An Omitted Variable Bias Framework for Sensitivity Analysis of Instrumental Variables

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Carlos Cinelli* Chad Hazlett†

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Abstract

We develop an "omitted variable bias" framework for sensitivity analysis of instrumental variable (IV) estimates that is immune to "weak instruments," naturally handles multiple "side-effects" (violations of the exclusion restriction assumption) and "confounders" (violations of the ignorability of the instrument assumption), exploits expert knowledge to bound sensitivity parameters, and can be easily implemented with standard software. Conveniently, many pivotal conclusions regarding the sensitivity of the IV estimate (e.g. tests against the null hypothesis of zero causal effect) can be reached simply through separate sensitivity analyses of the effect of the instrument on the treatment (the "first stage") and the effect of the instrument on the outcome (the "reduced form"). More specifically, we introduce sensitivity statistics for routine reporting, such as *robustness values* for IV estimates, describing the minimum strength that omitted variables need to have to change the conclusions of an IV study. Next we provide visual displays that fully characterize the sensitivity of IV point-estimates and confidence intervals to violations of the standard IV assumptions. Finally, we offer formal bounds on the worst possible bias under the assumption that the maximum explanatory power of omitted variables are no stronger than a multiple of the explanatory power of observed variables. We apply our methods in a running example that uses instrumental variables to estimate the returns to schooling.

*PhD Candidate, Department of Statistics, University of California Los Angeles.
Email: carloscinelli@ucla.edu. URL: carloscinelli.com.

†Assistant Professor, Departments of Statistics and Political Science, University of California Los Angeles. Email: chazlett@ucla.edu. URL: chadhazlett.com.

1 Introduction

Unobserved confounding often complicates efforts to make causal claims from observational data (Pearl, 2009; Imbens and Rubin, 2015; Rosenbaum, 2017). Instrumental variable (IV) regression offers a powerful and widely used tool to address unobserved confounding, by exploiting "exogenous" sources of variation of the treatment (Wright, 1928; Bowden and Turkington, 1990; Angrist et al., 1996; Angrist and Pischke, 2009); IV methods have also become a vital tool in the analysis of randomized experiments with imperfect compliance (Robins, 1989; Balke and Pearl, 1994, 1997; Angrist et al., 1996). These qualities have made instrumental variables "a central part of the econometrics canon since the first half of the twentieth century" (Imbens, 2014, p.324). Beyond economics, instrumental variables are prominent tools in the arsenal of investigators seeking to make causal claims across the social sciences, epidemiology, medicine, genetics, and other fields (see e.g. Hernán and Robins, 2006; Didelez and Sheehan, 2007; Baiocchi et al., 2014; Burgess and Thompson, 2015).

Yet, IV methods carry their own set of demanding assumptions. Principally, conditionally on certain observed covariates, an instrumental variable must not itself be confounded with the outcome, and it should influence the outcome *only* by influencing uptake of the treatment. These assumptions can be violated by omitted confounders of the instrument-outcome association, and by omitted "side-effects" of the instrument, which then influence the outcome through channels other than through the treatment.[1] Although in certain cases the IV assumptions may entail testable implications (Pearl, 1995; Bonet, 2001; Swanson et al., 2018; Gunsilius, 2020; Kédagni and Mourifié, 2020), they are often unverifiable and must be defended by appealing to domain knowledge and theoretical arguments. Whether a given IV study identifies the causal effect of interest, then, turns on debates as to whether these assumptions hold.

Particularly in recent years, economists and other scholars have adopted a more skeptical posture towards IV methods, emphasizing the importance of both defending the credibility of these assumptions as well as assessing the consequences of its failures (see e.g., Deaton, 2009; Heckman and Urzua, 2010). For instance, recent extensive reviews of many instrumental variables widely-used in applied work, such as weather, religion, sibling structure or ethnolinguistic fractionalization, have cataloged several plausible violations of the exclusion restriction for such instruments (Gallen, 2020; Mellon, 2020). More worrisome, if the IV assumptions fail to hold, it is well known that the bias of the IV estimate may be *worse* than the original confounding bias of the simple regression estimate that the IV was supposed to address (Bound et al., 1995). Therefore, researchers are also advised to perform *sensitivity analyses* to assess the degree of violation of the IV assumptions that would be required to alter the conclusions of an IV study. Although a variety of sensitivity methods for IV have been proposed (DiPrete and Gangl, 2004; Altonji et al., 2005; Small, 2007; Small and Rosenbaum, 2008; Conley et al., 2012; Wang et al., 2018; Jiang et al., 2018; Cinelli et al., 2019), such sensitivity analyses

[1] In the recent IV literature, the first assumption is usually called *exogeneity*, *ignorability*, *unconfoundedness* or *independence* of the instrument, whereas the second assumption is called the *exclusion* restriction (Angrist and Pischke, 2009; Pearl, 2009; Imbens and Rubin, 2015; Swanson et al., 2018). In earlier econometric works, these two assumptions were often combined into one, also labeled the "exclusion restriction" (Imbens, 2014).

are still rare in practice.

In this paper, we develop an omitted variable bias (OVB) framework for assessing the sensitivity of IV estimates against violations of its underlying assumptions.[2] Building on recent results of OVB for ordinary least squares (OLS) estimates (Cinelli and Hazlett, 2020), we develop a suite of sensitivity analysis tools for IV that: (i) has correct test size (or confidence interval coverage) regardless of instrument strength; (ii) naturally handles violations due to multiple "side-effects" and "confounders;" (iii) exploits expert knowledge to bound sensitivity parameters; and, (iv) can be easily implemented with standard software.

We first introduce two sensitivity statistics for IV estimates: (i) the *robustness value* describes the minimum strength of association (in terms of partial R^2) that omitted variables (side-effects or confounders) need to have, both with the instrument and with the untreated potential outcome, such that they are capable of changing the conclusions of the study; and (ii) the *extreme robustness value*, which describes the minimal strength of association that omitted variables need to have with the *instrument alone* (regardless of their association with the untreated potential outcome) in order to be problematic. We propose the routine reporting of those quantities to improve the transparency and facilitate the assessment of the credibility of IV studies. Next, we offer intuitive graphical tools for investigators to assess how postulated confounding of any degree would alter the IV hypothesis tests, as well as lower or upper limits of confidence intervals. Finally, these tools can be supplemented with formal bounds on the worst possible bias that side-effects or confounders could cause, under the assumption that the maximum explanatory power of these omitted variables are no stronger than a multiple of the explanatory power of one or more observed variables.

Conveniently, considering that investigators are already advised to carefully examine their "first stage" (the effect of the instrument on the treatment) and "reduced form" (the effect of the instrument on the outcome) (e.g. Angrist and Krueger, 2001; Angrist and Pischke, 2009), we show that many pivotal conclusions regarding the sensitivity of the IV estimate can in fact be reached simply through separate sensitivity analyses of these two familiar auxiliary OLS estimates. First, if researchers are interested in the null hypothesis of *zero effect,* all the OVB tools developed for OLS in Cinelli and Hazlett (2020) and Cinelli et al. (2020) can simply be directly applied to the reduced-form regression, and confounders or side-effects shown to be problematic there are equally problematic for IV. Second, if interest lies in assessing not just the null of zero, but biases that bring the estimate partway to zero or beyond it, then the robustness of the IV estimate formally reduces to the minimum of the robustness of the reduced-form and the robustness of the first-stage regressions.

[2] We focus on the "just-identified" case with one treatment and one instrument. One reason for our focus is that a thorough consideration of the identification assumptions and how they may be violated is already complicated enough with a single instrument (Angrist and Pischke, 2009). Second, and relatedly, in most applied settings, the single-instrument and single-treatment setup is the most common. For example, in a broad review of papers in the *American Economic Review* and 15 other journals of the *American Economic Association*, Young (2018) finds that 80% of IV regressions were of this type. Finally, in many "multiple instrument" studies, it is not uncommon for researchers to also report and give special focus to the analysis of their "best" instrument (Angrist and Pischke, 2009), or to combine multiple instruments into a single instrument, such as, for example, constructing an allele score in Mendelian Randomization (Burgess and Thompson, 2015). Extension of the tools we develop here to the scenario with multiple instruments and treatments is object of future investigations.

A final contribution of this paper is that, while developing OVB tools for IV, we extended the OVB results of Cinelli and Hazlett (2020) providing a new way to perform sensitivity analysis that simply replaces a conventional critical value (e.g. 1.96) with a novel "OVB-adjusted" critical value that accounts for a postulated degree of omitted variable bias. These new critical values depend only on the hypothetical partial R^2 of the omitted variables with the dependent and independent variables of the OLS regression. Researchers can thus easily perform sensitivity analysis with *any standard regression software* by substituting traditional thresholds with OVB-adjusted thresholds, when testing a particular null hypothesis, or when constructing confidence intervals. We believe the extreme simplicity of implementing this approach will further aid in the widespread adoption of sensitivity analysis in applied work.

In what follows, Section 2 introduces the running example and provides the essential background on the main IV estimators, all of which depend upon OLS. Next, Section 3 refines and extends the OVB framework of Cinelli and Hazlett (2020), which not only improves the sensitivity tools for OLS, but greatly simplifies the analysis for the IV setting. Section 4 then develops an OVB framework for IV, first showing what can be gleaned from the first-stage and reduced-form regressions alone, then establishing the necessary OVB-type results in the Anderson-Rubin approach. Section 5 returns to our running example to show how these results can can be deployed in practice. Finally, we offer concluding remarks in Section 6. Open-source software for `R` and `Stata` implements the methods discussed in this paper.[3]

2 Background

In this section we introduce the running example and use it to briefly review the required background on instrumental variables and the main approaches to IV estimation.

2.1 Running example: estimating the returns to schooling

Ordinary least squares and the OVB problem

Many observational studies have established a positive and large association between educational achievement and earnings using regression analysis (Card, 1999). Here we consider the work of Card (1993), which employed a sample of 3,010 individuals from the National Longitudinal Survey of Young Men (NLSYM). Considering the following multivariate linear regression

$$\text{Earnings} = \hat{\tau}_{\text{OLS,res}}\text{Education} + \boldsymbol{X}\hat{\beta}_{\text{OLS,res}} + \hat{\varepsilon}_{\text{OLS,res}} \tag{1}$$

where *Earnings* measures the log transformed hourly wages of the individual,[4] *Education* is an integer-valued variable indicating the completed years of education of the individual and the matrix

[3] Sensitivity analysis of the reduced form, first stage, and Anderson-Rubin regression for a specific null hypothesis can already be performed using the `R` and `Stata` package `sensemakr` (Cinelli et al., 2020). Additional functionality, such as contour plots with lower and upper limits of the Anderson-Rubin confidence interval, is forthcoming.

[4] In this case, regression coefficients can be conveniently interpreted, approximately, as percent changes in earnings.

$\boldsymbol{X}$ comprises race, experience, and a set of regional factors, Card concluded that each additional year of schooling was associated with approximately 7.5% higher wages (i.e, $\hat{\tau}_{\text{OLS,res}} \approx 0.075$) (see column "OLS" of Table 1).

Educational achievement, however, is not randomly assigned; perhaps individuals who obtain more education have higher wages due to other reasons, such as coming from wealthier families, or having higher levels of some unobserved characteristic, such as "ability" or "motivation." If data on these variables were available, then multivariate regression, further adjusting for such variables, would be able to capture the causal effect of educational attainment on schooling, as in

$$\text{Earnings} = \hat{\tau}_{\text{OLS}}\text{Education} + \boldsymbol{X}\hat{\beta}_{\text{OLS}} + \boldsymbol{U}\hat{\gamma}_{\text{OLS}} + \hat{\varepsilon}_{\text{OLS}} \tag{2}$$

where $\boldsymbol{U}$ denotes a set of variables that, along with $\boldsymbol{X}$, is sufficient to eliminate confounding concerns (i.e, the set $\{\boldsymbol{X}, \boldsymbol{U}\}$ is sufficient for making conditional ignorability hold). Such detailed information on individuals, however, is not available, and researchers will not even agree upon which variables $\boldsymbol{U}$ are needed. In the absence of such variables, regression estimates that adjust for only a partial list of characteristics (such as $\boldsymbol{X}$) may suffer from "omitted variable bias" (Angrist and Pischke, 2009; Cinelli and Hazlett, 2020) and are likely to overestimate the "true" returns to schooling.

Instrumental variables as a solution to the OVB problem

Instrumental variable methods offer an alternative route to estimate the causal effect of schooling on earnings without having data on the unobserved variables $\boldsymbol{U}$. The key for such methods to work is to find a new variable (the "instrument") that changes the incentives to educational achievement, but is associated with earnings *only through* its effect on education.

To that end, Card (1993) proposed exploiting the role of geographic differences in college accessibility. In particular, consider the variable *Proximity*, encoding an indicator of whether the individual grew up in an area with a nearby accredited 4-year college. Students who grow up far from the nearest college may face higher educational costs, discouraging them from pursuing higher level studies. Next, and most importantly, Card (1993) argues that, conditional on the set of observed variables $\boldsymbol{X}$ (available on the NLSYM), whether one lives near a college is not itself confounded with earnings, nor does proximity to college affect earnings apart from its effect on years of education.

If we believe such assumptions hold it is possible to recover a valid estimate of the (local) average treatment effect of *Education* on *Earnings* by simply taking the ratio of two OLS coefficients, one measuring the effect of *Proximity* on *Earnings*, and another measuring the effect of *Proximity* on

	Dependent variable:			
	Education	Earnings (log)		
	FS	RF	OLS	IV
	(1)	(2)	(3)	(4)
Proximity	0.320***	0.042**		
	(0.088)	(0.018)		
Education			0.075***	0.132**
			(0.003)	(0.055)
Black	−0.936***	−0.270***	−0.199***	−0.147***
	(0.094)	(0.019)	(0.018)	(0.054)
SMSA	0.402***	0.165***	0.136***	0.112***
	(0.105)	(0.022)	(0.020)	(0.032)
Other covariates	yes	yes	yes	yes
Observations	3,010	3,010	3,010	3,010
R^2	0.477	0.195	0.300	0.238
Note:				*p<0.1; **p<0.05; ***p<0.01

Table 1: Results of Card (1993). Columns show estimates and standard errors (in parenthesis) of the First Stage (FS), Reduced Form (RF), Ordinary Least Squares (OLS) and Two-Stage Least Squares (IV).

Education.[5] More precisely, consider the two OLS models

$$\text{Education} = \hat{\theta}_{\text{res}}\text{Proximity} + \boldsymbol{X}\hat{\psi}_{\text{res}} + \hat{\varepsilon}_{d,\text{res}} \tag{3}$$

$$\text{Earnings} = \hat{\lambda}_{\text{res}}\text{Proximity} + \boldsymbol{X}\hat{\beta}_{\text{res}} + \hat{\varepsilon}_{y,\text{res}} \tag{4}$$

Throughout the paper we refer to these equations as the "first stage" (Equation 3) and the "reduced form" (Equation 4), as these are now common usage (Angrist and Pischke, 2009, 2014; Imbens and Rubin, 2015; Andrews et al., 2019).[6] The results of both regressions are also shown in Table 1 (columns "FS" and "RF").

The coefficient for *Proximity* on the first-stage regression, $\hat{\theta}_{\text{res}} \approx 0.32$, reveals that those who grew up near a college indeed have higher educational attainment, having completed an additional 0.32 years of education, on average. Likewise, the coefficient for *Proximity* on the reduced-form regression, $\hat{\lambda}_{\text{res}} \approx 0.042$, suggests that those who grew up near a college have 4.2% higher earnings. The IV estimate is then given by the ratio of these two coefficients,

$$\hat{\tau}_{\text{res}} := \frac{\hat{\lambda}_{\text{res}}}{\hat{\theta}_{\text{res}}} \approx \frac{0.042}{0.319} \approx 0.132 \tag{5}$$

The value of $\hat{\tau}_{\text{res}} \approx 0.132$ suggests that, contrary to the OLS estimate of 7.5%, and perhaps surprisingly, each additional year of schooling instead raises wages by much more—13.2% (Table 1, column "IV").

The IV estimate itself may suffer from OVB

The previous IV estimate relies on the assumption that, conditional on $\boldsymbol{X}$, *Proximity* and *Earnings* are unconfounded, and the effect of *Proximity* on *Earnings* must go entirely through *Education*. As it is often the case, neither assumption is easy to defend in this setting. First, some of the same factors that might confound the relationship between *Education* and *Earnings* could similarly confound the relationship of *Proximity* and *Earnings* (e.g. family wealth or family connections). Second, as argued in Card (1993), the presence of a college nearby may be associated with high school quality, which in its turn also affects earnings. Finally, other geographic confounders can make some localities likely to both have colleges nearby and lead to higher earnings. These are only coarsely conditioned on by the observed regional indicators, and residual biases may still remain.

In sum, instead of adjusting only for $\boldsymbol{X}$ as in the previous Equations 4 and 3, we should have

[5] This identification result requires further functional restrictions on the data-generating process, such as linearity or monotonicity. Conditions that allow a causal interpretation of the IV estimand are extensively discussed elsewhere, and will not be reviewed here. See Angrist et al. (1996), Angrist and Pischke (2009) and Imbens (2014) for further discussion.

[6] Though now well established, these labels abuse the original meaning of the terminology, since both regressions are in their "reduced form." Equation 3 is called the "first stage" due to its operational role on two-stage least squares estimation, as we see next. See also Imbens (2014) and Andrews et al. (2019).

adjusted for *both* the observed covariates $\boldsymbol{X}$ *and unobserved* covariates $\boldsymbol{W}$ as in

$$\text{Education} = \hat{\theta}\text{Proximity} + \boldsymbol{X}\hat{\psi} + \boldsymbol{W}\hat{\delta} + \hat{\varepsilon}_d \tag{6}$$

$$\text{Earnings} = \hat{\lambda}\text{Proximity} + \boldsymbol{X}\hat{\beta} + \boldsymbol{W}\hat{\gamma} + \hat{\varepsilon}_y \tag{7}$$

Where $\boldsymbol{W}$ stands for all unobserved factors necessary to make *Proximity* a valid instrument for the effect of *Education* on *Earnings* (e.g, *Family Wealth*, *High School Quality*, *Place of Residence*, etc). The IV estimate we wished we had is then given by

$$\hat{\tau} := \frac{\hat{\lambda}}{\hat{\theta}} \tag{8}$$

Our previous estimate $\hat{\tau}_{\text{res}}$ deviates from the target estimate $\hat{\tau}$, but how badly? How strong would the omitted variables $\boldsymbol{W}$ have to be so that it would change our research conclusions? To develop a precise algebraic answer to this question, we must first review the mechanics of the main approaches to IV estimation.

