Effects of Beverage Alcohol Price and Tax Levels on Drinking: A Meta-analysis of 1003 Estimates from 112 Studies

Short Title: Meta-analysis of Alcohol Price

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Page Count: 39 (Including Appendix)

Word Count: 4179

Conflicts of Interest: None

Effects of Beverage Alcohol Price and Tax Levels on Drinking: A Systematic Review and Meta-analysis of 1003 Estimates from 112 Studies

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Abstract

Aims. We conducted a systematic review of studies examining relationships between measures of beverage alcohol tax or price levels and alcohol sales or self-reported drinking. A total of 112 studies of alcohol tax or price effects were found, containing1003 estimates of the tax/price→consumption relationship.

Design. Studies included analyses of alternative outcome measures, varying subgroups of the population, several statistical models, and use different units of analysis. Multiple estimates were coded from each study, along with numerous study characteristics. Using reported estimates, standard errors, t-ratios, sample sizes and other statistics, we calculated the partial correlation for the relationship between alcohol price or tax and sales or drinking measures for each major model or subgroup reported within each study. Random-effects models were used to combine studies for inverse variance weighted overall estimates of the magnitude and significance of the relationship between alcohol tax/price and drinking.

Findings. Simple means of reported elasticities are -0.46 for beer, -0.69 for wine, -0.80 for spirits. Meta-analytic results document the highly significant relationships ($p<.001$) between alcohol tax or price measures and indices of sales or consumption of alcohol (aggregate-level r = -0.17 for beer, -0.30 for wine, -0.29 for spirits, and -0.44 for total alcohol). Price/tax also significantly affects heavy drinking (mean reported elasticity = -0.28, individual-level r = -0.01, $p<.01$), but the magnitude of effect is smaller than effects on overall drinking.

Conclusions. A large literature establishes that beverage alcohol prices and taxes are inversely related to drinking. Effects are large compared to other prevention policies and programs. Public policies that raise prices of alcohol are an effective means to reduce drinking.

Keywords: alcohol, price, tax, systematic review, meta-analysis

INTRODUCTION

There is a large literature over the past several decades on the relationship between beverage alcohol tax and price levels and alcohol sales or consumption measures. Excise and sales taxes represent the most widespread public policy affecting retail price of alcohol; some studies measure prices directly, and many use tax rates as a surrogate measure for price, since differences in price across geographic areas are largely due to differing tax rates. Studies differ substantially in terms of methodological quality—some using longitudinal designs and others are simple cross-sectional surveys, some carefully controlled with comparison groups and covariates, others more rudimentary analyses. Economists, using contemporary econometric modeling methods, conduct a majority of these studies, but a substantial minority of studies are conducted by scientists in other disciplines related to health or social sciences. Beyond obvious differences in methodological quality from study to study, even studies most would consider of high quality vary in many details of measurement, jurisdiction studied, and statistical approach, and study authors' interpretation of a given pattern of empirical findings varies substantially across papers.

Several narrative reviews of this extensive literature have appeared. Early reviews were conducted by Ornstein [1] and Ornstein & Levy [2]. They suggest best estimates of price elasticities for beer, wine and spirits to be -0.30, -1.00 and -1.50, respectively. Leung and Phelps [3] report that studies using individual-level data tend to obtain larger (i.e., more elastic) demand estimates than aggregate-level studies. Also, they report that results from aggregate-level studies are sensitive to the choice of functional form in the demand model specification. Manning, et al. [4] examine several aggregate-level studies on the effects of excise taxes on alcohol consumption, reporting a range of price elasticities between -0.80 and -2.0 for spirits; -0.64 and -

1.0 for wine; -0.25 and +0.24 for beer; and -0.50 and -1.6 for overall alcohol consumption. Grossman, et al. [5] reviewed research on price effects of alcohol on consumption by youth, comparing different individual and aggregate data sets between 1974 and 1989. They conclude that both frequent and heavy consumption of alcohol by youth are negatively correlated with alcohol price. Edwards et al. [6] tabulated 52 sets of elasticity estimates for beer, wine, and distilled spirits for 18 countries across different time periods. The elasticities vary substantially across studies, countries, time, beverage type, and whether the elasticity estimate is elastic, unit elastic, or inelastic. But note that, all, with the exception of one estimate, are negative. Cook and Moore [7] summarized the economic literature on drinking and associated outcomes, concluding that the demand for alcohol is downward sloping, indicating excise taxes can be used as an effective alcohol-control policy. Chaloupka, et al. [8] reviewed studies that examine price effects on consumption and alcohol-related outcomes for youth. They conclude that frequency of youth drinking and probability of heavy drinking can be decreased by an increase in beer excise taxes.

Only two meta-analyses have been published, both of which have different goals and use a fundamentally different approach than our study reported here. Fogarty [9] took alcohol elasticity estimates from a review by Edwards, et al. [6] which only included studies through 1992; Gallet [10] includes 132 studies of alcohol price, income or advertising elasticities through 2003. The two studies [9, 10] report illuminating results on possible effects of study characteristics (i.e., data used, model specification, estimation method) on estimated elasticity. However, neither takes into account the variances attached to each estimate coming from the primary studies, implicitly weighting each estimate equally. In contrast, we explicitly cumulate the evidence by weighting each estimated effect by the inverse of its variance. Gallet [10] used dummy variables to account for study-author effects, while we used random effects models which into account the second-level variance at the study level in assessing the precision of the cumulative estimates. In short, the Fogarty and Gallet studies come from an econometric tradition and report results from simple OLS regressions of study characteristics on reported elasticity. The present study comes out of the systematic review and meta-analysis traditions in epidemiology and the social sciences, where evidence for an underlying relationship of theoretical and practical significance is cumulated across studies based on the point estimates and estimated variances from individual studies using multi-level random effects models.

METHODS

The core *independent* variable is measured in this literature in two main ways: direct measurement of retail price of alcoholic beverages, or use of tax rates as an indicator or surrogate for prices, since cross-jurisdiction and longitudinal variability in prices is heavily influenced by state/province/country tax rates on alcohol. As Kenkel [11] notes, economic theory predicts taxes will be fully passed through to consumers, given a competitive environment with constant marginal costs of production, and such a one-to-one pass through is a “standard assumption” in most of the research on alcohol taxes or prices. While empirical evidence is fairly limited, data indicate that taxes are passed through to prices, typically at pass-through rates of one to two (e.g., a 10 cent increase in tax produces a 10 to 20 cent increase in price; [11-13]). For the present meta-analysis, we included studies that use tax rates as a price indicator and those that measured retail prices directly, given the high correlation between the two. Nevertheless, the issue of potentially varying pass-through rates depending on local market and regulatory conditions must be noted in projecting tax policy effects. Finally, many jurisdictions control alcohol prices via mechanisms other than taxes (e.g., distribution monopolies, licensing fees); effects of such non-tax regulations (which in some cases affect prices) are not included in the current meta-analysis.

