors are arrayed on the Y-axis and elasticity estimates on the X-axis. The vertical line indicates the fixed-effect mean (-0.23) and diagonal lines are the 95% confidence interval. In the absence of publication bias, a funnel plot is symmetric about the mean effect size. The graph indicates negative bias because there is greater dispersion of estimates to the left of the mean, and a clear indication of "missing" studies to the lower right of the mean. The upper part of the graph shows a concentration of more precise estimates near the mean, but studies also are missing in the mid-range of values. A method for correcting for publication bias is the trim-and-fill procedure suggested by Duval and Tweedie (2000), which uses an iterative algorithm to add missing values until observations are symmetric about a recomputed mean effect size. Using trim-and-fill, the fixed-effect mean is -0.20 (se = .01) and the random-effects mean is -0.23 (.02). This correction for publication bias reduces mean estimates by 15-35%.

3.3 Meta-regression analysis of publication bias: FAT–PET results

In the absence of publication bias, effect size estimates and standard errors are independent. If not, they are correlated and summary statistics and standard errors are biased. For the *i*-th observation ($i = 1, \ldots, N$), a regression-based test of this association can be represented as (Card and Kruger, 1995; Stanley and Doucouliagos, 2012)

$$ES_i = \beta_1 + \beta_0 Se_i + \varepsilon_i \tag{1}$$

where *ES* is the estimated price elasticity in study *i*, *Se* is its estimated standard error, and ε is a stochastic error term. In the absence of selection and heterogeneity, observed effects should vary randomly about the true effect size, β_1, independent of standard errors.

However, if model specifications and estimates are selected based on significance of the main covariates, selection bias will vary directly with standard errors, i.e., larger *Se* values are associated generally with larger effect-size estimates. Because estimates are inherently heteroskedastic, it is appropriate to divide equation (1) by standard errors to yield (Egger et al., 1997; Sterne and Egger, 2005; Stanley, 2005)

$$t_i = \beta_0 + \beta_1(1/Se_i) + \upsilon_i \tag{2}$$

where t_i is the *t*-statistic for the *i*-th observation, $1/Se_i$ is its precision, and υ_i is a heteroskedasticity-corrected error term. In equation (2), a test of H_0: $\beta_1 = 0$ is a test of absence of publication bias or funnel-asymmetry test (FAT). If FAT rejects symmetry, the estimate of β_0 is interpreted as the effect size corrected for publication bias, referred to as the Egger intercept or precision-effect test (PET); see Stanley and Doucouliagos (2012).

Table 3 shows results for several FAT-PET regressions for beer price elasticities, estimated with different samples and model specifications. Applying FAT, all intercepts are statistically significant, and values between -1.0 and -2.0 are consistent with substantial selection bias (Doucouliagos and Stanley, 2012). Applying PET, precision coefficients indicate mean price elasticities in the range -0.14 to -0.19, with little difference between the full sample and an author-restricted sample of 161 observations (-0.153 compared to -0.148). Regressions (3) and (4) split the data into two samples based on reporting of primary errors. Price elasticities are -0.19 (*Se* reported) and -0.14 (*Se* estimated). Differences are probably more a function of underlying heterogeneity than indicative of different results due to estimation procedures. Lastly, regression (5) includes a set of country dummies, but none of these are statistically significant. Compared to weighted means in Table 2, meta-regression estimates corrected for publication bias are about 30-35% less elastic. All corrected price elasticities in Table 3 are about -0.20 or about 50% less elastic than conventional averages. Hence, correcting for publication bias has an important impact on the magnitude of average beer price elasticities.

3.4 Cumulative meta-analysis: mean elasticities by precision restrictions

An alternative to FAT-PET regressions is to order the data by precision, and focus attention on more precisely estimated effect sizes (Borenstein et al., 2009, p. 289; Stanley and Doucouliagos, 2012, p. 56). If less precise estimates are biased, it is possible to think of assigning zero weight to these estimates relative to those with smaller errors, regardless of effect sizes (Stanley et al., 2010). With data ordered by precision, a cumulative meta-analysis is an analysis run with the first observation, then repeated with the first two observations, and so on until all 191 observations are analyzed. In this framework, the analysis yields a fixed-effect mean of -0.17 (se = .01) for 20 observations with the smallest standard errors and a mean of -0.23 (.01) for the 50% most precise. Random-effects means are -0.18 (.02) and -0.30 (.02).[6]

In summary, a meta-analysis that corrects for publication bias yields mean price elasticities for beer that are less elastic, indicating negative bias in reported results. Using trim-and-fill, mean values are -0.20 and -0.23. Using meta-regression results, fixed-effect values are -0.14 to -0.19. Using a cumulative analysis, fixed- and random-effects means are -0.23 and -0.30 for the 50% most precise estimates.

