Testing the identification of causal effects in observational data

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Abstract

This study demonstrates the existence of a testable condition for the identification of the causal effect of a treatment on an outcome in observational data, which relies on two sets of variables: observed covariates to be controlled for and a suspected instrument. Under a causal structure commonly found in empirical applications, the testable conditional independence of the suspected instrument and the outcome given the treatment and the covariates has two implications. First, the instrument is valid, i.e. it does not directly affect the outcome (other than through the treatment) and is unconfounded conditional on the covariates. Second, the treatment is unconfounded conditional on the covariates such that the treatment effect is identified. We suggest tests of this conditional independence based on machine learning methods that account for covariates in a data-driven way and investigate their asymptotic behavior and finite sample performance in a simulation study. We also apply our testing approach to evaluating the impact of fertility on female labor supply when using the sibling sex ratio of the first two children as supposed instrument, which by and large points to a violation of our testable implication for the moderate set of socio-economic covariates considered.

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1 Introduction

Causal inference methods for assessing the effect of a treatment or policy intervention (e.g. a training or a marketing campaign) on an outcome variable (e.g. earnings or sales) typically make use of identifying assumptions that are deemed untestable. For instance, the popular selection-on-observables, unconfoundedness, conditional independence, or ignorability assumption imposes that the treatment is as good as randomly assigned after controlling for observed covariates, see e.g. Imbens and Wooldridge (2009) for a review of evaluation approaches in this context. Whether the set of covariates is sufficient for this assumption to hold is conventionally motivated based on theory, intuition, domain knowledge, or previous empirical findings.

In this paper, we demonstrate that the existence of a testable condition for the satisfaction of the selection-on-observables assumption in observational data in empirically relevant causal models, which implies that identification can be tested in the data. This condition relies on two types of observed characteristics, namely covariates to be controlled for and a suspected instrument. The condition arises if the following assumptions hold: First, there is no reverse causality from the outcome to the treatment, covariates, or the suspected instrument and from the treatment to the covariates or the suspected instrument. Second, the suspected instrument is statistically associated with the treatment conditional on the covariates, e.g. through a first stage effect of the suspected instrument on the treatment. If the suspected instrument is conditionally independent of the outcome given the treatment and the covariates in this context, then the instrument and the treatment satisfy the following two conditions: (A) the instrument is valid, i.e. does not directly affect the outcome (other than through the treatment) and is not associated with unobservables affecting the outcome conditional on the covariates and (B) the treatment is not associated with unobservables affecting the outcome conditional on the covariates. The latter means that the selection-on-observables assumption holds. Therefore, the conditional independence of the suspected instrument points to the identification of treatment effects. When focusing on average causal effects like the average treatment effects (ATE), it is sufficient to test mean (rather than full) conditional independence of the outcome and the suspected instrument.

There is an extensive literature on conditional independence tests of variables. Closely related to our testing problem, Fan and Li (1996), Racine (1997) and Racine et al. (2006) provide
methods to test the significance of regressors using kernel methods in semi- and nonparametric regression models. Further specification tests have been suggested in Bierens (1982), Hardle and Mammen (1993), Horowitz and Härdle (1994) and Wooldridge (1992). All of these methods assume the regressors to be fixed (i.e. a priori chosen by the researcher), while the testing approaches suggested in this paper permit selecting important control variables in a data-driven way. More specifically, we construct tests for the conditional independence of the instrument using doubly robust (DR) methods, see Robins et al. (1994) and Robins and Rotnitzky (1995). In our context, these DR approaches are based on statistical models for both the instrument and the outcome. We apply the double machine learning (DML) framework of Chernozhukov et al. (2018a), in which the models for the instrument and the outcome are learned in a data-driven way based on machine learning. This appears particularly attractive in high-dimensional settings where the number of available covariates is relatively large.

Under specific regularity conditions, testing is root-n-consistent despite the use of flexible machine learning methods. Following Athey and Imbens (2016), Wager and Athey (2018), Athey et al. (2019), and Lee et al. (2020), we also suggest a machine learning-based algorithm for detecting heterogeneity in the violations of the conditional independence as a function of the covariates. This permits detecting subgroups in which the violations are particularly large, in order to increase (asymptotic) testing power. Finally, we propose a test based on the average squared violation across all covariate values, which represents a global testing approach to account for covariate-dependent heterogeneity in violations. We show that under the null hypothesis, this test (similarly to DML) satisfies so-called Neyman (1959)-orthogonality, implying that we may account for covariates by machine learning without compromising on a desirable asymptotic behavior, given that specific regularity conditions hold. In a simulation study with 50 covariates, we find that the various tests perform decently in terms of empirical size and power even under moderate sample sizes of 1000 or 4000 observations.

As an empirical illustration, we apply our testing approach to US census data previously considered in Angrist and Evans (1998) for assessing the impact of fertility, which is the treatment, on female labor supply when using the sex ratio of the first two children as instrument. The intuition for this instrumental variable (IV) strategy is that if parents tend to have a preference for mixed sex children, then having two children of the same sex, which is arguably randomly
assigned by nature, increases the chances of getting a third child. Based on findings in Rosenzweig and Wolpin (2000) and Lee (2008), one might, however, challenge whether all identifying assumptions required for the IV-based identification of causal effects are satisfied. Here, we do not impose any IV assumptions a priori, but use the sibling sex ratio to test the joint satisfaction of IV validity and the selection-on-observables assumption. Our results point to a violation of the conditional independence of the instrument under our moderate set of covariates, which consists of several socio-economic characteristics like mother’s age and father’s income. Therefore, testing suggests that the treatment does not satisfy the selection-on-observables assumption, or the sex ratio is no conditionally valid instrument, or both.

This paper connects to several strands of the causal inference literature. Most closely related are studies assuming an instrument that is conditionally valid given covariates, in order to test the selection-on-observables assumption on the treatment based on the very same condition as in this paper, see de Luna and Johansson (2014) and Black et al. (2015). These contributions have in common that they a priori assume some form of IV validity like in condition (A) to test treatment exogeneity, e.g. the selection-on-observables condition (B). Here, we highlight that (A) and (B) can be tested jointly, such that one need not impose the existence of a valid instrument prior to testing. This is conceptually distinct from the previous approaches, as it implies that under some causal structure, we can test for identification in the data by checking a condition that implies the satisfaction of both (A) and (B).

Furthermore, our approach relates to Angrist and Rokkanen (2015), who consider the opposite scenario of a treatment satisfying a selection-on-observables assumption (B) in order to test whether a further variable is a valid instrument (A). More concisely, they consider the sharp RDD, in which the treatment is a deterministic function of a threshold in a running variable and therefore by design not associated with unobservables affecting the outcome. For instance, the admission to a college, which is the treatment, might be conditional on reaching a specific threshold in the score of an entrance exam, the running variable. Because admission is exclu-
sively determined by passing the score’s threshold, it is unconfounded conditional on the score.

RDD identification relies on comparing the outcomes of treated and non-treated subjects close to the threshold, i.e. with comparable values in the running variable. However, for subjects with scores further away from the threshold, the treatment effect is confounded if the running variable directly affects the outcome or is associated with unobservables affecting the outcome. For this reason, Angrist and Rokkanen (2015) test whether the score is independent of the outcome given the treatment and observed covariates. If this is the case, the running variable is (conditional on the covariates) a valid instrument, not a confounder. This permits identifying treatment effects away from the threshold, as one need not control for the running variable.

Our study is also related to Caetano et al. (2021), who consider a treatment with multiple values and bunching at a specific value. For instance, schooling laws may impose a minimum number of years of education, such that all subjects who would otherwise have acquired a lower level of education bunch at this minimum. For this reason, also the subjects’ unobserved characteristics affecting the treatment bunch at this point, which might for instance imply a comparably low level of ability at the minimum education requirement relative to other educational levels. One may then verify the selection-on-observables assumption by testing whether a dummy variable for the bunching point, which is a function of the unobserved characteristics, is conditionally independent of the outcome given the treatment and the covariates. Therefore, the dummy variable at the bunching point of the unobservables has the same role as the supposed instrument in our context, under the condition that the outcome model as a function of the treatment and covariates is correctly specified.

Our method of testing identification addresses in some sense a statistical problem ‘in between’ classical treatment evaluation, where both the treatment and the identifying assumptions are predetermined, and causal discovery, see e.g. Kalisch and Bühlmann (2014), Peters et al. (2017), Glymour et al. (2019), or Breunig and Buruel (2021). Causal discovery does typically not predefine the treatment and outcome, but aims at learning the causal relations between two or more variables in a data-driven way, possibly under parametric restrictions or the assumption that all relevant variables in the causal system (apart from random error terms) are observed. Here, we do not rely on such assumptions, but instead impose more causal structure to distinguish the treatment, outcome, covariates, and the supposed instrument. This structure appears
realistic in many empirical contexts with information about the timing of variable measurement. For instance, a treatment taking place in an earlier period can affect an outcome measured in a later period, but not vice versa. In contrast to classical treatment evaluation, we do, however, not pre-impose specific identifying assumptions, but test them in the data.\footnote{One subfield of causal discovery related to our study is so-called Y-learning, see e.g. Mani et al. (2012) and Sevilla and Mayn (2021). Conditional on covariates, Y-learning implies that if two variables are (1) independent of each other when not controlling for the treatment, (2) statistically associated with each other when controlling for the treatment, and (3) both independent of the outcome when controlling for the treatment, then these two variables are valid instruments. A further implication is that the selection-on-observables assumption holds, in analogy to the satisfaction of our testable implication under a single instrument. The difference to Y-learning is that here, we impose more structure by predefining a supposed instrument that must not be causally affected by the treatment. In contrast, testing identification by Y-learning hinges on detecting two instruments in a data-driven way, rather than predefining an instrument and ruling out causality from the treatment to the instrument.}

The remainder of this study is organized as follows. Section 2 discusses a set of identifying assumptions. The latter are used in Section 3 to derive a testable implication of the joint satisfaction of IV validity and selection-on-observables assumptions, implying the identifiability of treatment effects. Section 4 provides a modified testable implication when assuming that the IV validity and selection-on-observables assumptions hold with respect to the mean potential outcomes, rather than the entire potential outcome distributions. Section 5 discusses the null hypotheses to be tested. Section 6 proposes tests based on DML to control for (possibly high dimensional) covariates in a data-driven way. DML permits verifying whether the testable implication holds on average in the total sample or within subsamples defined as a function of the covariates. Section 7 proposes a test based on average squared violations, i.e. squared differences in conditional mean outcomes, which jointly tests for violations of the testable implication across all covariate values. Section 8 provides a simulation study. Section 9 presents an application to the evaluation of the impact of fertility on female labor supply. Section 10 concludes.

2 Assumptions

We are interested in the causal effect of a treatment $D$ on an outcome $Y$, and both variables might be discretely or continuously distributed. In our discussion of causality, we will make use of the potential outcome framework as for instance advocated in Neyman (1923) and Rubin (1974). We denote by $Y(d)$ the potential outcome when exogenously setting the treatment $D$ of a subject to some value $d$ in the support of the treatment. More generally, we will use capital...
and lower case letters for referring to random variables and specific values thereof, respectively. Importantly, denoting the potential outcome $Y(d)$ as a function of a subject’s treatment status $D = d$ alone implicitly imposes that (i) someone’s potential outcomes are not affected by the treatment status of others and (ii) there are no different versions of any treatment level $d$ across individuals. This is known as the ‘Stable Unit Treatment Value Assumption’ (SUTVA), see for instance the discussion in Rubin (1980) and Cox (1958), and is imposed throughout this paper. Furthermore, we denote by $X$ a vector of observed covariates to be used as control variables and by $Z$ one or several observed instrumental variables, whose properties are yet to be defined. Finally, let $\mathcal{X}$, $\mathcal{Z}$, $\mathcal{D}$, and $\mathcal{Y}$ denote the support of $X$, $Z$, $D$, and $Y$, respectively. We will subsequently discuss the assumptions which permit testing identification.

Our first assumption imposes some structure concerning which variables may causally affect other variables. It also states that any two variables which are associated with each other via causal paths (possibly conditional on other variables) are necessarily statistically dependent, which is known as causal faithfulness.

**Assumption 1 (Causal structure and faithfulness).**

\[
D(y) = D, \quad X(d, y) = X, \quad \text{and} \quad Z(d, y) = Z \quad \forall d \in \mathcal{D} \text{ and } y \in \mathcal{Y}.
\]

*Only variables which are $d$-separated in some causal model are statistically independent.*

The first line of Assumption 1 rules out reverse causal effects of outcome $Y$ on $D$, $X$, or $Z$. It also states that the treatment $D$ must not causally affect $X$ or $Z$, while both $X$ and $Z$ might affect $D$, $Y$, or each other. These conditions are satisfied in the causal framework in Figure 1, which represents the causal associations between instrument $Z$, treatment $D$, and outcome $Y$ by arrows in a directed acyclic graph (DAG), as e.g. considered in Pearl (2000) or Cunningham (2021). In the DAG, $Z$ and $X$ affect $D$ and $D$ and $X$ affect $Y$. Furthermore, $X$ might affect $Z$ or vice versa, as indicated by the bidirectional causal arrow. This scenario is in line with the practice of measuring covariates and instruments prior to treatment assignment, which rules out reverse causality of $D$ and $Y$ on the pre-treatment $X$ and $Z$. The DAG also includes the unobserved terms $U$ and $V$ which affect $Y$ and $D$, respectively, with the dashed
arrows indicating that these effect cannot be observed.

