Quasi-Experimental Shift-Share Research Designs

Kirill Borusyak | Peter Hull | Xavier Jaravel

Princeton | U Chicago | LSE*

July 10, 2018

Abstract

Many empirical studies leverage shift-share (or "Bartik") instruments by combining a set of aggregate shocks with measures of differential shock exposure. We derive a necessary and sufficient shock-level orthogonality condition for such instruments to identify causal effects. We then show that this condition holds when shocks are as-good-as-randomly assigned, growing in number, and sufficiently dispersed in terms of average exposure. Our quasi-experimental framework suggests several tests of shift-share instrument validity, extends to settings with conditional random assignment or multiple sets of shocks, and highlights a possible inconsistency from the growing number of shocks – similar to that of two-stage least squares with many instruments – which may be addressed by split-sample estimation.

*Contact: borusyak@gmail.com, hull@uchicago.edu, and x.jaravel@lse.ac.uk. We thank Rodrigo Adão, Moya Chin, Paul Goldsmith-Pinkham, Edward Glaeser, Larry Katz, Michal Kolesár, Jack Liebersohn, Eduardo Morales, Jörn-Steffen Pischke, Isaac Sorkin, and Itzchak Tzachi Raz for helpful comments. An earlier version of this draft was circulated in May 2017 under the title *Consistency and Inference in Bartik Research Designs*.

1 Introduction

A large and growing number of empirical studies exploit variation in shift-share (or "Bartik") instruments, which combine a set of aggregate shocks with measures of differential shock exposure. Bartik (1991) and Blanchard and Katz (1992) are widely considered to have pioneered this research design in studies of local labor market dynamics. Autor, Dorn, and Hanson (2013, henceforth ADH), which we will use in this paper as a main illustrative example, is a more modern application. ADH combine industry-specific shocks to Chinese import competition, as measured in non-U.S. countries, with the industrial composition of U.S. labor markets. They use this shift-share instrument to estimate the effects of import competition on regional employment growth.[1]

Despite the popularity of shift-share instrumental variable (IV) regressions, relatively few papers have studied the formal conditions underlying their validity. A recent exception is Goldsmith-Pinkham et al. (2018), who argue that identification in shift-share designs hinges on the exogeneity of shock exposure. This interpretation of ADH, for example, requires industrial composition to be as-good-as-randomly assigned to U.S. labor markets. The Goldsmith-Pinkham et al. (2018) reasoning is based on a numerical equivalence between the shift-share estimator and an overidentified generalized method of moments (GMM) procedure that uses the exposure profile as a set of instruments, with the aggregate shocks determining the weighting matrix. While providing a coherent econometric framework for shift-share IV, this shares-as-instruments interpretation has nevertheless proved controversial.[2]

This paper develops a novel framework for understanding shift-share research designs, based on the exogeneity of the aggregate shocks themselves. We first show that the shift-share IV exclusion restriction is satisfied if and only if a simple orthogonality condition holds in the space of shocks (the industry space, for ADH). This condition requires shocks to be uncorrelated with a relevant shock-level unobservable: an average of untreated potential outcomes weighted by their exposure to a given shock. In the ADH example, the shift-share IV is therefore invalid when the growth of Chinese import competition, measured outside the U.S., is systematically different for industries concentrated in regions where employment is falling for other reasons. As with the Goldsmith-Pinkham et al. (2018) interpretation, our orthogonality condition follows from a numerical equivalence: the shift-share IV coefficient is equivalently obtained by a just-identified shock-level IV procedure, with shocks instrumenting for an exposure-weighted average of treatment in a regression of exposure-weighted average outcomes.

Using this characterization, we next propose a set of intuitive shock-level restrictions that satisfy

[1] While most shift-share designs study regional variation in outcomes and treatment, observations may also represent firms differentially exposed to foreign market shocks (Hummels et al., 2014), product groups demanded by different types of consumers (Jaravel, 2017), or groups of individuals facing different income growth rates (Boustan et al., 2013). Other influential and recent examples of shift-share IVs, spanning many settings and topics, include Blanchard and Katz (1992), Luttmer (2006), Card (2009), Saiz (2010), Kovak (2013), Nakamura and Steinsson (2014), Oberfield and Raval (2014), Greenstone et al. (2014), Diamond (2016), Suárez and Zidar (2016), and Hornbeck and Moretti (2018).

[2] See, e.g., Tim Bartik's comment on a recent online discussion: `http://blogs.worldbank.org/impactevaluations/rethinking-identification-under-bartik-shift-share-instrument`.

the shift-share orthogonality condition. The key requirement of our approach is that shocks are as-good-as-randomly assigned, as if arising from a natural experiment. However, quasi-random shock assignment is not, by itself, enough. The equivalence result shows that for the law of large numbers to apply to shift-share quasi-experiments, the sample of independent shocks must grow with the sample of observations. Moreover, even though our approach allows each observation to be mostly exposed to only a small number of shocks, on average shock exposure must be sufficiently dispersed such that no finite set of shocks drives variation in the shift-share instrument. Under these conditions, we show that the shift-share instrument is valid, even when shock exposure is endogenous.

Our shocks-as-instruments interpretation bears several new insights for shift-share estimation and inference in practice. First, we outline different validations of the quasi-experimental framework, such as shock-level balance tests, pre-trend checks, and tests for auto- and intra-class correlation of shocks. Second, we show how researchers can weaken the key quasi-experimental assumptions by controlling for exposure-weighted averages of shock-level observables. For example when shocks are naturally clustered into larger groups (such as industry sectors in ADH), controlling for measures of cluster exposure allows for endogenous group shocks and avoids inconsistency from observing only a small number of clusters. Third, we show that for the orthogonality condition to hold when – as in ADH – the exposure weights do not sum to one, researchers must either control for the sum or de-mean the aggregate shocks. Fourth, we derive optimal shift-share IV estimators when multiple sets of shocks (such as country-specific China import shocks in the ADH setting) satisfy the orthogonality condition, along with the corresponding omnibus specification test of overidentifying restrictions. Finally, we note a potential for inconsistency when the aggregate shocks are estimated within the IV estimation sample, as in Bartik (1991). This issue is closely related to the classic bias of two-stage least squares with many instruments, which Angrist et al. (1999) show can be overcome with split-sample IV estimation. Correspondingly, we recommend that researchers use split-sample estimates of aggregate shocks when constructing shift-share instruments.

Regarding inference, we follow Adao et al. (2018) in arguing that researchers should account for the variance of the quasi-experimental shocks, in addition to usual observation sampling variation. In particular we show that conventional standard error formulas applied to a modified version of the shock-level IV regression from our equivalence result coincide with the formulas in Adao et al. (2018), and can thus be used for valid inference under their assumptions. This result extends to conventional diagnostics of first-stage instrument strength and instrument balance, suggesting researchers may wish to use the industry-level regressions in practice.

We argue that in many applications this quasi-experimental shock framework may be better aligned with researchers' motivations and goals, relative to the shares-as-instruments interpretation. For example Hummels et al. (2014), who combine country-by-product supply shocks with lagged firm-specific exposure to sources of intermediate inputs, motivate their approach by writing:

> "While these shocks are exogenous to Danish firms, their impact varies markedly across firms [...]. That is, if only one Danish firm buys titanium hinges from Japan, *idiosyncratic shocks* to the supply or transport costs of those hinges affects just that one firm." (p. 1598; emphasis added)

Similarly, ADH construct an instrument for Chinese import penetration in U.S. regions using the industry growth of Chinese imports in other developed economies. This research design attempts to purge Chinese industry import penetration from U.S.-specific factors, which one may view as an attempt to obtain quasi-random variation in the industry space. We emphasize, however, that the shocks-as-instruments interpretation may be less appropriate for some applications, particularly those involving only a small number of shocks.[3]

Econometrically, our approach is related to the Kolesar et al. (2015) study of consistency in IV designs with many invalid instruments. Identification in that setting follows when violations of individual instrument exclusion restrictions are uncorrelated with their first-stage effects. Here, per Goldsmith-Pinkham et al. (2018), the shock exposure measures can be thought of as a set of invalid instruments, while our key orthogonality condition requires their exclusion restriction violations to be uncorrelated with the aggregate shocks, rather than with their first-stage effects.

Our work also relates to other recent methodological studies of shift-share designs, including Jaeger et al. (2018) and Broxterman and Larson (2018). The former highlights biases of shift-share IV due to endogenous local labor market dynamics, while the latter explores the empirical performance of different shift-share instrument constructions. More broadly, our paper adds to a growing literature seeking to interpret, test, and extend the high-level assumptions of common applied research designs, including, among others, Goldsmith-Pinkham et al. (2018) for shift-share designs, Borusyak and Jaravel (2016) for event study designs, de Chaisemartin and D'Haultfoeuille (2018) for instrumented difference-in-difference designs, and Hull (2018) for mover designs.

The rest of this paper is organized as follows. Section 2 introduces the framework and notes the equivalence between conventional shift-share IV and a particular shock-level IV estimator. Section 3 then derives and interprets the key orthogonality condition, and Section 4 proposes quasi-experimental assumptions under which it is satisfied. Section 5 discusses inference and tests of the quasi-experimental framework, while Section 6 outlines extensions and practical implications, including the potential for inconsistency with many estimated shocks. Section 7 concludes.

[3]For example, Card (2001) and Acemoglu and Restrepo (2017) each construct a shift-share instrument with around twenty shocks. The Goldsmith-Pinkham et al. (2018) approach, which does not require a large shock sample, may thus be more appropriate framework for understanding these designs.