The core *dependent* variables for the current study are: (1) measures of the quantity, prevalence, or frequency of alcohol sold or consumed, stratified by beverage type when available (beer, wine, spirits), and (2) measures of heavy consumption or intoxication.

Data for this meta-analytic study are quantitative estimates of the magnitude or strength of these relationships, and estimates of the variability or error of those estimates, all coming from previously completed studies in the literature. Thus, major components of the project were identifying and obtaining the original study reports, calculating and coding multiple effect sizes and standard errors along with numerous population and other characteristics from each study, and statistically aggregating across all resulting estimates of the underlying relationships of conceptual interest. Each of these is now addressed in more detail.

Data Collection--Literature Search

A comprehensive literature search was conducted by a doctoral-level graduate student with expertise in econometric and statistical methods. Searches were conducted of nine databases to identify studies of interest: AgEcon Search (1960-present), Blackwell-Synergy (1879-present), EBSCO Host, which encompasses EconLit (1969-present), Academic Search Premier (1922-present), Business Source Premier (1922-present), and PsychInfo (1967-present), JSTOR (1838-present), MEDLINE (1950-present), Springer (1992-present), ScienceDirect (1823-present), ISI Web of Knowledge (1900-present), and Wiley (1961-present). The entire record for each document was included in each search; thus, any record with any search term in the title, keywords, subject headings, descriptors or abstract fields would be identified. The set of search terms that was used for each database is as follows, where * is the truncation indicator to include all forms of the root word: [(tax OR taxes OR taxation OR cost OR cost* OR price OR prices) AND (alcohol* OR drinking OR liquor OR drunk* OR beer OR wine OR spirits OR malt beverage*)]. The search process, particularly for older papers not in current on-line databases, was supplemented with an extensive reprint file of alcohol tax papers maintained over the last 30 years by the first author. Moreover, "snowball" sampling, using reference lists from each identified paper, was used to locate additional studies. The original document for each publication was obtained and reviewed for relevance and content. Studies were excluded from analysis if they: (1) are duplicate publications of a single study/dataset (keeping most recent); (2) are empirical studies but do not provide sufficient data for calculating some form of numeric estimate of effect and estimate of its standard error; (3) are commentaries, legal reviews, literature reviews, or otherwise report no new data; or (4) are not written in English. The above procedures produced 112 papers containing 1003 separate empirical estimates of the relationship between alcohol taxes/prices and drinking (see the Appendix for the complete list of citations).

Despite extensive procedures to locate relevant studies, there are always limitations to the search strategy. First, analyses were exclusively based on reports published in English. This was simply a practical consideration; however, it could potentially bias the results of the meta-analyses [14-16]. Research has shown that the proportion of studies with statistically significant results is higher among reports published in English compared with those in other languages [16-18]. Thus, exclusion of non-English studies could lend to inflated meta-estimates of effect, but note that one study found the inflation in effect size due to English-language restriction to be only 2% [19]. Second, publication bias (or, more generally, small-study bias) is always a threat to the validity of a meta-analysis [20-22]. Statistically significant findings are more likely to be published than those that are not significant [14, 20-22], with one estimate suggesting that the odds of publication are 2 to 4 times greater when results are statistically significant [14]. Thus, it is possible that a substantial number of studies with non-significant effects remain unpublished. Excluding these unpublished studies may introduce an upward bias into obtained effect estimates [23]. On the other hand, Sutton et al. [24] empirically examined publication bias across meta-analyses; while 54% of meta-analyses studied were determined to have had missing studies, these biases “did not affect the conclusions” of the meta-analyses. We did not exclude available unpublished studies, but we also did not implement extensive searching for difficult-to-find unpublished studies. While clearly beneficial, inclusion of explicit search procedures for such “fugitive” literature is prohibitively expensive. Methodologically, this creates a risk the results are subject to publication bias.

Coding—Determining Effect Sizes and their Precision

Meta-analysis aggregates and compares findings from different research studies, therefore it is necessary that those findings are both conceptually comparable and configured in a similar statistical form [25]. The first criterion requires that included studies deal with the same constructs and relationships in order for meaningful comparisons to be made. This can be problematic, because what is deemed conceptually comparable may differ across analysts, a particular issue when scientists in different disciplines analyze the same set of studies. In the present case, all the studies were conceptually very similar, examining relationships of identical concepts. Nevertheless, there is considerable variability in specific measures, research designs, and statistical models. For study findings to be meaningfully compared (and statistically aggregated and analyzed), they must lend to calculation of a single uniform effect-size metric that is appropriate to the conceptual nature of the relationship in the research findings and the statistical forms reported in the literature. Numerous population and study characteristics were coded, including multiple outcome measures (eg., beer, wine, spirits, if reported separately), age group, research design, level of analysis, sample design and size, and statistical methods.

Most important and complex is the coding of an effect size in a standardized comparable metric across all studies that represents both the direction and magnitude of the difference or relationship for each study finding. The individual studies identified used diverse research designs and statistical methods. Nevertheless, almost all provide results from some sort of regression equation showing estimated coefficients and standard errors or other statistics that indicate, or provide a basis for estimating, the standard error (e.g., a t-ratio or confidence interval). Multiple estimates from each study were coded, including multiple subgroups, multiple follow-ups, and from multiple statistical models for each subgroup. The measure of effect, its standard error, the analysis sample size, and the effect's significance level were coded for each separate estimate. For studies that report *p*-value cut-off values (0.05, for example) but not exact values, we (conservatively) assigned the value 0.05, even though the (unknown) true exact value was less than .05.

Statistical Analyses

Based on statistics reported in each study, and using conversion formulae from the meta-analysis literature programmed in Comprehensive Meta-analysis 2.0 software (CMA; [26]), we estimated a standardized effect size *r* for each separate estimate of the underlying relationship of interest, where $r = \sqrt{t^2 / (t^2 + (N-2))}$ and its associated Fisher's Z given by $ES_{Z_i} = .5\log_e \left[1 + r/1 - r\right]$. In addition to standardized effect size estimates, the standard errors of those estimates were calculated as $SE_{Z_i} = 1 / \sqrt{n-3}$ and $SE_r = (1 - r^2) * SE_z$. Note that the *r* estimates are also interpretable as the standardized slope of the relationship between price/tax and consumption.

