ASSESSING INFERENCE METHODS∗

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Abstract

We analyze different types of simulations that applied researchers may use to assess their inference methods. We show that different types of simulations vary in many dimensions when considered as inference assessments. Moreover, we show that natural ways of running simulations may lead to misleading conclusions, and we propose alternatives. We then provide evidence that even some simple assessments can detect problems in many different settings. Alternative assessments that potentially better approximate the true data generating process may detect problems that simpler assessments would not detect. However, they are not uniformly dominant in this dimension, and may imply some costs.

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1 Introduction

The credibility of scientific research depends crucially on the control of false-positive results. However, we may have an excess of false-positive results if inference is based on incorrect or unreliable methods. This may happen when (i) inference is based on methods that rely on unrealistic assumptions, (ii) inference is based on asymptotic theory when such asymptotic approximations are poor, and/or (iii) the inference method is invalid even asymptotically.

We analyze different types of assessments based on simulations that applied researchers may use to evaluate whether inference methods in their empirical applications are reliable. While the idea of using simulations to evaluate statistical methods is not new,\(^1\) we show that the use of simulations by applied researchers to assess inference methods in their empirical applications involve many subtle issues. We show that different assessments present advantages and disadvantages, varying in the set of problems they may detect, finite-sample performance, possibility of sequential-testing distortions, opportunities for cherry picking simulations, and implementation complexity. Moreover, we show that some commonly used ways of running simulations may lead to misleading conclusions, and propose alternatives to overcome these problems. We also show that analyzing simulated rejection rates for a single significance level may lead misleading conclusions in some settings.

We then provide evidence from a series of empirical applications that even some simple assessments, such as simulations replacing the outcome variable with iid normals, would be sufficient to detect a number of settings in which inference may be unreliable. While of course such approach has some limitations, we show that it is not as limited as it might appear at first sight. Therefore, the widespread use of even such low-cost procedure by applied researchers has the potential of making scientific evidence more reliable.

We also consider the use of alternative assessments that potentially better approximate

\(^1\)For example, the idea of relying on simulation studies tailored to the features of the data at hand has been proposed by Athey et al. (2020) and Blair et al. (2019) to select among alternative estimators and to diagnose research designs.
the true data generating process (DGP). We show that these assessments may detect problems that simpler assessments would not detect. However, applied researchers should consider such alternatives with caution, as they may not perform well in some settings, and may imply some costs, ranging from larger implementation costs to sequential-testing distortions.

As byproducts from our empirical illustrations, we provide contributions to the burgeoning literature on inference in shift-share designs. We show that a standard way of using simulations to assess whether spatial correlation is a problem in shift-share applications may be misleading, and present alternatives to assess the reliability of different inference methods in this setting. We also present some settings in which inference may be unreliable that have not received much attention in the literature, and that applied researchers would usually not suspect that there might be problems.\footnote{For example, when considering weighted OLS regressions, and when considering studentized block bootstraps.}

## 2 Assessing inference methods

We present the main ideas considering the OLS estimator, but the assessments we present are applicable to a wider range of applications. Let

\[ y_i = x_i' \beta + \epsilon_i , \]  

where \( y_i \) is an outcome, \( x_i \) is an \( K \times 1 \) vector of covariates, and \( \beta \) is the parameter of interest. We observe \( \{y_i, x_i\}_{i=1}^N \). Let \( y = [y_1 \ldots y_N]' \), \( X = [x_1 \ldots x_N]' \), \( \epsilon = [\epsilon_1 \ldots \epsilon_N]' \), and \( F(\epsilon) \) be the CDF of \( \epsilon \). We want to test the null hypothesis \( R\beta = q \), for a \( J \times K \) matrix \( R \) and a \( J \times 1 \) vector \( q \).

If we consider a sampling framework with \( X \) fixed and redraw \( \epsilon \), then the OLS estimator for \( \beta \) is unbiased if we assume \( E[\epsilon | X] = 0 \). Moreover, finite-sample inference is possible under strong assumptions on \( \epsilon \), such as normality, homoskedasticity, and non-autocorrelation.
Relaxing those assumptions, however, generally entails difficulties for inference in finite samples. An often-used alternative is to rely on asymptotic theory (Eicker, 1967; Huber, 1967; White, 1980; Liang and Zeger, 1986; Newey and West, 1987; Conley, 1999).

Even considering such alternatives, we may still have relevant inference distortions. First, the inference method may still rely on unreasonable assumptions. For example, Bertrand et al. (2004) show that heteroskedasticity-robust standard errors (henceforth, EHW) may lead to over-rejection in difference-in-differences (DID) due to serial correlation. Second, asymptotic theories may provide poor approximations, as documented by Young (2018). Finally, it may be that applied researchers consider inference methods that are invalid even asymptotically.

We consider different types of assessments based on simulations that applied researchers may use to evaluate whether inference methods are reliable in their applications. The idea is to consider a DGP based on their application, and analyze in simulations the rejection (or coverage) rates using the inference method being assessed.

The first step for applied researchers in choosing an assessment based on simulations is to define the source of uncertainty in their application, and what will be treated as fixed/stochastic in the simulations. We divide the different assessments in three categories: (i) assessments with fixed X; (ii) design-based assessments (X is stochastic/potential outcomes fixed); and (iii) assessments with both X and y stochastic. In Section 2.4.1, we discuss how applied researchers may decide on what is fixed/stochastic when constructing an assessment.

2.1 Assessments with fixed X

The idea in this case is to choose $\tilde{\beta}$ that satisfies the null hypothesis, and a distribution for the error $\tilde{F}(\epsilon)$. Then we simulate $B$ new datasets $y^b = X\tilde{\beta} + \epsilon^b$, where $\epsilon^b$ is drawn from $\tilde{F}(\epsilon)$, and for each draw we test the null using the inference method being assessed. The assessment is the proportion of times we reject the null, for a significance level $\alpha$, in these
simulations. We discuss below different alternatives for $\tilde{F}(\epsilon)$.

The data from these simulations is generated by a DGP such that the null is valid, and that has the same empirical design ($N$, $X$, sampling weights, and so on) as the empirical application, except potentially for the distribution of $\epsilon$. By construction, when $B \to \infty$, the assessment converges in probability to the rejection rate of this test, conditional on the empirical design, but given the distribution of the errors considered in the simulations. Since the null is satisfied, and considering $\tilde{F}(\epsilon)$ that satisfies the assumptions for asymptotic validity of the inference method, we should expect a rejection rate close to $\alpha$ for an $\alpha$-level test, if the test is asymptotically valid and such asymptotic theory provides a good approximation given the empirical design. Therefore, this assessment is informative about whether inference conditional on $X$ is reliable, given the design of the empirical application.

When we consider linear models and we are testing null hypotheses regarding linear combinations of $\beta$, the estimator considered in a given draw in the simulations ($\tilde{\beta}^b$) is such that $R\tilde{\beta}^b - q = R(X'X)^{-1}X'\epsilon^b$, while the residuals are given by $\hat{\epsilon}^b = (I - X(X'X)^{-1}X')\epsilon^b$. Therefore, $R\tilde{\beta}^b - q$ and $\hat{\epsilon}^b$ are invariant to the choice of $\tilde{\beta}$ (provided $R\tilde{\beta} = q$), and the relative magnitude between $\hat{\beta}^b$ and $\hat{\epsilon}^b$ is invariant to the scale of $\epsilon^b$. Therefore, in most cases, the assessment will be numerically invariant to the choice of $\tilde{\beta}$ and to the scale of the distribution of the errors. However, other variations in the distribution of the errors (such as heteroskedasticity, serial/spatial correlation, and heavier tails) might lead to different assessments.

We consider different alternatives for $\tilde{F}(\epsilon)$, varying crucially on whether they depend on the realization of the original errors.

2.1.1 Assessment resampling iid normal variables

We start with a simple assessment in which $\tilde{F}(\epsilon)$ is iid standard normal. As explained above, if we test the null that a specific coefficient equals zero in a linear model, we can consider $\tilde{\beta} = 0$. So the assessment can be computed by simply considering simulations
replacing the outcome variable in the dataset with iid standard normals, making it extremely easy to implement. There is no gain in correctly specifying the scale of the errors and how other covariates correlate with the outcome.

While this approach has some limitations, we show that it is not as limited as it might appear at first glance. The use of iid normal errors does not necessarily mean that we believe errors are normal or independent in specific applications. Likewise, the fact that we consider a distribution for $\epsilon$ that has conditional mean zero does not mean that we believe the conditional expectation function is linear. The idea is to consider a “favorable” setting, in the sense that OLS is the best linear unbiased estimator, and errors are normally distributed. So an applied researcher can assess whether, at least for such DGP, the inference method in his/her empirical application has good properties. If this assessment suggests relevant distortions, then we would know that the worst-case behavior of the inference method would be at least as bad as that, if we consider a class of distributions that include simple iid normal errors. This should at least lead applied researchers to consider this inference method with caution.

Moreover, there are instances in which applied researchers rely on standard errors that are invalid even asymptotically. For example, due to incorrect calculation of the degrees-of-freedom corrections, or from incorrect implementation of a bootstrap method (examples in Section 3.3). In such cases, the inference method is invalid for any distribution of the errors, including the case of iid normals. Therefore, such a simple assessment would be sufficient to detect such problems, without the need of specifying more complex DPGs.

Also, considering errors independent across observations in more complex settings, such as when assessing inference based on cluster robust variance estimator (CRVE), is not as limiting as it may appear at first glance. While CRVE relaxes the assumption of independence within cluster, it remains asymptotically valid when the number of clusters increases, even if errors are independent within clusters. Therefore, considering iid errors still provides a first screening for these inference methods, while avoiding more complex specifications on
the errors. The empirical application in Section 3.1 illustrates that.

In addition to being easy to implement, considering such a simple assessment presents other advantages. First, it requires minimal modifications to a wide range of applications. Also, since it does not depend on tuning parameters/implementation decisions, applied researchers would have less room to cherry pick simulations in which their inference methods look good. Moreover, this assessment does not depend on the realized $\epsilon$. Therefore, the true size of the test (conditional on $X$) would be the same regardless of whether we condition on a good assessment, so this procedure is immune to sequential-testing problems.

As we present in Section 3, even this simple assessment would be sufficient to detect that inference may be unreliable in a wide range of settings in which applied researchers may not suspect so. However, an important limitation is that it would not approximate $F(\epsilon)$ even when sample size increases. Therefore, it may fail to detect problems in case, for example, errors are not normal. Likewise, if we are assessing whether inference based on CRVE is reliable, it would not detect problems in case errors are correlated across clusters.

### 2.1.2 Estimating $F(\epsilon)$

Another alternative is to estimate $F(\epsilon)$ using the data. This way, it may be possible to take into account more complex features of the distribution of $\epsilon$, such as heteroskedasticity, serial/spatial correlations, and heavier tails. We discuss in Appendix A.1 different alternatives for $\tilde{F}(\epsilon)$, such as: resampling with replacement from the residuals, sign-changes on the residuals, parametric choices (for example, with estimated heteroskedasticity or spatial correlation), and non-parametric estimators for $F(\epsilon)$ (such as the Wasserstein Generative Adversarial Networks - WGAN). The idea is to simulate new datasets considering $\tilde{F}(\epsilon)$ as the distribution for $\epsilon$, and in each simulation we test the null using the inference method that is being assessed.

The main potential advantage of considering such alternatives is that we may better approximate the true DGP. This may provide assessments that are closer to the real rejection
rates, and may allow us to detect problems that an assessment based on, for example, iid
normals would not detect. However, there are some potential limitations/disadvantages in
considering such alternatives.