2 Shocks as Instruments in Shift-Share Designs

Suppose we observe an outcome y_ℓ, a treatment x_ℓ, and a vector of controls w_ℓ (which includes a constant) in a random sample of size L. For concreteness, we refer to the sampled units as "locations," as they are in Bartik (1991), ADH, and many other shift-share applications. We wish to estimate the causal effect of the treatment β, assuming a linear constant-effect model

$$y_\ell = \beta x_\ell + w_\ell' \gamma + \varepsilon_\ell, \tag{1}$$

where $\mathbb{E}[\varepsilon_\ell] = \mathbb{E}[w_\ell \varepsilon_\ell] = 0$.[4] Here ε_ℓ denotes the residual from projecting the untreated potential outcome of location ℓ – that is, the outcome we would observe there if treatment were set to zero – on the controls w_ℓ. In the ADH setting, for example, y_ℓ and x_ℓ denote the growth rates of employment and Chinese import penetration of local labor market ℓ, while w_ℓ contains measures of labor force demographics from a previous period.[5] The residual ε_ℓ thus contains all factors that are uncorrelated with lagged demographics but which would drive local employment growth in the absence of rising Chinese imports.

In writing equation (1) we do not require realized treatment x_ℓ to be uncorrelated with potential outcomes ε_ℓ, in which case the causal parameter β would be consistently estimable by ordinary least squares (OLS). Instead, we assume that, along with $(y_\ell, x_\ell, w_\ell')'$, we observe a set of N aggregate shocks g_n and weights $s_{\ell n} \geq 0$ predicting the exposure of location ℓ to each shock. Using these, we construct a shift-share instrument z_ℓ as the exposure-weighted average of shocks:

$$z_\ell = \sum_{n=1}^{N} s_{\ell n} g_n. \tag{2}$$

Here z_ℓ can thus be thought of as a predicted local shock for location ℓ. In the ADH design, g_n denotes industry n's average Chinese import penetration growth across eight developed non-U.S. economies, while $s_{\ell n}$ is the lagged employment share of industry n in location ℓ, as measured in a base period. The shift-share instrument z_ℓ is thus interpreted as a predicted local "China shock." Again for concreteness we refer to the space of shocks as "industries," as in Bartik (1991) and ADH. To start simply we first assume that the sum of exposure weights across industries is constant, i.e. $\sum_{n=1}^{N} s_{\ell n} = 1$, and treat the aggregate shocks g_n as known. In Section 6 we relax both of these assumptions.

The shift-share IV estimator $\hat{\beta}$ uses z_ℓ to instrument for x_ℓ in equation (1). Letting $v_\ell^\perp$ denote the

[4] It is straightforward to extend our framework to models with heterogeneous treatment effects. As shown in Appendix A, the shift-share IV estimator in general captures a convex average of location-specific linear effects β_ℓ under an additional first stage monotonicity assumption, as with the local average treatment effects of Angrist and Imbens (1994). This follows similarly to the results on heterogeneous effects in ordinary least square shift-share regressions considered by Adao et al. (2018).

[5] Because ADH do not observe imports by region, they proxy local import penetration in each industry by the national average to construct x_ℓ. We abstract from this issue in our discussion of their setting.

residual from the sample projection of a variable v_ℓ on the controls w_ℓ, we can write

$$\hat{\beta} = \frac{\frac{1}{L}\sum_{\ell=1}^{L} z_\ell y_\ell^\perp}{\frac{1}{L}\sum_{\ell=1}^{L} z_\ell x_\ell^\perp}, \tag{3}$$

where the numerator and denominator represent location-level covariances between the instrument and the residualized outcomes and treatment, respectively.

To build intuition for how identification in shift-share designs may come from the aggregate shocks, we first show that $\hat{\beta}$ can also be expressed as an industry-level IV estimator that uses the shocks as an instrument. Changing the order of summation across locations and industries in both the numerator and denominator of (3), we have

$$\begin{aligned}\hat{\beta} &= \frac{\frac{1}{L}\sum_{\ell=1}^{L}\sum_{n=1}^{N} s_{\ell n} g_n y_\ell^\perp}{\frac{1}{L}\sum_{\ell=1}^{L}\sum_{n=1}^{N} s_{\ell n} g_n x_\ell^\perp} \\ &= \frac{\sum_{n=1}^{N} \hat{s}_n g_n \bar{y}_n^\perp}{\sum_{n=1}^{N} \hat{s}_n g_n \bar{x}_n^\perp},\end{aligned} \tag{4}$$

where the weights $\hat{s}_n = \frac{1}{L}\sum_{\ell=1}^{L} s_{\ell n}$ measure the average exposure of locations to industry n and where $\bar{v}_n = \sum_{\ell=1}^{L} s_{\ell n} v_\ell / \sum_{\ell=1}^{L} s_{\ell n}$ denotes a weighted average of variable v_ℓ, with larger weights given to locations more exposed to industry n.[6] Equation (4) thus shows that the shift-share IV estimator is numerically equivalent to the coefficient from an s_n-weighted regression of $\bar{y}_n^\perp$, the average residualized outcome of locations specializing in n, on the same weighted average of residualized treatment, $\bar{x}_n^\perp$, instrumented by g_n.

In the ADH example, it is expected that industries with a high non-U.S. China shock g_n would be concentrated in U.S. regions facing increasing Chinese import penetration, so that the first stage covariance $\sum_{n=1}^{N} \hat{s}_n g_n \bar{x}_n^\perp$ is positive. By forming the reduced form numerator of (4) the researcher learns whether these industries are also concentrated in areas with large declines in employment. As usual for the ratio of these covariances (and thus $\hat{\beta}$) to reveal a causal effect, a particular industry-level exclusion restriction must hold. We next derive this restriction.

3 Shock Orthogonality

We seek conditions under which the shift-share IV estimator $\hat{\beta}$ is consistent for the causal parameter β; that is, $\hat{\beta} \xrightarrow{p} \beta$ as $L \to \infty$. As usual, IV consistency requires both instrument relevance (that z_ℓ and x_ℓ are asymptotically correlated, controlling for w_ℓ) and validity (that z_ℓ is asymptotically uncorrelated with ε_ℓ). Since relevance can be inferred from the data, we assume throughout that it is satisfied and focus our attention on validity. Applying the logic of (4) to the population covariance

[6] The weighted covariance interpretation follows from observing that the weighted means of $\bar{y}_n^\perp$ and $\bar{x}_n^\perp$ are zero: e.g. $\sum_{n=1}^{N} \hat{s}_n \bar{y}_n^\perp = \frac{1}{L}\sum_{\ell=1}^{L}\left(\sum_{n=1}^{N} s_{\ell n}\right) y_\ell^\perp = \frac{1}{L}\sum_{\ell=1}^{L} y_\ell^\perp = 0$, since the $y_\ell^\perp$ are regression residuals.

between the shift-share instrument and untreated potential outcomes, we have

$$\begin{aligned}\mathrm{Cov}\left[z_\ell, \varepsilon_\ell\right] &= \mathbb{E}\left[\sum_{n=1}^{N} s_{\ell n} g_n \varepsilon_\ell\right] \\ &= \sum_{n=1}^{N} s_n g_n \phi_n,\end{aligned} \tag{5}$$

where $s_n \equiv \mathbb{E}\left[s_{\ell n}\right]$ measures the expected exposure to industry n, and where $\phi_n \equiv \mathbb{E}\left[s_{\ell n}\varepsilon_\ell\right]/\mathbb{E}\left[s_{\ell n}\right]$ is an exposure-weighted expectation of untreated potential outcomes.[7] Given a law of large numbers, i.e. $\frac{1}{L}\sum_{\ell=1}^{L} z_\ell \varepsilon_\ell - \mathrm{Cov}\left[z_\ell, \varepsilon_\ell\right] \xrightarrow{p} 0$, the shift-share IV estimator is therefore consistent if and only if

$$\sum_{n=1}^{N} s_n g_n \phi_n \to 0. \tag{6}$$

Equation (6) is our key orthogonality condition. The left-hand side represents a covariance (measured in the set of N industries and weighted by s_n) between two variables: aggregate shocks g_n and industry-level unobservables ϕ_n. Thus in ADH, equation (6) characterizes the large-sample behavior of a covariance between industry-specific measures of Chinese import penetration (g_n) and a weighted average of unobserved factors affecting the employment growth of locations specializing in each industry (ϕ_n).

The industry-level orthogonality condition helps to formalize intuitive arguments in the ADH design. One may, for example, be concerned that orthogonality fails due to reverse causality: China may gain market shares in certain industries precisely because these industries, and thus locations specializing in them, are not performing well (as reflected in ϕ_n). Measuring import penetration outside of the U.S. addresses such a concern, to the extent that the underlying performance of U.S. and non-U.S. industries are not correlated. In addition, equation (6) helps understand potential concerns about omitted variable bias, because of either unobserved industry shocks or regional unobservables that are correlated with industrial composition. For example, industries with a higher growth of competition with China may also be more exposed to certain technological shocks (like automation), or these industries could happen to have larger employment shares in regions that are affected by other employment shocks (like low-skill immigration). These too would be captured by ϕ_n.