4. Meta-regression analysis of heterogeneity and publication bias

Adding moderator variables to Equation (1) yields a weighted least-squares (WLS) meta-regression model of heterogeneity and publication bias. Specifying all moderator variables as binary dummies preserves the interpretation of coefficients in equation (1), with the intercept as an estimate of the true effect size for the null case and precision coefficient as an index of distortion due to publication bias. As a correction for heteroskedasticity, weights are based on the inverse variances of elasticity estimates. Both fixed- and random-effects regressions are estimated. A general-to-specific approach was used to arrive at a preferred model specification (Stanley and Doucouliagos, 2012). All moderator variables were entered in a regression and then ten core significant variables were retained. Results for a robustness test are obtained by back-adding selected variables, which demonstrate that the core coefficients are generally stable and statistically significant. The core dummy variables describe methodological features of primary studies, including primary data, theoretical model, econometric methods, and precision.

4.1 Fixed-effect meta-regressions

Table 4 displays results for five meta-regressions estimated using WLS. Regression (1) is the preferred model, and the null category is a primary elasticity with the following features: (1) published in a journal article or book using annual data at the country level; (2) theoretical model for unconditional Hicksian compensated price elasticity; (3) estimated using a double log specification; (4) an index for the price variable, with no lag terms on the right-hand side; and (5) the primary study reports a standard error for the price elasticity. A negative coefficient for a covariate indicates the null category yields a less elastic price effect and a positive coefficient indicates the opposite. For example, other things equal, Hicksian price elasticities (null) should

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[6] If publication bias operates in part through a reading of past results, then ordering observations by publication date might reveal that bias is more important for more recent studies. That is, suppose primary investigators review previous estimates of beer price elasticities and selection is based on new results that are “favorable” compared to past results. Bias can creep into the temporal pattern of estimates as investigators condition their reported values to lie within an “acceptable” range of estimates. To test this proposition, I ran the cumulative meta-analysis with observations ordered by publication date. For the fixed-effect model, values are -0.30 (se = .01) in 1991, -0.25 (.01) in 2005, and -0.23 (.01) in 2011. Bias toward more elastic values in recent studies does not appear to be an appreciable problem. This is confirmed by meta-regression results reported in Table 4 below.

be smaller in absolute value compared to Marshallian elasticities, due to income effects (Fogarty, 2009, p. 23). This relationship is confirmed empirically in Table 4, e.g., in regression (1), the coefficient is -0.20 (se = .04). The intercepts in regressions (1) - (5) are negative and significant, indicating price elasticities from -0.14 to -0.17 for the null category. Precision coefficients are about -1.0 or slightly smaller. The dummy for estimated errors is insignificant.

Regressions (2) - (5) test for robustness by back-adding moderator variables to the model specification. None of the added coefficients are significant, and most have standard errors that are at least as large as the coefficient estimate. Regression (2) shows that adjusting for dependence due to author-specific effects is not important, given sampling procedures. Country-effects are insignificant in regression (3) and time effects are insignificant in regressions (4) and (5). Robustness tests show that core coefficients are generally stable and significant. Visual inspection of residuals suggests normality, although a few outliers remain. Overall, regression (1) captures the main features of primary data and econometric modeling that are important sources of dispersion in primary estimates.[7] R-square is 0.44 for regression (1), which is consistent with many other meta-analyses in economics.

4.2 Random-effects meta-regression

Regression (6) in Table 4 is a random-effects regression, using method-of-moments procedure in Stata's *metareg* command (Harbord and Higgins, 2008). As before, this is a more conservative approach because less precise estimates are given greater weight. The intercept is -0.20 and the precision slope is -0.95, which are modest changes in magnitude compared to regression (1). Fewer methodological variables are significant or have wider confidence intervals, but this is a generally expected outcome for random-effects models. As pointed out by Borenstein et al. (2010, p.107), random effects allow generalization to a wider variety of situations and scenarios, which is an important issue in discussions of alcohol tax policy. However, the difference between -0.17 and -0.20 is not substantial, indicating that different meta-regression methods give rise to similar results.