The second line of Assumption 1 imposes causal faithfulness, meaning that only variables which are d-separated, i.e. not associated with each other via some causal paths (possibly conditional on other variables) are statistically independent (or conditionally independent). More formally, the d-separation criterion of Pearl (1988) implies that two (sets of) variables $A$ and $B$ are d-separated when conditioning on a (set of) control variable(s) $C$ if and only if

1. the path between $A$ and $B$ contains a triple $A \rightarrow M \rightarrow B$ (causal chain) or $A \leftarrow M \rightarrow B$ (confounding) such that variable (set) $M$ is in $C$ (i.e. controlled for),

2. the path between $A$ and $B$ contains a collider $A \rightarrow S \leftarrow B$ such that variable (set) $S$ or any variable (set) causally affected by $S$ is not in $C$ (i.e. not controlled for).

d-separation is sufficient for the (conditional) independence of two variables, which will be useful for proving Theorems 1 and 2. Causal faithfulness imposes that d-separation is also a necessary condition, such that two variables are statistically independent if and only if d-separation holds.

One scenario in which faithfulness fails is that one variable affects another one via several causal paths (or mechanisms) which exactly cancel out such that the variables are independent, see e.g. the discussion in Spirtes et al. (2000). In the context of Figure 1, faithfulness rules for instance out that $Z$ affects $D$ via multiple causal mechanisms that fully offset each other. A further example is that one variable causally affects another one, but both variables are affected by a third factor in a way that exactly offsets the association of the first two variables such that they are independent. The implications of faithfulness for our testable condition will become apparent in the discussion in Section 3.

Our second assumption requires that for any values of $X$ in the population, any possible combination of treatment and instrument values exists, which is known as common support.

**Assumption 2** (Common support).

\[
\Pr(D = d, Z = z | X) > 0 \quad \forall d \in \mathcal{D} \text{ and } z \in \mathcal{Z}.
\]
Assuming discretely distributed treatments and instruments, Assumption 2 implies that the joint probabilities of any $D = d$ and $Z = z$ conditional on $X$ are larger than zero. In the case of continuously distributed treatments and/or instruments, the joint probabilities are to be replaced by joint density functions conditional on $X$. By applying basic probability theory,

$$ Pr(D = d, Z = z | X) = Pr(Z = z | D = d, X) \cdot Pr(D = d | X), $$

where $Pr(D = d | X)$ is the conditional treatment probability given $X$, known as treatment propensity score, and $Pr(Z = z | D = d, X)$ is conditional instrument probability given $D, X$, i.e. the instrument propensity score. Therefore, Assumption 2 requires that both the treatment and instrument propensity scores are larger than zero, i.e. $Pr(D = d | X) > 0$ and $Pr(Z = z | D, X) > 0 \forall d \in D$ and $z \in Z$.\(^4\)

Our third assumption requires the treatment and the instrument to be statistically dependent conditional on the covariates, where $\not\perp$ denotes statistical dependence.

**Assumption 3** (Conditional dependence between the treatment and instrument).

$$ D \not\perp Z | X. $$

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\(^4\)Common support in the treatment propensity score ensures that causal effects are identified conditional on any value of $X$ in the population, which is a precondition that aggregate treatment effects like the average treatment effect (ATE) given by $E[Y(d) - Y(d')]$ for any $d \neq d'$, are well defined. Under a violation of common support in the treatment propensity score for some values of $X$, one may identify causal effects only for subpopulations whose covariate values satisfy $Pr(D = d | X) > 0$. Common support in the instrument propensity score ensures that the testable implication suggested further below can be verified at any value of $X$ in the population and for any treatment value $d$. A violation of common support in the instrument propensity score implies that one may test the implication only among those covariate and treatment combinations satisfying $Pr(Z = z | D, X) > 0$. Assumption 2 is therefore strictly speaking not required for implementing our testing approach yet to be defined, if we contend ourselves with considering a subpopulation satisfying common support, which may, however, come with the potential caveat of reduced testing power.
Together with Assumption 1, which rules out effects of $D$ on $Z$, Assumption 3 either implies that $Z$ causally affects $D$, which is known as first stage effect in the IV literature, or that some (unobserved) characteristics jointly affect $Z$ and $D$ given $X$. This assumption is satisfied in Figure 1, where the instrument has an impact on the treatment.

Our fourth assumption invokes the conditional independence of the treatment and the potential outcomes given the covariates, with \( \perp \perp \) denoting statistical independence. This popular assumption in the treatment evaluation literature is also known as selection-on-observables, exogeneity, or unconfoundedness, see e.g. Imbens (2004) and Imbens and Wooldridge (2009).

\textbf{Assumption 4} (Conditional independence of the treatment).

\[ Y(d) \perp \perp D | X \quad \forall d \in D. \]

Assumption 4 implies that conditional on covariates $X$, there exist no unobserved confounders jointly affecting outcome $Y$ and treatment $D$.

Our fifth assumption invokes IV validity, requiring that the instrument is conditionally independent of the outcome give the covariates $X$.

\textbf{Assumption 5} (Conditional independence of the instrument).

\[ Y(d) \perp \perp Z | X \quad \forall d \in D. \]

Assumption 5 has two implications. First, $Z$ does not directly affect the outcome other than through the treatment conditional on $X$, such that potential outcome is a function of $d$ alone, rather than the instrument, too. For this reason, an IV exclusion restriction holds such that conditional on $X$, $Y(d, z) = Y(d, z') = Y(d)$ for any instrument values $z$ and $z'$, otherwise the conditional independence would be violated. Second, there exist no unobserved confounders jointly affecting $Y$ and $Z$ when controlling for $X$, which is analogous to Assumption 4, but now concerning the instrument rather than the treatment. We note that Assumption 5 is not sufficient for identifying causal effects based on the instrument, like the local average treatment
effect (LATE) on the subpopulation whose treatment reacts to (or complies with) the instrument, see Imbens and Angrist (1994) and Angrist, Imbens and Rubin (1996). The IV-based assessment of the LATE hinges on further assumptions, like e.g. the (conditional) monotonicity of $D$ in $Z$, which we do not consider here, because it is not required for our identification test.

To provide an example where Assumption 5 is violated, consider the following nonparametric outcome model: $Y = \psi(D, X, Z, U)$, where $\psi$ is an unknown function and $U$ denotes unobserved variables affecting the outcome. $Z$ directly affects the outcome, as considered in Figure 2. In this case, the potential outcome $Y(d) = \psi(d, X, Z, U)$ is not independent of $Z$ conditional on $X$. Let us now alternatively assume that the outcome model is $Y = \psi(D, X, U)$, such that $Y(d) = \psi(d, X, U)$. In this case, $Z$ does not directly affect $Y$ and Assumption (5) is satisfied if $U \perp \perp Z | X$ holds such that the unobservables and the instrument are conditionally independent. This is satisfied in Figure 1, where $Z$ is a conditionally valid instrument for $D$, as it neither directly shifts the outcome nor is associated with unobservables affecting the outcome.

Figure 2: Causal graph violating Assumption 5

3 Testable implication for identification

In the subsequent discussion, we use the assumptions introduced in Section 2 to derive a testable implication for the identification of causal effects. Assuming the causal structure and faithfulness
condition imposed by Assumption 1, we note that if Assumption 4 holds, then the additional satisfaction of Assumption 5 implies for some potential outcome $Y(d)$ that

$$Y(d) \perp \perp Z \mid D, X. \quad (3.1)$$

That is, controlling for $D$ in addition to $X$ cannot introduce statistical dependence between $Y(d)$ and $Z$, because the treatment and the potential outcomes are independent given $X$. This follows from the d-separation criterion of Pearl (1988), which under our model assumptions rules out such a spurious dependence between $Y(d)$ and $Z$, known as collider bias (see Pearl (2000)) or sample selection bias (see Heckman (1979)), when controlling for $X$ and $D$.$^5$

Next, we note that when setting $D = d$ in the conditioning set of (3.1), the potential outcome $Y(d)$ corresponds to the observed outcome $Y$ if SUTVA holds, because $Y(d) = Y$ conditional on $D = d$. For this reason, (3.1) implies that

$$Y \perp \perp Z \mid D = d, X. \quad (3.2)$$

As the conditional independence (3.2) is based on observed (rather than potential) outcomes, it provides a testable implication of the joint satisfaction of Assumptions 4 and 5 for the potential outcome under the (f)actually assigned treatment $D = d$. It appears important to point out the implications of causal faithfulness in this context, see Assumption 1. If it fails, then a violation of (3.2) might in particular parametric models not go together with a violation of Assumptions 4 and/or 5, respectively. For instance, de Luna and Johansson (2014) consider a direct effect of $Z$ on $Y$ (violating the exclusion restriction) and an association of $Z$ with unobservables affecting $Y$ which exactly offset each other in a linear model. In this case, Assumption 5 holds, while (3.2) may be violated. In more general (nonparametric) models, however, a violation of (3.2) necessarily points to a violation of Assumptions 4 and/or 5. Causal faithfulness, also referred to as stability in Pearl (2000), rules out parametric models for which this is not the case.

$^5$Indeed, when substituting $A$ by $Y(d)$, $B$ by $Z$, and $C$ by $(D, X)$ in the d-separation criterion established at the beginning of Section 2, we can verify that $Y(d)$ and $Z$ are d-separated conditional on $(D, X)$. This is the case because $Y(d)$ and $Z$ are d-separated conditional on $X$ alone and furthermore, $D$ is not a collider and therefore be included in the conditioning set $C$, too. As d-separation implies conditional independence, it follows that (3.1) holds under our assumptions.
It is worth noting that if $Z$ and $D$ are conditionally independent given $X$, i.e. $D \perp \perp Z | X$, condition (3.2) can hold even under a violation of Assumption 4, implying a lack of testing power. This follows from d-separation: If Assumption 5 holds such that $Z$ is conditionally independent of $Y(d)$ given $X$, then controlling for an endogenous treatment $D$ will not introduce a spurious dependence (or collider bias) between $Z$ and $Y(d)$ if $Z$ and $D$ are conditionally independent given $X$. Under the causal structure postulated in Assumption 1, it holds that $D \perp \perp Z | X$ and $Y(d) \perp \perp Z | X$ imply $Y(d) \perp \perp Z | D, X$ and $Y \perp \perp Z | D = d, X$, even if $Y(d) \perp \perp D | X$. Figure 3 provides a causal model satisfying this framework. $Z$ is not associated with $D$ given $X$, as neither $Z$ affects $D$, nor $D$ affects $Z$ and no variables jointly affect $D$ and $Z$ conditional on $X$. Therefore, (3.2) holds even if (4) is violated, due to confounding by $U$ which jointly affects $D$ and $Y$.

For this reason, we additionally impose Assumption 3, i.e. conditional dependence between $Z$ and $D$, to guarantee that (3.2) can be considered as a condition for the joint satisfaction of Assumptions 4 and 5. Under both Assumptions 1 and 3, (3.2) no longer holds if Assumption 4 is violated. Figure 4 provides several examples for such a causal framework, in which $Z$ affects $D$ such that Assumption 3 holds. However, the treatment-outcome association is confounded by unobservables $U$ given $X$. For this is reason, controlling for $D$ introduces a spurious dependence between $Z$ and $U$ even conditional on $X$. This in turn introduces statistical dependence (or collider bias) between $Z$ and $Y$, because $U$ affects $Y$. This issue arises in both graphs (a) and (b), implying that the causal effect of $D$ on $Y$ is not identified conditional on $X$. It is not identified conditional on both $Z$ and $X$ either, as additionally controlling for $Z$ does not tackle the confounding of the treatment-outcome relation due to $U$. 

Figure 3: A violation of Assumption 4 under a satisfaction of condition (3.2)
The previous discussion demonstrates that under the satisfaction of Assumptions 1, 5, and 3, a violation of Assumption 4, implying that the treatment effect is not identified conditional on $X$, entails a violation of (3.2). Such a violation also occurs under the satisfaction of Assumptions 1, 4, and 3, but a violation of Assumption 5: $Y(d) \not\perp Z \mid X$ entails $Y(d) \not\perp Z \mid D, X$ and thus $Y \not\perp Z \mid D = d, X$. Figure 5 provides two examples for such a causal framework, in which the unobserved characteristics $V$ that affect $D$ are not associated with $U$. While $Z$ and $Y$ is confounded by $U$ in graphs (a) and (b) such that Assumption 5 is violated, controlling for $X$ permits identifying the causal effect of $D$ on $Y$ such that Assumption 4 holds. In this case, identification is obtained conditional on $X$ but cannot be revealed through a satisfaction of (3.2) due to the invalidity of the instrument.

To gain further intuition about the conditional independence (3.2), Figure 6 provides two causal scenarios in which both Assumptions 4 and 5 are violated. In graphs (a) and (b),
conditioning on \( X \) alone introduces so-called M-bias, a form of collider bias. Because \( X \) is influenced by both \( Z \) and \( U \), controlling for \( X \) introduces a spurious association between \( Z \) (which affects \( D \)) and \( U \) (which affects \( Y \)), such that the treatment-outcome association is confounded. Nevertheless, the causal effect of \( D \) is identified based on observed variables, namely when controlling for both \( Z \) and \( X \). In graph (a), this approach simultaneously avoids M-bias by conditioning on the instrument and confounding of the treatment-outcome relation by conditioning on the covariates. In graph (b), the same strategy additionally blocks the direct impact of \( U \) on \( Z \). For this reason, a violation of (3.2) does not necessarily imply non-identification, as treatment effects might be just identified when controlling for both \( X \) and \( Z \). However, if (3.2) is violated, we cannot distinguish non-identification from identification in a data-driven way but need to rely on untestable assumptions.