At a broad level, statistical analyses involved combining diverse individual estimates of effect into a single (or small number of) meta-estimates of effect in a common metric, and standard error of that effect. We examined heterogeneity of effects and conducted sensitivity and robustness analyses to evaluate consistency of estimates across study characteristics, and risks to the meta-estimates due to publication bias and potential non-representativeness of the sample studies.

Combining the individual effect sizes into a single meta-estimate of effect involved several steps. First, we identified sets of statistically independent (e.g., separate youth and adult samples, separate states, etc.) and non-independent (e.g., multiple estimation models using the same population or sample) estimates. Intra-study effect sizes based on the same study population or sample were averaged such that only one value contributed to the meta-analyses [25]. Inverse variance weighting methods were applied to each resulting (independent) effect size, where the weight applied is $w_i = 1/v_i$, with $v_i = SE^2_{ES_i}$. Second, we evaluated the effect size distribution for outliers, to determine the need for trimming or Windsorizing (results for both the full analyses and trimmed models without outlier studies are shown in Tables 1-5). We computed the weighted mean effect size for each subgroup (based on study characteristics) by $\overline{ES} = \Sigma(w_i ES_i)/\Sigma w_i$, where ES_i are the values of the effect size statistic used (here *r* or Fisher's Z), w_i is the inverse variance weight for each effect size *i*, and *i* is equal to 1 to *k*, with *k* being the number of effect estimates.

Homogeneity tests within and across subgroups based on study characteristics were based on the *Q* statistic, $Q = \sum w_i (ES_i - \overline{ES})^2$, where ES_i is the individual effect size for i = 1 to *k*, $\overline{ES}$ is the weighted mean effect size over the *k* effect sizes, and w_i is the individual weight for ES_i. *Q* is distributed as a chi-square with $k - 1$ degrees of freedom [27]. A statistically significant Q indicates a heterogeneous effect size distribution [28].

Given our initial review of this literature and the diversity found, we expected statistically significant (residual) study-level heterogeneity, which was confirmed by significant Q-statistics. Therefore, we used random- (as opposed to fixed-) effects models when estimating average effects and their precision. Random-effects models are more conservative, producing wider confidence bounds around the meta-estimates of effect. Random effects modeling means that the variance for each effect size is a function of both underlying subject-level sampling error and random, between-studies variance component [25], such that $v_i^* = v_i + \tau^2$, where v_i is the initial subject-level sampling error and τ^2 is the random variance component, estimated by $\tau^2 = Q - (k-1) \big/ \sum w_i - (\Sigma w_i^2 / \Sigma w_i)$, where Q is the value of the homogeneity test, k is the number of effect sizes, and w_i is the inverse variance weight for each effect size defined under the fixed-effects model. The inverse variance weight applied to each effect size thus becomes $w_i^* = 1 \big/ v_i^*$ and the mean effect size is recomputed. Next, we computed the standard error of the mean effect size, $SE_{\overline{ES}} = \sqrt{1 \big/ \Sigma w_i}$, where w_i (or w_i^* for random effects models) is the inverse variance weight associated with effect size i with $i = 1$ to k effect sizes included in the mean [27]. We constructed confidence intervals and tested the significance of each mean effect size, where a 95% confidence interval is $\overline{ES} \pm Z_{(.95)}(SE_{\overline{ES}})$ and the significance of the mean effect size can be obtained with a Z-test as $z = \left|\overline{ES}\right| \big/ SE_{\overline{ES}}$.

There is a debate in the meta-methods literature on whether direct meta-analyses of the partial r estimate from each study is best, or meta-analyses of the Fisher’s Z transform of the r’s is preferred to avoid possible bias in calculation of the standard error of r. For completeness, we conducted all analyses twice, once using *r* values as inputs, and again using Fisher's Z's. However, the bias is known to be smaller than rounding error when study n's are over 40 [29]; consistent with expectations, we found very little difference in results between the two, and our presentation here is based on analyses of *r'*s instead of Fisher's Z's.

RESULTS

First, we present results from studies which examined effects of alcohol price or tax on *general alcohol consumption*, not stratified by beverage type. The simple mean of 91 elasticity estimates reported is -0.51. Because the underlying population variance of aggregate-level studies is considerably smaller than that of individual-level studies, we examined effect sizes separately for the two types of study. The inverse weighted overall partial *r* for 24 studies that examined these relationships using aggregate-level data (most often at the state/province level) is -0.44, which is highly significant ($Z = 7.55$, $p < .01$; Table 1). Examination of the top panel of Table 1 shows how strong this evidence is—all but one study found an inverse relationship, and 19 of the 24 studies show a significant inverse effect. At the individual level, the effect size in terms of standard deviation units is considerably smaller, as one would expect given the larger variation across individuals than across states/provinces, but the evidence remains very strong, with an overall $r = -0.06$, $Z = -4.27$, p<.01 (second panel of Table 1). Removing one outlier [30] has little effect on the overall results.

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INSERT TABLE 1 HERE

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The simple mean *beer* price/tax elasticity across 105 reported estimates in the 47 identified studies is -0.46. Variance weighted overall partial *r* estimate from 40 aggregate-level studies is -0.17 (Z = -6.91; p<.01; Table 2). All but five studies report an inverse relationship, and 11 of the 40 studies report an effect that is not statistically significant at the study level (using p<.05 criterion). Only seven individual-level studies specifically of price/tax effects on beer consumption were found. All seven report an inverse relationship, but four of the seven are not significant at the individual study level. Despite this, the seven studies as a group clearly show a significant inverse effect, with an overall inverse variance weighted r = -0.12, Z = -2.37, p=.02.

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Thirty-two studies examined the effects of tax or price on *wine* consumption (mostly overlapping with authors who also report effects for beer and spirits). The simple mean of 93 elasticities reported is -0.69. All but five studies report an inverse relationship, with five of the 32 not significant at the study level (using p<.05 criterion). Five of six individual-level studies similarly report an inverse relationship, although half are not significant at the study level. However, the meta-estimated effect across the seven studies is clearly significant (r = -0.30, Z = -8.03, p<.01; Table 3).