5. Discussion

This study uses meta-analysis to correct summary averages of beer price elasticities for heteroskedasticity, heterogeneity, dependence, and publication bias. First, weighted means for a sample of 191 estimates are -0.23 and -0.35 for fixed- and random-effects. Second, correcting for publication bias, fixed-effect estimate is -0.15, a reduction of 35% from the weighted-mean. Using a cumulative meta-analysis yields means of -0.23 and -0.30 for the 50% most precise estimates. Third, correcting for heterogeneity and publication bias, fixed-effect estimate for the null category is -0.17 and random-effects estimate is -0.20. Other samples or categories do not produce results that differ widely from these estimates, such as dispersion due to dependence,

[7] A moderator variable for short-run estimates also was insignificant. I also tried back-adding the variables individually, with the same basic results.

country-specific effects, time period, and estimation methods. Overall, the analysis indicates that the average price elasticity of beer is about -0.20, which is 50% less elastic than previously reported averages. A highly inelastic estimate has important implications for alcohol tax policy.

The justification for higher alcohol taxes to address social costs rests importantly on elastic demands, especially elastic demands for heavy or abusive drinkers and for beer as the beverage of choice among heavy and youthful drinkers. In the absence of this condition, tax increases on alcohol may fail a social benefit-cost test, although revenue, fairness, or "sin tax" objectives might be considerations (Cnossen, 2007; Heien, 1995/96; Kenkel and Manning, 1996). Motivated by studies of social costs of alcohol abuse and dependence, there have been a number of attempts to calculate optimal alcohol taxes. In principle, these calculations are complex and involve alcohol demands that vary by drinking patterns (heavy vs. moderate drinkers, acute vs. chronic consumption); beverage-type (beer vs. wine or spirits); drinker age and gender; and non-linear marginal external costs due to damage-cost thresholds and temporal patterns. As a result, current attempts to determine or simulate optimal taxes usually end up with fairly blunt policy instruments. For example, Lhachimi et al. (2012) use an alcohol price elasticity of -0.50, which is constant across populations and beverage types. In a study by van den Berg et al. (2008), elasticities are specified by beverage using average values from Clements et al. (1997), but these estimates ignore drinking patterns and publication bias. Other simulations involve some econometric modeling such as Purshouse et al. (2010). However, their elasticity estimates ignore some basic data issues, such as not accounting for zero observations in survey data (Purshouse et al., 2010, p. 1363). Using meta-analysis, this study demonstrates that the demand for beer, on average, is highly inelastic, so attempts to construct alcohol taxes that ignore this feature of beverage demand will be misguided. This result when combined with other survey evidence on the inelasticity of demands by heavy drinkers (Nelson, 2013a, 2013b) suggests that population-based alcohol tax policies are unlikely to achieve their desired or stated objectives. Other non-fiscal alcohol policies are deserving of greater attention and consideration.

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Table 1. Summary of meta-analysis variables

Category/variable	No. of beer obs., medians (191 total obs.)
Basic study information	
No. of studies	114 studies
Publication date (median)	1997
Country	see Table 2
Published in Applied Economics	49 obs.
Article/report or working paper/dissertation	164 article obs.
Basic sample information	
Median sample year	1977
No. of primary observations (median)	39 obs.
Data frequency: annual, qtr./monthly, or survey	133 annual
Data type: time series, cross-section, or panel	150 time series
Aggregation level: country, state, province, or person	138 country-level
Effect-size unwt. medians	
Price elasticity – beer (191 obs.)	-0.320
Std. error of price	0.150
Std. error reported or estimated	96 reported
Income elasticity – beer (169 obs.)	0.660
Std. error of income	0.130
Econometric model	
Double log	64
System (AIDS, Rotterdam, etc.)	105
Other models (linear, ARIMA, etc.)	22
Lagged dep. variable	44
Rational addiction model	4
Error correction model	15
Standard demand model	128
Effect-size features	
Short-run price estimate	176
Long-run price estimate	15
Conditional model	98
Unconditional model	93
Hicksian price elasticity	169
Marshallian price elasticity	22
Other data features	
Price index or other measure	65 index
Income/total exp. or other measure	84 income/exp.

Notes: Two articles analyze two data sets each and are counted as four studies. Effect-size means are: price, -0.424 (se = .348); and income, 0.604 (.430).