2.2 The mechanics of IV estimation

Let the random variable Y_i denote the outcome, D_i the treatment, Z_i the instrumental variable, $\boldsymbol{X}_i = [\boldsymbol{X}_{i1}, \dots, \boldsymbol{X}_{ip}]$ a vector of p observed covariates, and $\boldsymbol{W}_i = [\boldsymbol{W}_{i1}, \dots, \boldsymbol{W}_{il}]$ a vector of l unobserved covariates for an individual. The target quantity of IV estimation consists of a ratio of two *population* regression coefficients,

$$\tau := \frac{\lambda}{\theta} \tag{9}$$

where θ is the population regression coefficient of Z_i on D_i (the first stage) and λ the population regression coefficient of Z_i on Y_i (the reduced form), both adjusting for $\boldsymbol{X}_i$ and $\boldsymbol{W}_i$. We call the ratio τ the *IV estimand*. Here we briefly review the commonly used approaches to make inferences regarding this ratio.

2.2.1 Indirect Least Squares and Two-Stage Least Squares

Throughout the paper we consider exact algebraic results that holds for sample estimates. Denote by Y the $(n \times 1)$ *vector* of the outcome of interest with n observations; by D the $(n \times 1)$ treatment vector; by Z the $(n \times 1)$ vector of the instrument; by $\boldsymbol{X}$ an $(n \times p)$ *matrix* of observed covariates (including a constant), and by $\boldsymbol{W}$ an $(n \times l)$ matrix of *unobserved* covariates.

Indirect Least Squares. The first and perhaps most straightforward approach to instrumental variable estimation was outlined above: run two OLS models capturing the effect of the instrument on the treatment (first stage) and the effect of the instrument on the outcome (reduced form),

$$\textbf{First stage:} \quad D = \hat{\theta} Z + \boldsymbol{X}\hat{\psi} + \boldsymbol{W}\hat{\delta} + \hat{\varepsilon}_d \tag{10}$$

$$\textbf{Reduced form:} \quad Y = \hat{\lambda} Z + \boldsymbol{X}\hat{\beta} + \boldsymbol{W}\hat{\gamma} + \hat{\varepsilon}_y \tag{11}$$

Where $\hat{\theta}$, $\hat{\psi}$ and $\hat{\delta}$ are the OLS estimates of the regression of D on Z, $\boldsymbol{X}$ and $\boldsymbol{W}$, and $\hat{\varepsilon}_d$ its corresponding residuals; analogously, $\hat{\lambda}$, $\hat{\beta}$ and $\hat{\gamma}$ are the OLS estimates of the regression of Y on Z, $\boldsymbol{X}$ and $\boldsymbol{W}$, and $\hat{\varepsilon}_y$ its corresponding residuals. The estimator for τ is constructed by simply using the plug-in principle and taking the ratio of $\hat{\lambda}$ and $\hat{\theta}$

$$\hat{\tau}_{\text{ILS}} := \frac{\hat{\lambda}}{\hat{\theta}} \tag{12}$$

The ratio $\hat{\tau}_{\text{ILS}}$ may be called the *indirect least squares* (ILS) estimator, or the "ratio of coefficients" estimator. Inference in the ILS framework can be performed using the delta-method, resulting in the *estimated* variance

$$\widehat{\text{var}}(\hat{\tau}_{\text{ILS}}) := \frac{1}{\hat{\theta}^2}\left(\widehat{\text{var}}(\hat{\lambda}) + \hat{\tau}_{\text{ILS}}^2 \widehat{\text{var}}(\hat{\theta}) - 2\hat{\tau}_{\text{ILS}}\widehat{\text{cov}}(\hat{\lambda}, \hat{\theta})\right) \tag{13}$$

Where $\widehat{\text{var}}(\hat{\lambda})$, $\widehat{\text{var}}(\hat{\theta})$ and $\widehat{\text{cov}}(\hat{\lambda}, \hat{\theta})$ are the usual OLS variance and covariance estimates (see appendix).

Two-Stage Least Squares. A closely related approach for instrumental variable estimation is denoted by "two-stage least squares" (2SLS). As its name suggests, this involves two nested steps of OLS estimation: a first-stage regression given by Equation 10 to produce fitted values for the treatment ($\widehat{D}$), then regressing the outcome on these fitted values,

$$\textbf{Second stage:} \quad Y = \hat{\tau}_{\text{2SLS}}\widehat{D} + \boldsymbol{X}\hat{\beta}_{\text{2SLS}} + \boldsymbol{W}\hat{\gamma}_{\text{2SLS}} + \hat{\varepsilon}_{\text{2SLS}} \tag{14}$$

The 2SLS estimate corresponds to the coefficient $\hat{\tau}_{\text{2SLS}}$ in Equation 14, called the "second-stage" regression. By the Frisch-Waugh-Lovell (FWL) theorem (Frisch and Waugh, 1933; Lovell, 1963, 2008), one can readily show that $\hat{\tau}_{\text{2SLS}}$ and $\hat{\tau}_{\text{ILS}}$ are numerically identical,

$$\hat{\tau}_{\text{2SLS}} = \frac{\text{cov}(Y^{\perp \boldsymbol{X},\boldsymbol{W}}, \widehat{D}^{\perp \boldsymbol{X},\boldsymbol{W}})}{\text{var}(\widehat{D}^{\perp \boldsymbol{X},\boldsymbol{W}})} = \frac{\hat{\theta} \times \text{cov}(Y^{\perp \boldsymbol{X},\boldsymbol{W}}, Z^{\perp \boldsymbol{X},\boldsymbol{W}})}{\hat{\theta}^2 \times \text{var}(Z^{\perp \boldsymbol{X},\boldsymbol{W}})} = \frac{\hat{\lambda}}{\hat{\theta}} \tag{15}$$

Where $Y^{\perp \boldsymbol{X},\boldsymbol{W}}$,$\widehat{D}^{\perp \boldsymbol{X},\boldsymbol{W}}$ and $D^{\perp \boldsymbol{X},\boldsymbol{W}}$ denote the variables Y, $\widehat{D}$ and D after removing the components linearly explained by $\boldsymbol{X}$ and $\boldsymbol{W}$ and $\text{cov}(\cdot)$ and $\text{var}(\cdot)$ denote the *sample* covariance and variance of those variables. As with ILS, inference in 2SLS is performed by resorting to the asymptotic normality of the ratio, with estimated variance

$$\widehat{\text{var}}(\hat{\tau}_{\text{2SLS}}) := \frac{\text{var}(Y^{\perp \boldsymbol{X},\boldsymbol{W}} - \hat{\tau}_{\text{2SLS}} D^{\perp \boldsymbol{X},\boldsymbol{W}})}{\text{var}(\widehat{D}^{\perp \boldsymbol{X},\boldsymbol{W}})} \times \text{df}^{-1} \tag{16}$$

Where df denotes the appropriate degrees of freedom. Using the FWL theorem one can further show that $\widehat{\text{var}}(\hat{\tau}_{\text{2SLS}})$ and $\widehat{\text{var}}(\hat{\tau}_{\text{ILS}})$ are also numerically identical (see appendix).

2.2.2 Anderson-Rubin regression and Fieller's theorem

The methods of ILS and 2SLS make use of a normal approximation to the sampling distribution of the ratio $\hat{\lambda}/\hat{\theta}$, which may prove unreliable when θ is "close" to zero, relative to the sampling variability of $\hat{\theta}$—this is known as the "weak instrument" problem. Two alternatives that allow constructing confidence intervals with correct coverage, regardless of the "strength" of the first stage, are the proposals of Anderson and Rubin (1949) and Fieller (1954) (e.g. see Andrews et al., 2019).

Anderson-Rubin. The Anderson-Rubin approach starts by creating the random variable $Y_{\tau_0} := Y - \tau_0 D$ in which we subtract from Y a "putative" causal effect of D, namely, τ_0. If Z is a valid instrument, under the null hypothesis $H_0 : \tau = \tau_0$, we should not see an association between Y_{τ_0} and Z, conditional on $\boldsymbol{X}$ and $\boldsymbol{W}$. In other words, if we run the OLS model

$$\textbf{Anderson-Rubin:} \quad Y_{\tau_0} = \hat{\phi}_{\tau_0} Z + \boldsymbol{X}\hat{\beta}_{\tau_0} + \boldsymbol{W}\hat{\gamma}_{\tau_0} + \hat{\varepsilon}_{\tau_0} \tag{17}$$

we should find that $\hat{\phi}_{\tau_0}$ is equal to zero, but for sampling variation. To test the null hypothesis $H_0 : \phi_{\tau_0} = 0$ in the Anderson-Rubin regression is thus equivalent to test the null hypothesis $H_0 : \tau = \tau_0$. The $1 - \alpha$ confidence interval is constructed by collecting all values τ_0 such that the null hypothesis $H_0 : \phi_{\tau_0} = 0$ is not rejected at the chosen significance level α:

$$\text{CI}_{1-\alpha}(\tau) := \{\tau_0;\ t^2_{\hat{\phi}_{\tau_0}} \leq t^{*2}_{\alpha,\text{df}}\} \tag{18}$$

Where $t_{\hat{\phi}_{\tau_0}}$ is the t-value of the coefficient $\hat{\phi}_{\tau_0}$, and $t^*_{\alpha,\text{df}}$ the usual α level critical threshold for the t statistic, with the appropriate degrees of freedom. It is also convenient to define the point estimate $\hat{\tau}_{\text{AR}}$ as the value τ_0 which makes $\hat{\phi}_{\tau_0}$ exactly equal to zero

$$\hat{\tau}_{\text{AR}} := \{\tau_0;\ \hat{\phi}_{\tau_0} = 0\} \tag{19}$$

By the FWL theorem, we can write $\hat{\phi}_{\tau_0}$ as a linear combination of $\hat{\lambda}$ and $\hat{\theta}$,

$$\hat{\phi}_{\tau_0} = \frac{\text{cov}(Y^{\perp \boldsymbol{X},\boldsymbol{W}} - \tau_0 D^{\perp \boldsymbol{X},\boldsymbol{W}}, Z^{\perp \boldsymbol{X},\boldsymbol{W}})}{\text{var}(Z^{\perp \boldsymbol{X},\boldsymbol{W}})} = \hat{\lambda} - \tau_0\hat{\theta} \tag{20}$$

Thus resulting in $\hat{\tau}_{AR} = \frac{\hat{\lambda}}{\hat{\theta}}$, a point estimate numerically identical to the previous estimators.

Fieller's theorem. The connection between Fieller's theorem and the Anderson-Rubin approach follows from Equation 20. The central test statistic of Fieller's theorem is precisely the linear combination $\hat{\phi}_{\tau_0} = \hat{\lambda} - \tau_0\hat{\theta}$. Under the null hypothesis $H_0 : \tau = \tau_0$, if the estimators $\hat{\lambda}$ and $\hat{\theta}$ are asymptotically normal, it follows that $\hat{\phi}_{\tau_0}$ is also asymptotically normal with mean zero, and estimated variance

$$\widehat{\text{var}}(\hat{\phi}_{\tau_0}) := \widehat{\text{var}}(\hat{\lambda}) + \tau_0^2\widehat{\text{var}}(\hat{\theta}) - 2\tau_0\widehat{\text{cov}}(\hat{\lambda}, \hat{\theta}) \tag{21}$$

Confidence intervals are then constructed exactly as in Equation 18, and the two approaches are numerically identical (see appendix).

2.3 Problem statement

As we have seen, all main approaches for IV estimation result in the same point estimate—the ratio of the reduced-form and first-stage regression coefficients. They differ only in how to perform inference, with ILS/2SLS resorting to the asymptotic normality of the ratio estimator, and the Anderson-Rubin/Fieller approach inverting the test of the linear combination of both coefficients.

	Restricted IV regressions (omitting $\boldsymbol{W}$)	Full IV regressions (including $\boldsymbol{W}$)
First stage	$D = \hat{\theta}_{\text{res}}Z + \boldsymbol{X}\hat{\psi}_{\text{res}} + \hat{\varepsilon}_{d,\text{res}}$	$D = \hat{\theta}Z + \boldsymbol{X}\hat{\psi} + \boldsymbol{W}\hat{\delta} + \hat{\varepsilon}_d$
Reduced form	$Y = \hat{\lambda}_{\text{res}}Z + \boldsymbol{X}\hat{\beta}_{\text{res}} + \hat{\varepsilon}_{y,\text{res}}$	$Y = \hat{\lambda}Z + \boldsymbol{X}\hat{\beta} + \boldsymbol{W}\hat{\gamma} + \hat{\varepsilon}_y$
Anderson-Rubin	$Y_{\tau_0} = \hat{\phi}_{\tau_0,\text{res}}Z + \boldsymbol{X}\hat{\beta}_{\tau_0,\text{res}} + \hat{\varepsilon}_{\tau_0,\text{res}}$	$Y_{\tau_0} = \hat{\phi}_{\tau_0}Z + \boldsymbol{X}\hat{\beta}_{\tau_0} + \boldsymbol{W}\hat{\gamma}_{\tau_0} + \hat{\varepsilon}_{\tau_0}$

Table 2: The omitted variable bias problem for instrumental variable regressions.

The regression equations discussed in Section 2.2, summarized in the third column of Table 2, stand for the IV regressions our analyst *wished* she had run, adjusting for both $\boldsymbol{X}$ and $\boldsymbol{W}$. However, since $\boldsymbol{W}$ is *unobserved*, the investigator is forced to run instead the restricted models in the second column of Table 2. Our task is thus to characterize how point estimates and confidence intervals for the IV estimate, given by these regressions, would have changed due to the inclusion of $\boldsymbol{W}$. Since, at their core, all these IV approaches rely on OLS estimation, we should be able to leverage all OVB tools for OLS (Cinelli and Hazlett, 2020) for examining the sensitivity of IV.

A note on identification with instrumental variables

Before proceeding, it is worth making a brief note on the identification of causal effects using instrumental variables. There are many different sets of assumptions that allow different causal interpretations of the IV estimand given by Equation 9 Angrist et al. (1996); Brito and Pearl (2002); Pearl (2009); Swanson et al. (2018). The causal diagram of Figure 1 shows some of the most used "canonical" models illustrating the main traditional assumptions of IV. Equivalent assumptions can be articulated in the potential outcomes framework Pearl (2009); Swanson et al. (2018). Beyond those assumptions of exclusion and independence restrictions, some functional constraint is also

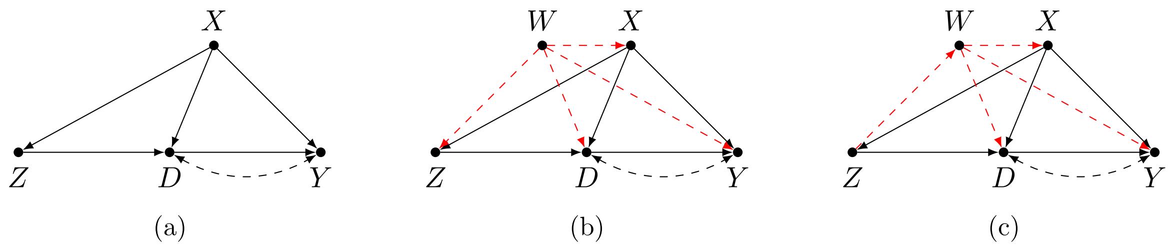


Figure 1: Causal diagrams illustrating the traditional IV assumptions. In Figure 1a, X is sufficient for rendering Z a valid instrumental variable. In Figures 1b and 1c, however, W is also needed to render Z a valid IV (in Figure 1b W is a confounder of the instrument-outcome relationship, whereas in Figure 1c W is a side-effect of the instrument). Graphically, conditional on a set of covariates $\{X, W\}$, a variable Z is a valid instrument for the causal effect of a treatment D on an outcome Y, if the set $\{X, W\}$ blocks all paths from Z to Y on the graph where the edge $D \rightarrow Y$ is removed Brito and Pearl (2002).

needed for point-identification. For instance, under certain assumptions of effect homogeneity (e.g., linearity), the IV estimand can be interpreted as the average treatment effect; another widely used example is the binary setting with the assumption of monotonicity, in which case the IV estimand can be interpreted as a local average treatment effect Angrist et al. (1996); Angrist and Pischke (2009); Swanson et al. (2018). Here we do not commit to a specific causal interpretation, and simply assume the researcher is interested in the IV estimand of Equation 9, adjusting for both $\boldsymbol{X}$ and $\boldsymbol{W}$. All sensitivity results we present here are thus valid for *any* set of IV assumptions, so long as the resulting estimand is still given by Equation 9.

3 Omitted variable bias with the partial R^2 parameterization

In this section, we extend the results of Cinelli and Hazlett (2020) regarding the partial R^2 parameterization of the OVB formula for OLS. In particular, we introduce the notion of *OVB-adjusted* critical values, and show how sensitivity analysis can be performed by simply substituting traditional critical values with the adjusted ones. We also introduce the idea of a set of compatible inferences given bounds on the strength of confounding, and formalize sensitivity statistics for routine reporting as answering an inverse question regarding those sets. These extensions are not only useful for the sensitivity of OLS estimates themselves, but will greatly simplify the generalization of these results to the IV setting in Section 4. To fix ideas, here we discuss the OVB framework in the context of the reduced-form regression coefficient, but the reader should have in mind that all results presented here are algebraic, and hold for *any* OLS estimate.

3.1 Sensitivity in an omitted variable bias framework

The OVB framework starts with a target coefficient obtained from a *full* regression equation that the analyst wished she could have estimated (such as those in the third column of Table 2). For concreteness, suppose we are interested in the coefficient $\hat{\lambda}$ of the regression equation of the outcome Y on the instrument Z, adjusting for a set of observed covariates $\boldsymbol{X}$ and a single *unobserved* covariate W (we generalize to multivariate W below),

$$Y = \hat{\lambda} Z + \boldsymbol{X}\hat{\beta} + \hat{\gamma} W + \hat{\varepsilon}_y \tag{22}$$

However, when W is unobserved, estimating the full regression equation is infeasible. Instead, the investigator is forced to estimate the *restricted* model given by

$$Y = \hat{\lambda}_{\text{res}} Z + \boldsymbol{X}\hat{\beta}_{\text{res}} + \hat{\varepsilon}_{y,\text{res}} \tag{23}$$

Where $\hat{\lambda}_{\text{res}}$ and $\hat{\beta}_{\text{res}}$ are the coefficients of the restricted OLS adjusting for Z and $\boldsymbol{X}$ alone, and $\hat{\varepsilon}_{y,\text{res}}$ its corresponding residual. The OVB framework seeks to answer the following question: how do the inferences for λ_{res} from the restricted OLS model (omitting W), compare with the inferences for λ from the full OLS model (adjusting for W)?