One potential limitation is that, while some of these approaches have good properties in
approximating the true DGP asymptotically, they may not perform well in finite samples.
We show an extreme example in Section 3.1 in which some of these assessments would provide
unreasonable DGPs for $\epsilon$, and this would prevent us from detecting relevant size distortions.
In contrast, an assessment based on iid normals in that setting (which, admittedly, does
not approximate the true DGP even asymptotically) would provide simulations based on a
more reasonable DGP, and would indicate relevant inference distortions. This is not to say
that those approaches to estimate $F(\epsilon)$ should not be used. Rather, we point out that they
should be considered with caution.

Also, relying on data to estimate $F(\epsilon)$ leads to the possibility of sequential-testing distor-
tions. If researchers rely on an inference method depending on the result of an assessment, we
should consider the properties of the estimator/inference method conditional on a satisfac-
tory assessment. If $\tilde{F}(\epsilon)$ depends on the realization of $\epsilon$, then conditioning on a satisfactory
assessment may induce bias and exacerbate over-rejection. See examples in Appendix A.1.

Some types of simulations may also require a number of implementation decisions and
tuning parameters. This is the case, for example, when we estimate $F(\epsilon)$ using a WGAN.
Since different implementations may lead to different assessments, this may open the possi-
bility for applied researchers to cherry pick among different assessments the ones that suggest
their inference methods are reliable. Finally, some of those assessments might involve non-
trivial implementation costs to applied researchers.

Overall, all else equal, having simulations that better approximate the true DGP of the
empirical setting should be preferred. However, constructing a DGP that aims to approxi-
mate the true DGP may come at some costs. Therefore, applied researchers should weight

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$^3$A series of papers analyze the implications of pre-testing on subsequent testing in different settings. A
non-exhaustive list of examples include Andrews (2018), Guggenberger (2010), and Roth (2019).
these cost and benefits to decide on which assessment(s) to run.

2.2 Design-based assessments

Another possibility is to consider a design-based approach for inference, where we treat potential outcomes as fixed, and uncertainty comes from the allocation of $X$ instead of, or in addition to, sampling-based uncertainty (Abadie et al., 2020, 2017).

If the distribution of $X$ is known (for example, in a randomized control trial - RCT), a natural way to construct an assessment is to hold $y$ fixed and consider different allocations of $X$. Then, for each allocation of $X$ we estimate the parameter of interest and test the null using the inference method being assessed. For example, this type of simulations has been considered by Adão et al. (2019) (AKM) to show that inference based on CRVE leads to over-rejection in shift-share design applications, and by Bertrand et al. (2004) to illustrate that inference in DID using EHW standard errors may be unreliable if errors are serially correlated. Chaisemartin and Ramirez-Cuellar (2019) also used this type of simulations to illustrate a problem with CRVE in stratified experiments.\footnote{They find in a survey of published papers many cases of inference problems, which suggests that the use of such simulations as an inference assessment is not widespread among applied researchers, even when we consider RCTs.}

We show that, even when the distribution of $X$ is known, this procedure does not generally recover the true size of the test. Consider an RCT in which the sample average treatment effect (ATE) is the estimand of interest, and let $Y_i(0)$ and $Y_i(1)$ be the potential outcomes. This procedure would recover the true size of the test (conditional on the potential outcomes of the sample), if $Y_i(0) = Y_i(1)$ for all $i$. However, if we have that $Y_i(1) \neq Y_i(0)$, then this assessment would consider a DGP in which potential outcomes $\{\tilde{Y}_i(0), \tilde{Y}_i(1)\}_{i=1}^N$ are given by $\tilde{Y}_i(1) = \tilde{Y}_i(0) = Y_i$, which would differ from the true DGP.

This can be particularly problematic if applied researchers consider this type of simulations to assess whether serial/spatial correlation is a problem in their applications. For example, if they consider the type of simulations presented by AKM to check whether inferen-
ence based on CRVE is reliable in their shift-share design applications, or if they consider simulations like the ones from Bertrand et al. (2004) to check whether serial correlation is a problem in their DID applications.

The problem with those simulations is that a true treatment effect would be confounded with spatial/serial correlation. Therefore, we may conclude that spatial/serial correlation is a problem even when it is not. We formalize this idea in Appendix A.2.1, and present evidence on this problem for shift-share designs in Section 3.2.

We consider two alternatives to circumvent this problem. First, if available, one could run design-based simulations on a placebo outcome that should exhibit similar spatial/serial correlation as the outcome of interest, but that was not affected by the treatment. For example, consider the China shock, analyzed by Autor et al. (2013). In this case, it would be possible to run simulations using a pre-shock outcome (so this placebo outcome would be $Y_i^P = Y_i^P(0)$ for all $i$). In this case, an assessment close to $\alpha$ would indicate that the inference method is reliable, at least when we consider a sharp null of no effect whatsoever. However, this assessment would not be informative about the possibility of heterogeneous treatment effects.\footnote{See AKM for the possibility of over-rejection due to heterogeneous treatment effects in shift-share designs.}

A second alternative is to consider a modified design-based assessment. Instead of considering potential outcomes $\{\widetilde{Y}_i(0), \widetilde{Y}_i(1)\}_{i=1}^{N}$, we consider $\{\tilde{Y}_i(0), \tilde{Y}_i(1)\}_{i=1}^{N}$, with $\tilde{Y}_i(0) = \tilde{Y}_i(1) = Y_i - \hat{\beta}T_i$, where $\hat{\beta}$ is the estimator for $\beta$.\footnote{We call this a “modified” design-based assessment because the papers cited above that considered these approaches to assess inference methods held $Y$ constant while varying $X$. An exception is Borusyak et al. (2021), who consider in their Appendix A.11 simulations holding the residuals fixed. We differ in that we provide a comparison between these two different approaches for design-based simulations (holding $Y$ versus holding residuals fixed). Moreover, we analyze in detail in Section 3.2 the use of these different simulations to assess the use of CRVE in shift-share applications, which is a case in which the different approaches can make substantial difference.} If we have that $Y_i(1) = Y_i(0) + \beta$, and $\hat{\beta}$ is a consistent estimator for $\beta$, then this modified design-based assessment would approximate the true size of the test when $N$ is large. This way we would have simulations with a structure for the potential outcomes when untreated that is more similar to the true structure of the potential outcomes in the application.
We present in Appendix A.2.2 simulations on the use of this kind of assessments to detect the possibility of spatial correlation. These simulations confirm that a design-based assessment holding \( y \) fixed may incorrectly indicate spatial correlation problems, if there is a true treatment effect. The modified assessment does not present this problem. We also show that these two alternatives may detect spatial correlation problems, even in settings in which placebo regressions are not statistically significant. Finally, we show that the modified assessment may detect problems in case there are heterogeneous treatment effects.

Note that these alternatives would still be unable to detect some potential problems for inference. For example, they would fail to detect problems when \( Y_i(1) \) is sampled from a lognormal distribution, while \( Y_i(0) \) is sampled from a normal distribution. In this case, \( \{\tilde{Y}_i(0), \tilde{Y}_i(1)\}_{i=1}^N \) would not approximate well the structure of potential outcomes \( \{Y_i(0), Y_i(1)\}_{i=1}^N \).

Overall, while the idea of considering this kind of simulations has been previously considered in DID, RCT, and shift-share design settings, we (i) show that this kind of simulations may lead to incorrect conclusions, and (ii) propose alternatives that circumvent (some) of these problems. In Section 3.2 we implement these assessments in shift-share design applications.

### 2.3 Treating \( X \) and \( y \) as stochastic

We may also consider assessments in which both \( X \) and \( y \) are stochastic. We discuss in Appendix A.3 the use of assessments based on a non-parametric bootstrap. While, under some conditions, a non-parametric bootstrap has the advantage of asymptotically recovering the joint distribution \( F_{X,Y}(x,y) \), it would be subject to some of the potential limitations discussed in Section 2.1.2.
2.4 Implementation decisions

2.4.1 What should be treated as fixed/stochastic?

In many applications, the decision on what is treated as stochastic is essentially linked to the target parameter. For example, consider an RCT. When uncertainty comes only from treatment assignment (holding potential outcomes fixed), we are implicitly defining the target parameter as the ATE for the sample. In contrast, when potential outcomes are also treated as stochastic, the target parameter becomes the ATE for a larger population or superpopulation from which the sample is drawn (Abadie et al., 2020; Miratrix et al., 2021).\footnote{See also Deeb and de Chaisemartin (2019) for a discussion different target parameters.}

In our view, it is up to the applied researchers to define what is the target parameter in their applications, and we do not intend to provide a definite answer to that in this paper.

Moreover, in some settings, we may have that the theory behind the inference method being assessed indicates what should be treated as stochastic in our simulations. For example, AKM and Borusyak et al. (2021) (BHJ) derive the validity of their inference methods for shift-share designs considering a setting in which shocks are stochastic, while potential outcomes and shares are fixed. Therefore, it is reasonable to consider in such settings assessments in which shocks are stochastic.

We also note that assessments conditional on either $X$ or $y$ might also be informative about whether an inference method is reliable for unconditional inference. For example, suppose an assessment conditional on $X$ suggests large over-rejection. In this case, we would generally need under-rejection conditional on other values for $X$, or that the realized $X$ is a very low probability event, so that, unconditionally, the test size is close to $\alpha$. Therefore, if a conditional assessment suggests relevant distortions, this should at least raise a red flag that there might be relevant distortions when we consider unconditional inference as well. Likewise, we also see a design-based assessment (conditional on potential outcomes) as informative about the reliability of inference methods when we consider unconditional inference. We discuss that further in the simulations presented in Appendix A.2.2.
We also discuss in Appendix A.4 two settings in which conditional assessments would (correctly) suggest distortions for conditional inference, but the inference method is reliable unconditionally. In our view, conditional assessments suggesting relevant distortions in these cases should at least lead researchers to think more carefully about whether conditional or unconditional inference is more appropriate (even if we believe the test is reliable unconditionally). While we do not intend to provide a definite answer on whether inference should be conditional or unconditional, we see good arguments to consider that conditional inference would be appropriate in those settings.

2.4.2 Which \( \alpha \) to choose?

Another issue is about which significance level to choose in the simulations. While in most cases assessments would lead to similar conclusion for any \( \alpha \) (for example, 1%, 5%, or 10%), there are some cases in which the choice of \( \alpha \) may lead to different conclusions.

Suppose we want to assess an inference method with standard errors that impose the null hypothesis to estimate the residuals, and let the null be \( \beta = 0 \). In this case, the residuals would tend to be larger when \( |\hat{\beta}| \) is larger, creating a positive correlation between the estimator for the standard errors and \( |\hat{\beta}| \). This creates downward bias in the rejection rates, that is stronger when \( \alpha \) is smaller. Therefore, we may have situations in which an assessment is close to \( \alpha \) (or even smaller) when, for example, \( \alpha = 5\% \), but it indicates large over-rejection when \( \alpha = 10\% \). We present in Appendix A.5 an empirical illustration of that.

Therefore, we recommend that applied researchers construct assessments using different significance levels, particularly when assessing inference methods that impose the null. It is also possible to plot the CDF of p-values in the simulations, which should be close to Uniform\([0, 1]\) if the inference method is reliable.