Table 2. Summary of average beer price and income elasticities: three meta-analyses

Study (smpl. obs. for price, income)	Ave. price elasticity (se)	Ave. income elasticity (se)
Gallet (2007, p. 124)		
unwt. median (315, 278 obs.)	-0.360	0.394
Fogarty (2009, p. 24)		
unwt. mean (154, 121 obs.)	-0.450	0.640
unwt. median (154, 121 obs.)	-0.330	0.670
Fogarty (2009, p. 25)		
Australia, unwt. mean (19, 14 obs.)	-0.340 (.220)	0.770 (.130)
Canada, unwt. mean (29, 19 obs.)	-0.450 (.310)	0.890 (1.41)
UK, unwt. mean (42, 33 obs.)	-0.470 (.540)	0.550 (.510)
USA, unwt. mean (36, 27 obs.)	-0.520 (.490)	0.450 (.570)
This study – full samples		
unwt. mean (191, 169 obs.)	-0.424 (.348)	0.604 (.430)
unwt. median (191, 169 obs.)	-0.320	0.660
wt. mean – fixed effect	-0.233 (.005)	0.390 (.006)
wt. mean – random effects	-0.350 (.018)	0.567 (.035)
This study – country samples		
Australia-NZ, unwt. med. (24, 22)	-0.215	0.795
Canada, unwt. median (28, 22)	-0.285	0.390
Nordic, unwt. median (23, 20)	-0.390	0.430
UK-Ireland, unwt. med. (49, 47)	-0.310	0.680
USA, unwt. median (42, 36)	-0.330	0.315
All other countries (25, 22)	-0.350	0.890
Australia-NZ, random effects	-0.250 (.028)	0.703 (.067)
Canada, random effects	-0.369 (.042)	0.432 (.078)
Nordic, random effects	-0.448 (.075)	0.450 (.055)
UK-Ireland, random effects	-0.361 (.045)	0.619 (.046)
USA, random effects	-0.287 (.034)	0.351 (.062)
All other countries	-0.423 (.082)	0.926 (.109)
This study – restricted samples		
unwt. mean (161, 139 obs.)	-0.464 (.359)	0.575 (.448)
unwt. median (161, 139 obs.)	-0.370	0.650
wt. mean – fixed effect	-0.238 (.005)	0.366 (.006)
wt. mean – random effects	-0.385 (.021)	0.537 (.039)
unwt. mean (96, 83 obs.)	-0.450 (.391)	0.570 (.453)
unwt. median (96, 83 obs.)	-0.315	0.500
wt. mean – fixed effect	-0.296 (.008)	0.335 (.009)
wt. mean – random effects	-0.414 (.033)	0.598 (.046)

Notes: Weighted means for this study computed using CMA 2.2 (Borenstein et al., 2008). The Q-statistic for price homogeneity using the fixed-effect model is 1598 (p = 0.000, 190 df) for the full sample; 1559 (p = 0.000, 160 df) for the first restricted sample; and 1118 (p = 0.000, 95 df) for the second restricted sample. For a sample of 95 observations with estimated standard errors, the fixed-effect mean price elasticity is -0.198 (se = .01) and the random-effects mean is -0.271 (.02).

Table 3. Regression-based tests of publication bias

Variable	(1)	(2)	(3)	(4)	(5)
Intercept	-1.577 (.235)*	-1.867 (.256)*	-1.643 (.455)*	-1.275 (.236)*	-0.939 (.337)*
Precision (inverse *Se*)	-0.153 (.027)*	-0.148 (.028)*	-0.190 (.059)*	-0.143 (.029)*	-0.155 (.028)*
Australia-NZ					-0.499 (.648)
Canada					-1.079 (.606)
Nordic					-0.595 (.522)
UK					-1.031 (.609)
Other countries (excl. USA)					-0.493 (.637)
R-square	0.243	0.230	0.162	0.462	0.260
Sample size	191	161	96	95	191
Mean dep. var.	-3.31	-3.42	-3.42	-2.87	-3.31