3.1.1 Adjusted estimates and standard errors

Let $R^2_{Y\sim W|Z,\boldsymbol{X}}$ denote the partial R^2 of W with Y, after controlling for Z and $\boldsymbol{X}$, and let $R^2_{Z\sim W|\boldsymbol{X}}$ denote the partial R^2 of W with Z after adjusting for $\boldsymbol{X}$. Given the estimates of the restricted model, $\hat{\lambda}_{\text{res}}$ and $\widehat{\text{se}}(\hat{\lambda}_{\text{res}})$, the values $R^2_{Y\sim W|Z,\boldsymbol{X}}$ and $R^2_{Z\sim W|\boldsymbol{X}}$ are sufficient to recover $\hat{\lambda}$ and $\widehat{\text{se}}(\hat{\lambda})$ (Cinelli and Hazlett, 2020). More precisely, define $\widehat{\text{bias}}(\lambda) := \hat{\lambda}_{\text{res}} - \hat{\lambda}$ as the difference between the restricted estimate and the full estimate. We then have,

$$|\widehat{\text{bias}}(\lambda)| = \sqrt{\frac{R^2_{Y\sim W|Z,\boldsymbol{X}} R^2_{Z\sim W|\boldsymbol{X}}}{1 - R^2_{Z\sim W|\boldsymbol{X}}}\,\text{df}} \times \widehat{\text{se}}(\hat{\lambda}_{\text{res}}) = \text{BF}\sqrt{\text{df}} \times \widehat{\text{se}}(\hat{\lambda}_{\text{res}}) \tag{24}$$

Where hereafter $\text{df} = n - p - 1$ stands for the degrees of freedom of the restricted model *actually run*. For notational convenience, and to aid interpretation, we define the term

$$\text{BF} := \sqrt{\frac{R^2_{Y\sim W|Z,\boldsymbol{X}} R^2_{Z\sim W|\boldsymbol{X}}}{1 - R^2_{Z\sim W|\boldsymbol{X}}}} \tag{25}$$

as the "bias factor" of W, which is the part of the bias solely determined by $R^2_{Y\sim W|Z,\boldsymbol{X}}$ and $R^2_{Z\sim W|\boldsymbol{X}}$. Likewise, the standard error of the full model can be recovered with

$$\widehat{\text{se}}(\hat{\lambda}) = \sqrt{\frac{1 - R^2_{Y\sim W|Z,\boldsymbol{X}}}{1 - R^2_{Z\sim W|\boldsymbol{X}}}\left(\frac{\text{df}}{\text{df}-1}\right)} \times \widehat{\text{se}}(\hat{\lambda}_{\text{res}}) = \text{SEF}\sqrt{\text{df}/(\text{df}-1)} \times \widehat{\text{se}}(\hat{\lambda}_{\text{res}}) \tag{26}$$

Where again, for convenience, we define

$$\text{SEF} := \sqrt{\frac{1 - R^2_{Y\sim W|Z,\boldsymbol{X}}}{1 - R^2_{Z\sim W|\boldsymbol{X}}}} \tag{27}$$

as the "standard error factor" of W, summarizing the factor of the adjusted standard error which is solely determined by the sensitivity parameters $R^2_{Y\sim W|Z,\boldsymbol{X}}$ and $R^2_{Z\sim W|\boldsymbol{X}}$. Note again that SEF consists of the square-root of the product of the familiar "variance inflation factor," $1/\left(1 - R^2_{Z\sim W|\boldsymbol{X}}\right)$ and what could be labeled the "variance reduction factor," $1 - R^2_{Y\sim W|Z,\boldsymbol{X}}$. Cinelli and Hazlett (2020, Sec. 4.2) provide further discussion. Although simple, Equations 24 and 26 form the basis of a rich set of sensitivity exercises regarding point estimates, standard errors and t-values in terms of sensitivity parameters $R^2_{Y\sim W|Z,\boldsymbol{X}}$ and $R^2_{Z\sim W|\boldsymbol{X}}$.

Multiple unobserved variables. For simplicity of exposition, throughout the text we usually refer to a single omitted variable W. These results, however, can be used for performing sensitivity analyses considering multiple omitted variables $\boldsymbol{W} = [W_1, W_2, \ldots, W_n]$, and thus also non-linearities and functional form misspecification of observed variables. In such cases, barring an adjustment in the degrees of freedom, the equations are conservative, and reveal the maximum bias a multivariate $\boldsymbol{W}$ with such pair of partial R^2 values could cause (Cinelli and Hazlett, 2020, Sec. 4.5).

3.1.2 Adjusted lower and upper limits of confidence intervals

We now closely examine how the confidence interval of a regression coefficient changes due to the inclusion of W. Traditional confidence intervals account for sampling uncertainty, and are constructed by multiplying the standard error of the coefficient by a critical value (for example, in large samples, 1.96 for a 95% confidence level). We show that replacing this traditional critical value with an *OVB-adjusted critical value*, which we introduce here, accounts for both sampling uncertainty and systematic biases due to the omission of W. Although simple, this perspective will prove useful for deriving and understanding OVB-type results for OLS in general, and for instrumental variables in particular, such as in the Anderson-Rubin approach of Section 4.

Specifically, let $t^*_{\alpha,\text{df}-1}$ denote the critical value for a standard t-test with significance level α and df -1 degrees of freedom. Now let $\text{LL}_{1-\alpha}(\lambda)$ be the lower limit and $\text{UL}_{1-\alpha}(\lambda)$ be the upper limit of a $1-\alpha$ confidence interval for λ in the full model, i.e.,

$$\text{LL}_{1-\alpha}(\lambda) := \hat{\lambda} - t^*_{\alpha,\text{df}-1} \times \widehat{\text{se}}(\hat{\lambda}), \quad \text{UL}_{1-\alpha}(\lambda) := \hat{\lambda} + t^*_{\alpha,\text{df}-1} \times \widehat{\text{se}}(\hat{\lambda}), \tag{28}$$

Considering the direction of the bias that further reduces the lower limit, or, alternatively, a direction that further increases the upper limit, Equations 24 and 26 imply that both quantities can be written as a function of the restricted estimates and a new multiplier (see appendix)

$$\text{LL}_{1-\alpha}(\lambda) = \hat{\lambda}_{\text{res}} - t^{\dagger}_{\alpha,\text{df}-1,\boldsymbol{R}^2} \times \widehat{\text{se}}(\hat{\lambda}_{\text{res}}), \quad \text{UL}_{1-\alpha}(\lambda) = \hat{\lambda}_{\text{res}} + t^{\dagger}_{\alpha,\text{df}-1,\boldsymbol{R}^2} \times \widehat{\text{se}}(\hat{\lambda}_{\text{res}}) \tag{29}$$

where $t^{\dagger}_{\alpha,\text{df}-1,\boldsymbol{R}^2}$ stands for the *OVB-adjusted critical value*

$$t^{\dagger}_{\alpha,\text{df}-1,\boldsymbol{R}^2} := \text{SEF}\sqrt{\text{df}/(\text{df}-1)} \times t^{*}_{\alpha,\text{df}-1} + \text{BF}\sqrt{\text{df}}. \tag{30}$$

The subscript $\boldsymbol{R}^2 = \{R^2_{Y\sim W|Z,\boldsymbol{X}}, R^2_{Z\sim W|\boldsymbol{X}}\}$ conveys the fact that $t^{\dagger}_{\alpha,\text{df}-1,\boldsymbol{R}^2}$ depends on both sensitivity parameters. The adjusted critical value $t^{\dagger}_{\alpha,\text{df}-1,\boldsymbol{R}^2}$ *uniquely determines* the extreme points of the confidence interval for λ that one could obtain after adjusting for an omitted variable W with a given pair of partial R^2. Equivalently, given any hypothetical strength of W, to test the general null hypothesis of a change of $(100 \times q^*)\%$ of the current estimate $\hat{\lambda}_{\text{res}}$ at the α level, it suffices to rescale the original t-value by q^* and compare this to the adjusted critical threshold $t^{\dagger}_{\alpha,\text{df}-1,\boldsymbol{R}^2}$.[7]

3.1.3 Compatible inferences given bounds on partial R^2

Given hypothetical values for $R^2_{Y\sim W|Z,\boldsymbol{X}}$ and $R^2_{Z\sim W|\boldsymbol{X}}$, the previous results allow us to determine the exact changes in inference regarding a parameter of interest due to the inclusion of W with such strength. Often, however, the analyst does not know the exact strength of omitted variables, and wishes to investigate the *worst* possible inferences that could be induced by a W with bounded strength, for instance, $R^2_{Y\sim W|Z,\boldsymbol{X}} \leq R^{2\max}_{Y\sim W|Z,\boldsymbol{X}}$ and $R^2_{Z\sim W|\boldsymbol{X}} \leq R^{2\max}_{Z\sim W|\boldsymbol{X}}$. That is, we wish to find the maximum adjusted critical value due to an omitted variable W with *at most* such strength. Writing $t^{\dagger}_{\alpha,\text{df}-1,\boldsymbol{R}^2}$ as a function of the sensitivity parameters $R^2_{Y\sim W|Z,\boldsymbol{X}}$ and $R^2_{Z\sim W|\boldsymbol{X}}$, we solve the maximization problem

$$\max_{R^2_{Y\sim W|Z,\boldsymbol{X}}, R^2_{Z\sim W|\boldsymbol{X}}} t^{\dagger}_{\alpha,\text{df}-1,\boldsymbol{R}^2} \quad \text{s.t.} \quad R^2_{Y\sim W|Z,\boldsymbol{X}} \leq R^{2\max}_{Y\sim W|Z,\boldsymbol{X}}, \quad R^2_{Z\sim W|\boldsymbol{X}} \leq R^{2\max}_{Z\sim W|\boldsymbol{X}} \tag{31}$$

Note that, although this maximum is often reached at the extrema of both coordinates, this is not always the case. Due to the variance reduction factor, increasing $R^2_{Y\sim W|Z,\boldsymbol{X}}$ may reduce the standard error more than enough to compensate for the increase in bias, resulting in tighter confidence intervals. Denoting the solution to the optimization problem in expression (31) as $t^{\dagger\max}_{\alpha,\text{df}-1,\boldsymbol{R}^2}$, the *most extreme possible* lower and upper limits after adjusting for W are given by

$$\text{LL}^{\max}_{1-\alpha,\boldsymbol{R}^2}(\lambda) = \hat{\lambda}_{\text{res}} - t^{\dagger\max}_{\alpha,\text{df}-1,\boldsymbol{R}^2} \times \widehat{\text{se}}(\hat{\lambda}_{\text{res}}), \quad \text{UL}^{\max}_{1-\alpha,\boldsymbol{R}^2} = \hat{\lambda}_{\text{res}} + t^{\dagger\max}_{\alpha,\text{df}-1,\boldsymbol{R}^2} \times \widehat{\text{se}}(\hat{\lambda}_{\text{res}}) \tag{32}$$

[7] For a numerical example of an adjusted critical value, consider a case with 100 degrees of freedom and a significance level of $\alpha = 5\%$. The traditional critical value, assuming no omitted variables, is $t^{*}_{.05,100} \approx 1.98$. If we now allow for an omitted variable with strength given by $R^2_{Y\sim W|Z,\boldsymbol{X}} = R^2_{Z\sim W|\boldsymbol{X}} = .1$, this leads to an increased OVB-adjusted critical value of $t^{\dagger}_{.05,100,.1,.1} \approx 3.05$. Further note $t^{\dagger}_{\alpha,\text{df}-1,\boldsymbol{R}^2}$ *increases* the larger the sample size—for instance, if the degrees of freedom were instead 1,000, the adjusted critical value would increase to approximately 5.30.

The interval composed of such limits,

$$\mathrm{CI}^{\max}_{1-\alpha,\boldsymbol{R}^2}(\lambda) = \left[\mathrm{LL}^{\max}_{1-\alpha,\boldsymbol{R}^2}(\lambda), \quad \mathrm{UL}^{\max}_{1-\alpha,\boldsymbol{R}^2}(\lambda)\right] \tag{33}$$

retrieves all inferences for λ which are compatible with an omitted variable with such strengths. In other words, without imposing further constraints on W, for any value λ_0 inside $\mathrm{CI}^{\max}_{1-\alpha,\boldsymbol{R}^2}(\lambda)$, we can find a W such that $R^2_{Y\sim W|Z,\boldsymbol{X}} \leq R^{2\max}_{Y\sim W|Z,\boldsymbol{X}}$ and $R^2_{Z\sim W|\boldsymbol{X}} \leq R^{2\max}_{Z\sim W|\boldsymbol{X}}$ and the confidence interval for λ after adjusting for W includes λ_0. Moreover, if the true partial R^2 of W lies within the posited bounds, then $\mathrm{CI}^{\max}_{1-\alpha,\boldsymbol{R}^2}(\lambda)$ is the union of all confidence intervals that would be obtained by including an omitted variable with that strength or less, and thus constitutes itself a confidence interval with *at least* $1-\alpha$ coverage (provided, of course, our "target" confidence interval adjusting for W has nominal coverage).

3.2 Sensitivity statistics for routine reporting

Widespread adoption of sensitivity analysis benefits from simple and interpretable statistics that quickly convey the overall robustness of an estimate. To that end, Cinelli and Hazlett (2020) proposed two sensitivity statistics for routine reporting: (i) the partial R^2 of Z with Y, $R^2_{Y\sim Z|\boldsymbol{X}}$; and, (ii) the *robustness value* (RV). Here we generalize the notion of a partial R^2 as a measure of robustness to extreme scenarios, by introducing the *extreme robustness value* (XRV), for which the partial R^2 is a special case. We also recast these sensitivity statistics as a solution to an "inverse" question regarding the interval of compatible inferences, $\mathrm{CI}^{\max}_{1-\alpha,\boldsymbol{R}^2}(\lambda)$—that is, given a threshold of inference for λ deemed to be of scientific importance (say, zero), what is the *minimum* strength of the sensitivity parameters $\boldsymbol{R}^2$ that could lead $\mathrm{CI}^{\max}_{1-\alpha,\boldsymbol{R}^2}(\lambda)$ to include such threshold? This framework facilitates extending these metrics to other contexts, in particular to the IV setting, as we show in Section 4.2.3.

3.2.1 The extreme robustness value

One benefit of the partial R^2 parameterization is that the parameter $R^2_{Y\sim W|Z,\boldsymbol{X}}$ can be left completely unconstrained; i.e, in the optimization problem of expression 31, one can set the bound for $R^2_{Y\sim W|Z,\boldsymbol{X}}$ to its trivial bound of 1, and this still results in non-trivial bounds on the set of possible inferences. This leads to our first inverse question: what is the *bare minimum* strength of association of the omitted variable W with Z that could bring its estimated coefficient to a region where it is no longer statistically different than zero (or another threshold of interest)?

To answer this question, we can see $\mathrm{CI}^{\max}_{1-\alpha,\boldsymbol{R}^2}(\lambda)$ as a function of the bound $R^{2\max}_{Z\sim W|\boldsymbol{X}}$ alone, obtained from maximizing the adjusted critical value in expression 31 where: (i) the parameter $R^2_{Y\sim W|Z,\boldsymbol{X}}$ is left completely unconstrained (i.e, $R^2_{Y\sim W|Z,\boldsymbol{X}} \leq 1$); and, (ii) the parameter $R^2_{Z\sim W|\boldsymbol{X}}$ is bounded by XRV (i.e, $R^{2\max}_{Z\sim W|\boldsymbol{X}} \leq \mathrm{XRV}$). The *Extreme Robustness Value* $\mathrm{XRV}_{q^*,\alpha}(\lambda)$ is defined as the greatest lower bound XRV such that the null hypothesis that a change of $(100 \times q^*)\%$ of the original estimate, $H_0 : \lambda = (1-q^*)\hat{\lambda}_{\text{res}}$, is not rejected at the α level,

$$\text{XRV}_{q^*,\alpha}(\lambda) := \inf\left\{\text{XRV};\ (1-q^*)\hat{\lambda}_{\text{res}} \in \text{CI}^{\max}_{1-\alpha,1,\text{XRV}}(\lambda)\right\} \tag{34}$$

The solution to this problem gives,

$$\text{XRV}_{q^*,\alpha}(\lambda) = \begin{cases} 0, & \text{if } \ f_{q^*}(\lambda) \leq f^*_{\alpha,\text{df}-1} \\ \dfrac{f^2_{q^*}(\lambda) - f^{*2}_{\alpha,\text{df}-1}}{1+f^2_{q^*}(\lambda)}, & \text{otherwise.} \end{cases} \tag{35}$$

Where $f_{q^*}(\lambda) := q^*|f_{Y\sim Z|\boldsymbol{X}}|$ (here $f_{Y\sim Z|\boldsymbol{X}}$ stands for the partial Cohen's f and we define the critical threshold $f^*_{\alpha,\text{df}-1} := t^*_{\alpha,\text{df}-1}/\sqrt{\text{df}-1}$).[8] Note $\text{XRV}_{q^*,\alpha}(\lambda)$ can be interpreted as an "adjusted partial R^2" of Z with Y. To see why, let us first consider the case of the minimal strength to bring the point estimate ($\alpha = 1$) to exactly zero ($q^* = 1$). We then have that $f^*_{\alpha=1,\text{df}-1} = 0$ and $f^2_{q^*=1}(\lambda) = f^2_{Y\sim Z|\boldsymbol{X}}$, resulting in

$$\text{XRV}_{q^*=1,\alpha=1}(\lambda) = \frac{f^2_{Y\sim Z|\boldsymbol{X}}}{1+f^2_{Y\sim Z|\boldsymbol{X}}} = R^2_{Y\sim Z|\boldsymbol{X}} \tag{36}$$

This recovers the result of Cinelli and Hazlett (2020), and shows that, for an omitted variable W to bring down the estimated coefficient to zero, it needs to explain at least as much residual variation of Z, as Z explains of Y. For the general case, we simply perform two adjustments that dampens the "raw" partial R^2 of Z with Y. First we adjust it by the proportion of reduction deemed to be problematic q^* through $f_{q^*} = q^*|f_{Y\sim Z|\boldsymbol{X}}|$; next, we subtract the threshold for which statistical significance is lost at the α level (via $f^{*2}_{\alpha,\text{df}-1}$).