Before proceeding, three general points on the orthogonality condition are worth highlighting. First, note that the weight each industry receives in the covariance (5) is the expected shock exposure s_n: for example, in the ADH setting s_n is the average employment share of industry n in the population. In practice researchers using employment shares as the exposure measure sometimes weight

[7] For initial simplicity in this section we derive the validity condition given fixed sequences of the set of (g_n, s_n, ϕ_n). In the quasi-experimental framing it is more natural to imagine a hierarchical sampling design, in which sets are first drawn from a larger population. All expectations and covariances in this section should then be thought to be conditional on the industry-level draws, with validity satisfied by $\sum_{n=1}^{N} s_n g_n \phi_n \xrightarrow{p} 0$. We formalize this framework in Section 4.

shift-share IV regressions by total location employment (e.g., Card (2009) and ADH themselves). In this case s_n has a more intuitive interpretation: the lagged employment of industry n. Second, note that local variation in industry exposure plays an important role in the orthogonality condition; if all locations have the same exposure to a given industry n, i.e. $s_{\ell n} = s_n$, then $\phi_n = \mathbb{E}[\varepsilon_\ell] = 0$ and this industry will not contribute to (6). Finally, note that if the exposure measures are as-good-as-randomly assigned, i.e. if $\mathbb{E}[s_{\ell n}\varepsilon_\ell] = 0$ for all n, then $\phi_n = 0$ so that equation (6) is satisfied for any shocks g_n. This is the preferred interpretation of shift-share IV in Goldsmith-Pinkham et al. (2018).

4 Quasi-Experimental Shock Assignment

When might the shift-share orthogonality condition be satisfied when exposure shares are endogenous? In this section we develop a framework in which the aggregate shocks are as-good-as-randomly assigned with respect to the relevant industry-level unobservable. We also give guidance on which individual controls a researcher might include to weaken the key quasi-experimental assumption in practice.

To formalize the shock quasi-experiment, we first take a step back. Section 3 viewed industry characteristics, including the aggregate shocks, as fixed given a sample of size L. We now imagine a hierarchical data-generating process, in which given the set of industry exposure weights $s_1, \ldots, s_N$ the set of aggregate shocks g_n and industry-level unobservables ϕ_n are drawn from some distribution. Our quasi-experimental framework assumes that shocks are drawn orthogonally from unobservables, while placing no restrictions on the joint distribution of ϕ_n itself. For example, in ADH we may imagine industry import shocks g_n arising quasi-randomly from an unanticipated policy or productivity change in China.

Specifically, our baseline case makes two assumptions on the industry-level data-generating process:

A1. Shocks are mean-independent of ϕ_n, with the same mean: $\mathbb{E}[g_n \mid \phi_n] = \mu$

A2. Shocks are mutually mean-independent, conditional on $\phi_1, \ldots, \phi_N$, and the Herfindahl index of industry exposure weights converges to zero: $\sum_{n=1}^{N} s_n^2 \to 0$

Under these and some weak regularity conditions, the orthogonality condition holds and the shift-share instrument is valid.[8]

Our key quasi-experimental assumption A1 states that the aggregate shocks are as-good-as-randomly assigned, as formalized by mean-independence with respect to the industry-level unobservables ϕ_n. The second assumption A2 then ensures that a law of large numbers applies to the weighted covariance (5) so that it satisfies the orthogonality condition asymptotically. Since $\sum_{n=1}^{N} s_n^2 \geq 1/N$, the

[8]Formally, we assume that the support of the distribution of ϕ_n is bounded by $[-P, P]$ and that $\text{Var}[g_n] < V$ for finite P and V. Then A1 implies $\mathbb{E}\left[\sum_n s_n\phi_n g_n\right] = \sum_n \mathbb{E}[s_n\phi_n]\mu = \mathbb{E}[\varepsilon_\ell]\mu = 0$, while by A2 and the Cauchy-Schwartz inequality $\text{Var}\left[\sum_n s_n\phi_n g_n\right] = \mathbb{E}\left[\left(\sum_n s_n\phi_n g_n\right)^2\right] \leq \sum_n \mathbb{E}\left[s_n^2\phi_n^2\right]\text{Var}[g_n] \leq PV\sum_n s_n^2 \to 0$. This guarantees L^2-convergence and thus weak convergence: $\sum_n s_n\phi_n g_n \xrightarrow{p} 0$.

restriction on exposure concentration implies that the number of industries grows with the sample, while the mutual mean-independence condition implies that additional shocks draw (5) closer to zero when A1 holds.[9] The concentration condition is stated in terms of the Herfindahl index but this is not the only possibility; since $\sum_{n=1}^{N} s_n^2 \leq \max_n s_n$, it is sufficient that the largest industry becomes vanishingly small as the sample grows.

As with other quasi-experimental designs, researchers may wish to assume that A1 and A2 only hold conditionally on a set of observables. For example, one could weaken the mean-independence restriction of A1 to allow the conditional mean of shocks to depend linearly on a vector of industry-level observables q_n,

$$\mathbb{E}\left[g_n \mid \phi_n, q_n\right] = \mu + q_n' \tau, \tag{7}$$

and similarly assume the mutual mean-independence condition of A2 holds for the residual $g_n^* = g_n - \mu - q_n'\tau$. In this case it is straightforward to use only the residual shock variation for identification by adding an exposure-weighted vector of industry controls $\sum_{n=1}^{N} s_{\ell n} q_n$ to the control vector w_ℓ. As shown in Appendix B, the resulting estimator is equivalent to a two-step procedure in which the residuals g_n^* are first estimated by a weighted regression of g_n on q_n and then used to construct a new shift-share instrument $\hat{z}_\ell^* = \sum_n s_{\ell n} \hat{g}_n^*$. Of course, researchers may also directly control for q_n in the equivalent industry-level regression (4) to weaken A1 and A2.

An intuitive application of this result is found in the case where industries are grouped into bigger clusters $c(n) \in \{1, \ldots, C\}$; for example, detailed industries in ADH can be grouped into larger industrial sectors. Here researchers may be more willing to assume that the aggregate shocks are exogenously assigned within clusters, but that the cluster-average shock is endogenous. With q_n including a set of cluster indicators, the shift-share IV would then be valid with the researcher controlling for the individual exposure to each cluster (e.g. the local employment shares of each sector in ADH), $s_{\ell c} = \sum_{n=1}^{N} s_{\ell n} \mathbf{1}\left[c(n) = c\right]$.[10] Interestingly, the same approach also relaxes A2. If shocks have a cluster-specific component, $g_n = \mu + g_{c(n)} + g_n^*$, and the number of clusters is small, then the law of large numbers can apply to g_n^* but not to g_n, even if the g_c are also as-good-as-randomly assigned. There are therefore two distinct reasons to control for cluster exposure in shift-share IV: either to remove a non-random component of the shocks or to remove a random component that causes shocks to be too correlated with one another asymptotically.

To conclude this section, we note that the quasi-experimental assumptions generalize to a panel setting in which outcomes, treatment, and shocks are observed for the same location over multiple periods. This setup, which we formally develop in Appendix C, delivers two insights. First, when the analogs of assumptions A1 and A2 hold (implying that shocks are mean-independent across both locations and time periods), identification can come either from having many industries N or from

[9] Note that while A1 and A2 only restrict industry-level variables, we still also require a law of large numbers to hold in the sample of locations; namely, we need both $L \to \infty$ and $N \to \infty$.

[10] Recently, Jaravel (2017) and Garin and Silverio (2017) follow this strategy.

observing a finite number of industries over many periods. Second, researchers can relax the two key assumptions by including location and time fixed effects in the regression: aggregate shocks are then allowed to be correlated with time-invariant aggregated local unobservables (even if exposure varies over time) and may have time-varying means. Controls can also be constructed to extract the idiosyncratic component from serially correlated shocks when their stochastic process belongs to a known parametric class, such as an autoregressive model.

5 Inference and Testing

When the aggregate shocks are random and growing in number, as in A1 and A2, conventional standard errors may fail to capture the asymptotic variance of the shift-share IV estimator. This point has been recently made by Adao et al. (2018), who derive corrected standard error formulas for such cases. Intuitively, the quasi-random assignment of g_n guarantees that the orthogonality condition holds in expectation but not in any given sample of N industries; when $N \ll L$, the finite-sample correlation between g_n and ϕ_n may be much more important than the location sampling variation targeted by conventional standard errors. In simulations Adao et al. (2018) show that a failure to account for this correlation can lead to large distortions in the coverage probabilities of standard confidence intervals.

Fortunately, the equivalence result (4) provides a straightforward path to correct quasi-experimental shift-share inference. In Appendix D we show that conventional standard error formulas from the industry-level regression of $\bar{y}_n^{\perp}$ on $\bar{x}_n^{\perp}$, instrumented by g_n and weighted by $\hat{s}_n$, match those derived in Adao et al. (2018) when w_ℓ includes just a constant. With additional controls, a slight modification is required: the shocks have to be residualized by regressing the residualized instrument, $z_\ell^{\perp}$, on the vector of shares and keeping the coefficients $\tilde{g}_n$. Computing $\hat{\beta}$ in this way via standard statistical software can therefore lead to valid inference under A1, A2, and the additional regularity conditions of the Adao et al. (2018) paper. This result also applies in the case of clustered standard errors in the industry-level regression; shocks may then have a random cluster component, yielding valid inference under a weaker form of A2. Notably, the Adao et al. (2018) standard errors remain valid even when untreated potential outcomes are themselves clustered by location, such as with the state-level clustering scheme of ADH.