Figure 6: Identification given \( Z \) and \( X \) under a violation of condition (3.2)

Finally, Figure 7 provides several causal graphs in which both Assumptions 4 and 5 are satisfied (in addition to Assumptions 1 and 3), such that (3.2) holds. In all cases considered, controlling for \( X \) is sufficient for the conditional independence of both the treatment and the instrument on the one hand and the potential outcomes on the other hand. In contrast to Figure 6, conditioning on the covariates does not introduce M-bias. Therefore, \( Z \) is conditionally independent of \( Y \) given both \( D \) and \( X \) and (3.2) provides a testable condition for the identification of the treatment effect of \( D \) on \( Y \) given \( X \).

In Theorem 1, we formalize the findings of our previous discussion. More concisely, we show that conditional on Assumptions 1 and 3, \( Y \perp Z | D = d, X \) is a necessary and sufficient condition for the joint satisfaction of \( Y(d) \perp D | X \) and \( Y(d) \perp Z | X \). The latter two conditions correspond
to Assumptions 4 and 5 when considering potential outcomes $Y(d)$ that match the factual treatment assignment $D = d$. This implies that in practice, we may test Assumptions 4 and 5 only for factual outcomes, e.g. for the potential outcomes $Y(1)$ of subjects with $D = 1$ and $Y(0)$ of subjects with $D = 0$. However, one cannot construct tests based on counterfactual outcomes, e.g. for potential outcomes $Y(0)$ of subjects with $D = 1$ and $Y(1)$ of subjects with $D = 0$. For this reason, our approach tests a necessary, but not a sufficient condition. If violations of Assumptions 4 and 5 solely concern counterfactual outcomes, i.e. $Y(0)$ of treated individuals and $Y(1)$ of non-treated individuals, testing cannot detect such violations. From a practical perspective, however, it seems unlikely that violations exclusively occur among counterfactual, but not among factual outcomes, because this would imply very specific modeling constraints.

**Theorem 1.** Conditional on Assumptions 1 and 3, it holds that

$$Y(d) \perp D | X, \quad Y(d) \perp Z | X \iff Y \perp Z | D = d, X \quad \forall d \in \mathcal{D}. \tag{3.3}$$

*Conditional on Assumptions 1 and 3, the testable implication $Y \perp Z | D = d, X$ is necessary and*
sufficient for the joint satisfaction of Assumptions 4 and 5 when considering potential outcomes $Y(d)$ matching the factual treatment assignment $D = d$.

The proof of the theorem is provided in Appendix A.1 and is related to, but yet different from that in de Luna and Johansson (2014), who consider the same testable implication under a different conceptual framework: They a priori impose Assumption 5 to test Assumption 4, i.e. selection-on-observables. That is, the same testable implication as considered in Theorem 1 also arises when imposing conditional IV validity and testing the selection-on-observables assumption w.r.t. treatment, see also Black et al. (2015). Taking in some sense the opposite approach, one may alternatively assume a treatment that is unconfounded to test IV validity, which entails the same testable implication. Angrist and Rokkanen (2015) consider this approach in an RDD context to test whether the running variable is a conditionally valid instrument. The latter implies the identification of causal effects away from the running variable’s threshold determining treatment assignment, at which the treatment is locally unconfounded by design.

In contrast to these contributions, Theorem 1 demonstrates that we can jointly test Assumptions 4 and 5, i.e. the conditional independence of both the treatment and the instrument, given that the two variables are conditionally dependent. We need not be convinced a priori that conditional instrument validity holds in order to test the selection-on-observables assumption, or vice versa. By jointly testing both assumptions, we conceptually move away from pre-imposing one specific set of identifying assumptions and towards verifying identification in the data. This idea is related to Caetano et al. (2021), who test whether conditional on the treatment and the covariates, the outcome is statistically independent of a function of unobservables. The latter function corresponds to a dummy variable for a specific treatment value under the conditions that bunching (i.e. a mass point) in the treatment occurs at that value and that the outcome model is correctly specified. In other words, the function of the unobservables is the instrument $Z$ in Theorem 1. Also in Caetano et al. (2021), it is a priori not clear whether the unobservables are valid instruments or whether the selection-on-observables assumption holds, but both assumptions are tested jointly based on the dummy variable at the bunching point.

Our approach of jointly testing several assumptions is also related to classical specification tests, like the Hausman (1978) test for comparing the results from an IV regression with those
of a method relying on selection-on-observables, e.g. OLS. Also in this case, one may assume conditional IV validity to test the selection-on-observables assumption or vice versa. And as in our setting, both assumptions could be tested jointly. However, as the Hausman test relies on comparing the causal effects obtained under IV and selection-on-observables methods, one disadvantage is that its behavior is generally affected by misspecification of the models underlying effect estimating. Even when basing testing on nonparametric models that allow for heterogeneous effects across subpopulations as in Donald et al. (2014), IV-based identification has to rely on further identifying assumptions like monotonicity of $D$ in $Z$. Because such functional form restrictions compromise on the generality of the model and the robustness of the tests, directly testing the conditional independence in Theorem 1 as in our approach seems preferable.

### 4 Conditional mean independence

The conditional independence assumptions 4 and 5 refer to the entire distributions of potential outcomes and permit the evaluation of distributional effects such as the quantile treatment effect (QTE), see for instance Firpo (2007). For assessing the average treatment effect (ATE), we may consider somewhat weaker conditional independence assumptions w.r.t. the means of potential outcomes. More formally, we may replace Assumption 4 by the following condition.

**Assumption 6** (Conditional mean independence of the treatment).

$$E[Y(d)|D, X] = E[Y(d)|X] \quad \forall d \in D.$$  

Assumption 6 is weaker than Assumption 4 as it only imposes the conditional mean independence of $Y(d)$ and $D$ given $X$, but not that of other moments. Analogously, we can weaken Assumption 5 to conditional mean independence of the instrument, see Assumption 7.

**Assumption 7** (Conditional mean independence of the instrument).

$$E[Y(d)|Z, X] = E[Y(d)|X] \quad \forall d \in D.$$
When considering Assumptions 6 and 7 rather than 4 and 5, Assumption 3 on the conditional dependence of $Z$ and $D$ needs to be replaced, too, by conditional mean dependence.

**Assumption 8** (Conditional mean dependence between the treatment and instrument).

\[ E[D|Z,X] \neq E[D|X]. \]

Assumption 8 imposes a nonzero first-stage effect of $Z$ in a regression of $D$ on a constant, $Z$, and $X$. It is stronger than the previously imposed Assumption 3, because it requires that the conditional dependence of $D$ and $Z$ necessarily affects the mean of these variables. Any conditional dependence in other moments (like the variance) is irrelevant for this assumption.

Theorem 2 states that conditional on Assumptions 1 and 8, the conditional mean independence of $Y$ and $Z$ given $D = d$ and $X$ implies that and is implied by $E[Y(d)|D = d, X] = E[Y(d)|X]$ and $E[Y(d)|Z, X] = E[Y(d)|X]$, where $Y(d)$ is the potential outcome associated with the factual treatment $D = d$. Therefore, Assumptions 6 and 7 are testable for potential outcomes that match the factual treatment assignment. The proof is provided in Appendix A.2.

**Theorem 2.** Conditional on Assumptions 1 and 8, it holds that

\[ E[Y(d)|D, X] = E[Y(d)|X], \quad E[Y(d)|Z, X] = E[Y(d)|X] \] (4.1)

\[ \iff E[Y|Z, D = d, X] = E[Y|D = d, X] \quad \forall d \in D. \]

Conditional on Assumptions 1 and 8, the testable implication $E[Y|Z, D = d, X] = E[Y|D = d, X]$ is necessary and sufficient for the joint satisfaction of Assumptions 6 and 7 when considering potential outcomes $Y(d)$ matching the factual treatment assignment $D = d$.

## 5 Testable hypotheses

This section presents the hypotheses for testing the conditional mean independence in (4.1), which implies the identification of average effects. We assume that the supposed instrument
Z to be a scalar, while X is a vector of observed covariates. To ease notation, we henceforth denote the conditional mean outcome as \( \mu(z, d, x) = E[Y|Z = z, D = d, X = x] \). Considering a discretely distributed instrument Z, the conditional mean independence (4.1) is equivalent to the following null hypothesis \( H_0 \):

\[
\mu(z, d, x) - \mu(z', d, x) = 0 \quad \forall z \neq z' \in Z, d \in D, \text{ and } x \in X. \tag{5.1}
\]

Under the null hypothesis, the mean conditional outcome is constant across \( Z \) given any value of \( D \) and \( X \). For a binary instrument, (5.1) for instance corresponds to \( \mu(1, d, x) - \mu(0, d, x) = 0 \).

For a continuous instrument \( Z \), (4.1) implies the following null hypothesis:

\[
H_0 : \frac{\partial \mu(z, d, x)}{\partial z} = 0 \quad \forall z \in Z, d \in D, \text{ and } x \in X. \tag{5.2}
\]

That is, the first derivative of the conditional mean outcome w.r.t. the instrument must be zero given any value of \( Z, D, \) and \( X \).

Under a linear regression model for the outcome with homogeneous effects, we can test the null hypotheses (5.1) or (5.2) based on regressing \( Y \) on a constant, \( Z, D, \) and \( X \) and verifying whether the coefficient on \( Z \) is statistically significantly different from zero. However, when assuming a more flexible nonparametric model, the conditional statistical association of \( Z \) and \( Y \) is allowed to be heterogeneous across different values of \( Z, D, \) and \( X \). In theory, one then might want to verify the respective null hypothesis at all values of the instrument, the treatment, and the covariates, to check for any possible violation. However, if some or all of these variables are continuously distributed, this implies an infinite number of hypotheses to be tested. Even under (mostly) discretely distributed variables, statistical power in finite samples quickly decreases in the conditioning set as a function of the dimension and support of \( X \). Therefore, a test statistic must necessarily involve an aggregate measure over its domain (or support), see e.g. Racine (1997) and Racine et al. (2006) who provide a nonparametric significance test based on an aggregate \( L_2 \)-norm that is related to our approach in Section 7.

Another way to circumvent such issues of limited finite sample power and multiple hypothesis
testing is to verify whether the null hypothesis holds on average. For a binary instrument as considered in our application in Section 9, this amounts to testing the following condition:

\[ H_0 : E[\mu(1, D, X) - \mu(0, D, X)] = 0. \] (5.3)

We henceforth denote this average difference in conditional means by \( \Delta = E[\mu(1, d, x) - \mu(0, d, x)] \). \( \Delta \) may be estimated by treatment evaluation methods for assessing the ATE, such as propensity score matching, see Rosenbaum and Rubin (1983) and Rosenbaum and Rubin (1985), inverse probability weighting (IPW), see Horvitz and Thompson (1952) and Hirano et al. (2003), or doubly robust (DR) methods, see Robins et al. (1994) and Robins and Rotnitzky (1995).

6 Testing based on doubly robust estimation

We henceforth suggest a testing approach based on doubly robust (DR) estimation, and to this end consider vectors \((Y, Z, D, X)\) such that

\[
Y = \mu(Z, D, X) + \varepsilon, \quad \mathbb{E}[\varepsilon|X, D, Z] = 0, \tag{6.1}
\]

\[
Z = p(D, X) + \nu, \quad \mathbb{E}[\nu|D, X] = 0, \tag{6.2}
\]

with \( p(D, X) = \Pr(Z = 1|D, X) \) denoting the conditional instrument probability or instrument propensity score and \( \varepsilon \) as well as \( \nu \) denoting deviations from the conditional mean of \( Y \) and \( Z \), respectively. The DR approach exploits both propensity scores and conditional means to estimate the following expression for \( \Delta \) (which is equivalent to \( E[\mu(1, d, x) - \mu(0, d, x)] \)):

\[
\Delta = E[\phi(D, X)] \tag{6.3}
\]

with

\[
\phi(D, X) = \mu(1, D, X) - \mu(0, D, X) + \frac{(Y - \mu(1, D, X)) \cdot Z}{p(D, X)} - \frac{(Y - \mu(0, D, X)) \cdot (1 - Z)}{1 - p(D, X)}. \tag{6.4}
\]
ϕ(D, X) is the efficient (and Neyman-orthogonal) score function, into which μ(Z, D, X) and 
p(D, X) enter as first-step or nuisance parameters. The sample analog of (6.3) consistently
estimates Δ if either the model for μ(Z, D, X) or for p(D, X) is correctly specified, which is
known as DR property.