The extreme robustness value establishes thus the equivalent of a "Cornfield condition" (Cornfield et al., 1959) for OLS estimates, and delineates the bare minimum strength of omitted variables necessary to overturn a certain conclusion—if W cannot explain at least $\text{XRV}_{q^*,\alpha}(\lambda)$ of the residual variation of Z, then such variable *is not* strong enough to bring about a change of $(100 \times q^*)\%$ on the original estimate, at the significance level of α, regardless of its association with Y.

3.2.2 The robustness value

Placing no constraints on the association of the omitted variable W with Y may be too conservative an exercise. An alternative measure of robustness of the OLS estimate is to consider the minimal strength of association that the omitted variable needs to have, *both* with Z and Y, so that a $1-\alpha$ confidence interval for λ will include a change of $(100 \times q^*)\%$ of the current restricted estimate.

Write $\text{CI}^{\max}_{1-\alpha,\boldsymbol{R}^2}(\lambda)$ as a function of both bounds varying simultaneously, that is, construct $\text{CI}^{\max}_{1-\alpha,\text{RV},\text{RV}}(\lambda)$ by maximizing the adjusted critical value with bounds given by $R^2_{Y\sim W|Z,\boldsymbol{X}} \leq \text{RV}$ and $R^2_{Z\sim W|\boldsymbol{X}} \leq \text{RV}$. The *Robustness Value* $\text{RV}_{q^*,\alpha}(\lambda)$ for not rejecting the null hypothesis that

[8] Cohen's f^2 can be written as $f^2 = R^2/(1-R^2)$.

$H_0 : \lambda = (1 - q^*)\hat{\lambda}_{\text{res}}$, at the significance level α, is defined as

$$\text{RV}_{q^*,\alpha}(\lambda) := \inf \left\{\text{RV};\ (1 - q^*)\hat{\lambda}_{\text{res}} \in \text{CI}^{\max}_{1-\alpha,\text{RV},\text{RV}}(\lambda)\right\} \tag{37}$$

We then have that,

$$\text{RV}_{q^*,\alpha}(\lambda) = \begin{cases} 0, & \text{if } \ f_{q^*}(\lambda) \leq f^*_{\alpha,\text{df}-1} \\ \frac{1}{2}\left(\sqrt{f^4_{q^*,\alpha}(\lambda) + 4f^2_{q^*,\alpha}(\lambda)} - f^2_{q^*,\alpha}(\lambda)\right), & \text{if } \ f^*_{\alpha,\text{df}-1} < f_{q^*}(\lambda) < f^{*-1}_{\alpha,\text{df}-1} \\ \text{XRV}_{q^*,\alpha}(\lambda), & \text{otherwise.} \end{cases} \tag{38}$$

Where $f_{q^*,\alpha}(\lambda) := q^*|f_{Y\sim Z|\boldsymbol{X}}| - f^*_{\alpha,\text{df}-1}$. In the appendix we show the conditions of Equation 38 are equivalent to those first derived in Cinelli and Hazlett (2020), with the advantage of being simpler to verify. The first case occurs when the confidence interval already includes $(1-q^*)\hat{\lambda}_{\text{res}}$ or the mere change of one degree of freedom achieves this. The second case occurs when both associations of W reach the bound. Finally, in the last case the solution is an interior point—this happens when the bound is large enough such that the constraint on the association with the outcome is not binding; in this case the RV reduces to the XRV.

The robustness value offers a simple interpretable measure that summarizes the strength of omitted variables necessary to change the estimate in problematic ways. If W explains $\text{RV}_{q^*,\alpha}(\lambda)$ of the residual variance of both Z and Y, then such variable is sufficiently strong to bring about a $(100 \times q)\%$ change in the estimate at the significance level of α, while any omitted variable that does not explain $\text{RV}_{q^*,\alpha}(\lambda)$ of the residual variance, neither of Z nor of Y, is not sufficiently strong to do so.

A visual depiction of the RV and XRV

Visually depicting the RV and the XRV in a sensitivity contour plot may be helpful. Consider Figure 2. The horizontal axis describes $R^2_{Z\sim W|\boldsymbol{X}}$ and the vertical axis describes $R^2_{Y\sim W|Z,\boldsymbol{X}}$. The contour lines show the adjusted t-value for testing the null hypothesis of zero effect for the reduced form regression (of Table 1), had we adjusted for W with such hypothetical strength (considering that adjustment reduces the t-value). The red dashed line shows a critical contour of interest, such as statistical significance at the $\alpha = 0.05$ level. The RV (when both values reach their bounds) summarizes the point of equal values on both axis of the critical contour, whereas the XRV summarizes the vertical line tangent to the critical contour, which will never be crossed.

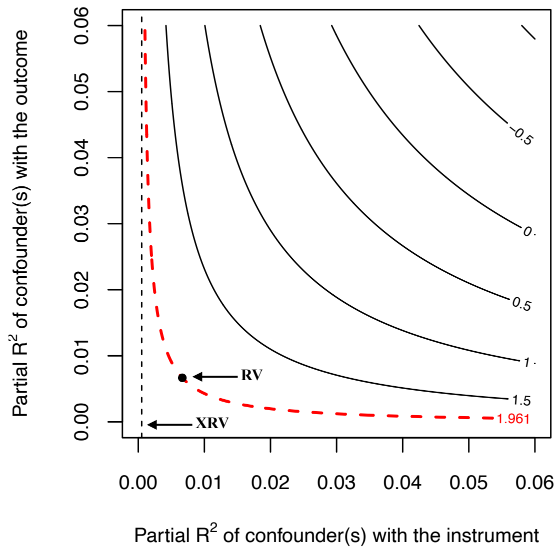


Figure 2: Sensitivity contours of the reduced form of Card (1993) depicting the RV and the XRV.

3.3 Bounding the strength of the omitted variable using observed covariates

One further result is required before turning to the sensitivity of IV estimates. Let X_j be a specific covariate of the set $\boldsymbol{X}$, and define

$$k_Z := \frac{R^2_{Z\sim W|\boldsymbol{X}_{-j}}}{R^2_{Z\sim X_j|\boldsymbol{X}_{-j}}}, \qquad k_Y := \frac{R^2_{Y\sim W|Z,\boldsymbol{X}_{-j}}}{R^2_{Y\sim X_j|Z\boldsymbol{X}_{-j}}}. \tag{39}$$

where $\boldsymbol{X}_{-j}$ represents the vector of covariates $\boldsymbol{X}$ excluding X_j. These new parameters, k_Z and k_Y, stand for how much "stronger" W is relatively to the observed covariate X_j in terms of residual variation explained of Z and Y. Our goal in this section is to re-express (or bound) the sensitivity parameters $R^2_{Z\sim W|\boldsymbol{X}}$ and $R^2_{Y\sim W|Z,\boldsymbol{X}}$ in terms of the relative strength parameters k_Z and k_Y.

We start by restating the bounds derived in Cinelli and Hazlett (2020). These are particularly useful when contemplating X_j and W both *confounders* of Z (violations of the ignorability of the instrument). Let $R^2_{W\sim X_j|\boldsymbol{X}_{-j}} = 0$ (or, equivalently, consider the part of W not linearly explained by $\boldsymbol{X}$). Then the previous sensitivity parameters can be written as (Cinelli and Hazlett, 2020, Sec. 4.4)

$$R^2_{Z\sim W|\boldsymbol{X}} = k_Z f^2_{Z\sim X_j|\boldsymbol{X}_{-j}}, \qquad R^2_{Y\sim W|Z,\boldsymbol{X}} \leq \eta^2 f^2_{Y\sim X_j|Z,\boldsymbol{X}_{-j}} \tag{40}$$

where η is a function of both parameters k_Y, k_Z and $R^2_{Z\sim X_j|\boldsymbol{X}_{-j}}$.

In the instrumental variable setting, however, W and X_j may be *side-effects* of Z, instead of causes of Z (violations of the exclusion restriction). In such cases, reasoning about the orthogonality of $\boldsymbol{X}$ and W may not be natural, as the instrument itself is a source of dependence between these variables. Therefore, here we additionally provide bounds under the alternative condition

$R^2_{W\sim X_j|Z,\boldsymbol{X}_{-j}} = 0$. We then have that

$$R^2_{Z\sim W|\boldsymbol{X}} = \eta' f^2_{Z\sim X_j|\boldsymbol{X}_{-j}}, \qquad R^2_{Y\sim W|Z,\boldsymbol{X}} \leq k_Y f^2_{Y\sim X_j|Z,\boldsymbol{X}_{-j}} \tag{41}$$

where η' is a function of k_Z and $R^2_{Z\sim X_j|\boldsymbol{X}_{-j}}$ (see appendix for details).

These results allow investigators to leverage knowledge of *relative importance* of variables (Kruskal and Majors, 1989) when making plausibility judgments regarding sensitivity parameters. For instance, if researchers have domain knowledge to argue that a certain observed covariate X_j is supposed to be a strong determinant of the instrument and the outcome variation, and that the omitted variable W is not likely to explain as much residual variance of Z and Y as that observed covariate, such results can be used to set plausible bounds on the maximum bias due to the omission of W.

4 An omitted variable bias framework for the sensitivity of IV

Having established the tools for analyzing the sensitivity of conventional OLS estimates, we are now in a position to develop a suite of sensitivity analysis tools for instrumental variable analyses. As explained, an OVB-approach to sensitivity begins by assuming that the researcher measured and included observed covariates $\boldsymbol{X}$, but would also have liked to adjust for W in order for the IV conditions to hold. In this section, we first show how separate sensitivity analysis of the reduced form and first stage is already sufficient to draw valuable conclusions regarding the sensitivity of IV. We then construct a complete OVB framework for sensitivity analysis of IV within the Anderson-Rubin approach, allowing one to investigate the sensitivity of tests to a specific null hypothesis, the sensitivity of lower and upper limits of confidence intervals, to define and compute sensitivity statistics for routine reporting for IV, such as (extreme) robustness values, as well as providing bounds on the sensitivity parameters, on the basis of comparison to observed covariates.

4.1 Sensitivity analysis of the reduced form and of the first stage

The recent literature on instrumental variables places strong emphasis on the first-stage and the reduced-form estimates. Not only are the first stage and reduced form often substantively meaningful on their own, but their critical examination plays an important role for motivating the causal story behind a particular instrumental variable. For example, in the “local average treatment effect” interpretation of the IV estimand, *both* the first stage and the reduced form must be unconfounded so that the resulting estimate can be interpreted as the average causal effect among compliers (Angrist et al., 1996). Therefore, beyond a means to the final IV estimate, researchers are advised to report and to interpret the first stage and the reduced form by, for example, assessing whether those results are in accordance to the postulated mechanisms that justify the choice of instrument (Angrist and Krueger, 2001; Angrist and Pischke, 2009; Imbens, 2014; Angrist and Pischke, 2014; Imbens and Rubin, 2015). While investigating these separate regressions, researchers can deploy all sensitivity analysis results discussed in the previous section.

Fortunately, such sensitivity analyses also provide answers to many pivotal sensitivity questions regarding the IV estimate itself. In particular, if the investigator is interested in assessing the strength of confounders or side-effects needed to bring the IV point estimate to zero, or to not reject the null hypothesis of zero effect, the results of the sensitivity analysis of the reduced form is all that is needed. If interest lies in also determining whether the IV estimate could be arbitrarily large in either direction, then the sensitivity of the first stage must also be assessed, as omitted variables capable of changing the direction of the first stage can lead to unbounded IV estimates. We now give a more precise meaning to these claims.

4.1.1 What the reduced form and first stage reveal about the IV point estimate

First let us consider the sensitivity of the point estimate. Recall that all estimators under consideration are algebraically equivalent, and are equal to the ratio of the reduced-form and the first-stage coefficients,

$$\hat{\tau} := \hat{\tau}_{\text{ILS}} = \hat{\tau}_{\text{2SLS}} = \hat{\tau}_{\text{AR}} = \frac{\hat{\lambda}}{\hat{\theta}} \tag{42}$$

This simple algebraic fact allows us to draw two important conclusions regarding the sensitivity of $\hat{\tau}$ from the sensitivity of $\hat{\lambda}$ and $\hat{\theta}$ alone.

First, residual biases can bring the IV point estimate to zero *if, and only if,* they can bring the reduced-form point estimate to zero. Therefore, if sensitivity analysis of the reduced form reveals that omitted variables are not strong enough to explain away $\hat{\lambda}$, then they also cannot explain away the IV point estimate $\hat{\tau}$. Or, more worrisome, if analysis reveals that it takes weak confounding or side-effects to explain away $\hat{\lambda}$, the same holds for the IV estimate $\hat{\tau}$. In sum, for all IV estimators considered here, to assess the strength of biases needed to bring the IV point estimate to zero, one needs only to perform a sensitivity analysis on the reduced-form regression coefficient.

Second, if we cannot rule out confounders or side-effects that are sufficiently strong to *change the sign* of the first-stage point estimate $\hat{\theta}$, then we also cannot rule out that the IV point estimate $\hat{\tau}$ could be *arbitrarily large* in either direction, even if not exactly equal to zero. This can be immediately seen by letting $\hat{\theta}$ approach zero on either side of the limit. Thus, whenever we are interested in biases as large *or larger* than a certain amount, the robustness of the first stage to the zero null puts an upper bound on the robustness of the IV point estimate.

4.1.2 What the reduced form and first stage reveal about IV hypothesis tests

Contrary to the point estimate, the different approaches presented here may lead to different conclusions regarding how omitted variables would have changed inferences. Let us start by examining the Anderson-Rubin/Fieller approach, as not only it has nominal coverage regardless of instrument strength, but its conclusions match the intuition of current guidelines when assessing the first-stage and reduced-form estimates (Angrist and Krueger, 2001; Angrist and Pischke, 2009, 2014).

Consider again the IV estimand

$$\tau = \frac{\lambda}{\theta}$$

Note that the same arguments we used before for the estimator hold for the estimand. Logically, provided the ratio is well defined ($\theta \neq 0$), we have that $\tau = 0 \iff \lambda = 0$. Therefore, a test of the null hypothesis $H_0 : \lambda = 0$ in the reduced-form regression is *logically equivalent* to a test of the null hypothesis $H_0 : \tau = 0$ for the IV estimand. Similarly, for a fixed λ, if we cannot rule out that θ is arbitrarily close to zero in either direction, then, logically, we also cannot rule out that τ is arbitrarily large in either direction—a test for the null hypothesis $H_0 : \theta = 0$ is thus *logically equivalent* to testing whether arbitrarily large sizes for τ can be ruled out.

The Anderson-Rubin/Fieller approach is coherent with respect to these logical implications. Recall the Anderson-Rubin test for the null hypothesis $H_0 : \tau = \tau_0$ is based on the test of $H_0 : \phi_{\tau_0} = 0$. By the FWL theorem, the point estimate and (estimated) standard error for $\hat{\phi}_{\tau_0}$ are given by

$$\hat{\phi}_{\tau_0} = \frac{\text{cov}(Y_{\tau_0}^{\perp \boldsymbol{X},W}, Z^{\perp \boldsymbol{X},W})}{\text{var}(Z^{\perp \boldsymbol{X},W})}, \qquad \widehat{\text{se}}(\hat{\phi}_{\tau_0}) = \frac{\text{sd}(Y_{\tau_0}^{\perp Z,\boldsymbol{X},W})}{\text{sd}(Z^{\perp \boldsymbol{X},W})}\sqrt{\frac{1}{\text{df} - 1}} \tag{43}$$

Which can be expressed in terms of the first-stage and reduced-form estimates (see appendix)

$$\hat{\phi}_{\tau_0} = \hat{\lambda} - \tau_0\hat{\theta}, \qquad \widehat{\text{se}}(\hat{\phi}_{\tau_0}) = \sqrt{\widehat{\text{var}}(\hat{\lambda}) + \tau_0^2\widehat{\text{var}}(\hat{\theta}) - 2\tau_0\widehat{\text{cov}}(\hat{\lambda}, \hat{\theta})} \tag{44}$$

Testing $H_0 : \phi_{\tau_0} = 0$ requires comparing the t-value for $\hat{\phi}_{\tau_0}$ with a critical threshold $t^*_{\alpha,\text{df}-1}$, and the null hypothesis is not rejected if $|t_{\hat{\phi}_{\tau_0}}| \leq t^*_{\alpha,\text{df}-1}$. Squaring and rearranging terms we obtain the quadratic inequality which must hold for non-rejection:

$$\underbrace{\left(\hat{\theta}^2 - \widehat{\text{var}}(\hat{\theta}) \times t^{*2}_{\alpha,\text{df}-1}\right)}_{a}\tau_0^2 + \underbrace{2\left(\widehat{\text{cov}}(\hat{\lambda}, \hat{\theta}) \times t^{*2}_{\alpha,\text{df}-1} - \hat{\lambda}\hat{\theta}\right)}_{b}\tau_0 + \underbrace{\left(\hat{\lambda}^2 - \widehat{\text{var}}(\hat{\lambda}) \times t^{*2}_{\alpha,\text{df}-1}\right)}_{c} \leq 0 \tag{45}$$

When considering the null hypothesis $H_0 : \tau_0 = 0$, only the term c remains, and c is less or equal to zero if, and only if, one cannot reject the null hypothesis $H_0 : \lambda = 0$ in the reduced-form regression. The Anderson-Rubin approach thus comports with the recommendation of Angrist and Krueger (2001) that "if you can't see the causal relation of interest in the reduced form, it's probably not there." Also note that arbitrarily large values for τ_0 will satisfy the inequality in Equation 45 if, and only if, $a < 0$, meaning that we cannot reject the null hypothesis $H_0 : \theta = 0$ in the first-stage regression. This supports the recommendation that, if one is unsure about the direction of the first stage, it is likely that very little can be said about the magnitude of the IV estimate.

Within the Anderson-Rubin framework, we thus reach analogous conclusions regarding hypothesis testing as those regarding the point estimate: (i) when interest lies in the zero null hypothesis, the sensitivity of the reduced form is exactly the sensitivity of the IV—no other analyses are needed. Confounders or side-effects sufficiently strong to bring the reduced form to a region where it is not statistically different than zero can also bring the IV estimate to a region where it is not statistically different than zero, and only omitted variables with such strength are capable of doing so; and, (ii) if one is interested in biases of a certain amount, *or larger,* then the sensitivity of the first stage to the zero null hypothesis needs also to be assessed. Specifically, for any null hypothesis of interest $H_0 : \tau = \tau_0$, omitted variables that are strong enough to make the first stage not statistically different from zero may also lead us to not reject values arbitrarily "worse" than τ_0.[9]

As is well known, it is not uncommon for frequentist statistical tests to lead to logically incoherent decisions (Gabriel, 1969; Schervish, 1996; Patriota, 2013; Fossaluza et al., 2017). While inferences made in the Anderson-Rubin approach have the expected behavior in this setting, inferences using ILS or 2SLS, however, do not necessarily comply with these logical expectations. Cases can be found for ILS and 2SLS where, for instance, one fails to reject the null hypothesis $H_0 : \lambda = 0$, yet still rejects the null hypothesis $H_0 : \tau = 0$ (and vice-versa). Such claims do not conform to current guidelines for interpreting the first-stage and reduced-form regressions (Angrist and Pischke, 2009).