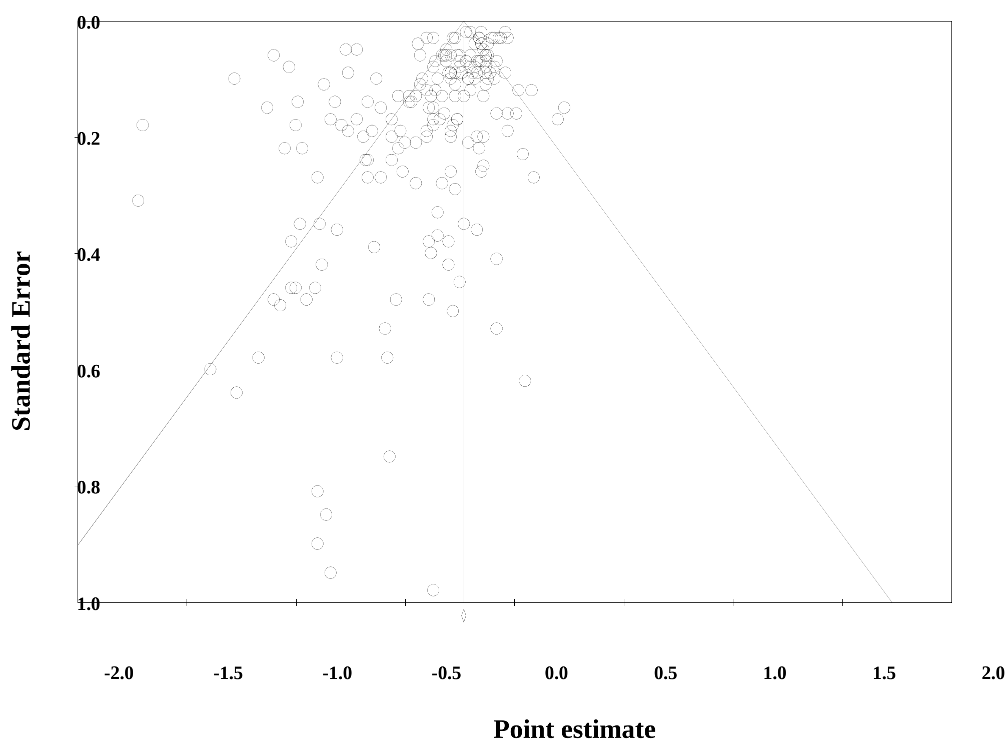
Notes: Dependent variable is t-statistic value for beer price elasticity estimates; standard errors in parentheses based on White's heteroskedastic robust OLS estimator. Asterisks indicate statistically significant at 95% level.

Table 4. Meta-regression results for beer price elasticities

Variable	(1)	(2)	(3)	(4)	(5)	(6)
Intercept (null category)	-0.166 (.059)*	-0.168 (.059)*	-0.165 (.059)*	-0.155 (.066)*	-0.135 (.061)*	-0.204 (.067)*
Precision (std. error)	-1.065 (.262)*	-1.144 (.269)*	-1.024 (.271)*	-1.023 (.264)*	-1.129 (.262)*	-0.951 (.218)*
Publication (article = 0)	-0.104 (.035)*	-0.093 (.037)*	-0.103 (.036)*	-0.112 (.036)*	-0.113 (.036)*	-0.120 (.057)*
Frequency (annual = 0)	-0.206 (.042)*	-0.199 (.042)*	-0.204 (.043)*	-0.222 (.044)*	-0.194 (.042)*	-0.110 (.051)*
Aggregation level (country = 0)	-0.096 (.042)*	-0.090 (.043)*	-0.097 (.043)*	-0.178 (.085)*	-0.118 (.049)*	-0.110 (.050)*
Theory model (uncond = 0)	0.080 (.042)	0.083 (.043)	0.084 (.043)	0.086 (.042)*	0.101 (.047)*	0.070 (.050)
Price model (Hicks = 0)	-0.201 (.042)*	-0.209 (.047)*	-0.202 (.042)*	-0.186 (.043)*	-0.230 (.045)*	-0.156 (.064)*
Lag model? (no = 0)	0.168 (.041)*	0.167 (.042)*	0.169 (.041)*	0.167 (.043)*	0.185 (.042)*	0.055 (.054)*
Econometric (double log = 0)	-0.101 (.047)*	-0.106 (.047)*	-0.104 (.049)*	-0.083 (.049)	-0.110 (.050)*	0.002 (.048)
Price index (index = 0)	0.126 (.050)*	0.120 (.050)*	0.123 (.050)*	0.116 (.051)*	0.158 (.054)*	0.027 (.055)
Se estimated? (no = 0)	-0.065 (.043)	-0.063 (.044)	-0.064 (.044)	-0.072 (.044)	-0.052 (.044)	-0.019 (.067)
Author restriction? (no = 0)	--	0.043 (.042)	--	--	--	--
Applied Econ pub.? (no = 0)	--	0.026 (.033)	--	--	--	--
Nordic? (no = 0)	--	--	-0.050 (.077)	--	--	--
Other non-Anglo? (no = 0)	--	--	0.001 (.050)	--	--	--
Data mid-year (< 1977 = 0)	--	--	--	-0.026 (.029)	--	--
Time series data? (yes = 0)	--	--	--	0.087 (.087)	--	--
Pub. year (< 1997 = 0)	--	--	--	--	-0.045 (.026)	--
Income meas. by dpi or exp.? (yes = 0)	---	--	--	--	-0.057 (.059)	--
R-square (wt.)	0.444	0.450	0.445	0.450	0.455	0.432
Estimation method	WLS	WLS	WLS	WLS	WLS	MM

Notes: Dependent variable is the price elasticity of beer (wt. mean = -0.233). Coefficient standard errors in parentheses, asterisks indicate significance at 95% level; sample size of 191 observations. All explanatory variables specified as binary dummies, except the precision variable; see Table 1 for additional information. WLS estimated using inverse variance weights. Regression (6) is random-effects regression, using method-of-moments (MM) and Stata *metareg* command for estimation.