This is important not only for the use of simulations as inference assessments, but also for Monte Carlo simulations in general.
3 Applications

3.1 Difference-in-Differences with Few Treated Clusters

As a first empirical illustration, we consider the Massachusetts 2006 health care reform, which was analyzed in a series of papers using DID designs (Sommers et al., 2014; Miller, 2012; Niu, 2014; Courtemanche and Zapata, 2014; Kolstad and Kowalski, 2012). We focus on Sommers et al. (2014), who compared 14 Massachusetts counties with 513 control counties. They reported CRVE at the state level. Their inference procedures were then revisited by Kaestner (2016) and Ferman (2020).

We can think about uncertainty in this setting coming from potentially different realizations of unobserved variables, conditional on the fact that Massachusetts implemented this reform in 2006. In this case, the error term in the DID model would reflect, for example, county × time specific weather or economics unobserved shocks.

We start assessing whether CRVE at the state level — which was used in the papers cited above — is reliable in this scenario. An assessment using iid normal errors in Sommers et al. (2014) would indicate a rejection rate of 63% for a 5% nominal-level test. Therefore, this simple assessment, without specifying more complex features of the errors, such as serial/across-county correlations, would have provided an immediate conclusion that CRVE should be considered with caution. We explain why CRVE works poorly in this particular setting in Appendix A.8. Kolstad and Kowalski (2012) also consider block bootstrap at the state level. However, in settings with few treated clusters, this bootstrap method also leads to substantial over-rejection, and this simple assessment would detect that.

As discussed in Section 2.1, the use of an assessment with iid normals does not mean that we believe errors are normal and serially/spatially uncorrelated in this application. Still, the performance of CRVE when we consider this simple DGP should indicate that it may also not work properly in potentially more complex settings. While the true over-rejection may not be as extreme as this simple assessment suggests, we would need unreasonable distributions
for the errors so that this inference method does not lead to large over-rejections (details in Appendix A.8).

Interestingly, some of the assessments discussed in Appendix A.1, in which we use the data to construct $\tilde{F}(\epsilon)$, would be close to 5%, failing to detect problems in this application. This would be the case if we consider an assessment based on sign-changes of residuals at the state level, or based on a non-parametric block bootstrap. The problem is that, while those assessments have good large-sample properties, they may recover unreasonable DGP’s in finite samples. In this example, they would consider a DGP in which the variance of the errors of the treated state is zero. While the assessment based on iid normals is limited in many dimensions, it would be based on a more reasonable DGP, at least in the sense of considering a distribution in which the variance of the treated is not zero.

This example illustrates that scientific evidence on important topics can be based on potentially misleading inference methods, even after peer-review processes. Moreover, the timing of these publications reveals a lag from the time in which inference problems are uncovered, and the widespread knowledge of these conclusions to applied researchers, editors, and referees. The problem in considering CRVE in DID applications with few treated clusters was discussed at least since Conley and Taber (2011) (first NBER version from 2005). This highlights that simple assessments may detect problems even before econometrics papers are written uncovering such problems. Moreover, they may remain relevant even after such papers have been published.

Given the conclusion that CRVE at the state level is unreliable, researchers should consider alternatives that do not rely on a large number of treated states. For example, clustering at a finer level, Conley and Taber (2011), or Ferman and Pinto (2019). These alternatives, however, generally rely on stronger assumptions on the errors (for example, absence of state-level shocks and/or restrictions on the heteroskedascitiy), and an assessment based on iid normals would not detect problems in these dimensions. In such cases, applied researchers

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\(^8\)Hagemann (2020) consider an heteroskedastic-robust alternative. The main trade-off is that it may have lower power relative to other alternatives.
should argue and provide evidence that these assumptions are reasonable in their applications (for example, this may be from other types of assessments that could detect violations of these assumptions).

3.2 Shift-share designs

We also consider the case of shift-share designs. Those are regression specifications in which one studies the impact of a set of shocks on units differentially exposed to them. In this case, the shift-share variable is given by

$$x_i = \sum_{f=1}^{F} w_{if} x_f,$$

where the shares $w_{if}$ reflect how shock $x_f$ affects unit $i$.

AKM show that inference based on EHW/CRVE can lead to over-rejection if units with similar shares have correlated errors, and/or when treatment effects are heterogeneous. AKM and BHJ propose interesting alternatives, which are asymptotically valid when the number of sectors goes to infinity, and the size of each sector becomes asymptotically negligible.

AKM and BHJ derive the validity of their inference methods in a design-based setting, in which shocks are stochastic, while potential outcomes and shares are fixed. Therefore, we focus on assessments with random draws of shocks. We consider three empirical applications, based on Autor et al. (2013), Dix-Carneiro et al. (2018), and Acemoglu and Restrepo (2020).

3.2.1 Assessing CRVE in shift-share design applications

We consider first the use of CRVE. There are two reasons why CRVE might be problematic in this setting: (i) if there is spatial correlation, and (ii) if there are few clusters. In Appendix A.7.1, we show evidence that CRVE is relatively unreliable for the weighted OLS specifications in these applications, even in the absence of spatial correlation. In this section,

\footnote{The fact that AKM and BHJ developed their theory in this framework does not necessarily mean that the focus in shift-share designs should be on conditional inference. As AKM explain page 1952, the idea of conditioning on potential outcomes is so that they “can allow for any correlation structure of the regression residuals across regions.” As discussed in Section 2.4.1, a design-based assessment in which only shocks are stochastic may be informative not only about conditional inference, but also about unconditional inference (in which potential outcomes are also stochastic).}
we consider unweighted specifications, since the focus is on understanding how different assessments may be used to detect spatial correlation problems. We emphasize, though, that these assessments would be able to detect both of these problems.

We consider first a design-based assessment in which $y$ is fixed and shocks are stochastic. This is exactly what AKM considered to assess the possibility of spatial correlation in shift-share designs. We find large rejection rates in columns 1, 3 and 5 of Table 1, ranging from 34% to 70%. However, as discussed in Section 2.2, we do not take that as evidence that CRVE leads to such substantial size distortions in these applications, since a true treatment effect of the shift-share variable may be confounded with spatially-correlated errors in these simulations. Therefore, we may find large assessments, even when errors are not spatially correlated.

We consider the two alternatives discussed in Section 2.2 to properly assess whether spatial correlation is a problem for CRVE in these applications. All results are presented in Table 1. For the application from Autor et al. (2013), we reinforce the conclusions from AKM that spatial correlation leads to relevant over-rejection due to spatial correlation in this application, although we find evidence that their assessments indeed over-state the magnitude of the problem (consistent with the discussion from Section 2.2).

For Acemoglu and Restrepo (2020), the modified design-based assessment remains large, but the design-based assessment using a pre-treatment (placebo) outcome is close to 5%. This provides evidence that inference based on CRVE might be reasonable for testing a sharp null of no effect whatsoever, but that we may have relevant heterogeneous treatment effects. As we show in Appendix A.6, the assessment using the pre-treatment outcome would have a relatively high probability of flagging a problem in the presence of spatial correlation.

Finally, for Dix-Carneiro et al. (2018), both alternatives lead to assessments smaller than 5%, providing some indication that inference based on CRVE is reasonable in this application. An important caveat for this application, however, is that those assessments would have a

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10 For the application from Autor et al. (2013), we use the specification considered by AKM in their Section II, so our results are comparable to theirs.
relatively lower probability of flagging a problem in case there is relevant spatial correlation. For example, we show in Appendix A.6 that, if there is spatial correlation leading to a 15% test size when relying on CRVE, then an assessment using a pre-treatment outcome would have only 35% probability of flagging a problem. One alternative in cases like that may be to run assessments for a number of pre-treatment outcomes, if available.

3.2.2 Assessing new inference methods for shift-share designs

We assess the inference methods proposed by AKM and BHJ. For Autor et al. (2013), we find evidence that these inference methods work well in this application, which is consistent with the conclusions from AKM. However, for the other two applications the assessments suggest that these inference methods can lead to large distortions, with rejection rates up to 57% (Table 1, Panel C). This is consistent with these applications having a smaller number of sectors.

3.2.3 Choosing among inference methods in shift-share designs

These results illustrate that it is not trivial to determine which inference methods are reliable in shift-share designs. If we have evidence that the methods from AKM/BHJ are reliable, then they should be preferred, as they impose less restrictions on the errors. This is the case for the application from Autor et al. (2013).

In some cases, however, we may have evidence that CRVE is reliable, while the asymptotic approximations for these new inference methods would be problematic. This is the case for the application from Acemoglu and Restrepo (2020) (for testing a sharp null), and for the one from Dix-Carneiro et al. (2018) (with the caveat that the assessments have lower probabilities of detecting problems in this application). Importantly, if applied researchers follow the simulations considered by AKM in their applications to assess whether CRVE would be reliable in their applications (instead of the alternatives we propose), they might incorrectly conclude that CRVE is less reliable than the new inference methods in these
Consistent with the conclusion that CRVE is more reliable than the AKM standard errors in these last two applications, if we consider AKM standard errors, then we would reject the null of no effect for their placebo specifications. In contrast, we do not reject these nulls if we use CRVE (Appendix Table A.4, columns 4 and 6).

Finally, another alternative in this case would be the RI tests proposed by Borusyak and Hull (2020) or Alvarez et al. (2022). However, these tests rely on assumptions on the shock assignment mechanism (such as correct specification of the distribution of shocks or exchangeability) for finite-sample validity.

3.3 Other applications

We consider in Appendix A.7 a series of other applications. We first discuss the large over-rejections when relying on CRVE for the weighted OLS regressions in the empirical applications discussed in Section 3.2, even in the absence of spatial correlation. We show that we may have large size distortions in weighted OLS regressions with 48 or 91 clusters (which is more than the usual rules of thumbs considered by applied researchers), and that even an assessment based on iid normals can detect that.

We also analyze the use of a studentized block-bootstrap, which is one of the recommendations from Bertrand et al. (2004) for inference in DID. In Section 3.1, we saw that this inference method may lead to large over-rejection when there are few treated clusters. In Appendix A.7, we show that we may have over-rejection even when there are many treated and many control clusters, depending on the implementation of the method. Again, an assessment based on iid normals would detect that.

We also discuss two settings in which CRVE leads to asymptotically invalid inference due to incorrect computation of the degrees-of-freedom corrections. The first one is when we include fixed effects at the cluster level. Cameron and Miller (2015) report that CRVE is over-estimated in Stata when we use areg command or when we manually include the fixed
effects dummies. In this case, an assessment based on iid normals would detect rejection rates lower than \( \alpha \), and should lead applied researchers to consider a correct degrees-of-freedom correction. The second case is when we have stratified experiments where, for example, treatment assignment is at the school level, but data is at the student level. Chaisemartin and Ramirez-Cuellar (2019) show that CRVE at the school level leads to over-rejection in this setting.

We note that, in these last three applications, the distortions arise from an incorrect implementation of a method, so we would have size distortions even asymptotically. Therefore, those are settings in which an assessment based on iid normals would detect the problem, and there would not be loss from the fact that this assessment does not approximate the true DGP even asymptotically. Moreover, those are settings in which many applied researchers may fail to suspect that there might be problems for inference, given the large number of observations/clusters.

### 4 Recommendations/concluding remarks

Overall, we show that the use of simulations to assess inference methods involve a number of issues that have not been previously considered in the literature, and provide a mapping on the advantages and disadvantages of different types of assessments. We also show that some natural ways of running simulations may lead to misleading conclusions, and propose alternatives to overcome these problems. Finally, we present empirical evidence on the use of different types of assessments in a number of empirical applications.