4.2 Sensitivity analysis of the IV in the Anderson-Rubin approach

We now apply the OVB framework for assessing the sensitivity of the IV estimate directly. We focus on the Anderson-Rubin approach for this task because: (i) it allows performing sensitivity analysis of the IV with only two interpretable sensitivity parameters; (ii) it has correct test size regardless of "instrument strength"; and, (iii) its conclusions conform to current recommendations regarding the interpretation of the first-stage and reduced-form regressions.

4.2.1 Sensitivity for testing a specific null hypothesis

We begin by examining the sensitivity of the t-value for testing a specific null hypothesis $H_0 : \tau = \tau_0$, as this is a straightforward application of the tools of Section 3. Recall that, in the Anderson-Rubin approach, a test for the null hypothesis $H_0 : \tau = \tau_0$ is a test for the null hypothesis $H_0 : \phi_{\tau_0} = 0$ in the regression of Y_{τ_0} on the instrument Z and covariates $\boldsymbol{X}$ and W. Therefore, standard OLS sensitivity analysis for testing the null hypothesis $H_0 : \phi_{\tau_0} = 0$ on the Anderson-Rubin regression gives the desired results for $H_0 : \tau = \tau_0$.

In detail, a sensitivity analysis for the null hypothesis that the IV estimate τ equals some τ_0 can be performed as follows:

1. Construct $Y_{\tau_0} = Y - \tau_0 D$ under the null value $H_0 : \tau = \tau_0$;
2. Run the OLS model $Y_{\tau_0} = \hat{\phi}_{\text{res},\tau_0} Z + \boldsymbol{X}\hat{\beta}_{\text{res},\tau_0} + \hat{\varepsilon}_{\tau_0,\text{res}}$;
3. Perform regular OLS sensitivity analysis for the null $H_0 : \phi_{\tau_0} = 0$.

This procedure can both tell us how omitted variables no worse than $\mathbf{R}^2 = \{R^2_{Z\sim W|\boldsymbol{X}}, R^2_{Y_{\tau_0}\sim W|Z,\boldsymbol{X}}\}$ would alter inferences regarding the null $H_0 : \tau = \tau_0$, or what is the minimal strength of $\mathbf{R}^2$ that is required to not reject the null $H_0 : \tau = \tau_0$, as given by the RV or XRV.

[9] Similar observations regarding the importance of the robustness of the first stage for hidden biases have been made before in the context of randomization inference (Small and Rosenbaum, 2008; Rosenbaum, 2017).

Making sense of the sensitivity parameters. While separate analyses of the first stage and reduced form regressions may suggest the need of three sensitivity parameters for the sensitivity of IV (e.g, $R^2_{Z\sim W|\boldsymbol{X}}$, $R^2_{D\sim W|Z,\boldsymbol{X}}$ and $R^2_{Y\sim W|Z,\boldsymbol{X}}$), note how within the Anderson-Rubin approach one is able to perform sensitivity with only two parameters ($R^2_{Z\sim W|\boldsymbol{X}}, R^2_{Y_{\tau_0}\sim W|Z,\boldsymbol{X}}$). The meaning of the parameter related with the instrument ($R^2_{Z\sim W|\boldsymbol{X}}$) is unchanged and straightforward, ie., the share of residual variation of the instrument explained by the omitted variable W. The main difference concerns the parameter $R^2_{Y_{\tau_0}\sim W|Z,\boldsymbol{X}}$, which stands for the share of residual variance of Y_{τ_0} explained by W. The substantive interpretation of Y_{τ_0} depends on the causal assumptions the researcher is willing to defend. For instance, under $H_0 : \tau = \tau_0$ and a constant treatment effects model, we have that $Y_{\tau_0} = Y - \tau_0 D$ equals the *untreated potential outcome* Y_0 and thus $R^2_{Y_{\tau_0}\sim W|Z,\boldsymbol{X}}$ could be interpreted as the share of residual variance of Y_0 explained by W. For simplicity of exposition, we adopt this interpretation throughout the text.

4.2.2 Compatible inferences given bounds on partial R^2

Instead of assessing the sensitivity of the test statistic for specific a null hypothesis, investigators may be interested in recovering the whole set of inferences compatible with plausibility judgments on the maximum strength of W. As discussed in Section 2, for a critical threshold $t^*_{\alpha,\text{df}-1}$, the confidence interval for τ in the Anderson-Rubin framework is given by

$$\text{CI}_{1-\alpha}(\tau) = \{\tau_0;\ t^2_{\phi_{\tau_0}} \leq t^{*2}_{\alpha,\text{df}-1}\} \tag{46}$$

Now consider bounds on sensitivity parameters $R^2_{Y_{\tau_0}\sim W|Z,\boldsymbol{X}} \leq R^{2\max}_{Y_0\sim W|Z,\boldsymbol{X}}$ (which should be judged to hold *regardless* of the value of τ_0) and $R^2_{Z\sim W|\boldsymbol{X}} \leq R^{2\max}_{Z\sim W|\boldsymbol{X}}$. Let $t^{\dagger\max}_{\alpha,\text{df}-1,\boldsymbol{R}^2}$ denote the maximum OVB-adjusted critical value under the posited bounds on the strength of W. The set of compatible inferences for τ, $\text{CI}^{\max}_{1-\alpha,\boldsymbol{R}^2}(\tau)$ is then simply given by

$$\text{CI}^{\max}_{1-\alpha,\boldsymbol{R}^2}(\tau) = \left\{\tau_0;\ t^2_{\hat{\phi}_{\text{res},\tau_0}} \leq \left(t^{\dagger\max}_{\alpha,\text{df}-1,\boldsymbol{R}^2}\right)^2\right\} \tag{47}$$

This interval can be found analytically using the same inequality as in Equation 45, now with the parameters of the restricted regression actually run, and the traditional critical value replaced by the OVB-adjusted critical value $t^{\dagger\max}_{\alpha,\text{df}-1,\boldsymbol{R}^2}$

$$\begin{aligned}\underbrace{\left(\hat{\theta}^2_{\text{res}} - \widehat{\text{var}}(\hat{\theta}_{\text{res}}) \times \left(t^{\dagger\max}_{\alpha,\text{df}-1,\boldsymbol{R}^2}\right)^2\right)}_{a}\tau_0^2 + \underbrace{2\left(\widehat{\text{cov}}(\hat{\lambda}_{\text{res}}, \hat{\theta}_{\text{res}}) \times \left(t^{\dagger\max}_{\alpha,\text{df}-1,\boldsymbol{R}^2}\right)^2 - \hat{\lambda}_{\text{res}}\hat{\theta}_{\text{res}}\right)}_{b}\tau_0 \\ + \underbrace{\left(\hat{\lambda}^2_{\text{res}} - \widehat{\text{var}}(\hat{\lambda}_{\text{res}}) \times \left(t^{\dagger\max}_{\alpha,\text{df}-1,\boldsymbol{R}^2}\right)^2\right)}_{c} \leq 0\end{aligned} \tag{48}$$

Note that users can easily obtain $\text{CI}^{\max}_{1-\alpha,\boldsymbol{R}^2}(\tau)$ with any software that computes Anderson-Rubin or Fieller's confidence intervals by simply providing the modified critical threshold $t^{\dagger\max}_{\alpha,\text{df}-1,\boldsymbol{R}^2}$.

It is now useful to discuss the possible shapes of $\text{CI}^{\max}_{1-\alpha,\boldsymbol{R}^2}$ as this will help understanding the robustness values for IV we derive next. Let $\mathbf{r} = \{r_{\min}, r_{\max}\}$ denote the roots of the quadratic equation, which can be written as $\mathbf{r} = -b \pm \sqrt{\Delta}/2a$, with $\Delta = b^2 - 4ac$. If $a > 0$ (i.e, we have a statistically significant first stage), the quadratic equation will be convex, and thus only the values between the roots will be non-positive. This leads to the connected confidence interval $\text{CI}^{\max}_{1-\alpha,\boldsymbol{R}^2} = [r_{\min}, r_{\max}]$. When $a < 0$ (i.e, the null hypothesis of zero for the first stage is not rejected), the curve is concave and this leads to unbounded confidence intervals. Here we have two sub-cases: (i) when $\Delta < 0$, the quadratic curve never touches zero, and thus the confidence interval is simply the whole real line $\text{CI}^{\max}_{1-\alpha,\boldsymbol{R}^2} = (-\infty, +\infty)$; and, (ii) when $\Delta > 0$ the confidence interval will be union of two disjoint intervals $\text{CI}^{\max}_{1-\alpha,\boldsymbol{R}^2} = (-\infty, r_{\min}] \cup [r_{\max}, +\infty)$.[10]

4.2.3 Sensitivity statistics for routine reporting

Armed with the notion of a set of compatible inferences for IV, $\text{CI}^{\max}_{1-\alpha,\boldsymbol{R}^2}(\tau)$, we are now able to formally define and derive (extreme) robustness values for instrumental variable estimates.

Extreme robustness values for IV. The extreme robustness value $\text{XRV}_{q^*,\alpha}(\tau)$ for the IV estimate is defined as the minimum strength of association of omitted variables with the instrument so that we cannot reject a reduction of $(100 \times q^*)\%$ of the original IV estimate; that is,

$$\text{XRV}_{q^*,\alpha}(\tau) := \inf\left\{\text{XRV};\ (1-q^*)\hat{\tau}_{\text{res}} \in \text{CI}^{\max}_{1-\alpha,1,\text{XRV}}(\tau)\right\} \tag{49}$$

It then follows immediately from Equation 47 that

$$\text{XRV}_{q^*,\alpha}(\tau) = \text{XRV}_{1,\alpha}(\phi_{\tau^*}) \tag{50}$$

where $\tau^* = (1-q^*)\hat{\tau}_{\text{res}}$. As in the general case, the extreme robustness value can be interpreted as a "dampened" partial R^2 of the instrument Z with the "putative" untreated potential outcome Y_{τ_0}. Also of interest is the special case of the minimum strength to bring the IV estimate to a region where it is no longer statistically different than zero ($q^* = 1$), in which we obtain $\text{XRV}_{1,\alpha}(\tau) = \text{XRV}_{1,\alpha}(\lambda)$. That is, for the null hypothesis of $H_0 : \tau = 0$, the extreme robustness value of the IV estimate equals the extreme robustness value of the reduced-form estimate, as we discussed in the last section.

The $\text{XRV}_{q^*,\alpha}(\tau)$ computes the minimal strength of W required to not reject a particular null hypothesis of interest. We might be interested, instead, in asking about the minimal strength of omitted variables to not reject a specific value *or worse*. When confidence intervals are connected, such as the case of standard OLS, the two notions coincide. But in the Anderson-Rubin case, as we have seen, confidence intervals for the IV estimate can sometimes consist of disjoint intervals.

[10]See Mehlum (2020) for an intuitive graphical characterization of Fieller's solutions using polar coordinates.

Therefore, let the upper and lower limits of $\mathrm{CI}^{\max}_{1-\alpha,\boldsymbol{R}^2}(\tau)$ be $\mathrm{LL}^{\max}_{1-\alpha,\boldsymbol{R}^2}(\tau)$ and $\mathrm{UL}^{\max}_{1-\alpha,\boldsymbol{R}^2}(\tau)$ respectively. The extreme robustness value $\mathrm{XRV}_{\geq q^*,\alpha}(\tau)$ for the IV estimate is defined as the minimum strength of association that confounders or side-effects need to have with the instrument so that we cannot reject a change of $(100 \times q^*)\%$ *or worse* of the original IV estimate;

$$\mathrm{XRV}_{\geq q^*,\alpha}(\tau) := \inf\left\{\mathrm{XRV};\ (1-q^*)\hat{\tau}_{\mathrm{res}} \in \left[\mathrm{LL}^{\max}_{1-\alpha,1,\mathrm{XRV}}(\tau), \quad \mathrm{UL}^{\max}_{1-\alpha,1,\mathrm{XRV}}(\tau)\right]\right\} \tag{51}$$

Now note that, whenever $\mathrm{CI}^{\max}_{1-\alpha,\mathrm{df}-1}(\tau)$ is connected, we must have that $\mathrm{XRV}_{\geq q^*,\alpha}(\tau) = \mathrm{XRV}_{q^*,\alpha}(\tau)$. On the other hand, recall that $\mathrm{CI}^{\max}_{1-\alpha,\mathrm{df}-1}(\tau)$ will be disjoint only if $t^2_{\hat{\theta}_{\mathrm{res}}} \leq (t^{\dagger\max}_{\alpha,\mathrm{df}-1})^2$, which is precisely the condition for the extreme robustness value of the first stage. Therefore,

$$\mathrm{XRV}_{\geq q^*,\alpha}(\tau) = \min\{\mathrm{XRV}_{1,\alpha}(\phi_{\tau^*}), \quad \mathrm{XRV}_{1,\alpha}(\theta)\} \tag{52}$$

This corroborates our previous conclusion that, when we are interested in biases as large or larger than a certain amount, the robustness of the IV estimate is bounded by the robustness of the first stage assessed at the zero null.

Robustness values for IV. The definitions of the robustness value for IV follow the same logic discussed above, but now considering both bounds on $\mathrm{CI}^{\max}_{1-\alpha,\boldsymbol{R}^2}$ varying simultaneously. That is,

$$\mathrm{RV}_{q^*,\alpha}(\tau) := \inf\left\{\mathrm{RV};\ (1-q^*)\hat{\tau}_{\mathrm{res}} \in \mathrm{CI}^{\max}_{1-\alpha,\mathrm{RV},\mathrm{RV}}(\tau)\right\} \tag{53}$$

Again from Equation 47 we have that

$$\mathrm{RV}_{q^*,\alpha}(\tau) = \mathrm{RV}_{1,\alpha}(\phi_{\tau^*}) \tag{54}$$

Which for the special case of $q^* = 1$ simplifies to $\mathrm{RV}_{1,\alpha}(\tau) = \mathrm{RV}_{1,\alpha}(\lambda)$, as before. We can also define robustness values for not rejecting the null hypothesis of a reduction of $(100 \times q^*)\%$ *or worse*

$$\mathrm{RV}_{\geq q^*,\alpha}(\tau) := \inf\left\{\mathrm{RV};\ (1-q^*)\hat{\tau}_{\mathrm{res}} \in \left[\mathrm{LL}^{\max}_{1-\alpha,\mathrm{RV},\mathrm{RV}}(\tau), \quad \mathrm{UL}^{\max}_{1-\alpha,\mathrm{RV},\mathrm{RV}}(\tau)\right]\right\} \tag{55}$$

By the same arguments articulated above, $\mathrm{RV}_{\geq q^*,\alpha}(\tau)$ must be the minimum of the robustness value of the Anderson-Rubin regression evaluated at $\tau^* = (1-q^*)\hat{\tau}_{\mathrm{res}}$ and the robustness value of the first-stage regression evaluted at the zero null

$$\mathrm{RV}_{\geq q^*,\alpha}(\tau) = \min\{\mathrm{RV}_{1,\alpha}(\phi_{\tau^*}), \quad \mathrm{RV}_{1,\alpha}(\theta)\} \tag{56}$$

For the special case of $q^* = 1$ (zero null hypothesis), $\mathrm{RV}_{\geq q^*,\alpha}(\tau)$ simplifies to the minimum of the robustness value of the first stage and of the reduced form, $\mathrm{RV}_{\geq q^*=1,\alpha}(\tau) = \min\{\mathrm{RV}_{1,\alpha}(\lambda), \quad \mathrm{RV}_{1,\alpha}(\theta)\}$.

4.2.4 Bounds on the strength of omitted variables

The bounds discussed in Section 3.3 work without any major modifications in the Anderson-Rubin setting. When testing a specific null hypothesis $H_0 : \tau = \tau_0$ in the AR regression, we have k_Z as before, and instead of k_Y we now have $k_{Y_{\tau_0}}$

$$k_{Y_{\tau_0}} := \frac{R^2_{Y_{\tau_0}\sim W|Z,\boldsymbol{X}_{-j}}}{R^2_{Y_{\tau_0}\sim X_j|Z\boldsymbol{X}_{-j}}}. \tag{57}$$

The plausibility judgment one is making here is that of how strong unobserved confounders or side-effects are, relative to observed covariates, in explaining the residual variance of the untreated potential outcome and of the instrument, *under the null hypothesis* $H_0 : \tau = \tau_0$.

Since the judgment is made under a specific null, the bounds will be different when testing different hypotheses. Therefore, it may be useful to compute bounds under a slightly more *conservative* assumption. More precisely, consider

$$k^{\max}_{Y_{\tau_0}} := \frac{\max_{\tau_0} R^2_{Y_{\tau_0}\sim W|Z,\boldsymbol{X}_{-j}}}{\max_{\tau_0} R^2_{Y_{\tau_0}\sim X_j|Z\boldsymbol{X}_{-j}}}. \tag{58}$$

That is, we can posit that the omitted variables are no stronger than (a multiple of) the *maximum* explanatory power of an observed covariate, regardless of the value of τ_0. This has the useful property of providing a unique valid bound for any value of the null hypothesis, and can be used to place bounds on sensitivity contours of the lower and upper limit of the AR confidence intervals, as we show next.

5 Using the OVB framework for the sensitivity analysis of IV

In this section we return to our running example of estimating the returns to schooling and show how these tools can be deployed to assess the robustness of those findings to violations of the IV assumptions. We propose investigators begin their sensitivity analysis by examining the robustness of the first-stage and reduced-form estimates. Not only are these analyses usually important on their own right, but in many cases—including this one—this exercise will be sufficient to establish that the instrumental variable estimate is not very informative of the causal effect of interest, since one is not in a position to rule out confounders or side-effects that can explain away those auxiliary estimates. We then turn to the sensitivity of the IV itself, and further show how sensitivity contour plots of the adjusted lower and upper limits of the AR confidence interval, supplemented with benchmark bounds, give a succinct yet complete picture of the whole range of sensitivity of the IV estimate.