Figure 1. Funnel plot for 191 beer price elasticity estimates



Appendix Table A1. Price elasticity and standard error estimates in 114 studies

Study (pub. date), country	Price (Se)	Study (pub. date), country	Price (Se)	Study (pub. date), country	Price (Se)
Adrian & Ferguson (1987), Canada	-0.37 (.15)	Clements et al. (1997), Australia	-0.40 (.03)[e]	Duffy (2001), UK	-0.12 (.06)
Adrian & Ferguson (1987), Canada	-0.84 (.17)	Clements et al. (1997), Canada	-0.31 (.07)[e]	Duffy (2001), UK	-0.13 (.08)
Alley et al. (1992), Canada	-0.15 (.26)[e]	Clements et al. (1997), Finland	-0.61 (.15)[e]	Duffy (2002), UK (X)	-0.37 (.98)[e]
Andrikopoulos et al. (1997), Canada	-0.48 (.13)	Clements et al. (1997), New Zealand	-0.37 (.08)[e]	Duffy (2002), UK (X)	-0.39 (.48)[e]
Andrikopoulos et al. (1997), Canada	-1.02 (.46)	Clements et al. (1997), Sweden	-0.30 (.38)[e]	Duffy (2003), UK	-0.40 (.19)
Andrikopoulos et al. (1997), Canada	-0.08 (.07)	Clements et al. (1997), UK	-0.44 (.04)[e]	Eakins & Gallagher (2003), Ireland	-0.53 (.13)[e]
Andrikopoulos et al. (1997), Canada	-1.00 (.46)	Clements & Daryl (2005), Australia	-0.17 (.09)	Eakins & Gallagher (2003), Ireland	-0.76 (.19)[e]
Andrikopoulos et al. (2000), Cyprus	-0.35 (.33)	Collis et al. (2010), UK	-0.77 (.05)	Fang & Pan (2003). China	-0.59 (.53)[e]
Andrikopoulos et al. (2000), Canada	-1.00 (.18)	Collis et al. (2010), UK	-1.10 (.06)	Fang & Pan (2003). China	-0.90 (.81)[e]
Angulo et al. (2001), Spain	-1.17 (.58)[e]	Comanor & Wilson (1974), USA	-1.39 (.60)[e]	Gallet (2007), USA	-0.03 (.03)
Ashton & Casswell (1987), New Zealand	-0.45 (.28)	Comanor & Wilson (1974), USA	-0.56 (.24)[e]	Gallet & List (1988), USA	-1.72 (.31)
Barnes (1984), Canada	-0.99 (.14)	Crawford & Tanner (1995), UK	-0.67 (.24)	Gao et al. (1995), USA	-0.23 (.13)
Bentzen et al. (1997), Denmark	-0.47 (.14)	Crawford et al. (1999), UK	-0.76 (.09)	Goldschmidt (1990), Australia	-0.10 (.03)[e]
Bentzen et al. (1997), Norway	-0.39 (.15)	Crooks (1989), UK	-1.05 (.22)	Gruber et al. (2002), Canada	-0.19 (.09)
Bentzen et al. (1997), Sweden	-0.67 (.27)	Duffy (1982a), UK (X)	0.04 (.23)	Gruenewald et al. (2006), Sweden	-1.70 (.18)
Berggren (1997), Sweden	-0.32 (.06)	Duffy (1982b), UK (X)	-0.14 (.20)	Hagan & Waterson (1983), UK	-0.34 (.17)
Blake & Nied (1997), UK	-0.95 (.48)[e]	Duffy (1982b), UK (X)	-0.17 (.36)	Heien & Pompelli (1989), USA	-0.84 (.95)[e]
Blake & Nied (1997), UK	-1.27 (.64)[e]	Duffy (1983), UK	0.20 (.17)	Hogarty & Elzinga (1972), USA	-0.89 (.35)[e]
Chang et al. (2002), Australia	-0.82 (.14)	Duffy (1983), UK	0.23 (.15)	Holm (1995), Finland	-0.51 (.26)[e]
Chetty et al. (2009), USA	-0.88 (.42)	Duffy (1987), UK (X)	-0.29 (.10)	Holm & Suoniemi (1992), Finland	-0.30 (.42)
Clements & Johnson (1983), Australia	-0.09 (.03)[e]	Duffy (1987), UK (X)	-0.36 (.12)	Huang (2003), UK	-1.03 (.08)
Clements & Johnson (1983), Australia	-0.36 (.07)[e]	Duffy (1990), UK	-0.48 (.14)	Jada et al. (2010), Czech Republic	-0.97 (.22)
Clements et al. (1988), UK	-0.21 (.10)	Duffy (1990), UK	-0.27 (.09)	Jithitikulchai (2010), USA	-0.57 (.75)[e]
Clements et al. (1988), USA	-0.09 (.08)	Duffy (1991), UK (X)	-0.09 (.10)	Johnson & Oksanen (1974), Canada	-0.22 (.07)[e]
Clements et al. (1991), Australia	-0.43 (.06)[e]	Duffy (1995), UK (X)	-0.29 (.20)	Johnson & Oksanen (1974), Canada	-0.38 (.13)[e]
Clements et al. (1991), Australia	-0.15 (.02)[e]	Duffy (1995), UK (X)	-0.03 (.16)	Johnson & Oksanen (1977), Canada	-0.25 (.08)[e]