In light of our results, we recommend that, whenever it is reasonable to consider that outcomes are stochastic, applied researchers should at least assess their inference methods using a simple assessment, such as the one replacing \( y \) with iid normals. As discussed in Section 2.4.1, this assessment can be informative about the reliability of an inference method not only conditional on \( X \), but also unconditionally. As we show, it works well in detecting
problems in settings in which asymptotic approximations are poor, and when the inference method is invalid even asymptotically. We provide evidence from a series of applications that such assessment would be sufficient to detect problems in a wide variety of settings, including settings in which applied researchers would likely not suspect of problems. At the same time, it is extremely easy to implement, and is immune to potential problems such as sequential-testing distortions. Therefore, its widespread use has large potential gains, at a low cost for applied researchers.

We also show that other types of assessments may improve relative to such simple assessment in terms of detecting a wider range of problems (for example, due to spatial correlation), and in better approximating the true DGP. However, we show that considering these alternatives may come at some costs. While we discuss in general terms potential costs/benefits of different types of assessments, we see the suitability of different types of assessments as an application-specific endeavor. For example, in one of our empirical applications, we analyze in detail how different types of assessments can be used to assess the reliability of different types of inference methods for shift-share designs. We see the analysis of the suitability of different assessments in other specific applications as an interesting avenue for further research. In the meantime, we provide evidence that the widespread use of even a simple assessment replacing $y$ with iid normals can already go a long way in making scientific evidence more reliable in a wide range of settings.
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Table 1: Assessments for shift-share designs

|                        | China shock          | Exposure to robots | Trade liberalization |
|------------------------|----------------------|--------------------|----------------------|
|                        | Main effects         | Placebo            | Main effects         | Placebo            |
| CRVE                   | 0.412                | 0.330              | 0.313                |
| Panel A: design-based assessment for CRVE | 0.237 | 0.059 | 0.001 |
| CRVE                   | 0.317                | 0.256              | 0.012                |
| Panel B: modified design-based assessment for CRVE | 0.226 | 0.049 | 0.000 |
| AKM                    | 0.076                | 0.355              | 0.547                |
| AKM (null imposed)     | 0.098                | 0.296              | 0.220                |
| # of clusters          | 48                   | 48                 | 91                   |
| # of observations      | 772                  | 7422               | 411                  |
| # of sectors           | 395                  | 19                 | 20                   |

Notes: this table presents different types of assessments for the shift-share applications analyzed in Section 3.2. We consider both specifications with the main outcome of interest in the original papers (columns 1, 3, and 5), and in which the outcome variable is a placebo (columns 2, 4, and 6). Panels A and C consider a design-based assessment holding $Y$ constant. In Panel A we assess inference based on CRVE with HC3 correction, while in Panel C we assess the new inference methods proposed by AKM (both the standard version and the version with the null imposed). In Panel B, we consider the modified design-based assessment discussed in Section 3.2 to assess inference based on CRVE with HC3 correction. In all cases, assessments are based on random draws of iid standard normal shocks, where we calculate the rejection rate for a 5%-level test. In column 1, we present results from the specifications considered by AKM in their Section II, which is based on the application from Autor et al. (2013). In column 2, we present the same results, but using a pre-treatment outcome (variation from 1980 to 1990). In columns 3 and 4 we present the assessments for a specification for the main effects and for a placebo specification from Acemoglu and Restrepo (2020). Finally, in columns 5 and 6 we present a specification for the main effects and a placebo specification from Acemoglu and Restrepo (2020). In all cases, we consider unweighted OLS regressions.
A Online Appendix

A.1 Alternative choices for $\tilde{F}(\epsilon)$

We present assessments using different choices for $\tilde{F}(\epsilon)$. In order to illustrate some of the differences between the different choices, we consider a simple setting of an RCT, where potential outcomes are such that $Y(0)$ is standard normal, while $Y(1)$ is log-normal with the mean normalized to zero.\(^{11}\) We want to test whether the average treatment effect (ATE) in a large population is equal to zero. If we have a sample of 20 treated and 180 control observations, then the true size of a 5% nominal-level test based on EHW is around 14%.

The assessment based on iid normals presented in Section 2.1.1 would be around 7%, understating the over-rejection problem. By construction, there is no sequential testing problem when we consider this assessment. We revisit this simple example when we consider alternative assessments, and summarize the results in Appendix Table A.1. All conclusions remain valid if we consider different alternatives for the EHW standard errors such as the ones proposed by MacKinnon and White (1985).

Assessments based on residual bootstrap: following the idea of a residual bootstrap (Efron and Tibshirani, 1994), we can also construct $\tilde{F}(\epsilon)$ by resampling with replacement from the residuals. Note that the idea is not to use the residual bootstrap as an inference method. Rather, we consider these simulations to assess whether another inference method is reliable.

This approach potentially captures, for example, settings in which errors are not normally distributed, so it may better approximate the real DGP relative to an assessment resampling iid normals. However, this potential advantage is an asymptotic property of bootstrap methods, while part of the idea of assessing inference methods is exactly to check whether asymptotic approximations are reliable. Moreover, if we resample at the individual level, the DGP in these simulations considers iid errors, so it would not take heteroskedasticity and serial/spatial correlation into account.\(^{12}\) Finally, different realizations of the errors lead to different $\tilde{F}(\epsilon)$, so this assessment is potentially subject to sequential-testing problems.

To illustrate these issues, consider a simple example in which $y_i$ is iid log-normal, with the mean normalized to zero. If we test the null $E[y_i] = 0$ with a 5% nominal t-test when $N = 20$, then the size of this test is 15%. We consider 5000 draws of $\{y_i\}_{i=1}^{20}$, and for each draw we calculate the assessment based on a residual bootstrap with 1000 simulations. In 52% of the draws of the original sample, the assessment is greater than, for example, 10%.

\(^{11}\) That is, we consider $Y_i(1) = \omega_i - \epsilon \omega^{1/2}$, where $\omega_i \sim \text{lognormal}(0, 1)$.

\(^{12}\) We can consider a residual bootstrap at the cluster level in case clusters are balanced (Cameron et al., 2008).
which would suggest that the inference method may be unreliable. However, conditional on having an assessment lower than 10%, the estimator would cease to be unbiased, and the rejection rate would be 22%, which is higher than the unconditional size of the test. We find similar results if we use alternative thresholds. In contrast, a simple assessment based on iid normals would never detect large distortions in this setting, but it would not lead to additional distortions due to sequential-testing, exposing a trade-off between these alternatives.

In the RCT example above, we find a zero probability (across realizations of the original data) that the assessment is greater than, for example, 10%. Therefore, this assessment would also fail to detect relevant distortions, despite the estimation of $\tilde{F}(\epsilon)$.

**Assessments based on wild bootstrap:** another alternative is to follow the idea of a wild bootstrap, and consider $\tilde{F}(\epsilon)$ in which we multiply the residuals from each observation by +1 or -1 with equal probabilities. This approach potentially takes into account heteroskedasticity (which is not captured by the two assessments considered above), and also settings in which errors are not normally distributed. We may also consider more complex sampling schemes. For example, when assessing CRVE, it may be natural to base the assessment on a wild bootstrap at the cluster level, to take within-cluster correlations into account.

However, these potential advantages are again asymptotic features of such procedure. As we illustrate in Section 3.1, this assessment may provide unreasonable simulations and fail to detect potential problems exactly in settings in which asymptotic approximations are poor. Moreover, such procedure may also be subject to sequential-testing problems. Finally, it is common in bootstrap methods to estimate the residuals imposing the null hypothesis, and the conclusions from such assessment may vary depending on whether we impose the null when constructing the DGP for the assessment.

If we consider these assessments for the RCT example above, they would also fail to detect relevant distortions, even though such procedures could potentially take into account a series of features of $F(\epsilon)$.

**Parametric choices for $\tilde{F}(\epsilon)$:** we can also consider a parametric model for the CDF, $g(\epsilon; \delta)$, and estimate the parameter $\delta$ using the original (or alternative) data, in order to better approximate the true distribution $F(\epsilon)$.

This may be natural, for example, in settings in which we suspect errors are heteroskedastic. We show in Appendix A.7.1 an empirical application in which $y_i$ represents the aggregate of region $i$, and we expect that regions with larger populations should have errors with lower variance. We show that an assessment based on a Gaussian model with estimated heteroskedasticity works better than other alternatives (such as assessments based on a non-parametric or wild bootstrap) in approximating the true DGP. As another example, when
assessing inference based on CRVE, applied researchers may consider distributions \( \tilde{F}(\epsilon) \) in which within-cluster observations are spatially correlated, and estimate the relevant parameters for the spatial correlation.

**Non-parametric choices for \( \tilde{F}(\epsilon) \):** we can also consider, for example, the use of Wasserstein Generative Adversarial Networks (WGAN), which allows for more flexible approximations for \( F(\epsilon) \) (Arjovsky et al., 2017; Athey et al., 2020).

We apply this method to the RCT example considered above. We first sample 20 treated and 180 control observations, and compute the residuals of the OLS regression. Then we estimate a WGAN with these residuals, allowing for different distributions for treated and control errors. Since the WGAN does not impose that the estimated conditional distributions for the errors have mean zero, we generate a large number of draws from the estimated distributions for treated and controls, and centralize these distributions so that they have conditional mean zero. Then, from these centralized distributions, we generate 1000 draws of 20 treated and 180 control observations to compute the assessment. We do this whole process 5000 times, varying the realization of the original sample.

Implementing the WGAN involves a number of implementation decisions and tuning parameters. We consider two different implementations, varying the batch size and maximum number of epochs, which can be either (50, 2000) or (100, 500).\(^\text{13}\) The results are presented in Appendix Table A.1.

In the first implementation, we would have a probability of 35% of finding an assessment greater than 10%, which would raise a red flag that inference may be unreliable. However, similar to our findings for the assessment based on non-parametric bootstrap (Section A.3), the estimator is biased if we condition on finding an assessment smaller than a given threshold. Moreover, conditioning on a good assessment exacerbates the over-rejection problems (rejection rates would be 21% in this case). In contrast, for the alternative implementation of the WGAN, we never find assessments greater than 10%.

Overall, these results show that the use of WGAN can capture features of \( F(\epsilon) \) that other choices for \( \tilde{F}(\epsilon) \) would not capture. However, since we use the estimated residuals to construct \( \tilde{F}(\epsilon) \), this type of assessment is subject to sequential-testing problems, and this shows to be more relevant exactly for the simulations that would detect with a positive probability problems due to the non-normal distribution of \( Y_i(1) \). Moreover, this method requires a number of choices on tuning parameters, and we show that different choices may lead to different conclusions. Therefore, applied researchers would be able to cherry pick

\(^{13}\)We have downloaded the Python package from [https://github.com/gsbDBI/ds-wgan/](https://github.com/gsbDBI/ds-wgan/), and have mostly used the package’s standard parameters in our implementation. Network architecture has 3 hidden layers with 128 neurons each for both the generator and the critic.
tuning parameters in which the assessment would not reveal much distortions.

A.2 Design-based assessments - theory and simulations

A.2.1 Design-based assessments may confound treatment effects for spatial correlation

We consider a simplified shift-share design model to analyze the placebo exercise considered by Adão et al. (2019). We show that this exercise may falsely detect spatial correlation problems for inference based on CRVE or EHW when the shift-share variable has an effect different from zero. This simplified shift-share model is equivalent settings in which we have a state-level treatment vs control comparison, and we want to check whether inference based on EHW in an individual-level regression is unreliable due to state-level shocks. The conclusions also apply to the placebo exercise considered by Bertrand et al. (2004) for DID.