In particular when the set of covariates X is large, one might estimate μ(Z, D, X) and
p(D, X) by machine learning (ML) algorithms, in order to control for important confounders in a
data-driven way (rather than using an ad-hoc rule which may introduce pre-testing issues). This
double machine learning (DML) approach is typically combined with cross-fitting, which consists
of estimating the the nuisance parameter models and the score function in non-overlapping
subsets of the data, with the roles of the subsets for the estimation steps being sequentially
swapped. As no observation enters both estimation steps at the same time, cross-fitting avoids
correlations between the estimation of the models of μ(Z, D, X) and p(D, X) on the one hand
and of the score function ϕ(D, X) on the other hand and thus, overfitting bias. Finally, taking
the sample average of the estimated score function yields an estimate of Δ, in analogy to the
population average in (6.3). Under specific regularity conditions, e.g. the convergence rate
of ML-based estimators of μ(Z, D, X) and p(D, X) being faster than n^{1/4}, DML is root-n-
consistent, see the discussion in Chernozhukov et al. (2018a).

However, verifying the condition (5.3) rather than (5.1) has its caveats. By averaging over
values of D and X (and Z, if it is non-binary) when testing the null hypothesis, there is a risk
of averaging out violations of the conditional mean independence (4.1) such that they cannot be
detected. By aiming at increasing finite sample power through averaging, we sacrifice asymptotic
power as we do not test the hypotheses separately for distinct values of our conditioning set.
To more optimally trade off asymptotic and finite sample power, we outline a further testing
approach, which borrows from the literature on estimating conditional average treatment effects
(CATE) and investigating effect heterogeneity across observed characteristics based on ML, see
e.g. Wager and Athey (2018). Denoting the conditional mean difference μ(1, d, x) − μ(0, d, x)
by Δ(d, x), we use the score function ϕ(D, X) for assessing whether Δ(d, x) is heterogeneous
across values of (D, X) and thus necessarily non-zero for some values of the conditioning set.

A practical issue is that we would like to focus on those variables in the set (D, X) which
importantly predict the effect heterogeneity of Δ(D, X) and thus, drive the statistical power
of our test. This suggests the use of ML for determining the crucial predictors of the estimate of $\phi(D, X)$. However, if the ML-based detection of important predictors of $\Delta(d, x)$ and hypothesis testing e.g. of (5.1) based on those predictors proceeds in the very same data, this may entail overfitting bias. This can entail spurious rejections of the null hypothesis due to a correlation of the estimation steps of predictor selection and testing, in analogy to the discussion in Athey and Imbens (2016). For this reason, we apply a sample splitting approach which avoids such correlations, see e.g. Lee et al. (2020), by randomly partitioning our sample into two non-overlapping subsamples. In the first subsample, we apply the previously discussed DML procedure to estimate the efficient score function, henceforth denoted by $\hat{\phi}(D, X)$. Still in the first subsample, $\hat{\phi}(D, X)$ is predicted as a function of the predictors $(D, X)$ using ML, for instance based on a so-called decision tree, see Morgan and Sonquist (1963) and Breiman et al. (1984). A decision tree consists of recursively splitting the covariate space into subsets (or leaves), such that predictive power w.r.t. $\hat{\phi}(D, X)$ is maximized. In the second subsample, we first estimate $\hat{\phi}(D, X)$ based on DML and then conduct the hypotheses tests conditional on the covariates that have been found to importantly predict the score in the first subsample.

More formally, let $n$ denote the total sample size (i.e. the sum of observations in the first and second subsample) and $i$ be the index of observations in the sample, i.e. $i \in \{1, \ldots, n\}$. Furthermore, denote by $L_m$ a specific leaf or subset and by $M$ the total number of leaves defined by the regression tree (or any other machine learning algorithm) in the first subsample, such that $m \in \{1, \ldots, M\}$. The average of $\Delta(D, X)$ within the respective leaf $L_m$ is denoted by

$$\Delta(L_m) = E[\Delta(D, X)|D, X \in L_m].$$

Hypothesis testing amounts to verifying whether the averages in the various leaves are statistically significantly different from zero. A leaf-specific null hypotheses is defined as follows:

$$H_0 : E[\mu(1, D, X) - \mu(0, D, X)|(D, X) \in L_m] = 0.$$ (6.5)

Under specific regularity conditions, the estimated parameters are root-n-consistent and asymptotically normally distributed with a variance which is not affected by the fact that
\(\hat{\phi}(D_i, X_i)\) is a ML-based estimate of the true (but unknown) score function \(\phi(D_i, X_i)\). As discussed in Semenova and Chernozhukov (2021), such a favorable behavior can be attained if the ML-based estimators of the nuisance parameters converge to the respective true models with a rate faster than \(n^{1/4}\) and if the number of leaves \(M\) is small relative to the sample size \(n\). In this case, we can directly consider the t-statistics for testing the null hypothesis in specific leaves. To account for multiple hypothesis testing issues due to running the test in several leaves, we may apply a statistical correction which controls for the expected proportion of spurious rejections among the rejected hypotheses, the so-called false discovery rate, as e.g. suggested Benjamini and Hochberg (1995), or the standard Bonferroni correction.\(^6\)

One drawback of such corrections for multiple testing is that they can drastically reduce testing power if the number of leaves \(M\) is non-negligible. To mitigate this issue, we base testing on uniformly valid confidence intervals for all target parameters \(\theta_m = E[(\mu(1, D, X) - \mu(0, D, X)) I\{D, X \in L_m\}], m = 1, \ldots, M\) obtained by the multiplier bootstrap. The target parameter \(\theta_m\) equals zero if and only if \(E[\mu(1, D, X) - \mu(0, D, X) | (D, X) \in L_m] = 0\), given that \(P(X, D \in L_m) > 0\). To construct uniformly valid confidence intervals, we consider the \(M\)-dimensional score function \(\phi^M = (\phi_1, \ldots, \phi_M)\) with

\[
\phi_m(D_i, X_i, \theta, \eta) = (\mu(1, D_i, X_i) - \mu(0, D_i, X_i) + \frac{Y_i - \mu(1, D_i, X_i) \cdot Z_i}{p(D_i, X_i)} - \frac{(Y_i - \mu(0, D_i, X_i)}{1 - p(D_i, X_i)}) I\{D_i, X_i \in L_m\} - \theta_m
\]

for all \(m = 1, \ldots, M\) with \(\eta = (\eta_1, \eta_2) = (\mu, p)\).

To derive the large sample behavior of this approach, we introduce further notation. Let \(\delta_N\) be sequences of positive constants approaching 0. Furthermore, let \(C\) and \(q\) be fixed strictly positive constants with \(q > 2\). In the following, we rely on cross-fitted DML and split the \(N\) observations in the second subsample (used for testing) into \(K\) folds \(I_k, k = 1, \ldots, K\), of size \(N/K\) as described in Definition 3.2 in Chernozhukov et al. (2018b). For simplicity, assume that

\(^6\)Alternatively, we can run an F-test for the joint satisfaction of the null hypothesis (6.5) across all leaves \(L_m\) if we assume a linear form of the regression function \(\mu\).
\( N/K \) is an integer. For each \( k \in [K] \), we obtain
\[
\hat{\eta}_k = (\hat{\mu}(\{W_i\}_{i \in I_k}^e), \hat{\rho}(\{W_i\}_{i \in I_k}^e)),
\]
which is an ML-based estimate of \( \eta_0 \). We then estimate the target parameter \( \theta_M \) by \( \hat{\theta}_M \), which solves
\[
\frac{1}{K} \sum_{k=1}^K \mathbb{E}_{N,k}[\phi^M(W, \hat{\theta}, \hat{\eta}_k)] = 0,
\]
where \( \mathbb{E}_{N,k} \) is the empirical expectation over the kth fold of the data. We now impose a set of regularity conditions required for the construction of uniform confidence intervals and the asymptotic normality of our testing approach, which is stated in Theorem 3.

**Assumption 9 (Uniform confidence intervals).** The following assumptions need to hold for all \( n \geq 3 \), \( P \in \mathcal{P} \) and \( q > 2 \):

i) It holds \( \|Y\|_{P,q} < C \), \( \mathbb{E}[\varepsilon^2I\{D, X \in L_m\}] > c \) for all \( m = 1, \ldots, M \) and \( \|\mathbb{E}[\varepsilon^2|D, X]\|_{P,\infty} < C \).

ii) It holds, \( \varepsilon < P(Z = 1|X, D \in L_m) < 1 - \varepsilon \) for all \( m = 1, \ldots, M \).

iii) Given a random subset \( I \) of \( [N] \) of size \( n = N/K \), the nuisance parameter estimator \( \hat{\eta}_0 = \hat{\eta}_0((W_i)_{i \in I}^e) \) obeys the following conditions: With \( P \)-probability not less than \( 1 - o(1) \), \( \max(\|\hat{\eta}_1 - \eta_{0,1}\|_{P,q}, \|\hat{\eta}_2 - \eta_{0,2}\|_{P,q}) \leq C \), \( \max(\|\hat{\eta}_1 - \eta_{0,1}\|_{P,2}, \|\hat{\eta}_2 - \eta_{0,2}\|_{P,2}) \leq \delta_N \), \( \|\hat{\eta}_1 - 1/2\|_{P,\infty} \leq 1/2 - \varepsilon \) and \( \|\hat{\eta}_1 - \eta_{0,1}\|_{P,2} \times \|\hat{\eta}_2 - \eta_{0,2}\|_{P,2} \leq \delta_N N^{-1/2} \).

**Theorem 3.** Conditional on Assumptions 9, under \( H_0 \), it holds that
\[
\sqrt{N} \Sigma^{-1} (\hat{\theta}_M - \theta_M) \sim N(0, I_M) \tag{6.7}
\]
uniformly over \( P \in \mathcal{P} \), where \( \Sigma^2 = \mathbb{E}[\phi^M(W, \theta_0, \eta_0)\phi^M(W, \theta_0, \eta_0)'|] \) with diagonal elements \( \sigma^2_m, m = 1, \ldots, M \). Moreover, the result continues to hold if \( \Sigma^2 \) is replaced by
\[
\hat{\Sigma}^2 := \frac{1}{K} \sum_{k=1}^K \mathbb{E}_{n,k} \left[ \phi^M(W, \hat{\theta}_M, \hat{\eta}_k)\phi^M(W, \hat{\theta}_M, \hat{\eta}_k)'ight].
\]
The proof of Theorem 3 is provided in Appendix A.3 and we note that its result also holds under the alternative hypothesis $H_1$ if the variance $\Sigma^2$ of the score is non-degenerate. Theorem 3 can be used to construct confidence regions for any scalar parameter $l^T \theta_M$ for some $M \times 1$ vector $l$. Using for instance the multiplier bootstrap, we can also construct a confidence interval for $\sup_{m=1,\ldots,M} |\theta_m|$ to simultaneously test the hypotheses (6.5) for all $m = 1, \ldots, M$. To this end, we define the process

$$\hat{G} = (\hat{G}_m)_{m=1,\ldots,M} = \left( \frac{1}{\sqrt{N}} \sum_{i=1}^N \zeta_i \hat{\sigma}_m^{-1} \phi_m(W_i, \hat{\theta}_m, \hat{\eta}) \right)_{m=1,\ldots,M},$$

where $(\zeta_i)_{i=1}^N$ are standard normal random variables, which are independent of each other and of the data $(W_i)_{i=1}^N$. $\hat{\sigma}_m^{-1}$, $m = 1, \ldots, M$, are the diagonal elements of $\hat{\Sigma}^{-1}$. The critical value $c_\alpha$ obtained through the multiplier bootstrap corresponds to the $(1-\alpha)$-quantile of the conditional distribution of $\sup_{m=1,\ldots,M} |\hat{G}_m|$ given the data $(W_i)_{i=1}^N$. The null hypothesis is rejected if

$$\sup_{m=1,\ldots,M} \sqrt{N} \hat{\sigma}_m^{-1} |\theta_m| > c_\alpha.$$

It is worth noting that one can also construct hypothesis tests based on the $l_p$-norm, $p \geq 1$, rather than the infinity norm of $\{\hat{\sigma}_m^{-1} \theta_m\}_{m=1}^M$. In this case, we simultaneously reject (6.5) if

$$\sqrt{N} \|\{\hat{\sigma}_m^{-1} \theta_m\}_{m=1}^M\|_p > c_\alpha^{(p)},$$

with $c_\alpha^{(p)}$ being the $(1-\alpha)$-quantile of the conditional distribution of $\|\{\hat{G}_m\}_{m=1}^M\|_p$ given the data $(W_i)_{i=1}^N$. This can lead to more efficient confidence intervals with lower volume compared to standard multiple testing approaches and hence to tests with higher power, see e.g. Klaassen (2021) and Klaassen et al. (2022).

Finally, we note that since Theorem 3 also holds for any $\Delta \neq 0$, we can reverse the role of the null and alternative hypotheses to construct hypotheses tests of the form

$$H_0 : \sqrt{N} \|\{\hat{\sigma}_m^{-1} \theta_m\}_{m=1}^M\|_p > \Delta_0,$$

for a predefined threshold $\Delta_0 > 0$. In such a framework as e.g. advocated by Bilinski and
Hatfield (2018), the null hypothesis $H_0$ postulates a non-negligible violation of the testable implication, while the alternative hypothesis $H_1$ states that the violation is close to zero, i.e. smaller than the absolute value of $\Delta_0$. For this reason, a statistically significant test statistic suggests that violations exceeding the threshold $\Delta_0$ can be ruled out in order to justify ATE estimation based on the selection-on-observables assumption.