Table A1. continued

Johnson & Oksanen (1977), Canada	-0.29 (.09)[e]	Norman (1976), USA	-0.87 (.11)	Selvanathan (1991), Finland (X)	-0.54 (.48)[e]
Johnson et al. (1992), Canada	-0.26 (.06)	Norstrom (2005), Sweden	-0.79 (.18)[e]	Selvanathan (1991), Japan (X)	-0.25 (.45)[e]
Johnson et al. (1992), Canada	-0.14 (.05)	Norstrom (2005), Sweden	-0.90 (.27)[e]	Selvanathan (1991), New Zealand (X)	-0.12 (.10)[e]
Johnson, LW (1985), UK	-0.42 (.10)[e]	Ogwong & Cho (2009), Canada	0.05 (.62)	Selvanathan (1991), Norway (X)	-0.14 (.25)[e]
Jones (1989), UK	-0.27 (.13)	Ornstein & Hanssens (1985), USA	-0.13 (.07)	Selvanathan (1991), Sweden (X)	-0.35 (.10)[e]
Jones (1989), UK	-0.40 (.12)	Osoro et al. (2005), Tanzania	-0.31 (.05)	Selvanathan (1991), UK (X)	-0.13 (.06)[e]
Keane (1965), USA	-0.69 (.20)[e]	Ozguven (2004), Turkey	-0.37 (.17)[e]	Selvanathan (1991), USA (X)	-0.11 (.09)[e]
Lariviere et al. (2000), Canada	-0.35 (.37)[e]	Pagoulatos & Sorenson (1986), USA	-0.28 (.18)	Selvanathan (1995), UK	-0.17 (.07)[e]
Lariviere et al. (2000), Canada	-0.38 (.40)[e]	Pan et al. (2006), China	-0.90 (.90)[e]	Selvanathan (2004), Australia	-0.16 (.03)[e]
Lau (1975), Canada	-0.08 (.41)	Pan et al. (2006), China	-0.58 (.58)[e]	Selvanathan (2004), Australia	-0.33 (.06)[e]
Lee & Tremblay (1992), USA	-0.61 (.27)	Partanen (1991), Kenya	-0.33 (.13)	Selvanathan et al. (2005a), Australia	-0.20 (.02)[e]
Lee & Tremblay (1992), USA	-0.81 (.36)	Pearce (1985), New Zealand	-0.15 (.07)[e]	Selvanathan et al. (2005a), Canada	-0.22 (.02)[e]
Leong & Wang (1994), USA	-0.52 (.19)[e]	Penm (1988), Australia	-0.45 (.21)[e]	Selvanathan et al. (2005a), Finland	-0.24 (.09)
Leong & Wang (1994), USA	-0.98 (.35)[e]	Pierani & Tiezzi (2007), Italy	-0.86 (.85)	Selvanathan et al. (2005a), France	-0.06 (.03)[e]
Mangeloja et al. (2009), Finland	-0.21 (.21)[e]	Pierani & Tiezzi (2007), Italy	-0.39 (.38)	Selvanathan et al. (2005a), New Zealand	-0.18 (.04)[e]
McGuinness (1983), UK	-0.30 (.09)[e]	Quek (1988), Canada	-0.28 (.03)	Selvanathan et al. (2005a), Sweden	-0.45 (.13)[e]
Meyerhoefer et al. (2005), Romania	-1.28 (.10)	Quek (1988), Canada	-0.16 (.22)	Selvanathan et al. (2005a), UK	-0.27 (.03)[e]
Nelson (1990a), USA	-0.56 (.20)	Ruhm et al. (2012), USA	-0.28 (.50)	Selvanathan et al. (2005b), Australia	-0.65 (.19)[e]
Nelson (1990b), USA	-0.56 (.17)	Sabuhoro et al. (1996), Canada	-0.72 (.05)	Selvanathan et al. (2005b), Canada	-0.43 (.11)[e]
Nelson (1997), USA	-0.27 (.11)	Salisu et al. (1997), UK	-0.21 (.10)	Selvanathan et al. (2005b), Finland	-0.81 (.58)[e]
Nelson (1997), USA	-0.16 (.07)	Salisu et al. (1997), UK	-0.32 (.16)[e]	Selvanathan et al. (2005b), France	-0.08 (.16)[e]
Nelson (1999), USA	-0.20 (.08)	Schweitzer et al. (1983), USA	-0.27 (.29)	Selvanathan et al. (2005b), Japan	-0.33 (.28)[e]
Nelson (2003), USA	-0.16 (.03)	Selvanathan (1988), UK	-0.13 (.06)	Selvanathan et al. (2005b), NZ	-0.23 (.35)[e]
Nelson (2003), USA	-0.12 (.04)	Selvanathan (1988), UK	-0.20 (.12)	Selvanathan et al. (2005b), Norway	0.09 (.27)[e]
Nelson & Moran (1995), USA	-0.37 (.18)[e]	Selvanathan (1989), UK (X)	-0.25 (.07)[e]	Selvanathan et al. (2005b), Sweden	-0.29 (.26)[e]
Nelson & Moran (1995), USA	-0.04 (.09)[e]	Selvanathan (1991), Australia (X)	-0.15 (.04)[e]	Selvanathan et al. (2005b), UK	-0.67 (.14)[e]
Niskanen (1962), USA	-0.50 (.21)[e]	Selvanathan (1991), Canada (X)	-0.26 (.17)[e]	Selvanathan et al. (2005b), USA	-0.29 (.09)[e]