Let \( Y_i = \beta X_i + \epsilon_i \), where \( X_i = \sum_{f=1}^{F} w_{i} X_{f} \), \( w_{i} \geq 0 \) for all \( f \), and \( \sum_{f=1}^{F} w_{i} = 1 \). Suppose observations \( i = 1, \ldots, N \) are partitioned into equally-sized groups \( \Lambda_1, \ldots, \Lambda_F \), with \( w_{i} = 1 \) if \( i \in \Lambda_{f} \), and \( w_{i} = 0 \) otherwise. Assume also that \( X_{f} \in \{0, 1\} \).\(^{14}\) We show that a placebo exercise in which we resample \( X_{f} \) and then conduct inference with EHW would suggest over-rejection if \( \beta \neq 0 \), even if \( \{\epsilon_{i}\}_{i=1}^{N} \) were drawn from a distribution in which the errors are independent. We assume for simplicity that \( \sum_{f=1}^{F} X_{f} = F/2 \), and consider random draws of \( \tilde{X}_{f} \) such that \( \sum_{f=1}^{F} \tilde{X}_{f} = F/2 \), while holding \( \{Y_{i}\}_{i=1}^{N} \) fixed.

Let \( \hat{\delta} \) be the estimator of the placebo regression. In this case, uncertainty in those simulations comes from the realizations of \( \tilde{X}_{f} \). Therefore, from Lemma 5 from Barrios et al. (2012),

\[
\mathbb{V}_{true} \equiv \mathbb{V} \left( \hat{\delta} \mid \{Y_{i}\}_{i=1}^{N} \right) = \frac{4}{F(F-2)} \sum_{f=1}^{F} (\beta \mathbb{I} \{f \in \mathcal{T}\} - \beta/2 + \bar{\epsilon}_{f} - \bar{\epsilon})^{2}, \tag{2}
\]

where \( \mathcal{T} \) is the set of sectors such that \( X_{f} = 1 \) (in the original data), \( \bar{\epsilon}_{f} \) is the average of \( \epsilon_{i} \) for \( i \in \Lambda_{f} \), and \( \bar{\epsilon} \) is the average across all \( i \). Note that in this exercise we implicitly assume a DGP in which potential outcomes are the same regardless of \( \{\tilde{X}_{f}\}_{f=1}^{F} \).

Likewise, if we consider EHW variance, it would asymptotically recover

\[
\mathbb{V}_{EHW} = \frac{4}{N(N-2)} \sum_{i=1}^{N} (\beta \mathbb{I} \{i \in \Lambda_{f} \text{ such that } f \in \mathcal{T}\} - \beta/2 + \epsilon_{j} - \bar{\epsilon})^{2}. \tag{3}
\]

\(^{14}\)This way, the model is similar to an RCT at the cluster level, or to the setting considered by Ferman (2022) for DID in its Appendix A.2.3.
Consider now a sequence in which $F \to \infty$, where we maintain the number of observations in each $\Lambda_f$ fixed, and that $\sum_{f=1}^{F} \mathcal{X}_f = F/2$. Given the assumption that $\epsilon_i$ was drawn from a distribution in which errors are independent, and assuming that such distribution has finite fourth moments, we have that the sequence $\{\epsilon_i\}_{i \in \mathbb{N}}$ is such that, with probability one,

$$F (\mathcal{V}_{true} - \mathcal{V}_{EHW}) = \beta^2 \left[ \frac{F}{F-2} - \frac{F}{N-2} \right] + o(1). \quad (4)$$

Therefore, except for the case in which $F/(N-2) \to 1$, EHW would asymptotically underestimate the variance of the true distribution of $\hat{\delta}$ whenever $\beta \neq 0$. In this case, the placebo exercise would suggest over-rejection even when there is no spatial correlation.

The same reasoning applies to settings with a state-level treatment vs control comparison and we do not cluster at the state level, or when we consider the placebo exercise presented by Bertrand et al. (2004) on DID settings.

A.2.2 Simulations for design-based assessments

We present simulations that illustrate the use to design-based assessments for assessing whether inference is distorted due to spatial correlation. Consider a setting in which we observe $Y_{is}$ for individual $i$ in state $s$. We have $N$ states, where half of them receive a treatment $T_s$. Each state has 10 individuals. We consider simulations in which $Y_{is}(0) = \omega \xi_s + \epsilon_{is}$ and $Y_{is}(1) = \beta + Y_{is}(0)$, where $\epsilon_{is}$ is iid $N(0, 1)$ across $i$ and $s$, and $\xi_s$ is iid $N(0, 1)$ across $s$. The parameter $\beta$ reflects the true treatment effect, while $\omega$ reflects the relevance of state-level shocks. The goal is to assess whether state-level shocks impose relevant distortions for inference based on EHW standard errors.

We consider a design-based assessment using permutations of $(T_1, \ldots, T_N)$, where for each permutation we estimate $\hat{\beta}^p$ and conduct inference using EHW standard errors. We consider that the parameter of interest is the ATE for a superpopulation. As discussed in Section 2.4.1, a design-based assessment can be used to assess the reliability of an unconditional test (in this case, for inference regarding a superpopulation ATE). We then discuss below the case in which the parameter of interest is the sample ATE. The simulation results are presented in Appendix Table A.2. We summarize the main conclusions.

1. When there is a true treatment effect ($\beta \neq 0$), a design based assessment would be larger than, for example, 0.1 (indicating relevant spatial correlation) with a high probability, even when there is no spatial correlation ($\omega = 0$). The modified design-based assessment would have a much lower probability of indicating relevant spatial correlation in this case. When there is no spatial correlation, this probability is decreasing in
the number of states (Panel A).

2. When there is spatial correlation ($\omega \neq 0$), both assessments would be larger than 0.1 with a high probability, correctly indicating that there are relevant spatial correlation problems. This probability is increasing in the number of states (Panels B and C).

3. These assessments are more informative about spatial correlation problems than checking whether there are significant effects in placebo regressions using pre-treatment outcomes (Panel C). For example, in this setting with $\omega = 0.3$, a placebo regression would be significant at 5% in 14% of the time. In contrast, the design-based assessment would be greater than 0.1 around 74% (91%) of the time when $N = 20$ ($N = 100$). In other words, we may have settings in which the p-value of a placebo regression would be large (which would not raise a red flag for the applied researcher), but these assessments would correctly indicate that there is a problem with a high probability.

4. When we consider the modified design-based assessment, there is evidence of sequential-testing distortions, although the magnitudes are extremely small (Panel C). When $\omega = 0.3$ and $N = 20$, the test size is 14% due to the spatial correlation. The modified assessment would be greater than 0.1 with a 69% probability. Conditional on this assessment being smaller than 0.1 (so the applied researcher would not have strong indication of spatial correlation problems), the test size would slightly increase to 14.5%.\footnote{Given that we consider a very large number of simulations (200,000), this difference is statistically significant with a p-value smaller than 0.001.}

5. We also show that these assessments would be able to detect problems for inference using EHW standard errors in case we have heterogeneous treatment effects at the state level (Panel E).\footnote{In this case, we consider simulations in which $Y_{is}(0) = \epsilon_{is}$ (so there is no spatial correlation in the error for the potential outcomes when untreated), but $Y_{is}(1) = 0.4\xi_s + Y_{is}(0)$, with $\xi_s \sim N(0, 1)$. Therefore, the superpopulation ATE is still zero, but we have heterogeneous treatment effects for different states.}

Now consider instead that the parameter of interest is the sample ATE (so inference is conditional on the potential outcomes). In this case, the test size would depend on the realization of the potential outcomes. Therefore, for some realizations of the potential outcomes we would have relevant size distortions, and for some we would not. Importantly, the design-based assessment would correctly recover the exact size of the test (conditional on the realization of the potential outcomes) for the simulations in which $\beta = 0$. This is because we have that $Y_{is}(1) = Y_{is}(0)$ in these simulations. When $\beta \neq 0$, the design-based
assessment would tend to over-state the relevance of spatial correlation, in the same way as presented in Appendix Table A.2, which considers a superpopulation ATE as the target parameter.

### A.3 Assessments based on non-parametric bootstrap

Another alternative is to consider an assessment based on the idea of a non-parametric bootstrap (Efron and Tibshirani, 1994). In this case, we sample with replacement from \[\{Y_i, x_i\}_{i=1}^N\]. Then, with the bootstrap sample, we estimate \(\hat{\beta}_b\) and test \(\beta = \hat{\beta}\) using the inference method being assessed, where \(\hat{\beta}\) is the original estimator.

Consider a setting in which uncertainty comes from random sampling from a large population with joint distribution \(F_{X,Y}(x,y)\), and we want to evaluate whether an inference method in this application is reliable. We consider here an unconditional test, since we also treat \(X\) as stochastic. An assessment based on a non-parametric bootstrap in this case would approximate this sampling scheme with simulations in which \((y_i, x_i)\) is sampled from a large population with joint distribution given by the empirical distribution of \(\{y_i, x_i\}_{i=1}^N\).

The advantage of this procedure is that the empirical distribution of \(\{y_i, x_i\}_{i=1}^N\) asymptotically recovers the joint distribution \(F_{X,Y}(x,y)\) when \(N \to \infty\). Therefore, such simulations potentially take into account heteroskedasticity and other features of the errors, such as settings in which the CEF is not linear. Moreover, if we consider a block bootstrap, we may take into account more complex features of the data, such as spatial/serial correlations. However, again these potential advantages are asymptotic properties of bootstrap methods, and this approach may provide unreasonable simulations, and fail to detect relevant problems, exactly when asymptotic approximations are poor (see example in Section 3.1). Also, this assessment may be subject to sequential-testing problems. Finally, it may require a number of decisions and modifications that may lead to misleading conclusions in case they are not properly implemented, as we discuss below.

If we apply this assessment to the RCT example discussed above, the median assessment across realizations of the original data is 10% (Appendix Table A.1). Therefore, this assessment would detect relevant over-rejection in many realizations of the original data, even though it would, on average, understate the over-rejection problem. Moreover, there is a 48% probability that it would be lower than, for example, 10%, which should not raise a red flag that we may have relevant size distortions. Also, if we condition on an assessment lower than 10%, the estimator ceases to be unbiased, and we have a larger rejection rate of 24% (which is higher than the unconditional test size). Therefore, while this assessment would detect inference problems in some cases, this comes at a cost of relevant sequential-testing...
To illustrate potential implementation problems that may arise even in large samples when we consider assessments based on a non-parametric bootstrap, we analyze the case of assessing whether inference based on CRVE is reliable in DID settings. Consider a setting with 500 states (half of them treated), so that it would be reasonable to rely on asymptotic theory to approximate the joint distribution of the data, since we have many treated and many control states.

We construct the assessment resampling blocks of states, to take serial correlation into account. In this case, we will generally end up with 2 or more blocks of some specific state in the bootstrap sample. The problem is that, once we cluster at the state variable in this simulated sample, we will consider all of those blocks in the same cluster, and this may lead to incorrect conclusions. If the true DGP is such that $Y_{st} \overset{iid}{\sim} N(0, 1)$, then an assessment based on such bootstrap procedure to evaluate whether CRVE is reliable would indicate a rejection rate of 0.6% (on average across simulations). Therefore, such assessment would (incorrectly) suggest that the inference method is extremely conservative.

If we generate a new state variable, so that each block in the bootstrap samples has its own cluster, the assessment would be close to 5%, leading to the correct conclusion. However, this correction would require a deeper analysis of the specific application, and a non-trivial amount of coding. For example, if we use the canned Stata bootstrap command, it will not take this correction into account, leading to incorrect conclusions.