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Forty-five aggregate-level studies of alcohol tax/price effects on *spirits* consumption have appeared, reporting 103 elasticity estimates with an overall simple mean of -0.80. Thirty-nine of the 45 studies report an inverse effect estimate, and 11 of the 45 studies report no statistically significant relationship. Combining all the estimates produces an estimated partial r = -0.29 (Z = -9.23, p<.01). Similar results are found for the six individual-level studies (r = -0.10, Z = -2.52, p<.01; Table 4).

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Finally, ten studies of the effects of alcohol prices or taxes on various indicators of *heavy drinking* have appeared, all studies are inherently at the individual level, since sales data do not differentiate by drinking status. The simple mean of the ten elasticities reported is -0.28. All but one study found an inverse effect, and eight of the ten studies found statistically significant effects at the study level. The meta-estimate of effect across the ten studies is r = -0.01 (Z = -2.54, p<.01; Table 5).

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Results are summarized in Figure 1 for the aggregate-level studies, and Figure 2 for the individual-level studies. The differing magnitude of estimated effects between the two types of studies is a consequence of a statistical artifact. The r estimates reported here represent the amount of change in standard deviation units in alcohol sales/consumption associated with a one standard deviation change in price/tax. Aggregating the population into larger units (such as cities, states or countries) substantially lowers the variability of the measure, since individual differences are "averaged out" (a longstanding known effect in sociology; see Blalock, Jr. [31], p. 106).

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INSERT FIGURES 1 & 2 HERE

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DISCUSSION

Results confirm previous reviews of this literature, but extend those results in important ways. The literature we analyzed contains 1003 separate estimates of the underlying conceptual relationship of interest. Narrative reviews inherently take short-cuts, often including only "major" studies, or only studies in the reviewer's discipline, or only recent studies. Narrative reviews often summarize the conclusions of the study authors, not necessarily the empirical results reported in data tables. And reviews often give disproportionate attention to a small number of studies with divergent results. The meta-analyses reported here demonstrate the statistically overwhelming evidence of effects of alcohol prices on drinking. Price affects drinking of all types of beverages, and across the population of drinkers from light drinkers to heavy drinkers. We know of no other preventive intervention to reduce drinking that has the numbers of studies and consistency of effects seen in the literature on alcohol taxes and prices.

A frequent criticism of meta-analyses is that they combine "apples and oranges," that is, combine results from studies that differ in important ways. Our sample of studies is conceptually very well-integrated, but diverse in terms of units analyzed, treatments (i.e., size of tax or price change evaluated), outcome measures, settings, time, and specific statistical models. On the last issue, a purist would argue that results from models with differing sets of covariates cannot be combined with the methods described here (and moreover, methods to address this issue have not yet been developed). Optimally, we would have available identical (bivariate) estimates of effect from all studies, but such estimates are not available in the published papers. We are not alone with this problem. Diversity in model covariates is fairly common in published meta-analyses, and does not prevent investigators from statistically aggregating the evidence, even though statistical theory that is the basis of meta-analyses was originally based on uniform bivariate estimates (and implicitly assume all studies used the same research design and statistical method). We used random-effects (rather than fixed-effects) models to combine studies, which help take into account such study-level variability, permitting a relaxation of the assumption that all studies are estimating the exact same underlying effect. Moreover, our use of random effects models is deemed a conservative approach, since estimated confidence intervals around point estimates are larger for random-effects models than fixed-effect models. Importantly, future studies are warranted that statistically model potential explanations of differences in estimated effect sizes across studies, and that examine price/tax effects on a range of relevant health and social outcomes. We are continuing such analyses, with meta-analyses of price/tax effects on morbidity and mortality outcomes currently in progress.

The meta-analyses reported here and much of the economic literature on alcohol may give the impression that price elasticities are somehow inherent properties of the different beverages studied. But results across studies suggest that the magnitude of price effects varies across groups, situations, and times. At the most basic level, price interacts with income in affecting consumption. Perhaps the effects of price/tax are not linear, but are characterized by effect thresholds such that effects qualitatively differ in communities or societies with very high or very low levels of consumption. All estimates of tax and price effects also reflect particular meanings and uses of alcoholic beverages across diverse social and cultural environments, and tax and price policies likely interact with a whole web of individual, community and societal influences on drinking behavior.

Finally, the effect sizes reported here are large. Cohen [32], one of the "founding fathers" of meta-analyses, suggested *d* (standardized difference) effect sizes under 0.20 are small, 0.50 are medium, and >0.80 are large; equivalent effects in terms of *r* mean a small effect is 0.10, medium is 0.24 and large is 0.37. Lipsey & Wilson [33] report from a study of over 300 meta-analyses of diverse behavioral and educational interventions showed a median effect size equivalent to $r = 0.24$. At the aggregate level where tax policy as a preventive intervention operates, the estimated effect sizes reported here for wine, spirits and overall alcohol consumption are clearly above such reported median level of prevention effectiveness. Given (1) very low cost of adjusting alcohol tax policies to achieve substantial prevention benefits, (2) the global burden of disease and injury due to alcohol consumption [34, 35], and (3) high levels of fiscal and social costs of alcohol-related problems [36-38], the magnitudes of effect that are clearly established in the extant literature on alcohol price effects are noteworthy.

Acknowledgements

This study was funded in part by the Robert Wood Johnson Foundation. Findings and conclusions are solely the authors. Special thanks to Amy L. Tobler for assistance with manuscript preparation.

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Table 1: Effects of Price on Alcohol Consumption