7 Testing based on squared differences

In this section, we suggest a further testing approach based on squared differences in conditional mean outcomes. More concisely, we aim at testing for violations of hypothesis (5.1) globally, i.e. across all values of $D$ and $X$, by verifying the following null hypothesis $H_0$:

$$\theta_0 := \mathbb{E} \left[ (\mu(1, D, X) - \mu(0, D, X))^2 \right] = 0.$$  \hfill (7.1)

In contrast to equation (6.3), expression (7.1) uses an aggregate $L_2$-type measure to test violations across values of $D$ and $X$, which is a common approach in specification tests for non-parametric regression, see e.g. Racine (1997), Racine et al. (2006), Hong and White (1995) and Wooldridge (1992). Here, we consider machine learning for estimating the regression function $\mu$, to allow the covariate vector $X$ to be high-dimensional. To this end, we test (7.1) based on a moment condition which uses the following Neyman-orthogonal score function:

$$\phi_2(W, \theta, \eta) = (\eta(1, D, X) - \eta(0, D, X))^2 - \theta + \zeta,$$

where $W = (Y, Z, D, X, \zeta)$ are the data, $\eta_0(Z, D, X) = \mu(Z, D, X)$ are the true nuisance parameters, and $\zeta$ is an independent mean-zero random variable, which satisfies $\|\zeta\|_{P,q} < C$ for $q > 2$ and has a variance of $\sigma_\zeta^2 > 0$. In contrast to the DR approach discussed in Section 6, testing exclusively relies on the conditional mean outcome as nuisance parameter, while propensity score estimation is not required. The additional random variable $\zeta$ is introduced to avoid a degenerate distribution of our estimator under $H_0$, which is a common problem in specification tests, see e.g. Hong and White (1995) and Wooldridge (1992). The variance $\sigma_\zeta^2$ acts like a tuning parameter and should be chosen as a function of the sample size $n$. A relatively high
variance should ensure that our test based on $\phi_2$ comes close to the nominal level $\alpha$ in small samples. On the negative side, a high variance $\sigma^2$ could importantly reduce testing power by whitewashing violations of the null hypothesis.

To estimate the target parameter $\theta_0$, we rely on cross-fitting and split the data into $K$ subsamples of size $N/K$. The cross-fitted estimator is given by

$$
\hat{\theta} = \frac{1}{K} \sum_{k=1}^{K} E_{N,k}[\hat{\eta}_k(0, D_i, X_i) - \hat{\eta}_k(1, D_i, X_i)]^2 + \zeta_i,
$$

The following assumption imposes several regularity conditions required for the asymptotic normality of our test under null hypothesis, as postulated in Theorem 4.

**Assumption 10 (Asymptotic Normality).** The following assumption needs to hold for all $n \geq 3$, $P \in \mathcal{P}$ and $q > 2$: Given a random subset $I$ of $[N]$ of size $n = N/K$, the nuisance parameter estimator $\hat{\eta}_0 = \hat{\eta}_0((W_i)_{i \in I})$ obeys $\|\hat{\eta} - \eta_0\|_{P,2q} \leq C$, $\|\hat{\eta} - \eta_0\|_{P,4} \leq \delta_N$, and $\|\hat{\eta} - \eta_0\|_{P,2} \leq \delta_N^{-1/2} N^{-1/4}$ with $P$-probability not less than $1 - o(1)$.

**Theorem 4.** Conditional on Assumptions 10, under $H_0$, it holds

$$
\sqrt{N}\sigma^{-1}\hat{\theta} \rightsquigarrow N(0, 1)
$$

(7.3)

uniformly over $P \in \mathcal{P}$, where $\sigma^2 = \mathbb{E}[(\eta_0(1, D, X) - \eta_0(0, D, X))^4] + \sigma^2_\xi$. Moreover, the result continues to hold if $\sigma^2$ is replaced by

$$
\hat{\sigma}^2 := \frac{1}{K} \sum_{k=1}^{K} E_{n,k}\left[(\hat{\eta}_k(1, D_i, X_i) - \hat{\eta}_k(0, D_i, X_i))^4\right] + \sigma^2_\xi.
$$

Consequently, a test that rejects the null hypothesis $H_0$ if $\sqrt{N}\hat{\sigma}^{-1}|\hat{\theta}| > \Phi^{-1}(1 - \alpha/2)$ has asymptotic level $\alpha$.

It is important to point out that Theorem 4 only holds under $H_0$. This is because Neyman orthogonality is only satisfied under $H_0$, as shown in the proof of Theorem 4 in Appendix A.4. To provide theoretical power results under $H_1$, one could orthogonalize the score (7.1) using the methodology introduced in Chernozhukov et al. (2015) and Chernozhukov et al. (2018b). This is an open question for future research.
8 Simulation study

This section provides a simulation study to investigate the finite sample behavior of our testing approaches introduced in Section 6 and Section 7 based on the following data generating process:

\[
Y = D + X'\beta + \gamma Z + \delta W + U,
\]
\[
D = I\{X'\beta + Z + W + V > 0\},
\]
\[
X \sim N(0, \sigma_X^2), Z \sim \text{bernoulli}(0.5),
\]
\[
W \sim N(0, \sigma_W^2), U \sim N(0, \sigma_U^2), V \sim N(0, \sigma_V^2),
\]

with \(X, Z, W, U, V\) being independent of each other. Outcome \(Y\) is a linear function of \(D\) (whose treatment effect is one), covariates \(X\) (for \(\beta \neq 0\)), the unobservables \(W\) (for \(\delta \neq 0\)) and \(U\), and the supposed instrument \(Z\) if the coefficient \(\gamma \neq 0\). The binary treatment \(D\) is a function of \(X, Z\), and the unobservables \(W\) and \(V\). While the supposed instrument \(Z\) is binary, the unobserved terms \(U, V, W\) are normally distributed random variables that are independent of each other, of \(Z\), and of \(X\) with \(\sigma_U = \sigma_V = 0.1\) and \(\sigma_W = 0.25\). \(X\) is a vector of covariates which follow a normal distribution with a zero mean and a covariance matrix \(\sigma_X^2\) that is obtained by setting the covariance of the \(i\)th and \(j\)th covariate in \(X\) to \(0.5^{|i-j|}\). The coefficient vector \(\beta\) gauges the effects of the covariates on \(Y\) and \(D\), respectively, and thus, the magnitude of confounding due to observables. The \(i\)th element of the coefficient vector \(\beta\) is set to \(0.7/i\) for \(i = 1, ..., p\), implying a linear decay of covariate importance in terms of confounding.

We analyze the performance of our testing approach in 1000 simulations under two sample sizes of \(n = 1000\) and \(4000\) when setting the number of covariates to 50. We estimate \(\Delta\) based on DML with cross-fitting using the default options of the ‘treatDML’ command in the ‘causalweight’ package by Bodory and Huber (2018) for the statistical software R. The command uses lasso regression, see Tibshirani (1996), as ML method for estimating the nuisance parameters, i.e. linear and logit specifications of the outcome and treatment equations (and more generally makes use of the ‘SuperLearner’ package by van der Laan et al. (2007) for selecting ML algorithms). Observations whose instrument propensity scores are close to zero, namely smaller than a threshold of 0.01 (or 1%), are dropped from the estimation in order to avoid an
explosion of the propensity score-based weights, which might heavily increase the variance of estimating $\Delta$. We also analyze the test performance when using the score based on the squared difference in equation (7.2) to estimate $\theta_0$ in (7.1). In this case, we choose $\zeta \sim \mathcal{N}(0, \sigma_\zeta^2)$ where the variance term $\sigma_\zeta$ decreases in the sample size, by setting $\sigma_\zeta = 500/n$ in (7.2).

Table 1: Simulations

| Assumptions 6 and 7 hold ($\delta = 0, \gamma = 0$) | Test based on $\Delta$ | Test based on $\theta$ |
|-----------------------------------------------|------------------------|------------------------|
| sample size | est | std | mean se | rej. rate | est | std | mean se | rej. rate |
| 1000 | -0.0030 | 0.0069 | 0.0066 | 0.151 | 0.0034 | 0.0152 | 0.0158 | 0.097 |
| 4000 | 0.0016 | 0.0034 | 0.0033 | 0.135 | 0.0009 | 0.0077 | 0.0079 | 0.091 |
| Ass. 6 violated, Ass. 7 holds ($\delta = 2, \gamma = 0$) | | | | | | |
| 1000 | -0.0695 | 0.0367 | 0.0328 | 0.657 | 0.0721 | 0.0192 | 0.0162 | 0.992 |
| 4000 | -0.0613 | 0.0170 | 0.0166 | 0.979 | 0.0232 | 0.0040 | 0.0020 | 1.000 |
| Ass. 6 holds, Ass. 7 violated ($\delta = 0, \gamma = 0.1$) | | | | | | |
| 1000 | 0.0970 | 0.0069 | 0.0066 | 1.000 | 0.0126 | 0.0152 | 0.0158 | 0.186 |
| 4000 | 0.0984 | 0.0034 | 0.0033 | 1.000 | 0.0101 | 0.0020 | 0.0020 | 1.000 |

Notes: columns ‘est’, ‘std’, and ‘mean se’ provide the average estimate of $\Delta$ and $\theta$, respectively, its standard deviation, and the average of the estimated standard error across all samples. ‘rej. rate’ gives the empirical rejection rate when setting the level of statistical significance to 0.1 (or 10%).

Table 1 reports the simulation results. The top panel focusses on the case that $\delta = \gamma = 0$, such that both Assumptions 6 and 7 are satisfied. Already under the smaller sample size of $n = 1000$, the average estimate of $\Delta$ (‘est’) across all simulations is close to zero, and quickly approaches this true value as the sample size increases. Accordingly, the empirical rejection rate amounts to 0.151 or 15.1% under the smaller sample size, which is only somewhat higher than the nominal rate of 10% when setting the level of statistical significance to 0.1. Under the larger sample size of $n = 4000$, the rejection rate corresponds to 13.5% and, thus, appears to approach the nominal level. Furthermore, the average standard error across all simulations (‘mean se’) is generally close to the actual standard deviation (‘std’) of DML. We also see a root-n consistent behavior in the sense that the standard deviation of the estimator is cut by half when the sample size is quadrupled, while the bias is close to zero.

The intermediate panel presents the results when Assumption 6 does not hold ($\delta = 2, \gamma = 0$), such that the treatment is not conditionally mean independent. The DML estimates are substantially different from zero, amounting to $-0.0695$ and $-0.0613$ for $n = 1000$ and $n = 4000$, respectively. Furthermore, the test’s statistical power to detect the violation quickly
increases in the sample size, with a rejection rate amounting to 65.7% under the lower and 97.9% under the higher sample size. Similar conclusions apply to the violation of Assumption 7 \((\delta = 0, \gamma) = 0.1\), such that the instrument is not conditionally mean independent. The lower panel of Table 1 shows that the rejection rates correspond to 100% for both sample sizes, \(n = 1000\) and \(n = 4000\). Summing up, we find the empirical size and power of our test based on \(\Delta\) to be very decent in these simulation scenarios.

The empirical size of our test based on the squared difference \(\theta\) is generally close to the nominal level of 10% under the null hypothesis, as indicated in the top panel of Table 1. Also the power is generally quite decent. Under a violation of Assumption 6 \((\delta = 2, \gamma = 0)\), the rejection rate is close to 100% under either sample size, see the intermediate panel. Under a violation of Assumption 7 \((\delta = 0, \gamma = 0.1)\), the power of the test based on \(\theta\) is quite low for \(n = 1000\) but increases fast in the sample size, with a rejection rate of 100% for \(n = 4000\), see the lower panel. In contrast to the estimation of \(\Delta\), we do not observe a root-\(n\) consistent behavior of the test based on \(\theta\) in the intermediate and lower panel of Table 1, which is not surprising as orthogonality only holds under the null hypothesis, see Theorem 4.

In a next step, we consider a simulation setting with effect heterogeneity:

\[
Y = D + X'\beta + \gamma Z X_1 + \gamma Z X_2 + \delta W X_1 + \delta W X_2 + U,
\]

\[
D = I\{X'\beta + Z + W + V > 0\},
\]

\[
X \sim N(0, \sigma_X^2), Z \sim \text{bernoulli}(0.5),
\]

\[
W \sim N(0, \sigma_W^2), U \sim N(0, \sigma_U^2), V \sim N(0, \sigma_V^2),
\]

with \(X, Z, W, U, V\) being independent of each other and \(X_1\) and \(X_2\) denoting the first and second covariates in \(X\), respectively. For \(\delta \neq 0\) and \(\gamma \neq 0\), Assumptions 6 and 7, respectively, are violated, but in contrast to our previous simulation design, the violations are now heterogeneous in \(X_1\) and \(X_2\). We also note that the violations cancel out when averaging over \(X_1\) and \(X_2\), because the covariates are normally distributed and centered around zero.