Overall, while considering assessments based on a non-parametric bootstrap presents some potential advantages, we show that (i) relying on such procedure may lead to sequential-testing problems, and (ii) we may have settings in which we end up with unreasonable assessments, because asymptotic approximations are poor (see Section 3.1). Moreover, if we decide to consider this assessment, then we should be sure that it is correctly implemented. As we show in an example above, if we want to use this assessment to assess whether CRVE is reliable, then we cannot use the canned Stata bootstrap command to do the simulations.

### A.4 Conditional vs unconditional inference

We present two examples in which we may have conditional inference with relevant distortions, but unconditional inference with no size distortion. In both cases, we argue that it would be useful for the applied researcher to consider a conditional assessment.

For the first one, consider an RCT with 200 observations, where treatment assignment is iid Bernoulli with probability 0.5. For simplicity, assume that potential outcomes are $N(0, 1)$. If $X$ is such that we have around 100 treated and 100 control observations, then a
Conditional inference on **X** would have a size close to \( \alpha \), given the large number of treated and of control observations. We also have that rejection rates are very close to \( \alpha \) when we consider unconditional inference, because the probability of having a sample with very few treated (or control) observations is very small.

We want to assess whether inference using EHW standard errors is reliable, using, for example, an assessment based on iid normals conditional on **X**. Now suppose that we are (extremely) unlucky, and the realization of **X** is such that only two or three observations got \( X_i = 1 \). In this case, the conditional assessment would suggest large over-rejection, even though the inference method has good properties unconditionally (assuming it is actually true that treatment assignment is iid with probability 0.5). The key point is that the realizations in which conditional inference is problematic in this setting are extremely low probability events, so once we integrate over the distribution of **X** we do not have much over-rejection for unconditional inference. In case we are certain that the distribution of \( X_i \) is iid Bernoulli with 0.5 treatment probability, then we could consider assessments treating **X** and potential outcomes as stochastic (instead of conditioning on **X**). In this case, we would find an assessments close to \( \alpha \), despite the fact that the conditional assessment would be larger than \( \alpha \) (in this unlikely event in which we end up with very few treated observations).

However, even if we are certain about the distribution of \( X_i \), it is not obvious whether we should focus on unconditional inference in this setting. We do not intend to provide in this paper a definite solution to the question on whether we should focus on conditional or unconditional inference. Still, given that **X** is observed, and the researcher knows that she should expect large over-rejections conditional on this realization, it seems odd not to take that information into account when conducting hypotheses testing. In our view, an assessment suggesting relevant distortions for conditional inference should at the very least lead applied researchers to carefully think about whether conditional or unconditional inference is more relevant in their settings (even if we believe the test is reliable unconditionally).

As a second example, consider a DID setting in which we observe individuals \( i \) in states **s**, where a single state is treated. Assume that potential outcomes for all individuals and all periods are iid \( N(0,1) \). Conditional on the treatment assignment, if the treated state has fewer individuals than the control states, then the inference method proposed by Conley and Taber (2011) would over-reject (Ferman and Pinto, 2019; MacKinnon and Webb, 2020). However, if all states have the same probability of being treated, then this test would have the correct size unconditionally. The reason is that the test would under-reject conditional on a treatment assignment in which the treated state is large relative to the controls. Therefore, an assessment based on iid normals would (correctly) suggest relevant over-rejection for a conditional test, while the test would be valid unconditionally.
This happens because the inference method being assessed, in this case, is not valid conditional on (some) \( X \), even asymptotically, and even though we considered a DGP with iid normal errors for the individual-level observations. In many cases, however, the inference method being assessed would be asymptotically valid for conditional inference. Therefore, if we find evidence of over-rejection conditional on \( X \), we should at least suspect that the inference method might also over-reject unconditionally in a simple DGP in which errors are iid normal.

Moreover, the conditional assessment in this case should, again, at the very least lead applied researchers to carefully think about whether conditional or unconditional inference is more relevant in their settings. For this particular example, Ferman and Pinto (2019) and MacKinnon and Webb (2020) argue that inference conditional on the treatment allocation is more appropriate.

### A.5 Choice of \( \alpha \)

We present an empirical illustration in which the choice of \( \alpha \) for the assessments leads to different conclusions. We consider the empirical application from Dix-Carneiro et al. (2018), which is analyzed in Section 3.2. However, we focus on the weighted OLS specification. We recommend reading Section 3.2 before reading this appendix section.

We consider the design-based assessment for this specification to evaluate whether the inference method proposed by AKM, with the null imposed, is reliable. In this case, instead of using the residuals from the shift-share regression to estimate the standard errors, we use the residuals from a regression that imposes the null hypothesis to calculate the residuals (see AKM for details).

When we consider \( \alpha = 5\% \), the design-based assessment is smaller than 5\% (at 3.5\%) which, at a first glance, could indicate that this inference method is, if anything, conservative. However, when we consider \( \alpha = 10\% \), then the design-based assessment becomes substantially bigger than \( \alpha \) (at 20\%), suggesting relevant over-rejection.

As discussed in Section 2.2, this happens because imposing the null leads to an upwards bias in the standard errors when \( \hat{\beta} \) is large, which is exactly when we should expect to reject the null at low significance levels. Since this downward bias on the rejection rates is stronger for lower significance levels, this explains why we may have under-rejection when \( \alpha \) is smaller, and over-rejection when it is larger.

In Appendix Figure A.1, we show the CDF of the p-values in the simulations used in the assessments. If the inference method were working well, then we should expect it to be approximately the CDF of a uniform variable \([0,1]\). In contrast, we see that we have
under-rejection for lower $\alpha$ and over-rejection for higher $\alpha$. Since the threshold in which the inference method does not lead to over-rejection in the application may be different from the one in the simulations used in the assessments, we should consider this inference method with caution in this empirical application.

A.6 Shift-share design applications: probability of flagging spatial correlation problems

In Section 3.2.1, we discuss the use of design-based assessments to detect inference problems due to spatial correlation in shift-share design applications. A natural question in this case is to consider the probability that those assessments would correctly flag that there are spatial correlation problems in case they are actually present.

In order to evaluate that, we consider the following exercise. Let $w_{if} \geq 0$ be the shares of a given application. We construct an outcome vector given by $Y_i = Z_i + \gamma \sum_{f=1}^{F} w_{if} \tilde{X}_f$, where $Z_i \sim N(0,1)$, while $\tilde{X}_f \sim N(0,1)$ are random shocks that will not be the shocks considered by the applied researcher to construct the shift-share variable. We can think of $\tilde{X}_f$ as unobserved variables in the error term that might be spatially correlated. Since those unobservables have the same structure of shares as the real shocks that the applied researcher observes, this spatially correlated shocks may generate relevant over-rejection, as discussed by Adão et al. (2019). Note that spatial correlation will be stronger if $|\gamma|$ is larger.

We consider then that the applied researcher runs a shift-share regression using the shift-share variable $X_i = \sum_{f=1}^{F} w_{if} X_f$ (where $X_f \sim N(0,1)$ are the true shocks that she observes), and $Y_i$ as the outcome variable. Again, all of those $N(0,1)$ variables are iid.

We consider simulations of this model with the structure of each of the three empirical applications considered in Section 3.2.1 (the number of observations, the matrix of shares, and covariates used in the regressions). For each realization of these random variables (that generates $Y_i$ and $X_i$), we (i) estimate the shift-share regression, and test the null using CRVE; (ii) calculate the design-based assessment and the modified design-based assessment. We do that for a number of different values for $\gamma$.

For each $\gamma$, we first calculate the test size. For all empirical applications, this is close to 5% when $\gamma = 0$ (because there is no spatial correlation in this case). However, when $\gamma$ increases, then we start to have over-rejection. Then we calculate the proportion of times in which each of the assessments would flag that there is a spatial correlation problem. We define that as the assessment being greater than 0.1 (using alternative thresholds would lead to similar conclusions). We plot in Appendix Figure A.2 the probability of flagging a problem as a function of the test size (which, in turn, is a function of $\gamma$).
For the application from Autor et al. (2013), we see that these assessments would have a large probability of detecting problems. For example, if the spatial correlation is such that the test size (ignoring spatial correlation) is around 17%, then there would be a 90% probability of having assessments greater than 0.1.

For the application from Acemoglu and Restrepo (2020), the probability of detecting problems is a bit lower. For example, if the spatial correlation is such that the test size is around 15%, then there would be a 60% probability of having assessments greater than 0.1. If the spatial correlation is stronger, then the probability of detecting problems would be larger (for example, at 90% if the spatial correlation is such that the test size is 29%). Finally, we note that the probability of detecting problems is much smaller for the application from Dix-Carneiro et al. (2018). When the test size is around 15%, we would only have a 35% probability of having a design-based assessment greater than 0.1.

In case the design-based assessments do not detect problems due to spatial correlation, we recommend that applied researchers consider this kind of simulations. This way, they can evaluate whether the assessments would have a high probability of detecting problems when there is meaningful spatial correlation problems.

A.7 Other applications

A.7.1 Weighted OLS

As we show in Appendix Table A.3, in the absence of spatial correlation, the assessments for CRVE suggest more distortions in the weighted OLS specifications for the applications considered in Section 3.2, relative to the unweighted ones. This suggests the possibility of large distortions when using CRVE with weighted OLS, even when we have a reasonably large number of clusters, and when errors are independent across clusters.

To understand how weights may affect the quality of asymptotic approximations, we start with a simple example in which we sample $N$ observations from $Y_i \sim N(0, 1)$. We estimate the mean of $Y_i$ with a weighted average, where the first half of the observations receives weight of one, and the other half receives weight of $W > 1$. In this case, when $W \to \infty$, this essentially means that this weighted average would only be based on $N/2$ observations, implying that asymptotic approximations would be poorer.

Simulating a t-test using the asymptotic critical value in this setting with $N = 10$ and $W = 10$, we find rejection rates of 8% when we do not use weights, and 13% when we consider a weighted average. We also consider the case in which $\mathbb{V}(Y_i) = 0.1$ for the observations that received weight $W = 10$. Interestingly, in this case the weighted average provides a more efficient estimator for the mean relative to the simple average. However,
we still find larger over-rejection for the weighted average (11% vs 7.6%). Therefore, in both settings, asymptotic approximations are poorer when we consider the weighted average. While Dickins (1990) discusses the disadvantages of using weighted regressions when using weights makes the estimator less efficient, we show that using weights may have a cost in terms of making asymptotic approximations poorer even when weighted estimators are asymptotically efficient.

In light of this simple example, we revisit the applications considered in Section 3.2. We abstract from the possibility of spatial correlation in shift-share designs, so we can focus on the consequences of using weighted OLS when inference is based on CRVE, even when the assumptions for CRVE are valid. When we consider assessments conditional on $X$, an assessment based on iid normals suggests relevant over-rejection when we consider the weighted OLS specifications (Appendix Table A.3, Panel A).

We also consider an assessment in which we allow the errors to be heteroskedastic. More specifically, we consider that $\epsilon_i \sim N(0, A + B/W_i)$, where $W_i$ represents the population of region $i$, and estimate the parameters $A$ and $B$ using the OLS residuals. This would provide a better approximation to the true DGP in case errors are heteroskedastic. When we run the assessment using this heteroskedastic distribution for the errors, we find very similar results to the ones with a simple assessment based on iid normals (Appendix Table A.3, Panel B). We also consider in Panel C assessments in which both $X$ and $y$ are treated as stochastic, leading to the same conclusions.