Study	*r*	lower CI	upper CI	*Z*	*p*
Bask, 2004	-0.72	-0.83	-0.53	-5.83	0.00
Bishai, 2005	-0.03	-0.04	-0.02	-4.63	0.00
Blake, 1997	-0.57	-0.75	-0.32	-3.96	0.00
Bourgeois, 1979	-0.06	-0.18	0.07	-0.85	0.40
Brinkley, 1999	0.03	-0.27	0.33	0.22	0.83
Clements, 1983	-0.71	-0.87	-0.42	-3.94	0.00
Clements, 1991	-0.84	-0.92	-0.70	-6.63	0.00
Clements, 1997	-0.60	-0.81	-0.23	-2.97	0.00
Duffy, 1987	-0.58	-0.81	-0.20	-2.84	0.00
Holm, 1992	0.01	-0.39	0.40	0.05	0.96
Leppanen, 2001	-0.64	-0.84	-0.30	-3.29	0.00
Levy, 1983	-0.62	-0.78	-0.38	-4.45	0.00
McGuinness, 1980	-0.39	-0.71	0.07	-1.69	0.09
Nelson, 1995	0.03	-0.35	0.41	0.15	0.88
Nelson, 1997	-0.41	-0.59	-0.19	-3.45	0.00
Nelson, 2003	-0.14	-0.22	-0.05	-3.01	0.00
Ruhm, 1995	-0.40	-0.46	-0.33	-10.69	0.00
Rush, 1986	-0.96	-0.98	-0.92	-9.95	0.00
Selvanathan, 1988	-0.61	-0.79	-0.33	-3.77	0.00
Smart, 1998	-0.83	-0.93	-0.60	-4.72	0.00
Treno, 1993	0.07	-0.26	0.38	0.40	0.69
Wilkinson, 1987	-0.10	-0.23	0.03	-1.48	0.14
Young, 2003	-0.12	-0.18	-0.05	-3.25	0.00
Zhang, 1999	-0.57	-0.73	-0.36	-4.53	0.00
AGGREGATE-LEVEL STUDIES	*-0.44*	*-0.54*	*-0.34*	*-7.55*	*0.00*
Cameron, 2001	-0.04	-0.06	-0.02	-3.74	0.00
Chaloupka, 1996	-0.02	-0.05	0.01	-1.31	0.19
Chaloupka, 1997	-0.03	-0.04	-0.01	-4.03	0.00
Cook, 1993	-0.09	-0.16	-0.01	-2.35	0.02
Cook, 1994	-0.04	-0.08	-0.01	-2.25	0.02
Dee, 1999	-0.05	-0.09	-0.01	-2.29	0.02
DiNardo, 2001	0.00	-0.01	0.00	-0.29	0.77
Farrell, 2003	-0.01	-0.02	0.00	-1.96	0.05
French, 2006	-0.03	-0.09	0.03	-0.99	0.32
Gao, 1995	-0.08	-0.11	-0.05	-5.53	0.00
Gius, 2005	0.00	-0.03	0.03	-0.12	0.90
Grossman, 1998	-0.02	-0.04	-0.01	-3.18	0.00
Grossman, 1999	-0.04	-0.05	-0.03	-13.75	0.00
Hamilton, 1997	0.00	-0.04	0.05	0.08	0.93
Harris, 2006	-0.02	-0.03	0.00	-1.58	0.11
Henderson, 2004	-0.11	-0.18	-0.05	-3.40	0.00
Kenkel, 1996	-0.06	-0.07	-0.04	-6.38	0.00
Laixuthai, 1993	-0.05	-0.06	-0.03	-5.42	0.00
Lyon, 1995	-0.05	-0.11	0.01	-1.71	0.09
Manning, 1995	-0.01	-0.03	0.00	-1.79	0.07
Pacula, 1998	-0.02	-0.05	0.00	-1.95	0.05
Sloan, 1995	0.09	0.08	0.10	17.73	0.00
Sutton, 1995	-0.04	-0.06	-0.01	-3.06	0.00
Wang, 1996	-0.09	-0.15	-0.03	-3.00	0.00
Waters, 1995	-0.18	-0.20	-0.16	-16.73	0.00
Williams, 2003	-0.01	-0.02	0.00	-2.03	0.04
Williams, 2004	-0.01	-0.02	0.00	-1.67	0.09
Williams, 2005	0.02	0.00	0.03	2.62	0.01
Yen, 1994	-0.10	-0.13	-0.07	-6.64	0.00
Zhao, 2004	0.00	-0.01	0.01	0.17	0.87
INDIVIDUAL-LEVEL STUDIES	*-0.03*	*-0.05*	*-0.02*	*-4.27*	*0.00*
Total w/o Rush, 1986	-0.06	-0.08	-0.04	-7.56	0.00
Total	*-0.06*	*-0.08*	*-0.05*	*-7.86*	*0.00*

Mean Elasticity: -0.51 n=91

Table 2 Effects of Price on Beer Consumption

Study	*r*	lower CI	upper CI	*Z*	*p*
Adrian, 1987	-0.30	-0.58	0.04	-1.72	0.08
Asplund, 2007	-0.23	-0.24	-0.22	-37.85	0.00
Beard, 1997	-0.20	-0.32	-0.07	-3.02	0.00
Bentzen, 1999	-0.02	-0.35	0.32	-0.11	0.91
Blake, 1997	-0.17	-0.46	0.15	-1.03	0.30
Bourgeois, 1979	-0.11	-0.23	0.02	-1.71	0.09
Clements, 1983	-0.61	-0.82	-0.27	-3.18	0.00
Clements, 1991	-0.36	-0.63	-0.01	-2.03	0.04
Duffy, 1982	-0.04	-0.33	0.26	-0.24	0.81
Duffy, 1983	0.17	-0.08	0.40	1.32	0.19
Duffy, 1987	-0.54	-0.79	-0.14	-2.55	0.01
Freeman, 2000	-0.04	-0.09	0.01	-1.68	0.09
Godfrey, 1988	-0.16	-0.52	0.25	-0.76	0.45
Hogarty, 1972	-0.21	-0.36	-0.05	-2.58	0.01
Holm, 1992	0.11	-0.13	0.33	0.89	0.37
Johnson, 1974	-0.23	-0.37	-0.08	-2.96	0.00
Johnson, 1977	-0.26	-0.41	-0.10	-3.15	0.00
Johnson, 1992	-0.12	-0.23	0.00	-1.94	0.05
Jones, 1989	-0.29	-0.48	-0.07	-2.60	0.01
Kubik, 2002	-0.09	-0.14	-0.04	-3.25	0.00
Kuo, 2003	-0.33	-0.35	-0.32	-36.20	0.00
Lee, 1992	-0.43	-0.68	-0.09	-2.46	0.01
Mast, 1999	-0.07	-0.16	0.03	-1.36	0.17
Nelson, 1990	-0.34	-0.57	-0.06	-2.39	0.02
Nelson, 1995	0.40	0.03	0.68	2.09	0.04
Nelson, 1997	-0.29	-0.50	-0.05	-2.37	0.02
Nelson, 2003	-0.21	-0.29	-0.12	-4.69	0.00
Norstrom, 2005	-0.39	-0.63	-0.09	-2.51	0.01
Ornstein, 1985	-0.15	-0.28	-0.01	-2.14	0.03
Saffer, 1989	-0.15	-0.28	-0.01	-2.06	0.04
Salisu, 1997	-0.18	-0.34	0.00	-1.99	0.05
Selvanathan, 1988	-0.34	-0.62	0.02	-1.85	0.06
Selvanathan, 1991	-0.33	-0.63	0.07	-1.61	0.11
Thom, 1984	0.55	0.31	0.72	4.09	0.00
Trolldal, 2005	-0.04	-0.11	0.03	-1.14	0.26
Uri, 1986	-0.30	-0.53	-0.02	-2.11	0.03
Walsh, 1970	0.03	-0.49	0.53	0.11	0.92
Walsh, 1982	-0.45	-0.74	-0.01	-1.99	0.05
Wette, 1993	-0.39	-0.65	-0.04	-2.20	0.03
Zhang, 1999	-0.33	-0.55	-0.06	-2.36	0.02
AGGREGATE-LEVEL	*-0.17*	*-0.22*	*-0.12*	*-6.91*	*0.00*
Angulo, 2001	-0.02	-0.03	0.00	-1.96	0.05
Coate, 1988	-0.04	-0.08	0.01	-1.51	0.13
Gao, 1995	-0.03	-0.06	0.00	-2.12	0.03
Grossman, 1987	-0.06	-0.13	0.01	-1.58	0.11
Heien, 1989	-0.01	-0.02	0.01	-0.78	0.44
Rabow, 1982	-0.06	-0.18	0.06	-1.00	0.32
Wang, 1996	-0.58	-0.62	-0.54	-22.60	0.00
INDIVIDUAL-LEVEL	*-0.12*	*-0.22*	*-0.02*	*-2.37*	*0.02*
Total w/o Thom, 1984	-0.18	-0.23	-0.12	-6.37	0.00
Total	*-0.17*	*-0.22*	*-0.11*	*-5.93*	*0.00*