Table 2 provides the results for testing based on \(\Delta\) and \(\theta\) under a violation of Assumption 6 (upper panel), Assumption 7 (intermediate panel), and both assumptions (lower panel). The
Table 2: Simulations: Effect Heterogeneity

| sample size | Test based on $\Delta$ | Test based on $\theta$ |
|-------------|-------------------------|------------------------|
|             | est | std | mean se | rej. rate | est | std | mean se | rej. rate |
| Assumptions 6 violated, 7 holds ($\delta = 2, \gamma = 0$) |     |     |         |           |     |     |         |           |
| 1000        | 0.0015 | 0.0656 | 0.0584 | 0.139 | 0.1738 | 0.0369 | 0.0178 | 1.000 |
| 4000        | 0.0181 | 0.0320 | 0.0300 | 0.185 | 0.0582 | 0.0122 | 0.0080 | 1.000 |
| Ass. 6 holds, Ass. 7 violated ($\delta = 0, \gamma = 0.1$) |     |     |         |           |     |     |         |           |
| 1000        | -0.0033 | 0.0089 | 0.0086 | 0.143 | 0.0336 | 0.0155 | 0.0159 | 0.678 |
| 4000        | -0.0017 | 0.0044 | 0.0043 | 0.120 | 0.0309 | 0.0077 | 0.0079 | 0.995 |
| Ass. 6 and Ass. 7 violated ($\delta = 2, \gamma = 0.1$) |     |     |         |           |     |     |         |           |
| 1000        | 0.0013 | 0.0657 | 0.0586 | 0.139 | 0.2035 | 0.0519 | 0.0187 | 1.000 |
| 4000        | 0.0181 | 0.0321 | 0.0300 | 0.188 | 0.0864 | 0.0212 | 0.0082 | 1.000 |

Notes: columns ‘est’, ‘std’, and ‘mean se’ provide the average estimate of $\Delta$ and $\theta$, respectively, its standard deviation, and the average of the estimated standard error across all samples. ‘rej. rate’ gives the empirical rejection rate when setting the level of statistical significance to 0.1 (or 10%).

DR estimator of $\Delta$ has a low power in all settings and under either sample size, due to averaging out violations across values of $X$. In contrast, the estimator based on the squared difference $\theta$ has a very high power in all settings investigated.

Finally, we investigate empirical size and testing power under effect heterogeneity when estimating violations based on DR in subsets of the data defined as a function of variables in $(D, X)$ that importantly predict such violations, as discussed in Section 6. To this end, we randomly split our data into two halves and use the first subsample to estimate the DR score functions based on cross-fitting and the random forest as implemented in the ‘grf’ package by Tibshirani et al. (2020) for nuisance parameter estimation. We then apply yet another random forest for determining the importance of the variables in $(D, X)$ for predicting the estimated scores based on the mean squared error-criterion, using the ‘randomForest’ package by Liaw and Wiener (2002). According to variable importance, we pick those three variables which explain most of the heterogeneity in violations. In the second subsample, we split the data at the median of these variables, such that we obtain $M = 6$ subsets in which we estimate the violation $\theta_m$ based on the DR score function (6.6). Finally, we control for multiple testing using the Bonferroni correction or the multiplier bootstrap based on the $\| \cdot \|_\infty$ and $\| \cdot \|_2$ norms.

Table 3 reports the results for sample sizes of $n = 4000$ and $n = 12000$. Under the satisfaction of both Assumptions 6 and 7 in the upper panel, the rejection rates of the Bonferroni correction and the multiplier bootstrap using the $\| \cdot \|_\infty$ norm non-negligibly exceed the nominal size of 10%,

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Table 3: Simulations: Multiple Testing approach

| sample size | Bonf. $\| \cdot \|_\infty$ | $\| \cdot \|_2$ |
|-------------|----------------|----------------|
|             | Ass. 6 and Ass. 7 hold ($\delta = 0, \gamma = 0$) | Ass. 6 and Ass. 7 violated ($\delta = 2, \gamma = 0.1$) | Ass. 6 violated and Ass. 7 holds ($\delta = 2, \gamma = 0$) | Ass. 6 holds and Ass. 7 violated ($\delta = 0, \gamma = 0.1$) |
| 4000        | 0.164 0.173 0.247 | 0.302 0.308 0.408 | 0.365 0.377 0.458 | 1.000 1.000 1.000 |
| 12000       | 0.154 0.158 0.245 | 0.824 0.829 0.869 | 0.867 0.874 0.915 | 1.000 1.000 1.000 |

Notes: column ‘Bonf.’ corresponds to rejection rate using Bonferroni correction for multiple testing. The columns ‘$\| \cdot \|_\infty$’ and ‘$\| \cdot \|_2$’ correspond to rejection rate using the multiplier bootstrap procedure described in (6.8) based on the norms $\| \cdot \|_\infty$ and $\| \cdot \|_2$, respectively. The level of statistical significance is 0.1 (or 10%).

while the size distortion is particularly severe for the $\| \cdot \|_2$ norm. When considering violations of one or both assumptions, the test has only limited power under a sample size of $n = 4000$. This was the motivation for also considering a substantially larger sample size of $n = 12000$, in which power is decent, in particular when considering the multiplier bootstrap with the $\| \cdot \|_2$ norm. Concerning the power issues, it is worth noting that testing within a subset relies on approximately 333 or 1000 observations under the smaller or larger sample size, respectively, since we consider 6 subsets in the second half of the sample for the DR approach.

9 Empirical application

This section provides an empirical application to US census data from Angrist and Evans (1998), who used the sex ratio of a mother’s first two children as instrument $Z$ for estimating the effect of (higher) fertility, defined as a treatment dummy $D$ for having at least three children, on mothers’ labor supply outcomes. The latter include mothers’ weeks in employment per year, which we consider as outcome $Y$. The intuition for this IV strategy is that if parents have a preference for mixed sex children, then getting two children of the same sex, which is arguably randomly assigned by nature, increases the chance of having a third child. This implies a
positive first stage effect of $Z$, defined as a dummy for having two kids of the same sex, on $D$.

However, Rosenzweig and Wolpin (2000) argue that having mixed sex siblings may violate IV validity by directly affecting both the marginal utility of leisure and child rearing costs and, thus, labor supply.\(^7\) In addition, a further IV assumption required for the identification of the LATE might be challenged, namely monotonicity, which states that no mother (or family) in the population prefers two children of the same sex over mixed sex children. Lee (2008) for instance finds that South Korean parents with one son and one daughter are more likely to continue childbearing than parents with two sons and it might be doubted that such cases can be fully excluded in the US data of Angrist and Evans (1998). Here, we do not a priori impose the IV assumptions required for LATE identification, but instead use the sibling sex ratio instrument to jointly test IV validity and the selection-on-observables assumption.

To this end, we consider a subsample of the 1980 wave in Angrist and Evans (1998) from the US Census Public Use Micro Samples, namely married white couples with mother’s education amounting to 12 years, all in all 143,410 observations. While this sample restriction already controls for ethnicity and mother’s education, we in addition consider mother’s age, mother’s age at first birth, father’s age, and father’s income as covariates $X$. As for the simulations in Section 8, we estimate $\Delta$ and $\theta$ based on lasso regression as ML, using the ‘causalweight’ package by Bodory and Huber (2018). To make the lasso regressions more flexible, we also include interaction terms between the variables in the conditioning set as well as squared and cubic terms of the age and income variables. Table 4 presents the estimates (‘est’) of $\Delta$ and $\theta$, respectively, along with the standard errors (‘se’) and p-values (‘pval’). The violation of our testable implication is statistically significant at the 5% level based on DR estimation of $\Delta$ (‘lasso-based DR’) and even considerably more significant (with a p-value close to zero) when applying the squared difference-based test based on $\theta$ (‘lasso-based SD’).

Finally, we aim at estimating the violations based on DR within subsets of the data which are defined as a function of elements in $(D, X)$ that importantly predict violations of our testable implication, as discussed in Section 6. We randomly split our sample into two halves and proceed

\(^7\)Furthermore, for a data set from rural India, they also provide empirical evidence that expenses for clothing of the third born are significantly lower if the older siblings are of the same sex, which may also affect labor supply decisions and thus, violate the exclusion restriction. Even though it is not clear to which extent this issue carries over to the US data of Angrist and Evans (1998), concerns about IV validity remain.
Table 4: DR and squared difference-based tests

|                  | est  | se  | pval |
|------------------|------|-----|------|
| lasso-based DR   | 0.22 | 0.11| 0.05 |
| lasso-based SD   | 0.50 | 0.02| 0.00 |

Notes: columns ‘est’, ‘se’, and ‘pval’ provide the estimates, the standard errors and the p-values for \( \Delta \) and \( \theta \), respectively.

analogously as in the simulations of Section 8. In the first subsample, we estimate the score functions based on cross-fitting and use the random forest of the ‘grf’ package to estimate the nuisance parameters. We apply another random forest for determining the variable importance of elements in \((D, X)\) in predicting the estimated scores based on the ‘randomForest’ package. The results suggest that father’s income is by far the most important predictor of heterogeneity in violations \( \Delta(D, X) \). Hence, we split the second subsample into subsets based on the median of father’s income and estimate the violation \( \theta_m \) based on the DR score function \((6.6)\) in either subset and test statistical significance separately and jointly across subsets.

Table 5: DR tests within subsets

| splitting variable | subset       | est  | se  | pval |
|--------------------|--------------|------|-----|------|
| father’s income    | in lower half| 0.076| 0.162| 0.641|
|                    | in upper half| 0.328| 0.152| 0.030|

Notes: columns ‘est’, ‘se’, and ‘pval’ provide the estimates for \( \Delta \) and the respective standard errors and p-values within subsets defined upon the quantiles of the variables in the first column. The three last columns ‘bonf’, ‘boot \((l_\infty)\)’, and ‘boot \((l_2)\)’ yield the p-value for the joint significance across all subsets using Bonferroni correction and multiplier bootstrap based on the \(l_\infty\)-norm and \(l_2\)-norm, respectively.

Table 5 reports the results of our DR approach across subsets. It provides the estimated violation (‘est’) separately for observations in the lower and upper halves of the distribution of father’s income. The estimate in the upper half (0.328) is statistically significantly different from zero at the 5% level (see ‘pval’), while that in the lower one is close to zero and insignificant. When testing the null hypothesis in both subsets jointly, the p-values amounts to roughly 6% when using the Bonferroni correction or the multiplier bootstrap based on the \(l_\infty\)-norm. The p-value of the test using multiplier bootstrap based on the \(l_2\)-norm is slightly higher (8.25%).

By and large, the results of our statistical tests presented in Tables 4 and 5 point to a violation of the testable implication \((4.1)\), such that identification might fail. Given the limited set of covariates in our empirical illustration, our findings are most likely driven by a violation of the selection-on-observables assumption, even though IV validity cannot be taken for granted either
for the reasons discussed at the beginning of this section.

10 Conclusion

In this paper, we demonstrated the existence of a testable condition for the identification of treatment effects, given that a set of covariates to be controlled for and a suspected instrument are available in the data and specific assumptions about the causal relations of the observed variables hold. The testable condition corresponds to the conditional independence of the suspected instrument and the outcome given the treatment and the covariates. It at the same time implies that the suspected instrument is valid (i.e. satisfies the exclusion restriction and is as good as random conditional on the covariates) and that the treatment is as good as random conditional on the covariates such that the treatment is identified given the covariates. We proposed tests of this conditional independence based on doubly robust estimators aimed at either detecting average violations in the total sample or violations in subsamples in a data-driven way as a function of observed covariates. We also suggested a further, global test accounting for covariate-related heterogeneities in violations which is based on the average squared violation in the sample. We derived the asymptotic distribution of newly suggested tests and investigated the finite sample behavior of all approaches in a simulation study. Finally, we applied our tests to the evaluation of the impact of fertility on female labor supply when using the sibling sex ratio of the first two children as suspected instrument. Our results pointed to a violation of our testable implication under the moderate set of socio-economic covariates considered.
A Appendix

A.1 Proof of Theorem 1

We subsequently provide a proof for Theorem 1. The latter states that conditional on Assumptions 1 and 3, $Y \perp \perp Z | D = d, X$ is a necessary and sufficient condition for the joint satisfaction of $Y(d) \perp \perp D | X$ and $Y(d) \perp \perp Z | X$. That is, Assumptions 4 and 5 hold when considering potential outcomes $Y(d)$ matching the factual treatment assignment $D = d$.

We show this result based on a proof by contradiction. To this end, let us assume that in addition to Assumptions 1 and 3, also Assumptions 4 and 5 hold, i.e. $Y(d) \perp \perp D | X$ and $Y(d) \perp \perp Z | X$, while $Y(d) \perp \perp Z | D, X$ is assumably violated. The latter violation necessarily implies that conditional on $X$, $D$ is a collider between $Z$ and $Y(d)$ in the sense of Pearl (2000), meaning that controlling for $D$ introduces statistical dependence between $Z$ and $Y(d)$. To see this, first note that by Assumptions 5 and 3, $Z$ is independent of $Y(d)$ and associated with $D$, respectively, conditional on $X$. Given the causal framework and faithfulness imposed by Assumption 1, the conditional dependence between $Z$ and $D$ may only be due to a causal effect of $Z$ on $D$ and/or to unobserved confounders affecting both $Z$ and $D$. If conditioning on $D$ introduces statistical dependence between $Z$ and $Y(d)$, then there must necessarily exist a confounder affecting both $D$ and $Y(d)$ conditional on $X$. This follows from a combination of the so-called d-separation criterion of Pearl (1988), which implies that $D$ is a collider if a confounder affects both $D$ and $Y(d)$ or $Y(d)$ affects $D$, and Assumption 1, which rules out that $Y(d)$ affects $D$ (reverse causality). However, confounding of $D$ and $Y(d)$ conditional on $X$ violates (and thus, contradicts) Assumption 4, namely $Y(d) \perp \perp D | X$. This in turn implies that if Assumption 4 holds such that $D$ is not a collider and controlling on $D$ does not introduce a dependence of $Y(d)$ and $Z$ given $X$, then $Y(d) \perp \perp Z | D, X$ must be due to statistical dependence between $Z$ and $Y(d)$ conditional $X$. The latter, however, violates (and thus, contradicts) Assumption 5.