In practice researchers may wish to exploit these results by estimating shift-share IV coefficients via the equivalent industry-level regression. An added benefit of such an approach is that it also sidesteps inferential issues with standard IV diagnostics, such as tests of instrument relevance and validity. For example, the usual first stage F-statistic test for weak instruments may be invalid when estimated at the location level, while conventional standard errors applied to the $\hat{s}_n$-weighted first stage regression of $\bar{x}_n^{\perp}$ on g_n (or $\tilde{g}_n$), and the corresponding F-statistic, are correct. Researchers should take note, however, that in the case of no location-level controls these computational shortcuts only apply when the industry-level regressions includes a constant term, even though this does not change the shift-share regression coefficient. Additional modifications with the same property are required in the case of controls – see Appendix D for details.

The industry-level approach is also useful for validating the identification assumptions A1 and A2. Suppose a researcher observes a variable r_ℓ that is plausibly correlated with untreated potential outcomes ε_ℓ and wishes to indirectly verify the orthogonality condition by testing $\text{Cov}\left[z_\ell, r_\ell^\perp\right] = 0$. For example, Appendix C shows that lagged outcomes are a natural candidate for r_ℓ in the panel setting, leading to familiar pre-trend tests. A valid industry-level balance test would regress the exposure-weighted average, $\bar{r}_n^\perp = \sum_{\ell=1}^L s_{\ell n} r_\ell^\perp / \sum_{\ell=1}^L s_{\ell n}$, on shocks (or residualized shocks) and weight by $\hat{s}_n$. Of course, instrument balance on industry-level observables thought to be correlated with ϕ_n may also be tested this way. When there are naturally-occurring clusters of industries, researchers may also validate A2 at the industry level by testing that the intra-class correlation of shocks is zero. When intra-class correlation is present, they may choose to control for cluster exposure, per the previous section. Alternatively a modified version A2 can be employed, with shocks independent across clusters, the number of clusters growing large with the sample size, and the concentration of cluster exposure becoming small. Clustered industry-level standard errors must be used in this case.

6 Extensions

In this section we consider three extensions to the basic shift-share IV approach. First, we relax the assumption that the exposure weights sum to one in each location. Second, we consider the case where multiple sets of shocks satisfy the industry-level orthogonality condition. Finally, we note the possibility of inconsistency in shift-share designs when the aggregate shocks are estimated in the IV sample, as well as a possible solution via split-sample estimation. In each case we highlight implications for practitioners and links to existing shift-share applications.

6.1 Incomplete Shares

In some settings the sum of exposure measures $S_\ell = \sum_{n=1}^N s_{\ell n}$ varies across locations. For example, in ADH the set of $s_{\ell n}$ correspond to lagged employment shares of manufacturing industries only, so that S_ℓ denotes the lagged share of manufacturing in local labor market ℓ. We show that to invoke the quasi-experimental framework in such cases, a researcher must either include S_ℓ into the set of location controls w_ℓ or first de-mean the aggregate shocks in a particular way.

To develop intuition for this result, suppose that the expected value of shocks, μ, is positive. Then locations with higher S_ℓ will tend to have higher values of the instrument $z_\ell = \sum_{n=1}^N s_{\ell n} g_n$, even when shocks are randomly assigned. Moreover, places with high S_ℓ (i.e. those with more manufacturing in ADH) may be differentially affected by other factors, undermining identification. To see this issue another way, note that the shift-share instrument can always be written $z_\ell = \sum_{n=1}^N s_{\ell n} g_n + s_{\ell 0} g_0$, where $s_{\ell 0} = 1 - S_\ell$ is location ℓ's exposure to the missing industry (non-manufacturing in ADH), $g_0 = 0$ is the shock to that industry, and the total exposure sums to one: $\sum_{n=1}^N s_{\ell n} + s_{\ell 0} = 1$. When the other shocks g_n are not mean-zero and the missing industry's weight $s_0 = E[s_{\ell 0}]$ remains large, its contribution to the industry-level covariance (5) violates the orthogonality condition. Controlling for the share of the missing industry, or equivalently for S_ℓ, solves the problem, as does recentering shocks to be mean-zero.

Formally, the orthogonality condition (6) requires $\sum_{n=1}^N s_n g_n \phi_n$ to converge to zero. Assumption A2 implies that as the sample grows, this sum converges to its expectation, which under A1 is

$$\begin{aligned} \mathbb{E}\left[\sum_{n=1}^N s_n g_n \phi_n\right] &= \mu \cdot \sum_{n=1}^N s_n \frac{\mathbb{E}\left[s_{\ell n}\varepsilon_\ell\right]}{\mathbb{E}\left[s_{\ell n}\right]} \\ &= \mu \cdot \mathbb{E}\left[S_\ell \varepsilon_\ell\right], \end{aligned} \tag{8}$$

where μ is the expected value of shocks. The expectation in (8) is zero only when the structural residual ε_ℓ is uncorrelated with the total exposure S_ℓ or when the shock mean μ is zero. The former is mechanically guaranteed when $S_\ell = 1$ or when S_ℓ is included in the list of controls, while the latter holds when shocks are recentered.

Although in principle it is not important how shocks are de-meaned, the numerical equivalence result (4) again suggests a straightforward approach via the industry-level regression. Namely, including a constant in the $\hat{s}_n$-weighted and g_n-instrumented regression of $\bar{y}_n^\perp$ on $\bar{x}_n^\perp$ automatically re-centers shocks to also have a weighted mean of zero, thus solving the incomplete shares issue. We recommend researchers include a constant whenever estimating the industry-level regression. Without incomplete shares ($S_\ell = 1$) or when S_ℓ varies but is controlled for, it is innocuous: the coefficient estimate will not change since weighted means of both $\bar{y}_n^\perp$ and $\bar{x}_n^\perp$ are zero. As discussed above, it may also be important to include a constant to ensure standard software packages produce correct standard errors.

6.2 Multiple Shocks

In some shift-share designs researchers may have access to multiple aggregate shocks plausibly satisfying A1 and A2. For example, while ADH measure Chinese import penetration by its average across eight non-U.S. countries, one may think that each set of country-specific growth rates g_{nk} for $k = 1, \ldots, 8$ is itself as-good-as-randomly assigned with respect to U.S. industry-level unobservables ϕ_n, and thus each may be used to consistently estimate β. In other settings additional shocks may be generated via non-linear transformations of the original g_n. As usual, such overidentification of β raises the possibility of an efficient GMM estimator which optimally combines the quasi-experimental variation, as well as a Hansen (1982) omnibus test of the identifying assumptions. Here we show that in the quasi-experimental framework such estimators and tests differ from those usually run at the location level, but are again easily produced with certain industry-level regressions.

Suppose a researcher uses multiple shocks to form K shift-share instruments $z_{\ell k} = \sum_{n=1}^{N} s_{\ell n} g_{nk}$. A typical overidentified IV procedure for estimating β is the two stage least squares (2SLS) regression of y_ℓ on x_ℓ, controlling for w_ℓ and instrumenting by $z_{\ell k}$. The resulting estimate can be interpreted as a weighted average of the instrument-specific estimates and is thus consistent when the orthogonality condition (6) holds for each of the K sets of shocks. However, it is known to only be the efficient estimate when the vector of structural errors $\varepsilon = (\varepsilon_1, \dots, \varepsilon_L)'$ is spherical conditional on the instrument matrix z, i.e. when $\mathbb{E}\left[\varepsilon\varepsilon' \mid z\right] = \sigma_\varepsilon^2 I$. This condition is unlikely to hold in the shift-share setting, even when shocks are independently assigned, as locations with similar exposure profiles to observed shocks may also be exposed to similar unobserved disturbances (Adao et al., 2018).[11]

To characterize the optimal GMM estimator of β, we again note and leverage a numerical equivalence. Namely, the location-level moment function based on the validity of the shift-share instrument vector $z_\ell = (z_{\ell 1}, \dots z_{\ell K})'$ and the structural error ε_ℓ can be rewritten

$$\begin{aligned} m(b) &= \sum_{\ell=1}^{L} z_\ell (y_\ell^\perp - b x_\ell^\perp) \\ &= \sum_{n=1}^{N} \hat{s}_n g_n (\bar{y}_n^\perp - b \bar{x}_n^\perp), \end{aligned} \tag{9}$$

where g_n is a $K \times 1$ vector collecting shocks g_{nk}. This corresponds to a weighted industry-level moment function, exploiting the asymptotic orthogonality of shocks g_n and industry-level unobservables ϕ_n. The optimal GMM estimator using $m(b)$ is then given by

$$\hat{\beta}^* = \arg\min_b m(b)' W^* m(b), \tag{10}$$

where W^* is a consistent estimate of the inverse asymptotic variance of $m(\beta)$'s limiting distribution.

As in Section 5, the asymptotic theory of Adao et al. (2018) can be used to characterize W^*. In the simple case of no controls and homoskedastic shocks, i.e. $\mathrm{Var}\left[g_n \mid \phi_n\right] = \mathrm{Var}\left[g_n\right]$, that theory shows that the optimal moment-weighting matrix is proportional to the inverse variance of shocks. In this case rearranging (10) shows that the optimal shift-share estimator is equivalent to an unweighted industry-level 2SLS regression of $\hat{s}_n \bar{y}_n^\perp$ on $\hat{s}_n \bar{x}_n^\perp$, using shocks as instruments.[12] Naturally, when $K = 1$

[11] Note that this 2SLS estimator also differs from the optimal IV estimator in the Goldsmith-Pinkham et al. (2018) setting of exogenous shares, even under homoskedasticity. The 2SLS regression of y_ℓ on x_ℓ controlling for w_ℓ and instrumenting by $s_{\ell 1}, \dots, s_{\ell N}$ will in general not involve growth rates at all. For example, when treatment has a shift-share structure, the first stage of 2SLS with shares as instruments produces perfect fit, and so shares-as-instruments 2SLS is the same as OLS. This almost corresponds to the ADH case (see footnote 5), except that they use employment shares from different years in constructing the instrument and treatment.