Mean Elasticity: -0.46 n=105

Table 3: Effects of Price on Wine Consumption

Study	***r***	**lower CI**	**upper CI**	***Z***	***p***
Adrian, 1987	-0.48	-0.70	-0.18	-2.95	0.00
Asplund, 2007	-0.10	-0.11	-0.09	-15.91	0.00
Bentzen, 1999	-0.38	-0.63	-0.06	-2.27	0.02
Blake, 1997	0.01	-0.30	0.32	0.06	0.96
Bourgeois, 1979	0.04	-0.08	0.17	0.69	0.49
Clements, 1983	-0.47	-0.74	-0.07	-2.27	0.02
Clements, 1991	-0.36	-0.63	-0.01	-2.03	0.04
Duffy, 1982	-0.48	-0.67	-0.23	-3.49	0.00
Duffy, 1983	-0.23	-0.45	0.02	-1.80	0.07
Duffy, 1987	-0.67	-0.86	-0.34	-3.47	0.00
Godfrey, 1988	-0.42	-0.70	-0.03	-2.12	0.03
Holm, 1992	-0.30	-0.49	-0.07	-2.57	0.01
Johnson, 1974	-0.37	-0.50	-0.23	-4.86	0.00
Johnson, 1977	-0.46	-0.58	-0.32	-5.85	0.00
Johnson, 1992	-0.31	-0.41	-0.20	-5.25	0.00
Jones, 1989	-0.41	-0.58	-0.21	-3.82	0.00
Labys, 1976	-0.51	-0.79	-0.06	-2.20	0.03
Nelson, 1990	-0.45	-0.65	-0.19	-3.27	0.00
Nelson, 1995	0.34	-0.04	0.64	1.76	0.08
Nelson, 1997	-0.28	-0.49	-0.04	-2.25	0.02
Nelson, 2003	-0.24	-0.32	-0.15	-5.30	0.00
Norstrom, 2005	-0.27	-0.54	0.04	-1.71	0.09
Saffer, 1989	0.08	-0.06	0.22	1.12	0.26
Salisu, 1997	-0.20	-0.36	-0.02	-2.22	0.03
Selvanathan, 1988	-0.70	-0.84	-0.45	-4.55	0.00
Selvanathan, 1991	-0.41	-0.69	-0.02	-2.06	0.04
Thom, 1984	-0.06	-0.35	0.23	-0.43	0.67
Trolldal, 2005	0.00	-0.07	0.08	0.09	0.93
Uri, 1986	-0.66	-0.79	-0.47	-5.47	0.00
Walsh, 1982	-0.52	-0.78	-0.10	-2.39	0.02
Wette, 1993	-0.49	-0.72	-0.17	-2.90	0.00
Zhang, 1999	-0.35	-0.57	-0.08	-2.54	0.01
AGGREGATE-Level	*-0.30*	*-0.36*	*-0.23*	*-8.03*	*0.00*
Angulo, 2001	-0.02	-0.03	0.00	-1.96	0.05
Gao, 1995	-0.01	-0.04	0.02	-0.81	0.42
Grossman, 1987	-0.03	-0.10	0.04	-0.86	0.39
Heien, 1989	-0.02	-0.04	-0.01	-2.75	0.01
Rabow, 1982	0.01	-0.10	0.13	0.23	0.82
Wang, 1996	-0.64	-0.67	-0.61	-25.72	0.00
INDIVIDUAL-LEVEL	*-0.14*	*-0.26*	*-0.01*	*-2.08*	*0.04*
Total	*-0.25*	*-0.30*	*-0.19*	*-8.86*	*0.00*