5.1 Minimal reporting and sensitivity plots of the reduced form

We start by examining the sensitivity of the reduced-form estimate, namely, the effect of *Proximity* on *Earnings*. Recall that if we cannot rule out that the reduced form is zero, we also cannot rule out the IV estimate is zero. In our running example we focus the discussion on violations of the ignorability of the instrument due to confounders, as this is the main threat of the study under investigation. Readers should keep in mind, however, that all analyses performed here can be equally used to assess violations of the exclusion restriction (or both). Table 3 shows the minimal sensitivity reporting initially proposed in Cinelli and Hazlett (2020), but now incorporating the new results of Section 3. Beyond the usual statistics such as the point estimate, standard-error and t-value, we recommend that researchers also report the: (i) partial R^2 of the instrument with the outcome ($R^2_{Y\sim Z|\boldsymbol{X}} = 0.18\%$), as well as (ii) the robustness value ($\text{RV}_{q^*,\alpha} = 0.67\%$), and (iii) the extreme robustness value ($\text{XRV}_{q^*,\alpha} = 0.05\%$), both for where the confidence interval would cross zero ($q^* = 1$), at a chosen significance level (here, $\alpha = 0.05$).

Outcome: *Earnings* (log)

Instrument	Estimate	Std. Error	t-value	$R^2_{Y\sim Z\|\boldsymbol{X}}$	$\text{XRV}_{q^*,\alpha}$	$\text{RV}_{q^*,\alpha}$
Proximity	0.042	0.018	2.33	0.18%	0.05%	0.67%

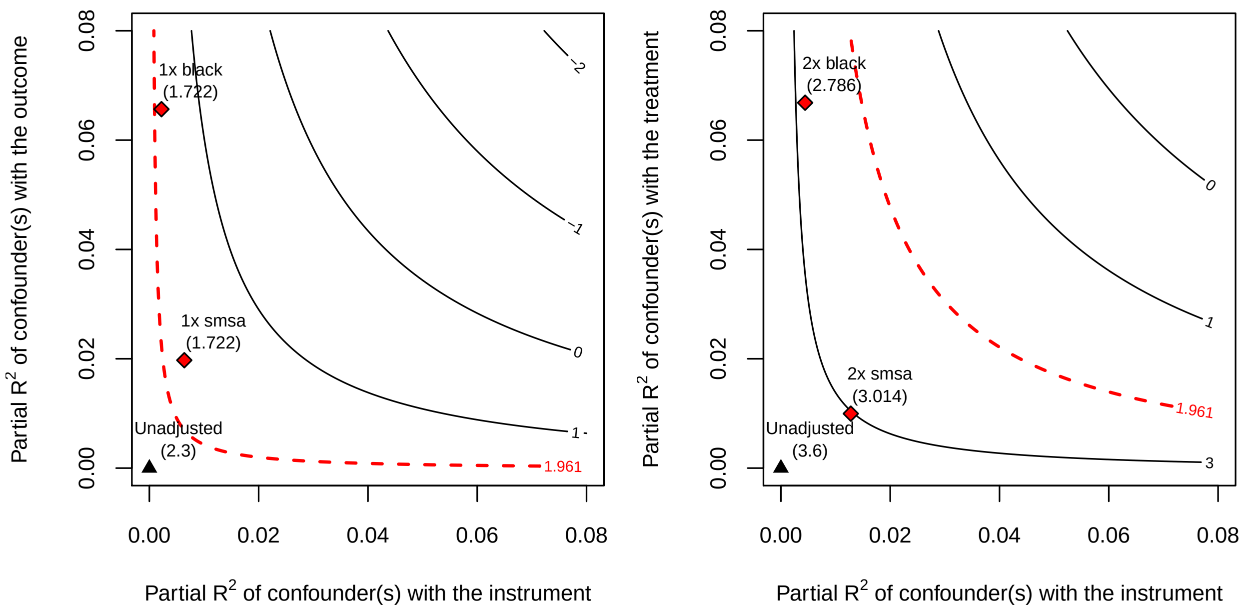
Bound (1x SMSA): $R^2_{Y\sim W|Z,\boldsymbol{X}} = 2\%$, $R^2_{W\sim Z|\boldsymbol{X}} = 0.6\%$, $t^{\dagger\max}_{\alpha,\text{df}-1,\boldsymbol{R}^2} = 2.55$

Note: df = 2994, $q^* = 1$, $\alpha = 0.05$

Table 3: Minimal sensitivity reporting of the reduced-form regression.

For our running example, the robustness value reveals that confounders that explain 0.67% of the residual variation both of *proximity* and of (log) *Earnings* are sufficiently strong to make the reduced-form estimate statistically insignificant, whereas confounders that explain less than 0.67% of the residual variation of both the instrument and of the outcome are not strong enough to do so. The extreme robustness value and the partial R^2 show that, if we are not willing to impose constraints on the strength of confounders with the outcome, then they would need to explain less than 0.05% or 0.18% of the instrument to escape concerns of eliminating statistical significance or fully eliminating the point estimate, respectively. To aid users in making plausibility judgments, the note of Table 3 provides the maximum strength of unobserved confounding if it were as strong as *SMSA* (an indicator variable for whether the individual lived in a metropolitan region) along with the OVB-adjusted critical value for a confounder with such strength, $t^{\dagger\max}_{\alpha,\text{df}-1,\boldsymbol{R}^2} = 2.55$. Since the observed t-value (2.33) is less than the adjusted critical threshold of 2.55, the table immediately reveals that confounding as strong as *SMSA* (for example, in the form of residual geographic confounding) is sufficiently strong to be problematic.

Beyond the results of Table 3, we also advise researchers to provide a sensitivity contour plot of the t-value for testing the null hypothesis of zero effect, while also showing different bounds on strength of confounding, under different assumptions of how they compare to the observed variables. This is shown in Figure 3a. The horizontal axis describes the partial R^2 of the confounder with the instrument whereas the vertical axis describes the partial R^2 of the confounder with the outcome.



(a) Sensitivity contours of the reduced form. (b) Sensitivity contours of the first stage.

Figure 3: Sensitivity contour plots with benchmark bounds for the t-value of: (a) the reduced form; and, (b) the first stage.

The contour lines show the t-value one would have obtained, had a confounder with such postulated strength been included in the reduced-form regression. The red dashed line shows the statistical significance threshold, and the red diamonds places bounds on strength of confounding as strong as *Black* (an indicator for race) and, again, *SMSA*. As we can see, confounders as strong as either *Black* or *SMSA* are sufficient to bring the reduced form, and hence also the IV estimate, to a region which is not statistically different from zero. Since it is not very difficult to imagine residual confounders as strong or stronger than those (e.g., parental income, finer grained geographic location, etc), these results for the reduced form are sufficient to call into question the reliability of the instrumental variable estimate.

5.2 Minimal reporting and sensitivity plots of the first stage

Outcome: *Education* (years)

Instrument	Estimate	Std. Error	t-value	$R^2_{D\sim Z\|\boldsymbol{X}}$	$\text{XRV}_{q^*,\alpha}$	$\text{RV}_{q^*,\alpha}$
Proximity	0.32	0.088	3.64	0.44%	0.31%	3.02%

Bound (1x SMSA): $R^2_{D\sim W|Z,\boldsymbol{X}} = 0.5\%$, $R^2_{Z\sim W|\boldsymbol{X}} = 0.6\%$, $t^{\dagger\,\max}_{\alpha,\text{df}-1,\boldsymbol{R}^2} = 2.26$

Note: df = 2994, $q^* = 1$, $\alpha = 0.05$

Table 4: Minimal sensitivity reporting of the first-stage regression.

We now turn to the sensitivity analysis of the first-stage regression. Table 4 performs the same sensitivity exercises as before, but now for the regression of *Education* (treatment) on *Proximity* (instrument). As expected, the association of proximity to college with years of education is stronger than its association with earnings, and this is also reflected in the robustness statistics, which are slightly higher ($R^2_{D\sim Z|\boldsymbol{X}} = 0.44\%$, $\text{XRV}_{q^*,\alpha} = 0.31\%$ and $\text{RV}_{q^*,\alpha} = 3.02\%$). As the note of Table 4 shows, confounding as strong as *SMSA* would not be sufficiently strong to bring the first-stage estimate to a region where it is not statistically different than zero. Figure 3b supplements those analysis with the sensitivity contour plot for the t-value of the first-stage regression. Here the horizontal axis still describes the partial R^2 of the confounder with the instrument, but now the vertical axis describes the partial R^2 of the confounder with the treatment. The plot reveals that, contrary to the reduced form, the first stage survives confounding once or twice as strong as *Black* or *SMSA*. The contrast of both sensitivity results suggests that, in our running example, the most prominent risk to the validity of the IV estimate comes from residual confounding on the reduced-form estimate.

5.3 Minimal reporting and sensitivity plots of the IV

Finally, we turn our attention to the sensitivity analysis of the IV, and Table 5 shows our proposed minimal sensitivity reporting. We start with the IV point estimate (0.132), as well as the lower limit ($\text{LL}_{1-\alpha} = 0.025$) and the upper limit ($\text{UL}_{1-\alpha} = 0.285$) of the Anderson-Rubin confidence interval. The t-value for testing the null hypothesis of zero effect is also shown (2.33). Next, we propose researchers to report the extreme robustness value $\text{XRV}_{\geq q^*,\alpha}$ and the robustness value $\text{RV}_{\geq q^*,\alpha}$ for bringing the lower limit of the confidence interval to *or beyond* zero (or another meaningful threshold), at the 5% significance level.

Outcome: *Earnings* (log)

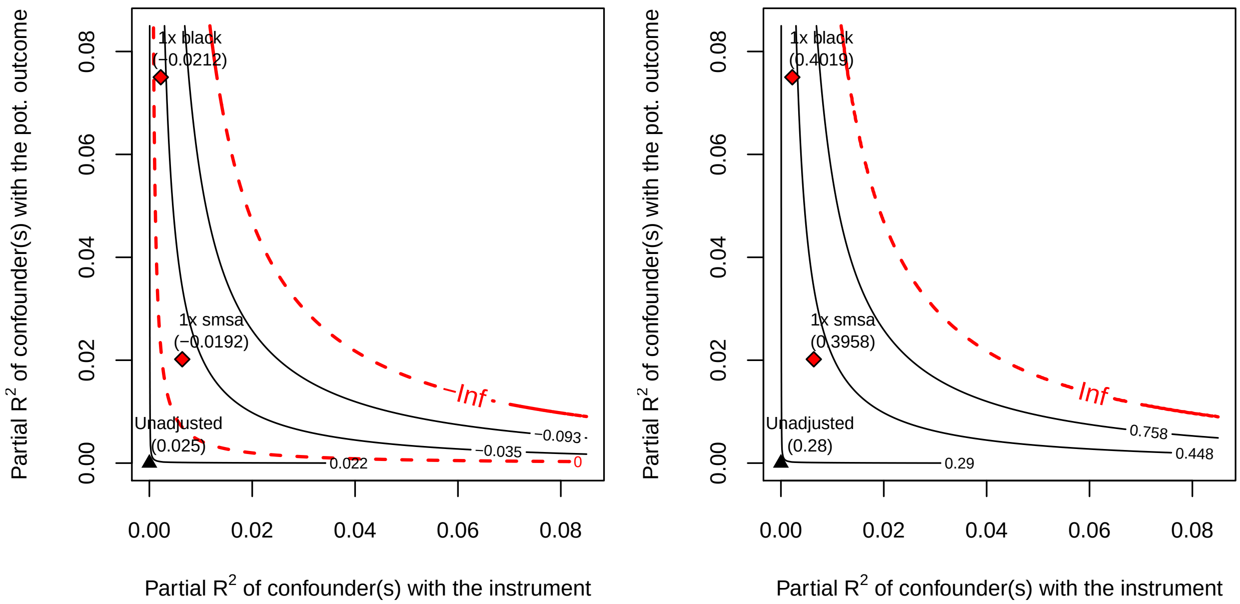
Treatment	Estimate	$\text{LL}_{1-\alpha}$	$\text{UL}_{1-\alpha}$	t-value	$\text{XRV}_{\geq q^*,\alpha}$	$\text{RV}_{\geq q^*,\alpha}$
Education (years)	0.132	0.025	0.285	2.33	0.05%	0.67%

Bound (1x SMSA): $R^2_{Y_0\sim W|Z,\boldsymbol{X}} = 2\%$, $R^2_{W\sim Z|\boldsymbol{X}} = 0.6\%$, $t^{\dagger\max}_{\alpha,\text{df}-1,\boldsymbol{R}^2} = 2.55$
Note: df = 2994, $q^* = 1$, $\alpha = 0.05$

Table 5: Minimal sensitivity reporting of IV estimate (Anderson-Rubin).

As derived in Section 4.2.3, we have that the (extreme) robustness value of the IV estimate for bringing the lower limit of the confidence interval to or below zero is the minimum of either the (extreme) robustness value of the reduced form and the (extreme) robustness value of the first stage. Therefore, the sensitivity statistics of Table 5 essentially reproduce the results of Table 3.

After examining the sensitivity of the first stage and reduced form it is thus, more informative to assess the sensitivity of the IV for null hypotheses *other than zero.* To that end, investigators may wish to examine sensitivity contour plots similar to those of Figure 3, but with contours now showing the adjusted *lower and upper limits* of the confidence interval. These contours are shown Figure 4. Here, as usual, the horizontal axis describes the partial R^2 of the confounder with the instrument, but now the vertical axis describes the partial R^2 of the confounder with the untreated *potential*



(a) Sensitivity contours for the lower limit.

(b) Sensitivity contours for the upper limit.

Figure 4: Sensitivity contour plots for the lower (a) and upper (b) limits of the 95% confidence interval for the IV estimate.

outcome. The contour lines show the worst lower (or upper) limit of the set of compatible inferences considering confounders bounded by such strength. Red dashed lines shows a critical contour line of interest (such as zero) as well as the boundary beyond confidence intervals become unbounded. As the plot reveals, even confounding as strong as *SMSA* could lead to an interval of compatible inferences for the causal effect of $\text{CI}^{\max}_{1-\alpha,\boldsymbol{R}^2}(\tau) = [-0.02, 0.40]$, which includes not only the original OLS estimate (7.5%), but also implausibly high values (40%), or even negative values (-2%), and is thus too wide for any meaningful conclusions regarding the "true" returns to schooling. That is, if we are concerned that omitted variables as strong as *SMSA* might exist, then we are unable to reject any estimates in this range, calling into question the strength of evidence provided by this IV study.

6 Conclusion

In this paper we developed a suite of sensitivity analysis tools for IV that naturally handles multiple "side-effects" and confounders of the instrument, does not require assumptions on the functional form of such omitted variables, and allows exploiting expert knowledge to bound sensitivity parameters. In particular, we introduced new sensitivity statistics for IV estimates that are suited for routine reporting, such as (extreme) robustness values, describing the minimum strength that omitted variables need to have, both with the instrument, and with the untreated potential outcome, to overturn the conclusions of an IV study. We also introduced a novel "OVB-adjusted" critical value that allows researchers to easily perform hypothesis tests or construct confidence intervals that accounts for omitted variable bias of any postulated strength, by simply replacing traditional critical values with the adjusted ones. Finally, we showed how intuitive visual displays can be deployed to fully characterize the sensitivity of IV to violations of its standard assumptions. Extension of these sensitivity analysis tools beyond the "just-identified" case, handling multiple instruments and multiple treatments, is an interesting direction for future work.

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Appendix for
"An Omitted Variable Bias Framework for Sensitivity Analysis of Instrumental Variables"

Carlos Cinelli & Chad Hazlett

A Main estimators for IV

For ease of reference, in this section we show in more detail some of the algebraic identities (and differences) of the main approaches to IV estimation.

A.1 Indirect Least Squares (ILS)

Point Estimate. The ILS estimate is defined as the ratio of the reduced-form and first-stage estimates

$$\hat{\tau}_{\text{ILS}} := \frac{\hat{\lambda}}{\hat{\theta}} \tag{59}$$

Inference. Inference in the ILS framework is usually performed using the delta-method, with estimated variance

$$\widehat{\text{var}}(\hat{\tau}_{\text{ILS}}) := \frac{1}{\hat{\theta}^2}\left(\widehat{\text{var}}(\hat{\lambda}) + \hat{\tau}^2\widehat{\text{var}}(\hat{\theta}) - 2\hat{\tau}\widehat{\text{cov}}(\hat{\lambda}, \hat{\theta})\right) \tag{60}$$

where, using the FWL formulation,

$$\widehat{\text{var}}(\hat{\lambda}) = \frac{\text{var}(Y^{\perp Z,\boldsymbol{X},\boldsymbol{W}})}{\text{var}(Z^{\perp \boldsymbol{X},\boldsymbol{W}})} \times \text{df}^{-1}, \qquad \widehat{\text{var}}(\hat{\theta}) = \frac{\text{var}(D^{\perp Z,\boldsymbol{X},\boldsymbol{W}})}{\text{var}(Z^{\perp \boldsymbol{X},\boldsymbol{W}})} \times \text{df}^{-1} \tag{61}$$

are the estimated variances of the reduced form and first stage, and

$$\widehat{\text{cov}}(\hat{\lambda}, \hat{\theta}) = \frac{\text{cov}(Y^{\perp Z,\boldsymbol{X},\boldsymbol{W}}, D^{\perp Z,\boldsymbol{X},\boldsymbol{W}})}{\text{var}(Z^{\perp \boldsymbol{X},\boldsymbol{W}})} \times \text{df}^{-1} \tag{62}$$

is the estimated covariance of $\hat{\lambda}$ and $\hat{\theta}$.

A.2 Two-Stage Least Squares (2SLS)

Point Estimate. By the FWL theorem, the 2SLS point estimate can be written as

$$\hat{\tau}_{\text{2SLS}} := \frac{\text{cov}(Y^{\perp \boldsymbol{X},\boldsymbol{W}}, \widehat{D}^{\perp \boldsymbol{X},\boldsymbol{W}})}{\text{var}(\widehat{D}^{\perp \boldsymbol{X},\boldsymbol{W}})} \tag{63}$$

In the just-identified case, the ILS and 2SLS point estimates are numerically identical. Expanding $\widehat{D}$ we have that

$$\hat{\tau}_{\text{2SLS}} = \frac{\text{cov}(Y^{\perp \boldsymbol{X},\boldsymbol{W}}, \widehat{D}^{\perp \boldsymbol{X},\boldsymbol{W}})}{\text{var}(\widehat{D}^{\perp \boldsymbol{X},\boldsymbol{W}})} = \frac{\text{cov}(Y^{\perp \boldsymbol{X},\boldsymbol{W}}, \hat{\theta} Z^{\perp \boldsymbol{X},\boldsymbol{W}})}{\text{var}(\hat{\theta} Z^{\perp \boldsymbol{X},\boldsymbol{W}})} \tag{64}$$

$$= \frac{\hat{\theta} \times \text{cov}(Y^{\perp \boldsymbol{X},\boldsymbol{W}}, Z^{\perp \boldsymbol{X},\boldsymbol{W}})}{\hat{\theta}^2 \times \text{var}(Z^{\perp \boldsymbol{X},\boldsymbol{W}})} = \frac{\hat{\lambda}}{\hat{\theta}} \tag{65}$$

Which establishes the equality $\hat{\tau}_{\text{2SLS}} = \hat{\tau}_{\text{ILS}}$.