[12] Letting P_g denote the industry-level matrix projecting onto the vector of shocks and a constant, we have $\hat{\beta}^* = \arg\min_b \left(\sum_n \left(\hat{s}_n \bar{y}_n^\perp - b \hat{s}_n \bar{x}_n^\perp\right) g_n'\right) W^* \left(\sum_n g_n \left(\hat{s}_n \bar{y}_n^\perp - b \hat{s}_n \bar{x}_n^\perp\right)\right) = \left(\bar{x}^{\perp\prime} \hat{s} P_g \hat{s} \bar{x}^\perp\right)^{-1} \bar{x}^{\perp\prime} \hat{s} P_g \hat{s} \bar{y}^\perp$ when W^* estimates the inverse sample variance of shocks, where here $\bar{x}^\perp$ and $\bar{y}^\perp$ collect observations of $\bar{x}_n^\perp$ and $\bar{y}_n^\perp$ and where $\hat{s}$ is a

and the model is just-identified this reduces to the earlier IV estimator (4).

The industry-level regression interpretation of $\hat{\beta}^*$ extends when there are controls or when shocks are heteroskedastic; in the latter case the IV estimator of White (1982) takes the place of 2SLS. In all cases, the minimized criteria function in (10) yields an omnibus chi-squared overidentification test, with $K-1$ degrees of freedom.[13] As before, these estimates and test statistics are straightforward to compute with standard statistical software at the industry level.

6.3 Estimated Shocks

So far we have assumed that shift-share researcher directly observes the set of aggregate shocks. In practice, however, shocks are typically estimated, often within the IV estimation sample. For example, Bartik (1991) uses national industry employment growth rates as shocks, which he estimates by the sample average growth of industry employment across observed locations, before instrumenting local employment growth in a regression of wage growth for the same location sample. Here we show that with the many shocks required by the quasi-experimental approach, such two-step estimation may lead to inconsistency of the shift-share IV estimator even if the orthogonality condition holds. Intuitively, this bias arises from the fact that shock estimation error need not vanish asymptotically and may be systematically correlated with the untreated potential outcome residual in large samples.

To illustrate the issue simply, we return to the case of a single set of shocks g_n, which we suppose the researcher estimates via a weighted average of observed variables $g_{\ell n}$.[14] That is, given weights $\omega_{\ell n}$, she computes

$$\hat{g}_n = \sum_{\ell=1}^{L} \frac{\omega_{\ell n} g_{\ell n}}{\sum_{m=1}^{L} \omega_{mn}}. \tag{11}$$

For example, in Bartik (1991) $g_{\ell n}$ is the local employment growth rate of industry n in location ℓ, with $\omega_{\ell n}$ denoting the local lagged level of industry employment. The researcher then uses $\hat{g}_n$ to form a feasible shift-share instrument $\hat{z}_\ell = \sum_{n=1}^{N} s_{\ell n}\hat{g}_n = z_\ell + \psi_\ell$, where we define

$$\psi_\ell = \sum_{n=1}^{N} s_{\ell n}(\hat{g}_n - g_n) \tag{12}$$

as a weighted average of industry-level estimation error $\hat{g}_n - g_n$. When the orthogonality condition for the infeasible shift-share instrument z_ℓ holds, validity of the feasible instrument $\hat{z}_\ell$ requires an additional condition:

$$\operatorname{Cov}\left[\psi_\ell, \varepsilon_\ell\right] \to 0, \tag{13}$$

$N \times N$ diagonal matrix of the $\hat{s}_n$. This corresponds to the formula for the above industry-level 2SLS regression.

[13] As usual, rejections of the overidentification test may come either from model misspecification or from different weightings of heterogeneous treatment effects across different shocks. See Appendix A for a derivation of shock-specific weights.

[14] For notational simplicity, we also return to the convention of treating the shocks g_n as fixed.

or that the measurement error ψ_ℓ is asymptotically uncorrelated with the structural residual ε_ℓ.

When might this condition fail? Note that we can rewrite (12) as the sum of two terms,

$$\psi_\ell = \sum_{n=1}^{N} s_{\ell n} \frac{\omega_{\ell n}(g_{\ell n} - g_n)}{\sum_{m=1}^{L} \omega_{mn}} + \sum_{n=1}^{N} s_{\ell n} \frac{\sum_{k \neq \ell} \omega_{kn}(g_{kn} - g_n)}{\sum_{m=1}^{L} \omega_{mn}}, \tag{14}$$

where the first term captures location ℓ's own contribution to estimation error $\hat{g}_n - g_n$ and the second is the contribution of all other locations. For $\text{Cov}[\psi_\ell, \varepsilon_\ell] \not\to 0$, it is sufficient for the first term to be systematically correlated with the structural error, though in principle both may be. Intuitively the first term may be asymptotically non-ignorable if, as the number of aggregate shocks grows large, the number of observations determining the $\hat{g}_n$ estimate through the weights $\omega_{\ell n}$ remains small. If these observations also have a large exposure to g_n (as measured by $s_{\ell n}$) with deviations $g_{\ell n} - g_n$ that are systematically correlated with ε_ℓ, the feasible shift-share IV estimator will be inconsistent. In the case of Bartik (1991), where both $\omega_{\ell n}$ and $s_{\ell n}$ reflect the size of industry n in location ℓ, condition (13) may thus fail if regions with faster untreated potential wage growth also see faster employment growth in their dominant industries. This is exactly the sort of endogeneity originally motivating the use of shift-share IV.

Inconsistency from many estimated shocks is analogous to the bias of conventional 2SLS estimation with many instruments. Appendix E makes this link explicit by considering the special case of an "examiner" or "judge" design, in which each location is exposed to only one shock and the shares (e.g., examiner dummies) are used as instruments for treatment. First-stage fitted values in this setting are examiner group-specific averages of treatment, so that 2SLS can be thought of as a shift-share IV estimator in which shocks are given by group-specific expectations of treatment.[15] Here $|\text{Cov}[\psi_\ell, \varepsilon_\ell]|$ is proportional to N/L, aligning with the original many-instrument 2SLS bias term of Nagar (1959). This bias persists when the number of industries (or examiner groups) is non-negligible relative to the sample size. While stark, the examiner example may be a reasonable approximation to many shift-share designs where locations tend specialize in a few industries, so that the exposure shares resemble "fuzzy" industry group assignment.[16]

Fortunately, as with the conventional bias of 2SLS (Angrist and Krueger (1995); Angrist et al. (1999)), this issue may have a simple solution in the form of sample splitting. Rather than using all observations to both estimate shocks and the shift-share IV coefficient, suppose the researcher randomly partitions the sample for these two distinct purposes. At the extreme, we could imagine

[15] Recent examples of examiner designs include Chetty et al. (2011), Maestas et al. (2013), Doyle et al. (2015), and Dobbie et al. (2018). Notably, Kolesar et al. (2015) study the Chetty et al. (2011) design under the assumption that examiner groups are invalid instruments for treatment, leveraging an orthogonality condition similar to ours. Our results in this section can thus be thought to generalize this analysis to settings where shocks are not given by the group-level expectation of treatment.

[16] Indeed, for the asymptotic variance of the shift-share instrument to be non-degenerate, exposure shares must be sufficiently concentrated in a small number of industries.

using leave-one-out estimates of the shocks

$$\tilde{g}_{\ell n} = \frac{\sum_{k \neq \ell} \omega_{kn} g_{kn}}{\sum_{k \neq \ell} \omega_{kn}} \tag{15}$$

to form a leave-one-out shift-share instrument $\tilde{z}_\ell = \sum_{n=1}^N s_{\ell n} \tilde{g}_{\ell n}$. Then, under independent sampling,

$$\begin{aligned} \text{Cov}\left[\tilde{z}_\ell, \varepsilon_\ell\right] &= \sum_{n=1}^{N} \mathbb{E}\left[s_{\ell n} \varepsilon_\ell \mathbb{E}\left[\tilde{g}_{\ell n} \mid s_{\ell n}, \varepsilon_\ell\right]\right] \\ &= \sum_{n=1}^{N} \hat{s}_n \mathbb{E}\left[\tilde{g}_{\ell n}\right] \phi_n, \end{aligned} \tag{16}$$

so that the validity condition for the feasible shift-share IV estimator is the same as the quasi-experimental orthogonality condition (6), with $\mathbb{E}\left[\tilde{g}_{\ell n}\right]$ replacing g_n. Of course when the leave-one-out shock estimator is unbiased, these conditions are the same.

This discussion of many-shock bias provides a formal justification for leave-one-out shock estimation in shift-share designs, a practice that has become common – tracing back at least as far as Autor and Duggan (2003) – though often with little theoretical underpinning. The split-sample solution also highlights a virtue of shift-share designs in which the aggregate shocks are measured in a separate sample for other substantive reasons, as with the non-U.S. shocks in ADH. It is worth emphasizing that this issue does not affect the consistency of the feasible shift-share IV estimator when, as in Goldsmith-Pinkham et al. (2018), the number of industries is fixed. For the shocks-as-instruments interpretation, however, the number of industries must grow large; split-sample shock estimation may then guard against inconsistency in otherwise valid shift-share designs.