Mean Elasticity: -0.69 n=93

Table 4: Effects of Price on Distilled Spirits Consumption

Study	***r***	**lower CI**	**upper CI**	***Z***	***p***
Adrian, 1987	-0.23	-0.52	0.12	-1.30	0.19
Asplund, 2007	-0.28	-0.29	-0.27	-45.49	0.00
Baltagi, 1990	-0.30	-0.54	-0.01	-2.04	0.04
Baltagi, 1995	-0.07	-0.13	0.00	-2.10	0.04
Baltagi, 2002	-0.08	-0.13	-0.03	-3.01	0.00
Beard, 1997	-0.29	-0.41	-0.17	-4.55	0.00
Bentzen, 1999	-0.42	-0.66	-0.10	-2.54	0.01
Blake, 1997	-0.18	-0.47	0.14	-1.12	0.26
Bourgeois, 1979	0.17	0.04	0.29	2.57	0.01
Clements, 1983	-0.71	-0.87	-0.42	-3.94	0.00
Clements, 1991	-0.77	-0.88	-0.58	-5.54	0.00
Cook, 1982	-0.19	-0.27	-0.10	-4.17	0.00
Duffy, 1982	-0.59	-0.75	-0.37	-4.54	0.00
Duffy, 1983	-0.43	-0.61	-0.20	-3.56	0.00
Duffy, 1987	-0.54	-0.79	-0.15	-2.58	0.01
Godfrey, 1988	-0.74	-0.88	-0.49	-4.49	0.00
Goel, 1995	-0.12	-0.19	-0.05	-3.47	0.00
Hoadley, 1984	-0.24	-0.48	0.02	-1.78	0.07
Holm, 1992	-0.08	-0.30	0.15	-0.68	0.50
Johnson, 1974	-0.48	-0.59	-0.35	-6.57	0.00
Johnson, 1977	-0.59	-0.69	-0.47	-7.91	0.00
Johnson, 1992	0.01	-0.11	0.12	0.10	0.92
Jones, 1989	-0.52	-0.66	-0.33	-5.01	0.00
McCornac, 1984	-0.18	-0.35	0.00	-1.99	0.05
Musgrave, 1988	-0.97	-0.99	-0.90	-6.08	0.00
Nelson, 1990	-0.34	-0.57	-0.07	-2.44	0.01
Nelson, 1995	0.48	0.13	0.73	2.58	0.01
Nelson, 1997	-0.48	-0.65	-0.27	-4.17	0.00
Nelson, 2003	0.02	-0.07	0.10	0.33	0.74
Norstrom, 2005	0.06	-0.26	0.36	0.34	0.74
Ornstein, 1985	-0.33	-0.45	-0.21	-4.99	0.00
Saffer, 1989	-0.31	-0.43	-0.18	-4.49	0.00
Salisu, 1997	-0.40	-0.54	-0.24	-4.69	0.00
Selvanathan, 1988	-0.51	-0.73	-0.19	-2.96	0.00
Selvanathan, 1991	-0.42	-0.70	-0.04	-2.14	0.03
Skog, 2006	-0.76	-0.90	-0.50	-4.28	0.00
Smith, 1976	-0.50	-0.69	-0.25	-3.61	0.00
Thom, 1984	0.31	0.02	0.55	2.11	0.04
Trolldal, 2005	-0.07	-0.14	0.00	-2.06	0.04
Uri, 1986	-0.29	-0.52	-0.01	-2.05	0.04
Wales, 1968	-0.21	-0.48	0.10	-1.34	0.18
Walsh, 1970	-0.29	-0.70	0.26	-1.03	0.30
Walsh, 1982	-0.58	-0.81	-0.18	-2.71	0.01
Wette, 1993	-0.04	-0.38	0.32	-0.20	0.84
Zhang, 1999	-0.03	-0.30	0.25	-0.19	0.85
AGGREGATE-LEVEL	*-0.29*	*-0.34*	*-0.23*	*-9.23*	*0.00*
Angulo, 2001	-0.02	-0.03	0.00	-1.96	0.05
Gao, 1995	-0.02	-0.05	0.01	-1.19	0.23
Grossman, 1987	-0.07	-0.14	0.00	-1.87	0.06
Heien, 1989	-0.02	-0.04	0.00	-2.53	0.01
Rabow, 1982	-0.03	-0.15	0.10	-0.43	0.67
Wang, 1996	-0.40	-0.45	-0.35	-14.48	0.00
INDIVIDUAL-LEVEL	*-0.10*	*-0.17*	*-0.02*	*-2.52*	*0.01*
Total w/o Musgrave, 1988	-0.24	-0.29	-0.19	-8.82	0.00
Total	*-0.25*	*-0.30*	*-0.20*	*-9.15*	*0.00*

Mean Elasticity -0.80 n=103

Table 5: Effects of Price on Heavy Alcohol Use (all individual-level studies)

Study	***r***	**lower CI**	**upper CI**	***Z***	***p***
Chaloupka, 1996	-0.01	-0.04	0.02	-0.77	0.44
Chaloupka, 1997	-0.01	-0.03	0.00	-2.14	0.03
Cook, 1994	-0.05	-0.09	0.00	-2.12	0.03
Keng, 2006	-0.01	-0.03	0.00	-2.70	0.01
Kenkel, 1993	-0.06	-0.11	-0.02	-2.72	0.01
Kenkel, 1996	0.01	-0.01	0.02	0.65	0.51
Laixuthai, 1993	-0.02	-0.04	0.00	-2.45	0.01
Powell, 2002	-0.02	-0.04	0.00	-2.58	0.01
Sloan, 1995	-0.03	-0.05	0.00	-2.32	0.02
Stout, 2000	0.01	0.00	0.02	2.69	0.01
Total	*-0.01*	*-0.03*	*0.00*	*-2.54*	*0.01*