Inference. By the FWL theorem, the standard two-stage least squares estimate of the variance can be written as

$$\widehat{\text{var}}(\hat{\tau}_{\text{2SLS}}) := \frac{\text{var}(Y^{\perp \boldsymbol{X},\boldsymbol{W}} - \hat{\tau} D^{\perp \boldsymbol{X},\boldsymbol{W}})}{\text{var}(\widehat{D}^{\perp \boldsymbol{X},\boldsymbol{W}})} \times \text{df}^{-1} \tag{66}$$

As with the point estimate, for the just-identified case, the estimated variance of ILS and 2SLS are numerically identical. To see why, note the denominator of Equation 66 can be expanded to

$$\text{var}(\widehat{D}^{\perp \boldsymbol{X},\boldsymbol{W}}) = \text{var}(\hat{\theta} Z^{\perp \boldsymbol{X},\boldsymbol{W}}) = \hat{\theta}^2 \, \text{var}(Z^{\perp \boldsymbol{X},\boldsymbol{W}}) \tag{67}$$

Finally, the numerator can be written as,

$$\text{var}(Y^{\perp \boldsymbol{X},\boldsymbol{W}} - \hat{\tau} D^{\perp \boldsymbol{X},\boldsymbol{W}}) = \text{var}(Y^{\perp \boldsymbol{X},\boldsymbol{W}} - \hat{\tau}(\hat{\theta} Z^{\boldsymbol{X},\boldsymbol{W}} + D^{\perp Z,\boldsymbol{X},\boldsymbol{W}})) \tag{68}$$

$$= \text{var}((Y^{\perp \boldsymbol{X},\boldsymbol{W}} - \hat{\lambda} Z^{\boldsymbol{X},\boldsymbol{W}}) - \hat{\tau} D^{\perp Z,\boldsymbol{X},\boldsymbol{W}}) \tag{69}$$

$$= \text{var}(Y^{\perp Z,\boldsymbol{X},\boldsymbol{W}} - \hat{\tau} D^{\perp Z,\boldsymbol{X},\boldsymbol{W}}) \tag{70}$$

$$= \text{var}(Y^{\perp Z,\boldsymbol{X},\boldsymbol{W}}) + \hat{\tau}^2 \, \text{var}(D^{\perp Z,\boldsymbol{X},\boldsymbol{W}}) - 2\hat{\tau} \, \text{cov}(Y^{\perp Z,\boldsymbol{X},\boldsymbol{W}}, D^{\perp Z,\boldsymbol{X},\boldsymbol{W}}) \tag{71}$$

Plugging in Equations 71 and 67 back in Equation 66, then using Equations 61 and 62 establishes the desired equality.

A.3 Anderson-Rubin (AR)

Point Estimate. We define the Anderson-Rubin point estimate to be the value of τ_0 that makes $\hat{\phi} = 0$, ie,

$$\hat{\tau}_{\text{AR}} = \{\tau_0; \; \hat{\phi}_{\tau_0} = 0\} \tag{72}$$

Resorting again to the FWL theorem, we can write the regression coefficient of the AR regression, $\hat{\phi}_{\tau_0}$, as a function of the regression coefficients of the first stage and reduced form,

$$\hat{\phi}_{\tau_0} = \frac{\text{cov}(Y^{\perp \boldsymbol{X},\boldsymbol{W}} - \tau_0 D^{\perp \boldsymbol{X},\boldsymbol{W}}, Z^{\perp \boldsymbol{X},\boldsymbol{W}})}{\text{var}(Z^{\perp \boldsymbol{X},\boldsymbol{W}})} \tag{73}$$

$$= \frac{\text{cov}(Y^{\perp \boldsymbol{X},\boldsymbol{W}}, Z^{\perp \boldsymbol{X},\boldsymbol{W}})}{\text{var}(Z^{\perp \boldsymbol{X},\boldsymbol{W}})} - \tau_0 \frac{\text{cov}(D^{\perp \boldsymbol{X},\boldsymbol{W}}, Z^{\perp \boldsymbol{X},\boldsymbol{W}})}{\text{var}(Z^{\perp \boldsymbol{X},\boldsymbol{W}})} \tag{74}$$

$$= \hat{\lambda} - \tau_0 \hat{\theta} \tag{75}$$

Thus solving for the condition $\hat{\phi}_{\tau_0} = 0$ gives us

$$\hat{\tau}_{AR} = \frac{\hat{\lambda}}{\hat{\theta}} \tag{76}$$

Which establishes the equality $\hat{\tau}_{AR} = \hat{\tau}_{ILS}$. Therefore, all the point estimates of ILS, 2SLS and AR are numerically identical.

Inference. The AR confidence interval with significance level α is defined as all values of τ_0 such that we cannot reject the null hypothesis $H_0 : \phi_{\tau_0} = 0$ at the chosen significance level

$$\text{CI}_{1-\alpha}(\tau) = \{\tau_0; t^2_{\hat{\phi}_{\tau_0}} \leq t^{*2}_{\alpha,\text{df}}\} \tag{77}$$

This confidence interval can be obtained analytically as functions of the estimates of the first-stage and reduced form regressions. As shown in Equation 75, $\hat{\phi}_{\tau_0}$ can be written as the linear combination

$$\hat{\phi}_{\tau_0} = \hat{\lambda} - \tau_0\hat{\theta} \tag{78}$$

Likewise, by the FWL theorem, the estimated variance is given by

$$\begin{aligned}
\widehat{\text{var}}(\hat{\phi}_{\tau_0}) &= \frac{\text{var}(Y^{\perp Z,\boldsymbol{X},\boldsymbol{W}} - \tau_0 D^{\perp Z,\boldsymbol{X},\boldsymbol{W}})}{\text{var}(Z^{\perp \boldsymbol{X},\boldsymbol{W}})} \times \text{df}^{-1} &(79)\\
&= \left(\frac{\text{var}(Y^{\perp Z,\boldsymbol{X},\boldsymbol{W}})}{\text{var}(Z^{\perp \boldsymbol{X},\boldsymbol{W}})} + \tau_0^2 \frac{\text{var}(D^{\perp Z,\boldsymbol{X},\boldsymbol{W}})}{\text{var}(Z^{\perp \boldsymbol{X},\boldsymbol{W}})} - 2\tau_0 \frac{\text{cov}(Y^{\perp Z,\boldsymbol{X},\boldsymbol{W}}, D^{\perp Z,\boldsymbol{X},\boldsymbol{W}})}{\text{var}(Z^{\perp \boldsymbol{X},\boldsymbol{W}})}\right) \times \text{df}^{-1} &(80)\\
&= \widehat{\text{var}}(\hat{\lambda}) + \tau_0^2 \widehat{\text{var}}(\hat{\theta}) - 2\tau_0 \widehat{\text{cov}}(\hat{\lambda}, \hat{\theta}) &(81)
\end{aligned}$$

Thus, we obtain that the t-value $t_{\hat{\phi}_{\tau_0}}$ for testing the null hypothesis $H_0 : \phi_{\tau_0} = 0$ equals to

$$t_{\hat{\phi}_{\tau_0}} = \frac{\hat{\lambda} - \tau_0\hat{\theta}}{\sqrt{\widehat{\text{var}}(\hat{\lambda}) + \tau_0^2 \widehat{\text{var}}(\hat{\theta}) - 2\tau_0 \widehat{\text{cov}}(\hat{\lambda}, \hat{\theta})}} \tag{82}$$

And our task is to find all values of τ_0 such that the following inequality holds

$$\frac{(\hat{\lambda} - \tau_0\hat{\theta})^2}{\widehat{\text{var}}(\hat{\lambda}) + \tau_0^2 \widehat{\text{var}}(\hat{\theta}) - 2\tau_0 \widehat{\text{cov}}(\hat{\lambda}, \hat{\theta})} \leq t^{*2}_{\alpha,\text{df}} \tag{83}$$

First, note that the empty set is not possible here. If we pick $\tau_0 = \hat{\tau}_{\text{AR}}$, then the numerator in Equation 83 is zero, and the inequality trivially holds—therefore, the point-estimate is always included in the confidence interval. Now squaring and rearranging terms we obtain

$$\underbrace{\left(\hat{\theta}^2 - \widehat{\text{var}}(\hat{\theta}) \times t^{*2}_{\alpha,\text{df}}\right)}_{a} \tau_0^2 + \underbrace{2\left(\widehat{\text{cov}}(\hat{\lambda}, \hat{\theta}) \times t^{*2}_{\alpha,\text{df}} - \hat{\lambda}\hat{\theta}\right)}_{b} \tau_0 + \underbrace{\left(\hat{\lambda}^2 - \widehat{\text{var}}(\hat{\lambda}) \times t^{*2}_{\alpha,\text{df}}\right)}_{c} \leq 0 \tag{84}$$

Our task has simplified to find all values of τ_0 that makes the above quadratic equation, with coefficients a, b and c, non-positive. As discussed in Section 4.2.2, this confidence intervals can take three different forms, depending on the instrument strength: (i) finite and connected, (ii) the union two disjoint half lines; or, (iii) the whole real line.

A.4 Fieller's theorem

Fieller's proposal to test the null hypothesis $H_0 : \tau = \tau_0$ is to construct the linear combination $\hat{\phi}_{\tau_0} = \hat{\lambda} - \tau_0\hat{\theta}$, and to test the null hypothesis $H_0 : \phi_{\tau_0} = 0$. The standard estimated variance for $\hat{\phi}_{\tau_0}$ equals Equation 81, resulting in a test statistic equal to Equation 82, and thus numerically identical to the AR approach.

B OVB-adjusted critical values and set of compatible inferences

B.1 OVB-adjusted critical values

As in the main text, using the reduced form as an example, let $\text{LL}_{1-\alpha}(\lambda) := \hat{\lambda} - t^*_{\alpha,\text{df}-1} \times \widehat{\text{se}}(\hat{\lambda})$ be the lower limit of a $1-\alpha$ level confidence interval of the full reduced form regression, where $t^*_{\alpha,\text{df}-1}$ denotes the critical α-level threshold of the t-distribution with df -1 degrees of freedom. Considering the direction of the bias that reduces the lower limit, Equations 24 and 26 imply

$$\begin{aligned}
\text{LL}_{1-\alpha}(\lambda) &:= \hat{\lambda} - t^*_{\alpha,\text{df}-1} \times \widehat{\text{se}}(\hat{\lambda}) && (85)\\
&= \hat{\lambda}_{\text{res}} - \text{BF}\sqrt{\text{df}} \times \widehat{\text{se}}(\hat{\lambda}_{\text{res}}) - t^*_{\alpha,\text{df}-1} \times \text{SEF}\sqrt{\text{df}/(\text{df}-1)} \times \widehat{\text{se}}(\hat{\lambda}_{\text{res}}) && (86)\\
&= \hat{\lambda}_{\text{res}} - \left(\text{SEF}\sqrt{\text{df}/(\text{df}-1)} \times t^*_{\alpha,\text{df}-1} + \text{BF}\sqrt{\text{df}}\right) \times \widehat{\text{se}}(\hat{\lambda}_{\text{res}}) && (87)
\end{aligned}$$

Similarly, now let $\text{UL}_{1-\alpha}(\lambda)$ the upper limit of the confidence interval and consider the direction of the bias that increases the upper limit. By the same algebraic manipulations, we obtain

$$\text{UL}_{1-\alpha}(\lambda) = \hat{\lambda}_{\text{res}} + \left(\text{SEF}\sqrt{\text{df}/(\text{df}-1)} \times t^*_{\alpha,\text{df}-1} + \text{BF}\sqrt{\text{df}}\right) \times \widehat{\text{se}}(\hat{\lambda}_{\text{res}}) \tag{88}$$

Note that, in both Equations 87 and 88, the only part that depends on the omitted variable W is the common multiple of the observed standard error, which defines the new *OVB-adjusted critical value*,

$$t^{\dagger}_{\alpha,\text{df}-1,\boldsymbol{R}^2} := \text{SEF}\sqrt{\text{df}/(\text{df}-1)} \times t^*_{\alpha,\text{df}-1} + \text{BF}\sqrt{\text{df}}. \tag{89}$$

B.2 Compatible inferences given bounds on the partial R^2

Now suppose the analyst wishes to investigate the worst possible lower (or upper) limits of the confidence intervals induced by a confounder with strength no stronger than certain bounds, for instance, $R^2_{Y\sim W|Z,\boldsymbol{X}} \leq R^{2\max}_{Y\sim W|Z,\boldsymbol{X}}$ and $R^2_{Z\sim W|\boldsymbol{X}} \leq R^{2\max}_{Z\sim W|\boldsymbol{X}}$. As per the last section, this amounts to finding the largest *OVB-adjusted critical value* induced by an omitted variable W with at most such strength. That is, we need to solve the following maximization problem

$$\max_{R^2_{Y\sim W|Z,\boldsymbol{X}},R^2_{Z\sim W|\boldsymbol{X}}} t^{\dagger}_{\alpha,\text{df}-1,\boldsymbol{R}^2} \quad \text{s.t.} \quad R^2_{Y\sim W|Z,\boldsymbol{X}} \leq R^{2\max}_{Y\sim W|Z,\boldsymbol{X}}, \quad R^2_{Z\sim W|\boldsymbol{X}} \leq R^{2\max}_{Z\sim W|\boldsymbol{X}} \tag{90}$$

Dividing $t^{\dagger}_{\alpha,\text{df}-1,\boldsymbol{R}^2}$ by $\sqrt{\text{df}}$ and letting $f^*_{\alpha,\text{df}-1} := t^*_{\alpha,\text{df}-1}/\sqrt{\text{df}-1}$, we see that the derivative of $t^{\dagger}_{\alpha,\text{df}-1,\boldsymbol{R}^2}$ with respect to $R^2_{Z\sim W|\boldsymbol{X}}$ is always increasing, since

$$\frac{\partial(t^{\dagger}_{\alpha,\text{df}-1,\boldsymbol{R}^2}/\sqrt{\text{df}})}{\partial R^2_{Z\sim W|\boldsymbol{X}}} = \frac{\partial\,\text{BF}}{\partial R^2_{Z\sim W|\boldsymbol{X}}} + f^*_{\alpha,\text{df}-1} \times \frac{\partial\,\text{SEF}}{\partial R^2_{Z\sim W|\boldsymbol{X}}} \tag{91}$$

$$= \frac{(R^2_{Y\sim W|Z,\boldsymbol{X}})^{1/2}}{2(1-R^2_{Z\sim W|\boldsymbol{X}})^{3/2}(R^2_{Z\sim W|\boldsymbol{X}})^{1/2}} + f^*_{\alpha,\text{df}-1}\frac{(1-R^2_{Y\sim W|Z,\boldsymbol{X}})^{1/2}}{2(1-R^2_{Z\sim W|\boldsymbol{X}})^{3/2}} \tag{92}$$

$$= \frac{(R^2_{Y\sim W|Z,\boldsymbol{X}})^{1/2} + f^*_{\alpha,\text{df}-1}(1-R^2_{Y\sim W|Z,\boldsymbol{X}})^{1/2}(R^2_{Z\sim W|\boldsymbol{X}})^{1/2}}{2(1-R^2_{Z\sim W|\boldsymbol{X}})^{3/2}(R^2_{Z\sim W|\boldsymbol{X}})^{1/2}} \geq 0 \tag{93}$$

Therefore, the "optimal" $R^{2*}_{Z\sim W|\boldsymbol{X}}$ (the one the minimizes (maximizes) the lower (upper) limit of the confidence interval) always reaches the bound. However, the same is not true for the derivative with respect to $R^2_{Y\sim W|Z,\boldsymbol{X}}$. To see that, write,

$$\frac{\partial(t^{\dagger}_{\alpha,\text{df}-1,\boldsymbol{R}^2}/\sqrt{\text{df}})}{\partial R^2_{Y\sim W|Z,\boldsymbol{X}}} = \frac{\partial\,\text{BF}}{\partial R^2_{Y\sim W|Z,\boldsymbol{X}}} + f^*_{\alpha,\text{df}-1} \times \frac{\partial\,\text{SEF}}{\partial R^2_{Y\sim W|Z,\boldsymbol{X}}} \tag{94}$$

$$= \frac{(R^2_{Z\sim W|\boldsymbol{X}})^{1/2}}{2(1-R^2_{Z\sim W|\boldsymbol{X}})^{1/2}(R^2_{Y\sim W|Z,\boldsymbol{X}})^{1/2}} + \frac{-f^*_{\alpha,\text{df}-1}}{2(1-R^2_{Y\sim W|Z,\boldsymbol{X}})^{1/2}(1-R^2_{Z\sim W|\boldsymbol{X}})^{1/2}} \tag{95}$$

$$= \frac{(R^2_{Z\sim W|\boldsymbol{X}})^{1/2}(1-R^2_{Y\sim W|Z,\boldsymbol{X}})^{1/2} - f^*_{\alpha,\text{df}-1}(R^2_{Y\sim W|Z,\boldsymbol{X}})^{1/2}}{2(R^2_{Y\sim W|Z,\boldsymbol{X}})^{1/2}(1-R^2_{Y\sim W|Z,\boldsymbol{X}})^{1/2}(1-R^2_{Z\sim W|\boldsymbol{X}})^{1/2}} \tag{96}$$

That is, due to the variance reduction factor of the omitted variable (VRF in Equation 26), it could be the case that increasing $R^2_{Y\sim W|Z,\boldsymbol{X}}$ reduces the standard error more than enough to compensate for the increase in bias, resulting in tighter confidence intervals.