7 Conclusion

Shift-share instruments combine variation in the local exposure to aggregate shocks with variation in the shocks *per se*. We provide a general framework for understanding the validity of these instruments, while focusing on the shock variation. Ours framework is motivated by a simple equivalence result: shift-share IV estimates can be reframed as coefficients from weighted industry-level regressions, which use shocks to instrument for an exposure-weighted average of treatment. Shift-share instruments are therefore valid when shocks are idiosyncratic with respect to an exposure-weighted average of the unobserved factors determining outcomes. While this orthogonality condition can technically be satisfied when either the exposure measures or the shocks are as-good-as-randomly assigned, we argue that the latter may be more plausible and better aligned with researchers' motivations in many settings, such as Autor et al. (2013). The quasi-experimental approach assumes shocks are drawn as-good-as-randomly and independently across industries, perhaps conditional on observables, with the average exposure to any one industry becoming small as the sample grows..

We then outline various tests and extensions of the quasi-experimental shift-share framework. Several of these – such as the checks of instrument relevance and balance or the handling of controls, incomplete shares, and multiple shock instruments – are easily applied at the industry level, with standard statistical software, by exploiting the equivalence result. In practice researchers may therefore wish to conduct shift-share inference and validation with the industry-level regressions we derive. For other practical recommendations, we argue that researchers should adopt the quasi-experimental mindset and take a stand on which variation in growth rates is plausibly random: for instance, within industry clusters, across clusters, or both. This choice matters for what industry- and location-level controls to include and (in the case of clusters) how to compute valid standard errors. Finally, we recommend researchers use leave-one-out or other types of split sample methods for estimating shocks, as the failure to do so may cause quasi-experimental shift-share IV estimates to be inconsistent. Each of these recommendations draw on intuitions that applied researchers are likely to have from other quasi-experimental settings, bringing shift-share IV estimators to familiar econometric territory.

A Heterogeneous Treatment Effects

In this section we extend our shift-share IV identification result to allow for location-specific treatment effects. Maintaining linearity, suppose the structural outcome model is

$$y_\ell = \alpha + \beta_\ell x_\ell + \varepsilon_\ell, \tag{17}$$

where now β_ℓ denotes the treatment effect for location ℓ, and where we abstract away from other controls for simplicity. Without loss of generality suppose the aggregate shocks g_n are mean-zero. The shift-share IV estimator can then be written

$$\begin{aligned}\hat{\beta} &= \frac{\widehat{Cov}(z_\ell, y_\ell)}{\widehat{Cov}(z_\ell, x_\ell)} \\ &= \frac{\frac{1}{L}\sum_{\ell=1}^L \beta_\ell z_\ell x_\ell}{\frac{1}{L}\sum_{\ell=1}^L z_\ell x_\ell} + \frac{\frac{1}{L}\sum_{\ell=1}^L z_\ell \varepsilon_\ell}{\frac{1}{L}\sum_{\ell=1}^L z_\ell x_\ell},\end{aligned} \tag{18}$$

where $\widehat{Cov}(\cdot,\cdot)$ denotes a sample covariance. When our orthogonality condition holds $\frac{1}{L}\sum_{\ell=1}^L z_\ell \varepsilon_\ell \xrightarrow{p} 0$ so that, assuming instrument relevance ($p\lim \frac{1}{L}\sum_{\ell=1}^L z_\ell x_\ell \neq 0$), we have

$$\hat{\beta} = \sum_{\ell=1}^L \beta_\ell \frac{z_\ell x_\ell}{\sum_{k=1}^L z_k x_k} + o_p(1). \tag{19}$$

This shows that the shift-share IV coefficient approximates a weighted average of heterogeneous treatment effects β_ℓ, with weights $z_\ell x_\ell / \sum_{k=1}^L z_k x_k$ that sum to one. As with the classic result of Angrist and Imbens (1994), a further monotonicity assumption ensures the weighted average IV captures is convex. Suppose treatment is generated from a linear, heterogeneous-effects first stage model

$$x_\ell = \kappa + \sum_{n=1}^N \pi_{\ell n} g_n + \upsilon_\ell, \tag{20}$$

where the $\pi_{\ell n}$ denote industry- and location-specific effects of the aggregate shocks on treatment. Suppose further that the shift-share orthogonality condition holds not only for the second-stage relevant unobservables $\phi_n = \mathbb{E}[s_{\ell n}\varepsilon_\ell]/\mathbb{E}[s_{\ell n}]$, but that g_n is also uncorrelated with unobserved $\mathbb{E}[s_{\ell n}\upsilon_\ell]/\mathbb{E}[s_{\ell n}]$ and $\mathbb{E}[s_{\ell n}\beta_\ell \upsilon_\ell]/\mathbb{E}[s_{\ell n}]$, when weighted by s_n. Finally, assume shocks are mutually mean independent conditional on $\mathbb{E}[s_{\ell n}\pi_{\ell n}]$, similar to A2. Then plugging (20) into (19) and simplifying gives

$$\hat{\beta} = \sum_{\ell=1}^L \beta_\ell \frac{\omega_\ell}{\sum_{k=1}^L \omega_k} + o_p(1), \tag{21}$$

where

$$\omega_\ell = \sum_{n=1}^{N} \pi_{\ell n} s_{\ell n} \mathrm{Var}\left[g_n\right]. \tag{22}$$

This shows that the shift-share IV coefficient approximates a convex average of heterogenous treatment effects when the first-stage effects of shocks on treatment satisfy $\pi_{\ell n} \geq 0$ almost-surely.

B Controlling for Industry Observables

This section shows that adding an exposure-weighted vector of industry-level controls $\sum_n s_{\ell n} q_n$ as a location-level control is equivalent to a two-step procedure in which industry-level residuals g_n^* are first estimated by a matrix-weighted regression of g_n on q_n and then used to construct a new shift-share instrument $\hat{z}_\ell^* = \sum_n s_{\ell n} \hat{g}_n^*$. For simplicity, we abstract from other location-level controls. By the Frisch-Waugh-Lovell theorem, the resulting shift-share IV estimator can then be written

$$\hat{\beta} = \frac{\sum_{\ell=1}^{L} \hat{z}_\ell^* y_\ell}{\sum_{\ell=1}^{L} \hat{z}_\ell^* x_\ell}, \tag{23}$$

where $\hat{z}_\ell^*$ is the sample residual from regressing the instrument on the controls:

$$z_\ell = \mu + \sum_{n=1}^{N} s_{\ell n} q_n' \tau + z_\ell^*. \tag{24}$$

Since $\sum_{n=1}^{N} s_{\ell n} = 1$, this regression can be written

$$\sum_{n=1}^{N} s_{\ell n} g_n = \sum_{n=1}^{N} s_{\ell n} \mu + \sum_{n=1}^{N} s_{\ell n} q_n' \tau + \sum_{n=1}^{N} s_{\ell n} z_\ell^*. \tag{25}$$

Let s be the $L \times N$ matrix collecting observations of $s_{\ell n}$, g be the $N \times 1$ vector stacking the g_n, and $\bar{q}$ be a matrix with rows $[1, q_n']$. Then we can write OLS estimates of the parameters of (25) as

$$\begin{aligned}(\hat{\mu}, \hat{\tau})' &= \left((s\bar{q})' s\bar{q}\right)^{-1} (s\bar{q})' sg \\ &= \left(\bar{q}' (s's)\, \bar{q}\right)^{-1} \bar{q}' (s's)\, g,\end{aligned} \tag{26}$$

which is a matrix-weighted projection of g_n on q_n, with weight matrix $s's$. Thus

$$\begin{aligned}\hat{z}_\ell^* &= z_\ell - \hat{\mu} - \sum_{n=1}^{N} s_{\ell n} q_n' \hat{\tau} \\ &= \sum_{n=1}^{N} s_{\ell n} \left(g_n - \hat{\mu} - q_n' \hat{\tau}\right).\end{aligned} \tag{27}$$

This shows that $\hat{\beta}$ is equivalent to a shift-share IV regression coefficient that uses a modified aggregate shock $g_n - \hat{\mu} - q_n'\hat{\tau}$. This modified shock reflects the residual from the industry-level projection (26).

C Shift-Share Instruments in Panels

In this section we consider a panel extension of the cross-sectional shift-share IV setting. We derive the orthogonality condition and quasi-experimental assumptions, paralleling Sections 3 and 4. We also show how these conditions could be relaxed by including location and time fixed effects or with assumptions on the stochastic process for aggregate shocks, and propose a simple pre-trend test.

Suppose we observe T repeated observations t of outcomes, treatment, exposure, and shocks over time. We continue with a constant effects model for outcomes and treatment, but now decompose the structural error term into a fixed location component and its residual: $\varepsilon_{\ell t} = \alpha_\ell + \nu_{it}$ and

$$y_{\ell t} = \beta x_{\ell t} + w_{\ell t}'\gamma + \alpha_\ell + \nu_{\ell t}, \tag{28}$$

where α_ℓ denotes the location-specific mean of $\varepsilon_{\ell t}$. We also construct a time-varying shift-share instrument

$$z_{\ell t} = \sum_{n=1}^{N} s_{\ell t n} g_{nt}, \tag{29}$$

where g_{nt} is now the shock to industry n in time t. Note that this can be rewritten

$$z_{\ell t} = \sum_{n=1}^{N} \sum_{p=1}^{T} s_{\ell t n p} g_{np}, \tag{30}$$

where here $s_{\ell t n p} = s_{\ell t n} \mathbf{1}[n = p]$ denotes the exposure of location ℓ in time t to industry n in time p, which is zero for $n \neq p$.