Mean Elasticity: -0.28 n=10

Figure 1. Effects of Price on Alcohol Consumption: Aggregate-level Studies

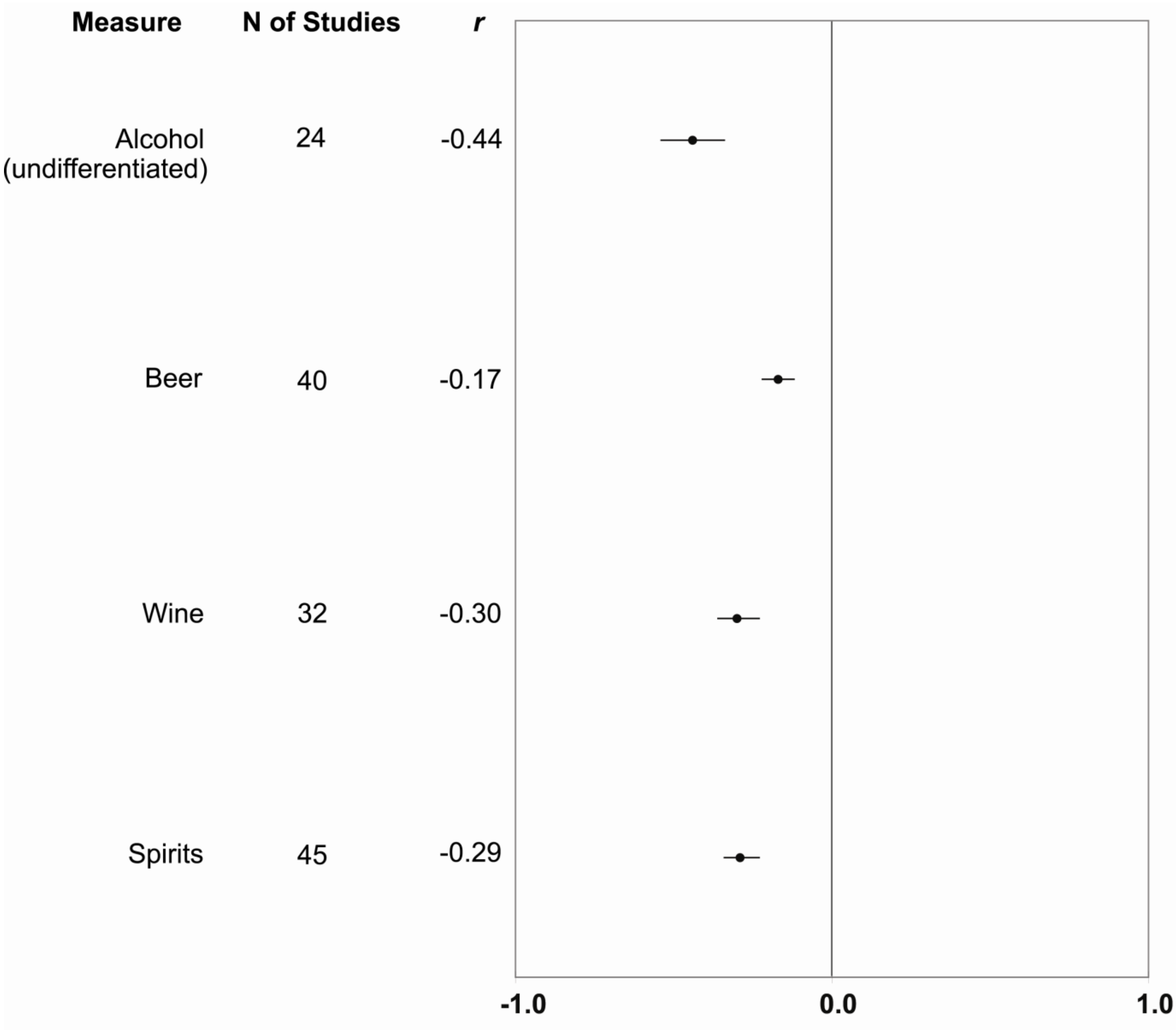
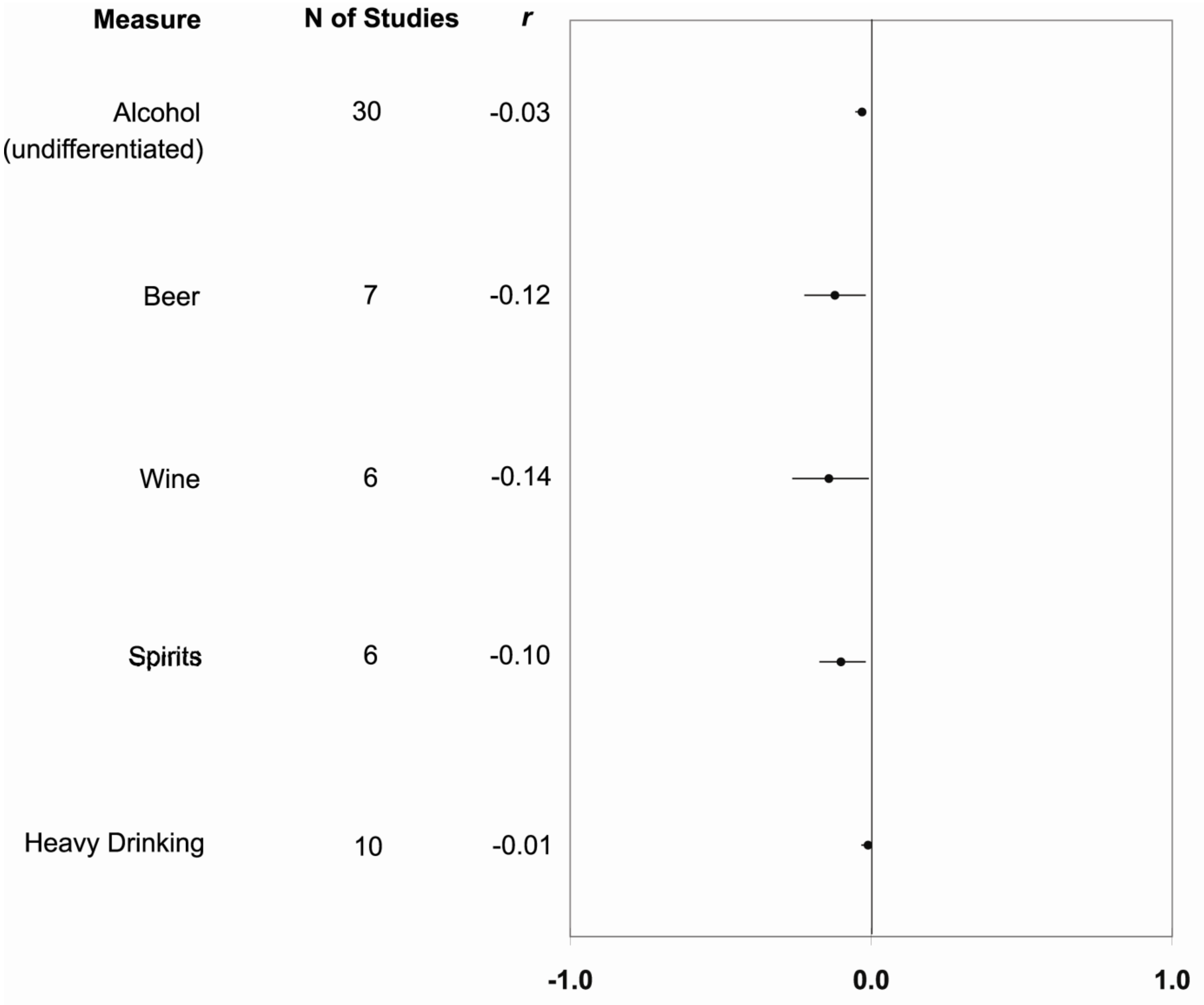


Figure 2. Effects of Price on Alcohol Consumption: Individual-level Studies



APPENDIX: STUDIES INCLUDED IN META-ANALYSIS

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