We have demonstrated that the joint satisfaction of Assumptions 1, 3, 4, and 5 necessarily implies $Y(d) \perp \perp Z | D, X$. Next, we also show the converse, namely that conditional on Assumptions 1 and 3, the satisfaction of $Y(d) \perp \perp Z | D, X$ necessarily implies the joint satisfaction of Assumptions 4 and 5. In our proof by contradiction, we assume that $Y(d) \perp \perp Z | D, X$ holds while Assumptions 4 and/or 5 are violated. The latter violations imply that conditional on $X$, there exist confounders of $D$ and $Y(d)$ and/or statistical dependence between $Z$ and $Y(d)$ (due to confounders or a violation of the exclusion restriction). For the reasons discussed before, any of these violations imply $Y \not\perp \perp Z | D = d, X$ and thus
create a contradiction. Therefore, it follows that

\[ Y(d) \perp \perp D|X, \quad Y(d) \perp \perp Z|X \iff Y(d) \perp \perp Z|D = d', X \quad \forall d, d' \in D, \]  

(A.1)
such that \( Y(d) \perp \perp Z|D, X \) is necessary and sufficient for Assumption 4 and 5, given the remaining assumptions.

To derive the testable implication, we set the factual treatment in the conditioning set of \( Y(d) \perp \perp Z|D, X \) to \( D = d \) and note that

\[ Y(d) \perp \perp Z|D = d, X \iff Y \perp \perp Z|D = d, X. \]  

(A.2)

The equivalence follows from the observational rule stating that \( Y(d) = Y \) if \( D = d \) under the Stable Unit Treatment Value Assumption (SUTVA). It then follows by (A.1) that

\[ Y(d) \perp \perp D|X, \quad Y(d) \perp \perp Z|X \iff Y \perp \perp Z|D = d, X \quad \forall d \in D. \]  

(A.3)

Therefore, conditional on Assumptions 1 and 3, the testable implication \( Y \perp \perp Z|D = d, X \) is necessary and sufficient for the joint satisfaction of Assumptions 4 and 5 when considering potential outcomes \( Y(d) \) matching the factual treatment assignment \( D = d \).

A.2 Proof of Theorem 2

We subsequently provide a proof for Theorem 2. The latter states that conditional on Assumptions 1 and 8, \( E[Y|Z, D = d, X] = E[Y|D = d, X] \) is a necessary and sufficient condition for the joint satisfaction of \( E[Y(d)|D = d, X] = E[Y(d)|X] \) and \( E[Y(d)|Z, X] = E[Y(d)|X] \). That is, Assumptions 6 and 7 hold when considering potential outcomes \( Y(d) \) that match the factual treatment assignment \( D = d \).

We show this result based on a proof by contradiction. To this end, let us assume that in addition to Assumptions 1 and 8, also Assumptions 6 and 7 hold, while \( E[Y(d)|Z, D, X] \neq E[Y(d)|D, X] \). The latter inequality necessarily implies that conditional on \( X, D \) is a collider between \( Z \) and \( Y(d) \) in the sense of Pearl (2000), meaning that controlling for \( D \) in addition to \( X \) introduces a non-zero correlation between \( Z \) and \( Y(d) \). To see this, first note that by Assumptions 7 and 8, \( Z \) is mean independent of \( Y(d) \), but not of \( D \) conditional on \( X \). Given the causal framework and faithfulness imposed by Assumption 1, the
conditional dependence between $Z$ and $D$ may only be due to a non-zero average causal effect of $Z$ on $D$ and/or to unobserved confounders affecting both $Z$ and $D$. If conditioning on $D$ introduces mean dependence between $Z$ and $Y(d)$, then there must necessarily exist a confounder affecting both $D$ and $Y(d)$ conditional on $X$. This follows from a combination of the so-called d-separation criterion of Pearl (1988), which implies that $D$ is a collider if a confounder affects both $D$ and $Y(d)$ or $Y(d)$ has a non-zero average effect on $D$ (reverse causality). However, confounding of $D$ and $Y(d)$ conditional on $X$ violates (and thus, contradicts) Assumption 6. This in turn implies that if Assumption 6 holds such that $D$ is not a collider and controlling on $D$ does not introduce a correlation between $Y(d)$ and $Z$ conditional on $X$, then $E[Y(d)|Z, D, X] \neq E[Y(d)|D, X]$ must be due to a correlation of $Z$ and $Y(d)$ conditional $X$. The latter, however, violates (and thus, contradicts) Assumption 7.

We have demonstrated that the joint satisfaction of Assumptions 1, 8, 6, and 7 necessarily implies $E[Y(d)|Z, D, X] = E[Y(d)|D, X]$. Next, we also show the converse, namely that conditional on Assumptions 1 and 8, $E[Y(d)|Z, D, X] = E[Y(d)|D, X]$ necessarily implies the joint satisfaction of Assumptions 6 and 7. In our proof by contradiction, we assume that $E[Y(d)|Z, D, X] = E[Y(d)|D, X]$ holds while Assumptions 6 and/or 7 are violated. The latter violations imply that conditional on $X$, there exist confounders of $D$ and $Y(d)$ and/or a correlation of $Z$ and $Y(d)$ (due to confounders or a violation of the exclusion restriction). For the reasons discussed before, any of these violations imply $E[Y(d)|Z, D, X] \neq E[Y(d)|D, X]$ and thus create a contradiction. Therefore, it follows that

$$E[Y(d)|D, X] = E[Y(d)|X], \quad E[Y(d)|Z, X] = E[Y(d)|X]$$

$$\iff E[Y(d)|Z, D = d', X] = E[Y(d)|D = d', X] \quad \forall d, d' \in D,$$

such that $E[Y(d)|Z, D, X] = E[Y(d)|D, X]$ is necessary and sufficient for Assumption 6 and 7, given the remaining assumptions.

To derive the testable implication, we set the factual treatment in the conditioning set of $E[Y(d)|Z, D, X]$ and $E[Y(d)|D, X]$ to $D = d$ and note that

$$E[Y(d)|Z, D = d, X] = E[Y(d)|D = d, X] \iff E[Y|Z, D = d, X] = E[Y|D = d, X].$$

The equivalence follows from the observational rule stating that $Y(d) = Y$ if $D = d$ under the Stable
Unit Treatment Value Assumption (SUTVA). It then follows by (A.4) that

\[ E[Y(d)|D, X] = E[Y(d)|X], \quad E[Y(d)|Z, X] = E[Y(d)|X] \]

\[ \iff \quad E[Y|Z, D = d, X] = E[Y|D = d, X] \quad \forall d \in D. \]

Therefore, conditional on Assumptions 1 and 8, the testable implication \( E[Y|Z, D = d, X] = E[Y|D = d, X] \) is necessary and sufficient for the joint satisfaction of Assumptions 6 and 7 when considering potential outcomes \( Y(d) \) that match the factual treatment assignment \( D = d \).

### A.3 Proof of Theorem 3

To prove Theorem 3, we adapt the proof of Theorem 5.1 in Chernozhukov et al. (2018b). All bounds in the proof hold uniformly over \( P \in \mathcal{P} \) but we omit this qualifier for brevity. We use \( C \) to denote a strictly positive constant that is independent of \( n \) and \( P \in \mathcal{P} \). The value of \( C \) may change at each appearance.

**Proof.** First, we observe that the score in (6.6) is linear

\[ \phi_m(W, \theta, \eta) = \phi^\alpha_m(W, \eta)\theta + \phi^b_m(W, \eta) \]

with \( \phi^\alpha_m(W, \eta) = -1 \) and

\[ \phi^b_m(W, \eta) = \left( \mu(1, D, X) - \mu(0, D, X) \right) \\
+ \frac{(Y - \mu(1, D, X)) \cdot Z}{p(D, X)} - \frac{(Y - \mu(0, D, X)) \cdot (1 - Z)}{1 - p(D, X)} \right) I\{D, X \in L_m\}. \]

The score fulfills the moment condition, e.g.

\[ E[\phi_m(W, \theta_0, \eta_0)] = E \left[ \left( \frac{(Y - \mu(1, D, X)) \cdot Z}{p(D, X)} - \frac{(Y - \mu(0, D, X)) \cdot (1 - Z)}{1 - p(D, X)} \right) I\{D, X \in L_m\} \right] = 0 \]

for each \( m = 1, \ldots, M \) by construction of the score since \( E[\varepsilon|Z, D, X] = 0 \). Next, we verify Neyman
orthogonality. For all $\eta = (\mu, p)$, it holds

$$
\partial_r \mathbb{E}[\phi_m(W, \theta_0, \eta_0 + r(\eta - \eta_0))]|_{r=0} = 
= \mathbb{E} \left[ \partial_r \phi_m, (W, \theta_0, \eta_0 + r(\eta - \eta_0)) \right]|_{r=0} = 
= \mathbb{E} \left[ (\mu(1, D, X) - \mu_0(1, D, X))I\{D, X \in L_m\} \right] - \mathbb{E} \left[ (\mu(0, D, X) - \mu_0(0, D, X))I\{D, X \in L_m\} \right] 
- \mathbb{E} \left[ \left( \frac{Z(\mu(1, D, X) - \mu_0(1, D, X))}{p_0(D, X)} \right) I\{D, X \in L_m\} \right] 
+ \mathbb{E} \left[ \left( \frac{(1-Z)(\mu(0, D, X) - \mu_0(0, D, X))}{1-g_0(D, X)} \right) I\{D, X \in L_m\} \right] 
- \mathbb{E} \left[ \left( \frac{Z(Y - \mu_0(1, D, X))}{g_0(D, X)^2} \right) (p(D, X) - p_0(D, X)) I\{D, X \in L_m\} \right] 
- \mathbb{E} \left[ \left( \frac{(1-Z)(Y - \mu_0(0, D, X))}{(1-g_0(D, X))^2} \right) (p(D, X) - p_0(D, X)) I\{D, X \in L_m\} \right] = 0
$$

since $\mathbb{E}[Z|D, X] = p_0(D, X)$, $\mathbb{E}[Z(Y - \mu_0(1, D, X))|D, X] = 0$ and $\mathbb{E}[(1-Z)(Y - \mu_0(0, D, X))|D, X] = 0$ due to model (6.1) and (6.2). Also, the identification condition holds, namely, $J_0 := \mathbb{E}[\phi^2_m(W, \eta)] = -1.$ This gives us Assumption 3.1 in Chernozhukov et al. (2018b). To prove Assumption 3.2 d) in Chernozhukov et al. (2018b), we observe that $\Sigma^2 := \mathbb{E}[\phi_M(W, \theta_0, \eta_0)\phi_M(W, \theta_0, \eta_0)^T]$ with diagonal elements

$$
\mathbb{E}[\phi_m(W, \theta_0, \eta_0)^2]
= \mathbb{E}[\mathbb{E}[\phi_m(W, \theta_0, \eta_0)^2|X, D]]
= \mathbb{E}\left[ \left( \frac{(\mu(1, D, X) - \mu(0, D, X))I\{D, X \in L_m\} - \theta_m}{p(D, X)} \right)^2 I\{D, X \in L_m\} \right]|X, D]
= \mathbb{E}\left[ \left( \frac{(\mu(1, D, X) - \mu(0, D, X))I\{D, X \in L_m\} - \theta_m}{p(D, X)} \right)^2 + \left( \frac{(Y - \mu(1, D, X))}{p(D, X)} \cdot \frac{(Y - \mu(0, D, X))}{1-p(D, X)} \right)^2 I\{D, X \in L_m\} \right]|X, D]
$$
with the same arguments as above. Thus, due to Assumption 9,

\[
\begin{align*}
\mathbb{E}[\phi_m(W, \theta_0, \eta_0)^2] & \geq \mathbb{E} \left[ \frac{(Y - \mu(1, D, X)) \cdot Z}{p(D, X)} - \frac{(Y - \mu(0, D, X)) \cdot (1 - Z)}{1 - p(D, X)} \right]^2 I\{D, X \in L_m\} \\
& = \mathbb{E} \left[ \frac{(Y_i - \mu(1, D, X))^2 \cdot Z^2}{p(D, X)} + \frac{(Y_i - \mu(0, D, X))^2 \cdot (1 - Z)^2}{1 - p(D, X)} \right] I\{D, X \in L_m\} \\
& \geq \frac{1}{(1 - \epsilon)^2} \mathbb{E} \left[ \left( (Y - \mu(1, D, X))^2 \cdot Z^2 + (Y - \mu(0, D, X))^2 \cdot (1 - Z)^2 \right) I\{D, X \in L_m\} \right] \\
& = \frac{1}{(1 - \epsilon)^2} \mathbb{E} \left[ \epsilon^2 I\{D, X \in L_m\} \right] \\
& \geq \frac{c}{(1 - \epsilon)^2}.
\end{align*}
\]

Further, for \( m \neq n \), it holds

\[
\mathbb{E}[\phi_m(W, \theta_0, \eta_0)\phi_n(W, \theta_0, \eta_0)] = 0
\]

since \( I\{D_i, X_i \in L_m\} \cdot I\{D_i, X_i \in L_n\} = 0 \) almost sure and \( \theta_n = \theta_m = 0 \) under \( H_0 \). Thus, all eigenvalues of \( \Sigma^2 \) are bounded from below under \( H_0 \). Next, we show Assumption 3.2 a)-c) to complete the proof.