Table A1. continued					
Selvanathan et al. (2007), Australia (X)	-0.20 (.06)[e]	Stone & Rowe (1958), UK	-0.53 (.22)	Wang et al. (1997), China	-0.63 (.10)
Selvanathan et al. (2007), Canada (X)	-0.25 (.06)[e]	Tegene (1990), USA	-0.72 (.17)	Wette et al. (1993), New Zealand	-1.10 (.48)
Selvanathan et al. (2007), Finland (X)	-0.64 (.39)[e]	Thom (1984), Ireland	-0.68 (.24)[e]	Wohlgenant (2011), USA	0.02 (.12)
Selvanathan et al. (2007), France (X)	0.01 (.16)[e]	Tian & Liu (2011), China	-0.29 (.06)	Wohlgenant (2011), USA	-0.14 (.13)
Selvanathan et al. (2007), Japan (X)	-0.03 (.19)[e]	Treisman (2010), Russia	-0.18 (.10)	Yu & Chen (1998), Canada	-1.13 (.15)
Selvanathan et al. (2007), NZ (X)	-0.13 (.11)[e]	Trolldal & Ponicki (2005), USA	-0.07 (.03)	Zereyesus (2010), USA	-0.04 (.02)[e]
Selvanathan et al. (2007), Norway (X)	0.08 (.12)[e]	Uri (1986), USA	-1.07 (.49)	Zereyesus (2010), USA	-0.91 (.46)[e]
Selvanathan et al. (2007), Sweden (X)	-0.26 (.17)[e]	Volland (2009), Germany	-0.08 (.53)	Zhang & Casswell (1999), New Zealand	-1.02 (.38)
Selvanathan et al. (2007), UK (X)	-0.31 (.06)[e]	Walsh (1982), UK	-0.13 (.09)[e]	Zhuk (2011), USA	-0.18 (.08)
Selvanathan et al. (2007), USA (X)	-0.15 (.04)[e]	Walsh & Walsh (1970), Ireland	-0.17 (.20)[e]	Zoltan (2006), Hungary	-0.29 (.19)
Stone & Rowe (1958), UK	-0.40 (.20)	Wang et al. (1996), USA	-0.37 (.03)	--	--

Notes: (X) = study excluded in author-restricted sample and e = estimated standard error. Eight outliers were trimmed: two based on the size of the price elasticity and six based on the size of the estimated standard error. Two studies are based on two data sets each, and are counted as four studies; see Pierani and Tiezzi (2007) and Selvanathan and Selvanathan (2005) . A complete list of references is available upon request from the author.