Given the large size distortions in the weighted OLS regressions, an alternative would be to consider CRVE's with improved finite-sample properties (Bell and McCaffrey, 2002; Imbens and Kolesár, 2016). For the weighted specification from Acemoglu and Restrepo (2020), the clustered standard errors with HC3 correction become almost six times larger than the ones with the standard HC1 correction.\(^{17}\) If we consider assessments with iid normals or with estimated heteroskedasticity, then we would have evidence that inference using such standard errors is extremely conservative, with rejection rates smaller than 0.1% under the null (Appendix Table A.3). Therefore, both the use of CRVE with the standard HC1 and with the HC3 corrections should be considered with caution in this application. For the weighted specification from Dix-Carneiro et al. (2018), using the HC3 option leads to standard errors 25% larger, and the assessments are close to 5%, suggesting that, in this application, such standard errors better control for size, without becoming too conservative.

We also consider a small simulation study based on the specification presented in Column

\(^{17}\)We computed these standard errors using the R function vcovCL. It is not possible to compute the CRVE with the HC2 option in this application. The results with the HC0 option are very similar to the ones using HC1 option.
4 of Appendix Table A.3. We consider two scenarios, one in which the true errors are homoskedastic normal, and another one in which errors are normal with variance $1/W_i$. In the homoskedastic case, the true rejection rates would be 31% (as seen in Appendix Table A.3). In the heteroskedastic case, the true rejection rate would be lower, at 14%. We simulate 3000 realizations from each of these DGP’s and then compute different types of assessments. For each draw, the assessments are based on 1000 simulations. We present the results in Appendix Table A.5.

First, note that a simple assessment based on iid normals would be 31% for all realizations of the original data in both DGP’s. Therefore, it would recover the true rejection rate when errors are indeed iid normal, but would overstate the rejection rates when errors are heteroskedastic. We also consider an assessment based on normal errors with estimated heteroskedasticity. These assessments lead to rejection rates close to the true ones in both scenarios, with very small variation across realizations of the original data.

We also consider assessments based on different types of bootstraps, that could also potentially take heteroskedasticity into account. Overall, these alternatives provide poorer approximations to the true DGP. The median assessments (across realizations of the original sample) are smaller than the true rejection rates. Moreover, there is large variation in the assessments depending on the realization of the errors, and in some cases the assessments are correlated with the p-value in the original regression. Finally, an assessment based on a non-parametric bootstrap performs particularly poorly without the fix described in Section A.3, which again reinforces that we should be careful to implement this type of assessment correctly.

A.7.2 Studentized block-bootstrap

We consider the studentized block-bootstrap suggested by Bertrand et al. (2004) to deal with serial correlation in DID settings. Following their guidelines, we first compute the absolute $t$-statistic $t = \text{abs}(\hat{\beta}/SE(\hat{\beta}))$ using the OLS estimate of $\beta$ and its standard error. Then, we construct a bootstrap sample by drawing with replacement $N$ matrices $(\bar{Y}_s, V_s)$, where $\bar{Y}_s$ is the entire time series of observations for state $s$, and $V_s$ is the matrix of state dummies, time dummies, and treatment dummy for state $s$. The bootstrap distribution for $t$ is then constructed using $t_b = \text{abs}(\hat{\beta}_b/SE(\hat{\beta}_b))$. Bertrand et al. (2004) are not clear about how the standard errors to compute the $t$-statistic should be constructed. We show that a standard choice may lead to large over-rejections, depending on how it is implemented.

Let $N$ be the total number of states, and suppose a state $s$ is drawn $n$ times in a given bootstrap sample. The main problem is that, if we compute the standard errors using CRVE at the state level, then we would treat all the observations from these $n$ “states” in the
bootstrap sample as being in the same cluster. Therefore, we will generally have fewer than \( N \) clusters in the bootstrap sample, while the CRVE in the original regression was estimated using \( N \) clusters. This implies that the bootstrap distribution will not approximate the distribution of the \( t \)-statistic, even asymptotically. Note that this is how the bootstrap p-values would be calculated if we use the canned bootstrap Stata command.

To illustrate the problem, consider a DID setting with 2 periods, and 500 states. Half of the states are treated, so that we do not have to worry about the problem of few treated states. Assuming \( Y_{st} \overset{iid}{\sim} N(0,1) \), we find a rejection rate of 16% for a 5% nominal level test. Therefore, even with a large number of states, a studentized block-bootstrap would be unreliable in this setting. Importantly, even simple assessments resampling iid standard normal variables or permuting the treatment assignment would detect this problem.

There are two alternative implementations of the block bootstrap that would work in this case. The first one is to use EHW standard errors instead of CRVE to construct the \( t \)-statistic.\(^{18}\) Alternatively, we can create a new set of state dummies in the bootstrap samples, so that we continue to have \( N \) different clusters in the bootstrap samples. In this case, however, we would not be able to use the canned bootstrap command in Stata.

Overall, these results show that standard inference procedures suggested in the literature may present relevant distortions depending on how they are constructed, and that even some simple assessments may be very efficient in detecting that. Finally, we note that an inference assessment based on a non-parametric bootstrap without the modification described in Section A.3 would (incorrectly) be around 5%, failing to detect this problem.

A.7.3 Clustering with fixed effects

A setting in which applied researchers commonly use asymptotically invalid CRVE is when fixed effects at the cluster level are included. As Cameron and Miller (2015) report, CRVE is over-estimated in Stata when we use \texttt{areg} command or when we manually include the fixed effect dummies. In such settings, even simple assessments based on iid normals or on permutations would indicate rejection rates lower than \( \alpha \), which should prompt applied researchers to use a valid degrees-of-freedom correction.

A.7.4 Stratified randomized control trials

Consider a setting in which we have a total of \( N \) schools, and those schools are divided into \( S \) strata of \( G \) schools each, so \( N = G \times S \). For each strata, half of the schools receive

\(^{18}\)Note that EHW would be wrong if there is serial correlation. However, this is not a problem, because the block-bootstrap would asymptotically recover the distribution of the \( t \)-statistic, despite the fact that it would not asymptotically be standard normal in this case.
treatment. For simplicity, consider that each school has \( n \) students. A common approach in this setting is to estimate the treatment effect using OLS regression of the outcome on a treatment dummy and strata fixed effects, and to use CRVE at the school level. However, Chaisemartin and Ramirez-Cuellar (2019) show that inference based on CRVE at the school level in this case leads to significant over-rejection when \( G \) is small. They recommend clustering at the strata level to solve this problem.

We present detailed simulations of this setting in an earlier version of the paper (Ferman, 2019). The main conclusions are that (i) inference using CRVE at the school level is invalid even asymptotically when \( G \) is fixed, and a simple assessment based on iid normals would detect that;\(^{19}\) (ii) clustering at the strata level is asymptotically valid, but leads to over-rejection if there are few strata (again, an assessment based on iid normals would detect that); (iii) there is a trade-off between clustering at the school versus at the strata level, and a simple assessment based on iid normals can shed some light on that. In Ferman (2019) we also consider the use of alternative inference assessments in this setting. We show that a design-based assessment may be misleading depending on the true ATE, with some non-trivial patterns depending on the number of schools (which is consistent with the discussion in Section 2.2, that a design-based assessment may not work well when \( Y(0) \neq Y(1) \)). The modified design-based assessment presented in Section 2.2 would solve this problem.

### A.8 DID with a single treated cluster

Consider a DID model

\[
Y_{cst} = \theta_c + \gamma_t + \beta d_{st} + \epsilon_{cst},
\]

where \( Y_{cst} \) is the outcome of county \( c \), in state \( s \), in time \( t \); \( \theta_c \) and \( \gamma_t \) are county and time fixed effects; \( \epsilon_{cst} \) is the error term; \( d_{st} \) is an indicator variable that equal one if state \( s \) is treated at time \( t \). We consider the case in which only state \( s = 1 \) is treated, and treatment starts after \( t^* \). We have data from periods \( t = 1, ..., T \) and states \( s = 1, ..., S \). For simplicity, we consider the case in which each state has \( N \) counties. Let \( \epsilon_{cs,post} = \frac{1}{T-t^*} \sum_{t=t^*+1}^{T} \epsilon_{cst} \), while \( \epsilon_{cs,pre} = \frac{1}{t^*} \sum_{t=1}^{t^*} \epsilon_{cst} \). In this case, the DID estimator is given by

\[
\hat{\beta} = \beta + \frac{1}{N} \sum_{c=1}^{N} [\epsilon_{c1,post} - \epsilon_{c1,pre}] - \frac{1}{(S-1)N} \sum_{s=2}^{S} \sum_{c=1}^{N} [\epsilon_{cs,post} - \epsilon_{cs,pre}].
\]

\(^{19}\)Note that calculating the effective number of clusters proposed by Carter et al. (2017) would not detect a problem in this setting.
We recall that we consider the treatment allocation as fixed. If $\epsilon_{cst}$ has expected value equal to zero, then the DID estimator is unbiased. Moreover, assuming errors independent across states, but potentially correlated within states, we have that

$$V(\hat{\beta}) = V\left( \frac{1}{N} \sum_{c=1}^{N} [\epsilon_{c1,post} - \epsilon_{c1,pre}] \right) + \frac{1}{(S-1)^2} \sum_{s=2}^{S} V\left( \frac{1}{N} \sum_{c=1}^{N} [\epsilon_{cs,post} - \epsilon_{cs,pre}] \right).$$

(7)

The CRVE will estimate the first term in the right-hand side of Equation 7 using the residuals counterparts of the treated counties, that is, $\hat{V}_{treated} = \left( N^{-1} \sum_{c=1}^{N} [\hat{\epsilon}_{c1,post} - \hat{\epsilon}_{c1,pre}] \right)^2$. However, if we have only a single treated state, then $\hat{V}_{treated} = 0$, so the true variance of $\hat{\beta}$ would be severely underestimated. See Conley and Taber (2011) and Ferman and Pinto (2019) for a more detailed analysis of this setting.

If the true variance of treated counties is smaller than the variance of the control counties, the true over-rejection would be smaller than an assessment based on iid normals would suggest. Still, we would need the variance of the treated to be 100 times smaller than the variance of the controls so that we have a rejection rate of 10% for a 5% nominal level test.
Figure A.1: Assessment of AKM inference method with the null imposed

Notes: This figure presents the CDF of the p-values in the simulations used to construct the design-based assessment for the inference method proposed by Adão et al. (2019) with the null imposed. We consider the weighted OLS specification from Dix-Carneiro et al. (2018) (which is also analyzed in column 6 of Appendix Table A.3). The dashed line is the CDF of an uniform \([0, 1]\) random variable.
Figure A.2: **Probability of detecting spatial correlation problems**

A: China shocks

![Graph A: China shocks](image)

B: Exposure to robots

![Graph B: Exposure to robots](image)

C: Trade liberalization

![Graph C: Trade liberalization](image)