We define the following nuisance realization set \( T_n \) as the set of all \( P \)-square-integrable functions \( \eta = (\eta_1, \eta_2) = (\mu, p) \) such that

\[
\max(\|\eta_0,1 - \eta_1\|_{P,2}, \|\eta_0,2 - \eta_2\|_{P,2}) \leq C \\
\max(\|\eta_0,1 - \eta_1\|_{P,q}, \|\eta_0,2 - \eta_2\|_{P,q}) \leq \delta_N \\
\|\eta_2 - 1/2\|_{P,\infty} \leq 1/2 - \epsilon \\
\|\eta_0,1 - \eta_1\|_{P,2} \times \|\eta_0,2 - \eta_2\|_{P,2} \leq \delta_N N^{-1/2}
\]

for \( \delta_N = o(1) \) and a constant \( q > 2 \). Note that Assumption 3.2(a) holds by construction of the set \( T_N \) and Assumption 9. Next, we verify Assumption 3.2(b). First, we note that

\[
(\|\mu_0(0, D, X)\|_{P,q} \vee \|\mu_0(0, D, X)\|_{P,q}) \leq \|\mu_0(Z, D, X)\|_{P,q}/\epsilon^{1/q} \leq \|Y\|_{P,q}/\epsilon^{1/q} \leq C/\epsilon^{1/q},
\]

due to Assumption 9 i) where we use that \( P(Z = 1|D, X) = p_0(D, X) \geq \epsilon \) and \( P(Z = 0|D, X) = 1 - p_0(D, X) \geq \epsilon \). Similarly, for any \( \mu \in T_n \), it holds

\[
\|\mu(1, D, X) - \mu_0(1, D, X)\|_{P,q} \leq C/\epsilon^{1/q}, \quad \|\mu(0, D, X) - \mu_0(0, D, X)\|_{P,q} \leq C/\epsilon^{1/q}
\]
and

\[ |\theta_m| \leq |E[(\mu_0(1, D, X) - \mu_0(0, D, X))]| \leq 2C/\epsilon^{1/q}. \]

Therefore, for any \( \eta \in \mathcal{T}_n \), it holds

\[
E[\|\phi_M(W, \theta_0, \eta_0)\|^{q}]^{1/q} \\
\leq \sqrt{M}E \left[ \left( \sup_{m=1, \ldots, M} \phi_m(W, \theta_0, \eta_0) \right)^q \right]^{1/q} \\
\leq \sqrt{M} \left( (1 + \epsilon^{-1}) (\|\mu(1, D, X)\|_{P,q} + \|\mu(0, D, X)\|_{P,q}) + \|Y\|_{P,q}/\epsilon + 2C/\epsilon^{1/q} \right) \\
\leq \sqrt{M} \left( 4(1 + \epsilon^{-1})/\epsilon^{1/q} + 2C/\epsilon + 2C/\epsilon^{1/q} \right).
\]

Also, we have

\[
\sup_{\eta \in \mathcal{T}_N} E[\|\phi_m^a(W, \theta_0, \eta)\|^{q}]^{1/q} = 1.
\]

Finally, we verify Assumption 3.2 c). It holds

\[
\sup_{\eta \in \mathcal{T}_N} |E[\phi_m^a(W, \theta_0, \eta) - \phi_m^a(W, \theta_0, \eta_0)]| = |(-1) - (-1)| = 0.
\]

Analogously to the verification of Assumption 3.2 b) following the proof of Theorem 5.1 in Chernozhukov et al. (2018b), we conclude

\[
\sup_{\eta \in \mathcal{T}_N} E[\|\phi_M(W, \theta_0, \eta) - \phi_M(W, \theta_0, \eta_0)\|^2]^{1/2} \\
\leq \sqrt{M} \sup_{\eta \in \mathcal{T}_N} E \left[ \sup_{m=1, \ldots, M} (\phi_m(W, \theta_0, \eta) - \phi_m(W, \theta_0, \eta_0))^2 \right]^{1/2} \\
\leq \sqrt{M} (I_1 + I_{II} + I_{III})
\]

with

\[
I_1 := \|\mu(1, D, X) - \mu_0(1, D, X)\|_{P,q} + \|\mu(0, D, X) - \mu_0(0, D, X)\|_{P,2} \leq 2\delta_n/\epsilon^{1/2};
\]

\[
I_{II} := \left\| \frac{Y - \mu(1, D, X)}{p(D, X)} \cdot Z - \frac{Y - \mu_0(1, D, X)}{p_0(D, X)} \cdot Z \right\|_{P,2} \leq \epsilon^{-2} \left( \epsilon^{-1/2} + \sqrt{C} \right) 2\delta_n
\]

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\[ I_{III} := \left\| \frac{(Y - \mu(0, D, X)) \cdot (1 - Z)}{1 - p(D, X)} - \frac{(Y - \mu_0(0, D, X)) \cdot (1 - Z)}{1 - p_0(D, X)} \right\|_{\mathcal{P}, 2} \leq \epsilon^{-2} \left( \epsilon^{-1/2} + \sqrt{C} \right) 2\delta_n. \]

Finally,
\[
\sup_{\eta \in T_N, r \in (0,1)} \| \partial^2_r \mathbb{E}[\phi_M(W, \theta_0, \eta_0 + r(\eta - \eta_0)) \|
\leq \sqrt{M} \sup_{\eta \in T_N, r \in (0,1), m = 1, \ldots, M} \mathbb{E} \left[ \partial^2_r \phi_m(W, \theta_0, \eta_0 + r(\eta - \eta_0)) \right]
\leq \delta_N N^{-1/2}
\]
completes the proof.

\[\]

### A.4 Proof of Theorem 4

To prove Theorem 4, we apply Theorem 3.1 in Chernozhukov et al. (2018b) (analog to Theorem 5.1 and 5.2). All bounds in the proof hold uniformly over \( P \in \mathcal{P} \) but we omit this qualifier for brevity. We use \( C \) to denote a strictly positive constant that is independent of \( n \) and \( P \in \mathcal{P} \). The value of \( C \) may change at each appearance.

**Proof.** First, we observe that the score in (7.2) is linear

\[ \phi_2(W, \theta, \eta) = \phi^a_2(W, \eta) \theta + \phi^b_2(W, \eta) \]

with \( \phi^a_2(W, \eta) = -1 \) and \( \phi^b_2(W, \eta) = (\eta_1(W) - \eta_2(W))^2 + \zeta \). The score fulfills the moment condition, e.g.

\[ \mathbb{E}[\phi_2(W, \theta_0, \eta_0)] = \mathbb{E}[\mu(1, X, D) - \mu(0, X, D)^2 - \mathbb{E}[\mu(1, X, D) - \mu(0, X, D)^2] + \zeta] = 0 \]

by construction of the score. Under \( H_0 \), it holds

\[ \partial_r \mathbb{E}[\phi_2(W, \theta_0, \eta_0 + r(\eta - \eta_0))]|_{r=0} = 0 \]

and therefore the score fulfills the Neyman orthogonality condition. Also, the identification condition
holds, namely, \( J_0 := \mathbb{E}[\phi_2^2(W, \eta)] = -1 \). This gives us Assumption 3.1 in Chernozhukov et al. (2018b).

Next, we show Assumption 3.2 to complete the proof. We define the following nuisance realization set \( \mathcal{T}_n \) as the set of all P-square-integrable functions \( \eta \) such that

\[
\| \eta_0 - \eta \|_{P,2} \leq C
\]

\[
\| \eta_0 - \eta \|_{P,4} \leq \delta_N
\]

\[
\| \eta_0 - \eta \|_{P,2} \leq \delta_N^{1/2} N^{-1/4}
\]

for \( \delta_N = o(1) \) and a constant \( q > 2 \). Note that Assumption 3.2(a) holds by construction of the set \( \mathcal{T}_n \) and Assumption 10. Next, we verify Assumption 3.2(b). For \( q > 2 \), we have

\[
\sup_{\eta \in \mathcal{T}_n} \mathbb{E}[|\phi_2(\eta, \theta_0, \eta)|^q]^{1/q} = \sup_{\eta \in \mathcal{T}_n} \mathbb{E}\left[\left(\left((\eta(1, X, D) - \eta(0, X, D))^2 - \theta_0 + \nu\right)^q\right)^{1/q}\right]
\]

\[
\leq \sup_{\eta \in \mathcal{T}_n} \| (\eta(1, X, D) - \eta(0, X, D))^2 \|_{P,q} + |\theta_0| + \| \zeta \|_{P,q}
\]

\[
\lesssim \sup_{\eta \in \mathcal{T}_n} \| (\eta(1, X, D) - \eta(0, X, D))^2 \|_{P,q} + C
\]

due to \( \| \zeta \|_{P,q} < C \) with

\[
\sup_{\eta \in \mathcal{T}_n} \| (\eta(1, X, D) - \eta(0, X, D))^2 \|_{P,q}
\]

\[
= \sup_{\eta \in \mathcal{T}_n} \| \eta(1, X, D) - \eta(0, X, D) \|_{P,2q}^2
\]

\[
\leq \sup_{\eta \in \mathcal{T}_n} \| (\eta - \eta_0)(1, X, D) + (\eta_0(1, X, D) - \eta_0(0, X, D)) + (\eta_0 - \eta)(0, X, D) \|_{P,2q}^2
\]

\[
\leq \sup_{\eta \in \mathcal{T}_n} \| (\eta - \eta_0)(1, X, D) \|_{P,2q} + \| \eta_0 - \eta \|_{P,2q} \}
\]

\[
\leq C
\]

by construction of the nuisance realization set. Also, we have

\[
\sup_{\eta \in \mathcal{T}_n} \mathbb{E}[|\phi_2^2(W, \theta_0, \eta)|^q]^{1/q} = 1.
\]

Now, we verify Assumption 3.2(c). It holds

\[
\sup_{\eta \in \mathcal{T}_n} |\mathbb{E}[\phi_2^2(W, \theta_0, \eta) - \phi_2^2(W, \theta_0, \eta_0)]| = |(-1) - (-1)| = 0.
\]
In addition, we have

\[
\sup_{\eta \in T_N} \mathbb{E}[\phi_2(W, \theta_0, \eta) - \phi_2(W, \theta_0, \eta_0)]^2 = \sup_{\eta \in T_N} \mathbb{E}\left[\left(\frac{1}{2}((\eta(1, X, D) - \eta(0, X, D)) - (\mu(1, X, D) - \mu(0, X, D))^2\right)^2\right]^{1/2}
\]

\[
= \sup_{\eta \in T_N} \mathbb{E}\left[\left(\frac{1}{2}((\mu(1, X, D) - \mu(0, X, D)) + (\eta(1, X, D) - \eta(0, X, D))^2\right)^2\right]^{1/2}
\]

\[
\leq \sup_{\eta \in T_N} \mathbb{E}\left[\left(\frac{1}{2}((\mu(1, X, D) - \mu(0, X, D)) + (\eta(1, X, D) - \eta(0, X, D)))\right)^4\right]^{1/4}
\]

\[
\leq C \sup_{\eta \in T_N} (\mathbb{E}[(\mu(1, X, D) - \eta(1, X, D)) + (\eta(0, X, D) - \mu(0, X, D)))^4]^{1/4}
\]

\[
\leq 2C \sup_{\eta \in T_N} \|\eta - \eta_0\|_{p,4} \lesssim \delta_N.
\]

Further, for all \(r \in (0, 1)\), it holds

\[
\sup_{\eta \in T_N} \partial_r^2 \mathbb{E}[\phi_2(W, \theta_0, \eta_0 + r(\eta - \eta_0))]
\]

\[
= \sup_{\eta \in T_N} \mathbb{E}\left[\partial_r^2 \phi_2(W, \theta_0, \eta_0 + r(\eta - \eta_0))\right]
\]

\[
= \sup_{\eta \in T_N} \mathbb{E}\left[\partial_r^2(\mu(1, X, D) + r(\eta(1, X, D) - \mu(1, X, D)) - (\mu(0, X, D) + r(\eta(0, X, D) - \mu(0, X, D)))^2\right]
\]

\[
= \sup_{\eta \in T_N} \mathbb{E}\left[\partial_r^2(\mu(1, X, D) + r(\eta(1, X, D) - \mu(1, X, D)) - (\mu(0, X, D) + r(\eta(0, X, D) - \mu(0, X, D)))\right]
\]

\[
\leq C \sup_{\eta \in T_N} \|\eta - \eta_0\|_{p,2}^2
\]

\[
\leq C(\delta_N^2 N^{-1/4})^2 = C\delta_N N^{-1/2}.
\]

Finally, it is easy to show that the variance of the score \(\phi_2\) is non-degenerate, namely

\[
\mathbb{E}[\phi_2(W, \theta_0, \eta_0)]^2 = \mathbb{E}[((\mu(1, X, D) - \mu(0, X, D)) - \theta_0 + \nu)^2]
\]

\[
= \mathbb{E}[(\mu(1, X, D) - \mu(0, X, D)) - \theta_0 + \nu]^2 + \mathbb{E}[\nu^2]
\]

\[
\geq \mathbb{E}[\nu^2] = \sigma^2_\nu
\]

since the variance \(\sigma^2_\nu\) of \(\nu\) is chosen to bounded from below. This completes the proof. \(\square\)
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