With this expanded share notation, the key orthogonality condition for a the validity of a fixed effects shift-share IV regression of $y_{\ell t}$ on $x_{\ell t}$, controlling for $w_{\ell t}$ and location fixed effects, is

$$\text{Cov}\left[z_{\ell t}, \nu_{\ell t}\right] = \sum_{n=1}^{N} \sum_{t=1}^{T} s_{nt} g_{nt} \phi_{nt} \to 0, \tag{31}$$

where $s_{np} = \mathbb{E}\left[s_{\ell t n p}\right]$ and $\phi_{np} = \mathbb{E}\left[s_{\ell t n p} \nu_{\ell t}\right] / \mathbb{E}\left[s_{\ell t n p}\right]$. This is a weighted covariance of the aggregate shocks g_{nt}, now time-varying, and a time-varying measure of relevant unobservables ϕ_{nt}. Due to the location fixed effects, the industry-level unobservables here reflect a weighted average of only the time-varying component of structural residuals, $\nu_{\ell t}$.

The quasi-experimental assumptions A1 and A2 now map easily to the panel setting. The mean-independence condition is $\mathbb{E}\left[g_{nt} \mid \phi_{nt}\right] = \mu$, so that A1 is satisfied if the time varying shocks are mean-independent of the time-varying component unobservables. Importantly here need not be as-good-as-randomly assigned with respect to an exposure-weighted average of time-invariant heterogeneity, $\mathbb{E}\left[s_{\ell ntp}\alpha_\ell\right]/\mathbb{E}\left[s_{\ell tnp}\right]$. As before, A1 can be further weakened with the inclusion of industry-by-time observables. In the panel setting, a natural choice is a set of time fixed effects; by the equivalence result in Appendix B, including time fixed effects in the shift-share IV regression allows the mean of the aggregate shocks to vary over periods.

In the panel setting, assumption A2 requires the time-varying shocks to be mutually mean-independent, conditional on $\left\{\{s_{nt}, \phi_{nt}\}_{n=1}^{N}\right\}_{t=1}^{T}$, with $\sum_{n=1}^{N}\sum_{t=1}^{T} s_{nt}^2 \to 0$. Note that in a balanced panel, $s_{np} = \mathbb{E}\left[s_{\ell tnp}\mathbf{1}[t=p]\right] = \mathbb{E}\left[s_{\ell tnp}\right]/T$. Thus the latter condition would be satisfied either when the number of periods T is fixed and $\max_{n,p} s_{np} \to 0$, or when N is fixed but $T \to \infty$; in long panels, shift-share IV may be consistent even with only a small number of industries.

The assumption of mutually mean-independent shocks in the panel setting rules out autoregressive shock processes: for example g_{nt} can not be conditionally correlated with $g_{n,t-1}$. One may imagine replacing assumption A2 to allow for strongly mixing or ergodic quasi-experimental shocks – we leave formalizing this approach for future work. Given a particular time series model for g_{nt}, however, one could easily use the earlier result on industry-level controls to satisfy the current A2. For example, suppose the researcher assumes a first-order autoregressive process:

$$g_{nt} = \rho_0 + \rho_1 g_{n,t-1} + g_{nt}^*, \tag{32}$$

where the residuals g_{nt}^* are idiosyncratic. Then a researcher may choose to control for $\sum_{n=2}^{N} s_{\ell n,t} g_{n,t-1}$ in the panel IV specification.

With assumption A2 holding either on the original shocks or their idiosyncratic residual, researchers can validate the panel identifying assumptions by testing for pre-trends. This entails correlating the outcome $y_{\ell t}$ with leads of the shift-share instrument, for example with $z_{\ell,t+1} = \sum_{n=1}^{N} s_{\ell n,t+1} g_{n,t+1}$. Under A1 and A2,

$$\mathrm{Cov}\left[y_{\ell t}, z_{\ell,t+1}\right] = \sum_{n=1}^{N} g_{n,t+1}\mathrm{Cov}\left[s_{\ell n,t+1}, y_{\ell t}\right] \to 0. \tag{33}$$

Validating this pre-trend condition is a special case of our test for assumption A1, where a lagged outcome is used as an observable proxy for the current-period error term.

D Industry-Level Regression Standard Errors

In this section we show that conventional standard errors from the industry-level IV regression coincide with the formulas of Adao et al. (2018). We prove this equivalence for heteroskedasticity-robust standard errors, but it can be similarly shown for homoskedastic or industry-clustered standard errors.

Typical standard errors for the $\hat{s}_n$-weighted regression of $\bar{y}_n^\perp$ on $\bar{x}_n^\perp$ and a constant, instrumented by g_n, are given by

$$\widehat{se}\left(\hat{\beta}\right) = \frac{\sqrt{\sum_{n=1}^{N} \hat{s}_n^2 \hat{\varepsilon}_n^2 \left(g_n - \bar{g}\right)^2}}{\left|\sum_{n=1}^{N} \hat{s}_n \bar{x}_n^{\perp} g_n\right|}, \tag{34}$$

where $\hat{\varepsilon}_n = \bar{y}_n^{\perp} - \hat{\beta}\bar{x}_n^{\perp}$ is the estimated industry-level regression residual and $\bar{g} = \sum_{n=1}^{N} \hat{s}_n g_n$ is the $\hat{s}_n$-weighted average of shocks. Note that

$$\begin{aligned}\hat{\varepsilon}_n &= \frac{\sum_{\ell=1}^{L} s_{\ell n}\left(y_\ell^{\perp} - \hat{\beta} x_\ell^{\perp}\right)}{\hat{s}_n} \\ &= \frac{\sum_{\ell=1}^{L} s_{\ell n}\hat{\varepsilon}_\ell}{\hat{s}_n},\end{aligned} \tag{35}$$

where $\hat{\varepsilon}_\ell$ is the estimated residual from the location-level shift-share IV regression by the equivalence (4). Therefore, the numerator of (34) can be rewritten as

$$\sqrt{\sum_{n=1}^{N} \hat{s}_n^2 \hat{\varepsilon}_n^2 \left(g_n - \bar{g}\right)^2} = \sqrt{\sum_{n=1}^{N} \left(\sum_{\ell=1}^{L} s_{\ell n}\hat{\varepsilon}_\ell\right)^2 \left(g_n - \bar{g}\right)^2}. \tag{36}$$

The expression in the denominator of (34) estimates the absolute value of the industry-level first-stage covariance, which matches the covariance at the location level:

$$\begin{aligned}\sum_{n=1}^{N} \hat{s}_n \bar{x}_n^{\perp} g_n &= \sum_{n=1}^{N} \left(\sum_{\ell=1}^{L} s_{\ell n} x_\ell^{\perp}\right) g_n \\ &= \sum_{\ell=1}^{L} x_\ell^{\perp} z_\ell.\end{aligned} \tag{37}$$

Thus

$$\widehat{se}\left(\hat{\beta}\right) = \frac{\sqrt{\sum_{n=1}^{N} \left(g_n - \bar{g}\right)^2 \left(\sum_{\ell=1}^{L} s_{\ell n}\hat{\varepsilon}_\ell\right)^2}}{\left|\sum_{\ell=1}^{L} x_\ell^{\perp} z_\ell\right|}. \tag{38}$$

We now compare this expression to the corresponding standard errors in Adao et al. (2018). Absent location-level controls, equation (44) in that paper derives a valid IV standard error estimate as

$$\widehat{se}_{\text{AKM}}\left(\hat{\beta}\right) = \frac{\sqrt{\sum_{n=1}^{N} \tilde{g}_n^2 \left(\sum_{\ell=1}^{L} s_{\ell n}\hat{\varepsilon}_\ell\right)^2}}{\left|\sum_{\ell=1}^{L} x_\ell^{\perp} z_\ell\right|}, \tag{39}$$

where $\hat{g}_n$ are coefficients from regressing $z_\ell - \frac{1}{L}\sum_{\ell=1}^{L} z_\ell$ on all shares $s_{\ell n}$, without a constant. To understand these coefficients, note that

$$\frac{1}{L}\sum_{\ell=1}^{L} z_\ell = \frac{1}{L}\sum_{\ell=1}^{L}\sum_{n=1}^{N} s_{\ell n} g_n$$
$$= \sum_{n=1}^{N} \hat{s}_n g_n$$
$$= \bar{g}, \tag{40}$$

and, since $\sum_{n=1}^{N} s_{\ell n} = 1$, we can rewrite

$$z_\ell - \frac{1}{L}\sum_{\ell=1}^{L} z_\ell = \sum_{\ell=1}^{L} s_{\ell n} g_n - \bar{g}$$
$$= \sum_{\ell=1}^{L} s_{\ell n} \left(g_n - \bar{g}\right). \tag{41}$$

Therefore the auxiliary regression Adao et al. (2018) run has exact fit and produces $\tilde{g}_n = g_n - \bar{g}$, making equations (38) and (39) identical. We emphasize that for this equivalence to hold a constant must be included in the industry-level regression; otherwise, standard statistical software packages would use g_n^2 in the numerator of (38) instead of $\left(g_n - \bar{g}\right)^2$. Including a constant does not change the shift-share IV coefficient estimate, as the weighted means of both $\bar{y}_n^\perp$ and $\bar{x}_n^\perp$ are already zero.

As noted in the text, with additional location-level controls the set of $\tilde{g}_n$ in equation (44) in Adao et al. (2018) are the coefficients from projecting $z_\ell^\perp$ on all shares, without a constant. Instrumenting $\bar{x}_n^\perp$ with $\tilde{g}_n$ defined this way, instead of $g_n$will produce standard error estimates that match those of Adao et al. (2018).