Notes: these figures present the results from the simulations described in Appendix A.6. Each point in the graphs represent one choice of $\gamma$, which determines the strength of the spatial correlation in the DGP used in the simulations. For that $\gamma$, we plot the implied rejection rate for inference using CRVE (which is increasing in $|\gamma|$), and the probabilities that the assessments are greater than 0.1. We consider the application from Autor et al. (2013) in Figure A, the one from Acemoglu and Restrepo (2020) in Figure B, and from Dix-Carneiro et al. (2018) in Figure C.
Table A.1: **RCT example with lognormal distribution for $Y_i(1)$ (true size = 14%)**

|                           | median (1) | p10 (2) | p90 (3) | Pr($A > 0.1$) (4) | $E[\beta | A < 0.1]$ (5) |
|---------------------------|------------|---------|---------|-------------------|-------------------|
| **Panel A: Assessments conditional on $X$** |            |         |         |                   |                   |
| $\tilde{F}(\epsilon)$ used in the assessment: |            |         |         |                   |                   |
| iid normal                | 0.070      | 0.070   | 0.070   | 0                 | 0                 |
| Residual bootstrap        | 0.066      | 0.062   | 0.071   | 0.000             | 0.010             |
| Wild bootstrap (without null imposed) | 0.061      | 0.025   | 0.069   | 0.000             | 0.010             |
| Wild bootstrap (with null imposed) | 0.063      | 0.021   | 0.070   | 0.000             | 0.010             |
| WGAN (batch size = 50; max epochs = 2000) | 0.090      | 0.071   | 0.141   | 0.355             | -0.122            |
| WGAN (batch size = 100; max epochs = 500) | 0.069      | 0.063   | 0.076   | 0.000             | -0.007            |

| **Panel B: Assessments based on non-parametric bootstrap** |            |         |         |                   |                   |
| Resampling (X,Y)          | 0.102      | 0.074   | 0.202   | 0.521             | -0.154            |
| Resampling Y conditional on X | 0.099      | 0.072   | 0.202   | 0.491             | -0.145            |

| **Panel C: Design-based Assessments** |            |         |         |                   |                   |
| Permutation of X          | 0.065      | 0.059   | 0.071   | 0.000             | 0.010             |

Notes: this table presents the results for different types of assessments considered in Section 2, for the RCT example in which $Y_i(0)$ is standard normal, $Y_i(1)$ is lognormal with the mean normalized to zero, and $(N_1, N_0) = (20, 180)$. All assessments are constructed based on tests with nominal level of 5%. The assessment based on iid normals does not depend on the draw of the original data, so we report the rejection rate of 10,000 simulations using iid normals as the outcome variable. For the other assessments, we generate 5000 draws of the original data, and then for each draw we construct the assessment using 1000 simulations. Columns 1 to 3 present the median, 10th percentile and 90th percentile of the assessments (across realizations of the original data). Column 4 presents the probability that the assessment is greater than 0.1, while column 5 presents the bias of the estimator conditional on an assessment greater than 0.1. The choice of 0.1 as the threshold for flagging that the inference assessment is unreliable is admittedly arbitrary. Other choices lead to qualitatively similar results.
Table A.2: Simulations - design-based assessments

|                | Test size | \( Pr(A > 0.1) \) (cond. on \( A < 0.1 \)) | \( Pr(A_m > 0.1) \) (cond. on \( A_m < 0.1 \)) |
|----------------|-----------|---------------------------------------------|---------------------------------------------|
| **Panel A:**   |           |                                             |                                             |
| \( N = 20 \)   | 0.051     | 0.632                                       | 0.056                                       |
| \( N = 100 \)  | 0.049     | 0.715                                       | 0.068                                       |
| **Panel B:**   |           |                                             |                                             |
| \( N = 20 \)   | 0.140     | 0.927                                       | 0.688                                       |
| \( N = 100 \)  | 0.138     | 0.998                                       | 0.902                                       |
| **Panel C:**   |           |                                             |                                             |
| \( N = 20 \)   | 0.140     | 0.743                                       | 0.070                                       |
| \( N = 100 \)  | 0.138     | 0.913                                       | 0.902                                       |
| **Panel D:**   |           |                                             |                                             |
| \( N = 20 \)   | 0.051     | 0.114                                       | 0.043                                       |
| \( N = 100 \)  | 0.049     | 0.009                                       | 0.049                                       |
| **Panel E:**   |           |                                             |                                             |
| \( N = 20 \)   | 0.129     | 0.672                                       | 0.075                                       |
| \( N = 100 \)  | 0.130     | 0.840                                       | 0.101                                       |

Notes: this table presents the results from the simulations discussed in Appendix A.2.2. Column 1 presents test size for inference based on EHW standard errors, with a nominal level of 5%. Column 2 (column 4) presents the probability that the design based assessment (modified design-based assessment) is greater than 0.1. This would flag that inference based on EHW might be problematic due to spatial correlation (all results remain similar if we consider alternative thresholds). Columns 3 and 5 present test sizes conditional on having assessments smaller than 0.1 (which would not flag that spatial correlation is problematic). We run 20,000 simulations (draws of the original data) for each scenario, and for each draw the assessment is constructed using 500 permutations. The only exception is the setting with \( (N, \beta, \omega) = (20, 0, 0.3) \), in which we report results from 200,000 simulations. We do that so that we have more precision to compare the rejection rates unconditional, and conditional on a good assessment. For the settings in which the probability of having an assessment larger than 0.1 is larger than 0.9, we omit the conditional test size (because in this case there will be very few observations).
### Table A.3: Shift-share designs: assessments for alternative CRVE's

|                      | China shock | Exposure to robots | Trade liberalization |
|----------------------|-------------|--------------------|----------------------|
|                      | unweighted  | weighted           | unweighted           | weighted           | unweighted | weighted |
| (1)                  | (2)         | (3)                | (4)                  | (5)                | (6)        |
| CRVE                 | 0.073       | 0.093              | 0.090               | 0.311              | 0.062      | 0.146    |
| CRVE - HC3           | 0.059       | 0.019              | 0.049               | 0.000              | 0.056      | 0.031    |

Panel A: Assessment conditional on X (iid normal errors)

|                      | CRVE | CRVE - HC3 |
|----------------------|------|------------|
| (1)                  | 0.073| 0.059      |
| (2)                  | 0.093| 0.019      |
| (3)                  | 0.090| 0.049      |
| (4)                  | 0.311| 0.000      |
| (5)                  | 0.062| 0.056      |
| (6)                  | 0.146| 0.031      |

Panel B: Assessment conditional on X (normal errors with estimated heteroskedasticity)

|                      | CRVE | CRVE - HC3 |
|----------------------|------|------------|
| (1)                  | 0.073| 0.059      |
| (2)                  | 0.093| 0.019      |
| (3)                  | 0.087| 0.047      |
| (4)                  | 0.310| 0.000      |
| (5)                  | 0.061| 0.056      |
| (6)                  | 0.146| 0.031      |

Panel C: Assessment with X and Y stochastic

|                      | CRVE | CRVE - HC3 |
|----------------------|------|------------|
| (1)                  | 0.070| 0.060      |
| (2)                  | 0.063| 0.007      |
| (3)                  | 0.067| 0.042      |
| (4)                  | 0.123| 0.000      |
| (5)                  | 0.064| 0.055      |
| (6)                  | 0.140| 0.028      |

| # of clusters | 48  | 48  | 48  | 48  | 91  | 91  |
|---------------|-----|-----|-----|-----|-----|-----|
| # of observations | 772 | 772 | 722 | 722 | 411 | 411 |

Notes: this table presents different types of assessments to assess the reliability of CRVE and CRVE with the HC3 correction for the empirical applications presented in Section 3.2. Panel A considers an assessment conditional on X with iid normal errors. Panel B estimates the variance of each observation as a function of population sizes. We then consider a DGP with errors independent normal with this estimated heteroskedasticity. Panel C considers an assessment in which the outcome is iid normal and shocks are iid normal (so X is also stochastic). Columns 1 and 2 refer to the application from Autor et al. (2013). We consider the specification that AKM used in their Table I. Column 1 uses unweighted OLS, while column 2 uses weighted OLS. Columns 3 and 4 refer to the application from Acemoglu and Restrepo (2020). Column 3 uses the specification from column 6 in their Table 2 (unweighted), while column 4 uses the specification from column 4 in their Table 2 (weighted). Columns 5 and 6 refer to the application from Dix-Carneiro et al. (2018). Column 5 uses the specification from column 1 in their Table 2 (unweighted), while column 6 uses the specification from column 2 in their Table 2 (weighted).
|                          | China shock          |          | Exposure to robots |          | Trade liberalization |          |
|--------------------------|----------------------|----------|-------------------|----------|---------------------|----------|
|                          | Main effects         | Placebo  | Main effects       | Placebo  | Main effects         | Placebo  |
|                          | (1)                  | (2)      | (3)               | (4)      | (5)                 | (6)      |
| Estimate                 | -0.504               | -0.110   | -0.516            | -0.217   | -1.976              | 0.727    |
| CRVE                     | Standard error       | 0.103    | 0.048             | 0.118    | 0.151               | 0.822    |
|                          | p-value              | 0.000    | 0.023             | 0.000    | 0.152               | 0.016    |
| CRVE - HC3               | Standard error       | 0.116    | 0.052             | 0.150    | 0.226               | 0.839    |
|                          | p-value              | 0.000    | 0.034             | 0.001    | 0.338               | 0.018    |
| AKM                      | Standard error       | 0.138    | 0.036             | 0.053    | 0.070               | 0.311    |
|                          | p-value              | 0.000    | 0.003             | 0.000    | 0.002               | 0.000    |
| AKM (null imposed)       | Standard error       | 0.208    | 0.048             | 0.115    | 0.342               | 0.545    |
|                          | p-value              | 0.016    | 0.022             | 0.000    | 0.525               | 0.000    |
| # of clusters            | 48                   | 48       | 48                | 48       | 91                  | 91       |
| # of observations        | 772                  | 772      | 722               | 722      | 411                 | 411      |
| # of sectors             | 395                  | 395      | 19                | 19       | 20                  | 20       |

Notes: this table presents the estimates, standard errors, and p-values when we consider inference based on CRVE, CRVE with HC3 correction, and the procedure proposed by Adão et al. (2019) (without and with the null imposed). Columns 1 and 2 refer to the application from Autor et al. (2013). We consider the specification that Adão et al. (2019) used in their Table I. Column 1 uses estimates for the main effects, while column 2 uses a placebo specification (considering variations in the outcome variable from 1980 to 1990). Columns 3 and 4 refer to the application from Acemoglu and Restrepo (2020). Column 3 uses the specification from column 6 in their Table 2, while column 4 uses the specification from column 4 in their Table 4. Columns 5 and 6 refer to the application from Dix-Carneiro et al. (2018). Column 5 uses the specification from column 1 in their Table 2, while column 6 uses the specification from column 1 in their Table 4.
Table A.5: Simulations with weighted OLS

| F(ε) used in the assessment: | Homoskedastic DGP (true size 31%) | Heteroskedastic DGP (true size 14%) |
|-----------------------------|-----------------------------------|-----------------------------------|
|                             | Median (1) p10 (2) p90 (3) Corr. (4) | Median (5) p10 (6) p90 (7) Corr. (8) |
| Normal with parametric heteroskedasticity | 0.299 0.280 0.316 0.001 [0.001] | 0.139 0.121 0.199 -0.001 [0.002] |
| Non-parametric bootstrap    | 0.174 0.102 0.289 0.013 [0.005] | 0.090 0.059 0.157 0.019 [0.003] |
| Non-parametric bootstrap (without correction) | 0.092 0.040 0.194 0.007 [0.004] | 0.031 0.015 0.072 0.013 [0.002] |
| Wild bootstrap without null imposed | 0.142 0.069 0.311 -0.005 [0.006] | 0.078 0.050 0.144 0.013 [0.003] |
| Wild bootstrap with null imposed | 0.219 0.084 0.518 -0.240 [0.008] | 0.094 0.053 0.206 -0.067 [0.004] |

Notes: this table presents results for the simulation study described in Appendix A.7.1. For each type of assessment, we present the median, 10th percentile, and 90th percentile of the distribution of assessments across realizations of the original data. We also present the coefficient (and standard errors in brackets) of a regression of the assessment on the p-value in the original data in columns 4 and 8.