We have, thus, two cases. First, consider the case in which the optimal point reaches both bounds. In that case, the numerator of Equation 96 must be positive when evaluated at the solution. Rearranging and squaring, we see that this happens when

$$R^{2\max}_{Z\sim W|\boldsymbol{X}} \geq f^{*2}_{\alpha,\text{df}-1} \times f^{2\max}_{Y\sim W|Z,\boldsymbol{X}} \tag{97}$$

Clearly, when considering the sensitivity of the point estimate, we have $f^*_{\alpha,\text{df}-1} = 0$, and the condition always holds. If condition of Equation 97 fails, then the optimal $R^{2*}_{Y\sim W|Z,\boldsymbol{X}}$ will be an interior point. This will happen when the numerator of Equation 96 equals zero. Since we know $R^2_{Z\sim W|\boldsymbol{X}}$ reaches its maximum, the optimal $R^{2*}_{Y\sim W|Z,\boldsymbol{X}}$ will be,

$$R^{2*}_{Y\sim W|Z,\boldsymbol{X}} = \frac{R^{2\max}_{Z\sim W|\boldsymbol{X}}}{f^{*2}_{\alpha,\text{df}-1} + R^{2\max}_{Z\sim W|\boldsymbol{X}}} \tag{98}$$

Denoting the solution to the optimization problem as $t^{\dagger\max}_{\alpha,\text{df}-1,\boldsymbol{R}^2}$, the *most extreme possible* lower and upper limits after adjusting for W are given by

$$\text{LL}^{\max}_{1-\alpha,\boldsymbol{R}^2}(\lambda) = \hat{\lambda}_{\text{res}} - t^{\dagger\max}_{\alpha,\text{df}-1,\boldsymbol{R}^2} \times \widehat{\text{se}}(\hat{\lambda}_{\text{res}}), \quad \text{UL}^{\max}_{1-\alpha,\boldsymbol{R}^2} = \hat{\lambda}_{\text{res}} + t^{\dagger\max}_{\alpha,\text{df}-1,\boldsymbol{R}^2} \times \widehat{\text{se}}(\hat{\lambda}_{\text{res}}) \tag{99}$$

And interval composed of such limits

$$\text{CI}^{\max}_{1-\alpha,\boldsymbol{R}^2}(\lambda) = \Big[\text{LL}^{\max}_{1-\alpha,\boldsymbol{R}^2}(\lambda), \quad \text{UL}^{\max}_{1-\alpha,\boldsymbol{R}^2}(\lambda)\Big] \tag{100}$$

Defines the set of compatible inferences given the bounds on the partial R^2, $R^2_{Y\sim W|Z,\boldsymbol{X}} \leq R^{2\max}_{Y\sim W|Z,\boldsymbol{X}}$ and $R^2_{Z\sim W|\boldsymbol{X}} \leq R^{2\max}_{Z\sim W|\boldsymbol{X}}$.

C (Extreme) Robustness Values

C.1 The Extreme Robustness Value

The *Extreme Robustness Value* $\text{XRV}_{q^*,\alpha}(\lambda)$ is defined as the greatest lower bound XRV on the sensitivity parameter $R^2_{Z\sim W|\boldsymbol{X}}$, while keeping the parameter $R^2_{Y\sim W|Z,\boldsymbol{X}}$ unconstrained, such that the null hypothesis that a change of $(100 \times q)\%$ of the original estimate, $H_0 : \lambda = (1 - q^*)\hat{\lambda}_{\text{res}}$, is not rejected at the α level:

$$\text{XRV}_{q^*,\alpha}(\lambda) := \inf\Big\{\text{XRV};\ (1 - q^*)\hat{\lambda}_{\text{res}} \in \text{CI}^{\max}_{1-\alpha,1,\text{XRV}}(\lambda)\Big\} \tag{101}$$

First, consider the case where $f_{q^*}(\lambda) < f^*_{\alpha,\text{df}-1}$. Note the XRV will be zero, since we already cannot reject the null hypothesis $H_0 : \lambda = (1 - q^*)\hat{\lambda}_{\text{res}}$ even assuming zero omitted variable bias. Next, note that, when $f^*_{\alpha,\text{df}-1} > 0$, we can always pick a large enough value for $R^2_{Y\sim W|Z,\boldsymbol{X}}$ until condition 97 fails (since $f^2_{Y\sim W|Z,\boldsymbol{X}}$ is unbounded). Therefore, XRV will be given by an interior point solution. Using Equation 98 to express $t^{\dagger\max}_{\alpha,\text{df}-1,\boldsymbol{R}^2}$ solely in terms of the optimal $R^2_{Z\sim W|\boldsymbol{X}}$, and solving for the value that gives us $(1 - q^*)\hat{\lambda}_{\text{res}}$, we obtain

$$\text{XRV}_{q^*,\alpha}(\lambda) = \begin{cases} 0, & \text{if } \ f_{q^*}(\lambda) \leq f^*_{\alpha,\text{df}-1} \\ \dfrac{f^2_{q^*}(\lambda) - f^{*2}_{\alpha,\text{df}-1}}{1 + f^2_{q^*}(\lambda)}, & \text{otherwise.} \end{cases} \tag{102}$$

C.2 The Robustness Value

The *Robustness Value* $\text{RV}_{q^*,\alpha}(\lambda)$ for not rejecting the null hypothesis that $H_0 : \lambda = (1 - q^*)\hat{\lambda}_{\text{res}}$, at the significance level α, is defined as

$$\text{RV}_{q^*,\alpha}(\lambda) := \inf\Big\{\text{RV};\ (1 - q^*)\hat{\lambda}_{\text{res}} \in \text{CI}^{\max}_{1-\alpha,\text{RV},\text{RV}}(\lambda)\Big\} \tag{103}$$

Where now we consider both sensitivity parameters bounded by RV. Again, consider the case where $f_{q^*}(\lambda) < f^*_{\alpha,\text{df}-1}$. The RV then must be zero, since we already cannot reject the null hypothesis $H_0 : \lambda = (1 - q^*)\hat{\lambda}_{\text{res}}$ given the current data. Next, let's consider the case when the bound on $R^2_{Y\sim W|Z,\boldsymbol{X}}$ is not biding—here our optimization problem reduces to the XRV case. Finally, we have the solution in which both coordinates achieve the bound, resulting in a quadratic equation as solved in Cinelli and Hazlett (2020). We thus have,

$$\mathrm{RV}_{q^*,\alpha}(\lambda) = \begin{cases} 0, & \text{if } \; f_{q^*}(\lambda) \leq f^*_{\alpha,\mathrm{df}-1} \\ \frac{1}{2}\left(\sqrt{f^4_{q^*,\alpha}(\lambda) + 4f^2_{q^*,\alpha}(\lambda)} - f^2_{q^*,\alpha}(\lambda)\right), & \text{if } \; f^*_{\alpha,\mathrm{df}-1} < f_{q^*}(\lambda) < f^{*-1}_{\alpha,\mathrm{df}-1} \\ \mathrm{XRV}_{q^*,\alpha}(\lambda), & \text{otherwise.} \end{cases} \tag{104}$$

The condition $f_{q*}(\lambda) < f^{*-1}_{\alpha,\mathrm{df}-1}$, stems from the fact that the XRV solution cannot satisfy Equation 97. We now show that this is equivalent to the condition $\mathrm{RV}_{q^*,\alpha}(\lambda) > 1 - 1/f^2_{q^*}(\lambda)$ that Cinelli and Hazlett (2020) had previously established. If $f_{q^*}(\lambda) < 1/f^*_{\alpha,\mathrm{df}-1}$ then,

$$\begin{aligned}
\mathrm{RV}_{q^*,\alpha}(\lambda) &= \frac{1}{2}\left(\sqrt{f^4_{q^*,\alpha}(\lambda) + 4f^2_{q^*,\alpha}(\lambda)} - f^2_{q^*,\alpha}(\lambda)\right) && (105)\\
&= \frac{1}{2}\left(\sqrt{(f_{q^*}(\lambda) - f^*_{\alpha,\mathrm{df}-1})^4 + 4(f_{q^*}(\lambda) - f^*_{\alpha,\mathrm{df}-1})^2} - (f_{q^*}(\lambda) - f^*_{\alpha,\mathrm{df}-1})^2\right) && (106)\\
&> \frac{1}{2}\left(\sqrt{(f_{q^*}(\lambda) - 1/f_{q^*}(\lambda))^4 + 4(f_{q^*}(\lambda) - 1/f_{q^*}(\lambda))^2} - (f_{q^*}(\lambda) - 1/f_{q^*}(\lambda))^2\right) && (107)\\
&= \frac{1}{2}\left(\sqrt{\left(\frac{f^2_q(\lambda) - 1}{f_{q^*}(\lambda)}\right)^4 + 4\left(\frac{f^2_{q^*}(\lambda) - 1}{f_{q^*}(\lambda)}\right)^2} - \left(\frac{f^2_{q^*}(\lambda) - 1}{f_{q^*}(\lambda)}\right)^2\right) && (108)\\
&= \left(\frac{1}{2}\right)\left(\frac{f^2_{q^*}(\lambda) - 1}{f^2_{q^*}(\lambda)}\right)\left(\sqrt{(f^2_q(\lambda) - 1)^2 + 4f^2_{q^*}(\lambda)} - f^2_{q^*}(\lambda) + 1\right) && (109)\\
&= \left(\frac{1}{2}\right)\left(1 - 1/f^2_{q^*}(\lambda)\right)\left(\sqrt{f^4_q(\lambda) + 1 - 2f^2_{q^*}(\lambda) + 4f^2_{q^*}(\lambda)} - f^2_{q^*}(\lambda) + 1\right) && (110)\\
&= \left(\frac{1}{2}\right)\left(1 - 1/f^2_{q^*}(\lambda)\right)\left(\sqrt{f^4_q(\lambda) + 1 + 2f^2_{q^*}(\lambda)} - f^2_{q^*}(\lambda) + 1\right) && (111)\\
&= \left(\frac{1}{2}\right)\left(1 - 1/f^2_{q^*}(\lambda)\right)\left(f^2_{q^*}(\lambda) + 1 - f^2_{q^*}(\lambda) + 1\right) && (112)\\
&= 1 - 1/f^2_{q^*}(\lambda) && (113)
\end{aligned}$$

Therefore, $f_{q^*}(\lambda) < 1/f^*_{\alpha,\mathrm{df}-1} \implies \mathrm{RV}_{q^*,\alpha}(\lambda) > 1 - 1/f^2_{q^*}(\lambda)$. By the same argument one can derive $\mathrm{RV}_{q^*,\alpha}(\lambda) > 1 - 1/f^2_{q^*}(\lambda) \implies f_q(\lambda) > 1/f^*_{\alpha,\mathrm{df}-1}$. Hence, both conditions are equivalent. The new condition, however, is much simpler to verify.

D Bounds on the strength of W

Let X_j be a specific covariate of the set $\boldsymbol{X}$. Now define

$$k_Z := \frac{R^2_{Z\sim W|\boldsymbol{X}_{-j}}}{R^2_{Z\sim X_j|\boldsymbol{X}_{-j}}}, \qquad k_Y := \frac{R^2_{Y\sim W|Z,\boldsymbol{X}_{-j}}}{R^2_{Y\sim X_j|Z\boldsymbol{X}_{-j}}}. \tag{114}$$

Where $\boldsymbol{X}_{-j}$ is the set $\boldsymbol{X}$ excluding covariate X_j. Our goal in this section is to re-express (or bound) both sensitivity parameters as a function of the new parameters k_Z and k_Y and the observed data.

Cinelli and Hazlett (2020) showed how to obtains bounds for the strength of W under the assumption that $R^2_{W\sim X_j|\boldsymbol{X}_{-j}} = 0$, or, equivalently, when we consider the part of W not linearly explained by $\boldsymbol{X}$. This result may be particularly useful when considering both $\boldsymbol{X}$ and W as *causes* of Z, as in such cases contemplating the marginal orthogonality of W (or its part not explained by observed covariates) is more natural.

Here we additionally provide bounds under the assumption that $R^2_{W\sim X_j|Z,\boldsymbol{X}_{-j}} = 0$. This condition may be helpful when contemplating the strength of W against X_j whenever these variables are *side-effects* of Z, instead of causes of Z. In such cases, reasoning about the marginal orthogonality of W with respect to $\boldsymbol{X}$ may not be natural, as Z itself is also a source of dependence between these variables.

We can thus start by re-expressing $R^2_{Y\sim W|Z,\boldsymbol{X}}$ in terms of k_Y, which in this case is straightforward. Using the recursive definition of partial correlations, and considering our two conditions $R^2_{W\sim X_j|Z,\boldsymbol{X}_{-j}} = 0$ and $R^2_{Y\sim W|Z,\boldsymbol{X}_{-j}} = k_Y R^2_{Y\sim X_j|Z\boldsymbol{X}_{-j}}$, we obtain

$$\left|R_{Y\sim W|Z,\boldsymbol{X}}\right| = \left|\frac{R_{Y\sim W|Z,\boldsymbol{X}_-j} - R_{Y\sim X_j|Z,\boldsymbol{X}_-j}R_{W\sim X_j|Z,\boldsymbol{X}_-j}}{\sqrt{1-R^2_{Y\sim X_j|Z,\boldsymbol{X}_-j}}\sqrt{1-R^2_{W\sim X_j|Z,\boldsymbol{X}_-j}}}\right| \tag{115}$$

$$= \left|\frac{R_{Y\sim W|Z,\boldsymbol{X}_-j}}{\sqrt{1-R^2_{Y\sim X_j|Z,\boldsymbol{X}_-j}}}\right| \tag{116}$$

$$= \left|\frac{\sqrt{k_Y}R_{Y\sim X_j|Z,\boldsymbol{X}_{-j}}}{\sqrt{1-R^2_{Y\sim X_j|Z,\boldsymbol{X}_-j}}}\right| \tag{117}$$

$$= \sqrt{k_Y}\left|f_{Y\sim X_j|Z,\boldsymbol{X}_{-j}}\right| \tag{118}$$

Hence,

$$R^2_{Y\sim W|Z,\boldsymbol{X}} = k_Y \times f^2_{Y\sim X_j|Z,\boldsymbol{X}_{-j}} \tag{119}$$

Moving to bound $R^2_{Z\sim W|\boldsymbol{X}}$, it is useful to first note that the conditions $R^2_{W\sim X_j|Z,\boldsymbol{X}_{-j}} = 0$ and $R^2_{Z\sim W|\boldsymbol{X}_{-j}} = k_Z R^2_{Z\sim X_j|\boldsymbol{X}_{-j}}$ allow us to re-express $R_{W\sim X_j|\boldsymbol{X}_{-j}}$ as a function of k_Z

$$R_{W\sim X_j|Z,\boldsymbol{X}_{-j}} = 0 \implies \frac{R_{W\sim X_j|\boldsymbol{X}_{-j}} - R_{W\sim Z|\boldsymbol{X}_{-j}}R_{X_j\sim Z|\boldsymbol{X}_{-j}}}{\sqrt{1-R^2_{W\sim Z|\boldsymbol{X}_{-j}}}\sqrt{1-R^2_{X_j\sim Z|\boldsymbol{X}_{-j}}}} = 0 \tag{120}$$

$$\implies R_{W\sim X_j|\boldsymbol{X}_{-j}} - R_{W\sim Z|\boldsymbol{X}_{-j}}R_{X_j\sim Z|\boldsymbol{X}_{-j}} = 0 \tag{121}$$

$$\implies R_{W\sim X_j|\boldsymbol{X}_{-j}} = R_{W\sim Z|\boldsymbol{X}_{-j}}R_{X_j\sim Z|\boldsymbol{X}_{-j}} \tag{122}$$

$$\implies R_{W\sim X_j|\boldsymbol{X}_{-j}} = R_{Z\sim W|\boldsymbol{X}_{-j}}R_{Z\sim X_j|\boldsymbol{X}_{-j}} \tag{123}$$

$$\implies |R_{W\sim X_j|\boldsymbol{X}_{-j}}| = \sqrt{k_Z}R^2_{Z\sim X_j|\boldsymbol{X}_{-j}} \tag{124}$$

Now we can re-write $R^2_{Z\sim W|\boldsymbol{X}}$ using the recursive definition of partial correlations

$$\left|R_{Z\sim W|\boldsymbol{X}}\right| = \left|\frac{R_{Z\sim W|\boldsymbol{X}_{-j}} - R_{Z\sim X_j|\boldsymbol{X}_{-j}} R_{W\sim X_j|\boldsymbol{X}_{-j}}}{\sqrt{1-R^2_{Z\sim X_j|\boldsymbol{X}_{-j}}}\sqrt{1-R^2_{W\sim X_j|\boldsymbol{X}_{-j}}}}\right| \tag{125}$$

$$\leq \frac{\left|R_{Z\sim W|\boldsymbol{X}_{-j}}\right| + \left|R_{Z\sim X_j|\boldsymbol{X}_{-j}} R_{W\sim X_j|\boldsymbol{X}_{-j}}\right|}{\sqrt{1-R^2_{Z\sim X_j|\boldsymbol{X}_{-j}}}\sqrt{1-R^2_{W\sim X_j|\boldsymbol{X}_{-j}}}} \tag{126}$$

$$= \frac{\left|\sqrt{k_Z} R_{Z\sim X_j|\boldsymbol{X}_{-j}}\right| + \left|\sqrt{k_Z} R^3_{Z\sim X_j|\boldsymbol{X}_{-j}}\right|}{\sqrt{1-R^2_{Z\sim X_j|\boldsymbol{X}_{-j}}}\sqrt{1-k_Z R^4_{Z\sim X_j|\boldsymbol{X}_{-j}}}} \tag{127}$$

$$= \left(\frac{\sqrt{k_Z} + \left|R^3_{Z\sim X_j|\boldsymbol{X}_{-j}}\right|}{\sqrt{1-k_Z R^4_{Z\sim X_j|\boldsymbol{X}_{-j}}}}\right) \times \left(\frac{\left|R_{Z\sim X_j|\boldsymbol{X}_{-j}}\right|}{\sqrt{1-R^2_{Z\sim X_j|\boldsymbol{X}_{-j}}}}\right) \tag{128}$$

$$= \eta' |f_{Z\sim X_j|\boldsymbol{X}_{-j}}| \tag{129}$$

Hence we have that

$$R^2_{Z\sim W|\boldsymbol{X}} \leq {\eta'}^2 f^2_{Z\sim X_j|\boldsymbol{X}_{-j}} \tag{130}$$

Where $\eta' = \left(\frac{\sqrt{k_Z} + \left|R^3_{Z\sim X_j|\boldsymbol{X}_{-j}}\right|}{\sqrt{1-k_Z R^4_{Z\sim X_j|\boldsymbol{X}_{-j}}}}\right)$.