E Many-Shock Bias in the Examiner Case

This section shows how the asymptotic bias of shift-share IV with many estimated shocks reduces to that of Nagar (1959) in an examiner design. Exposure shares in this setting are binary, $s_{\ell n} \in \{0, 1\}$, with shocks given by the group-level expectation of treatment: $g_n = \mathbb{E}\left[x_\ell \mid s_{\ell n} = 1\right]$. The researcher estimates shocks by the corresponding sample average,

$$\hat{g}_n = \frac{\sum_{\ell=1}^{L} s_{\ell n} x_\ell}{\sum_{\ell=1}^{L} s_{\ell n}}, \tag{42}$$

so that in terms of the general equation (11), $g_{\ell n} = x_\ell$ and $\omega_{\ell n} = s_{\ell n}$.

For simplicity here we assume the control vector w_ℓ contains only a constant. Projecting treatment and the structural error onto the shares, we have

$$x_\ell = \sum_{n=1}^{N} g_n s_{\ell n} + \eta_\ell \tag{43}$$

$$\varepsilon_\ell = \sum_{n=1}^{N} \phi_n s_{\ell n} + \nu_\ell, \tag{44}$$

where $\mathbb{E}\left[\eta_\ell \mid s_{\ell 1}, \ldots, s_{\ell N}\right] = \mathbb{E}\left[\nu_\ell \mid s_{\ell 1}, \ldots, s_{\ell N}\right] = 0$ by construction. Let s_ℓ be a $N \times 1$ vector collecting $s_{\ell n}$, s be a $L \times N$ matrix collecting s_ℓ', g be an $N \times 1$ vector collecting g_n, x be an $L \times 1$ vector collecting x_ℓ, and η be an $L \times 1$ vector collecting η_ℓ. Then in matrix form the measurement error term (12) is

$$\begin{aligned} \psi_\ell &= s_\ell' \left(\left(s's\right)^{-1} s'x - g \right) \\ &= s_\ell' \left(s's\right)^{-1} s'\eta. \end{aligned} \tag{45}$$

Under independent sampling and conditional homoskedasticity of (η_ℓ, ν_ℓ), we then have

$$\begin{aligned} \operatorname{Cov}\left[\psi_\ell, \varepsilon_\ell\right] &= \mathbb{E}\left[s_\ell' \left(s's\right)^{-1} s'\eta\varepsilon_\ell\right] \\ &= \mathbb{E}\left[s_\ell' \left(s's\right)^{-1} s_\ell \operatorname{Cov}\left[\varepsilon_\ell, \eta_\ell \mid s\right]\right] \\ &= \mathbb{E}\left[s_\ell' \left(s's\right)^{-1} s_\ell \sigma_{\eta\nu}\right] \\ &= \frac{N}{L}\sigma_{\eta\nu}, \end{aligned} \tag{46}$$

where $\sigma_{\eta\nu} = \operatorname{Cov}\left[\eta_\ell, \nu_\ell\right] = \operatorname{Cov}\left[\varepsilon_\ell, \eta_\ell\right]$.

References

Acemoglu, D. and P. Restrepo (2017): "Robots and Jobs: Evidence from US Labor Markets," NBER Working Paper 23285.

Adao, R., M. Kolesar, and E. Morales (2018): "Inference in Shift-Share Designs: Theory and Inference," Working Paper.

Angrist, J., G. Imbens, and A. Krueger (1999): "Jackknife Instrumental Variables Estimation," *Journal of Applied Econometrics*, 14, 57–67.

Angrist, J. D. and G. W. Imbens (1994): "Identification and Estimation of Local Average Treatment Effects," *Econometrica*, 62, 467–475.

Angrist, J. D. and A. B. Krueger (1995): "Split-Sample Instrumental Variables Estimates of the Returns to Schooling," *Journal of Business and Economic Statistics*, 13, 225–235.

Autor, D. H., D. Dorn, and G. H. Hanson (2013): "The China Syndrome: Local Labor Market Impacts of Import Competition in the United States," *American Economic Review*, 103, 2121–2168.

Autor, D. H. and M. G. Duggan (2003): "The Rise in the Disability Rolls and the Decline in Unemployment," *Quarterly Journal of Economics*, 118, 157–205.

Bartik, T. J. (1991): *Who Benefits from State and Local Economic Development Policies?*, W. E. Upjohn Institute for Employment Research.

Blanchard, O. J. and F. Katz (1992): "Regional Evolutions," *Brookings Papers on Economic Activity*, 1–75.

Borusyak, K. and X. Jaravel (2016): "Revisiting Event Study Designs, with an Application to the Estimation of the Marginal Propensity to Consume," Working Paper.

Boustan, L., F. Ferreira, H. Winkler, and E. M. Zolt (2013): "The effect of rising income inequality on taxation and public expenditures: Evidence from U.S. Municipalities and school districts, 1970-2000," *Review of Economics and Statistics*, 95, 1291–1302.

Broxterman, D. A. and W. D. Larson (2018): "An Examination of Industry Mix Demand Indicators : The Bartik Instrument at Twenty-Five," Working Paper.

Card, D. (2001): "Immigrant Inflows, Native Outflows, and the Local Labor Market Impacts of Higher Immigration," *Journal of Labor Economics*, 19, 22–64.

——— (2009): "Immigration and Inequality," *American Economic Review: Papers & Proceedings*, 99, 1–21.

Chetty, R., J. N. Friedman, N. Hilger, E. Saez, W. Schanzenbach, and D. Yagan (2011): "How Does Your Kindergarten Classroom Affect Your Earnings? Evidence from Project STAR," *Quarterly Journal of Economics*, 126, 1593–1660.

de Chaisemartin, C. and X. D'Haultfoeuille (2018): "Two-way Fixed Effects Estimators with Heterogeneous Treatment Effects," Working Paper.

Diamond, R. (2016): "The Determinants and Welfare Implications of US Workers' Diverging Location Choices by Skill: 1980-2000," *American Economic Review*, 106, 479–524.

Dobbie, W., J. Goldin, and C. Yang (2018): "The Effects of Pre-Trial Detention on Conviction, Future Crime, and Employment: Evidence from Randomly Assigned Judges," *American Economic Review*, 108(2), 201–240.

Doyle, J., J. Graves, J. Gruber, and S. Kleiner (2015): "Measuring Returns to Hospital Care: Evidence from Ambulance Referral Patterns," *Journal of Political Economy*, 123, 170–214.

Garin, A. and F. Silverio (2017): "How Does Firm Performance Affect Wages? Evidence from Idiosyncratic Export Shocks," Working Paper.

Goldsmith-Pinkham, P., I. Sorkin, and H. Swift (2018): "Bartik Instruments: What, When, Why, and How," Working Paper.

Greenstone, M., A. Mas, and H.-L. Nguyen (2014): "Do Credit Market Shocks Affect the Real Economy? Quasi-Experimental Evidence from the Great Recession and 'Normal' Economic Times," NBER Working Paper 20704.

Hansen, L. P. (1982): "Large Sample Properties of Generalized Method of Moments Estimators," *Econometrica*, 50, 1029–1054.

Hornbeck, R. and E. Moretti (2018): "Who Benefits From Productivity Growth? Direct and Indirect Effects of Local TFP Growth on Wages, Rents, and Inequality," NBER Working Paper 24661.

Hull, P. (2018): "Estimating Treatment Effects in Mover Designs," Working Paper.

Hummels, D., R. Jorgensen, J. Munch, and C. Xiang (2014): "The Wage Effects of Offshoring: Evidence From Danish Matched Worker-Firm Data," *American Economic Review*, 104, 1597–1629.

Jaeger, D. A., J. Ruist, and J. Stuhler (2018): "Shift-Share Instruments and Dynamic Adjustments: The Case of Immigration," Working Paper.

Jaravel, X. (2017): "The Unequal Gains from Product Innovations: Evidence from the US Retail Sector," Working Paper.

Kolesar, M., R. Chetty, J. Friedman, E. Glaeser, and G. W. Imbens (2015): "Identification and Inference With Many Invalid Instruments," *Journal of Business and Economic Statistics*, 33, 474–484.

Kovak, B. K. (2013): "Regional Effects of Trade Reform: What is the Correct Measure of Liberalization?" *American Economic Review*, 103, 1960–1976.

Luttmer, E. F. (2006): "Neighbors as Negatives: Relative Earnings and Well-being," *Quarterly Journal of Economics*, 120, 963–1002.

Maestas, N., K. J. Mullen, and A. Strand (2013): "Does Disability Insurance Receipt Discourage Work? Using Examiner Assignment to Estimate Causal Effects of SSDI Receipt," *American Economic Review*, 103, 1797–1829.

Nagar, A. L. (1959): "The Bias and Moment Matrix of the General k-class Estimators of the Parameters in Simultaneous Equations," *Econometrica*, 27, 575–595.

Nakamura, E. and J. Steinsson (2014): "Fiscal Stimulus in a Monetary Union: Evidence from US Regions," *American Economic Review*, 104, 753–792.

Oberfield, E. and D. Raval (2014): "Micro Data and Macro Technology," .

Saiz, A. (2010): "The Geographic Determinants of Housing Supply," *Quarterly Journal of Economics*, 125, 1253–1296.

Suárez, J. C. and O. Zidar (2016): "Who Benefits from State Corporate Tax Cuts? A Local Labor Markets Approach with Heterogeneous Firms," *American Economic Review*, 106, 2582–2624.

White, H. (1982): "Instrumental Variables Regression with Independent Observations," *Econometrica*, 50, 483–499.