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QUASI-EXPERIMENTAL SHIFT-SHARE RESEARCH DESIGNS

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Quasi-Experimental Shift-Share Research Designs
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ABSTRACT

Many studies use shift-share (or “Bartik”) instruments, which average a set of shocks with exposure share weights. We provide a new econometric framework for shift-share instrumental variable (SSIV) regressions in which identification follows from the quasi-random assignment of shocks, while exposure shares are allowed to be endogenous. The framework is motivated by an equivalence result: the orthogonality between a shift-share instrument and an unobserved residual can be represented as the orthogonality between the underlying shocks and a shock-level unobservable. SSIV regression coefficients can similarly be obtained from an equivalent shock-level regression, motivating shock-level conditions for their consistency. We discuss and illustrate several practical insights of this framework in the setting of Autor et al. (2013), estimating the effect of Chinese import competition on manufacturing employment across U.S. commuting zones.

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A Replication programs and data is available at <https://github.com/borusyak/shift-share>

1 Introduction

A large and growing number of empirical studies use shift-share instruments: weighted averages of a common set of shocks, with weights reflecting heterogeneous shock exposure. In many settings, such as those of Bartik (1991), Blanchard and Katz (1992) and Autor et al. (2013), a regional instrument is constructed from shocks to industries with local industry employment shares measuring the shock exposure. In other settings, researchers may combine shocks across countries, socio-demographic groups, or foreign markets to instrument for treatments at the regional, individual, or firm level.¹

The claim for instrument validity in shift-share instrumental variable (SSIV) regressions must rely on some assumptions about the shocks, exposure shares, or both. This paper develops a novel framework for understanding such regressions as leveraging exogenous variation in the shocks, allowing the variation in exposure shares to be endogenous. Our approach is motivated by an equivalence result: the orthogonality between a shift-share instrument and an unobserved residual can be represented as the orthogonality between the underlying shocks and a shock-level unobservable. Given a first stage, it follows that the instrument identifies a parameter of interest if and only if the shocks are uncorrelated with this unobservable, which captures the average unobserved determinants of the original outcome among observations most exposed to a given shock. SSIV regression coefficients can similarly be obtained from an equivalent IV regression estimated at the level of shocks. In this regression the outcome and treatment variables are first averaged, using exposure shares as weights, to obtain shock-level aggregates. The shocks then directly instrument for the aggregated treatment. Importantly, these equivalence results only rely on the structure of the shift-share instrument and thus apply to outcomes and treatments that are not typically computed at the level of shocks.

We use these equivalence results to derive two conditions sufficient for SSIV consistency. First, we assume shocks are as-good-as-randomly assigned as if arising from a natural experiment. This is enough for the shift-share instrument to be valid: i.e. for the shocks to be uncorrelated with the relevant unobservables in expectation. Second, we assume that a shock-level law of large numbers applies—that the instrument incorporates many sufficiently independent shocks, each with sufficiently small average exposure. Instrument relevance further holds when individual units are mostly exposed to only a small number of shocks, provided those shocks affect treatment. Our two quasi-experimental conditions are similar to ones imposed in other settings where the underlying shocks are directly used as instruments, bringing SSIV to familiar econometric territory.²

¹Observations in shift-share designs may, for example, represent regions impacted by immigration shocks from different countries (Card 2001; Peri et al. 2016), firms differentially exposed to foreign market shocks (Hummels et al. 2014; Berman et al. 2015), product groups purchased by different types of consumers (Jaravel 2019), groups of individuals facing different national income trends (Boustan et al. 2013), or countries differentially exposed to the U.S. food aid supply shocks (Nunn and Qian 2014). We present a taxonomy of existing shift-share designs, and how they relate to our framework, in Section 6.1.

²For example, Acemoglu et al. (2016) study the impact of import competition from China on U.S. industry employment using industry (i.e. shock-level) regressions with shocks constructed similarly to those underlying the regional shift-share instrument used in Autor et al. (2013). Our framework shows that both studies can rely on similar econometric assumptions, though the economic interpretations of the estimates differ.

We extend our quasi-experimental approach to settings where shocks are as-good-as-randomly assigned only conditionally on shock-level observables, to SSIVs with exposure shares that do not add up to a constant for each observation, and to panel data. For conditional random assignment, we show that quasi-experimental shock variation can be isolated with regression controls that have a shift-share structure. Namely, it is enough to control for an exposure-weighted sum of the relevant shock-level confounders. Relatedly, in SSIVs with “incomplete shares,” where the sum of exposure shares varies across observations, it is important to control for the sum of exposure shares as the exposure-weighted sum of a constant. In panel data, we show that the SSIV estimator can be consistent both with many shocks per period and with many periods. We also show that unit fixed effects only isolate variation in shocks over time when exposure shares are time-invariant. In other extensions we show how SSIV with multiple endogenous variables can be viewed quasi-experimentally and how multiple sets of quasi-random shocks can be combined with new overidentified shock-level IV procedures.

Our framework also bears practical tools for SSIV inference and testing. Adão et al. (2019) show that conventional standard errors in SSIV regressions may be invalid because observations which similar exposure shares are likely to have correlated residuals. They are also the first to propose a solution to this inference problem in a framework based on ours, with identifying variation in shocks. We present a convenient alternative based on our equivalence result: estimating SSIV coefficients at the level of identifying variation (shocks) can yield asymptotically valid standard errors. The validity of this solution requires an additional assumption on the structure of the included controls (producing standard errors that are conservative otherwise). However, it offers several practical features: it can be implemented with standard statistical software, extended to various forms of shock dependence (e.g. autocorrelation), and computed in some settings where the estimator of Adão et al. (2019) fails (e.g. when there are more shocks than observations). Appropriate measures of first-stage relevance and valid falsification tests of shock exogeneity can also be obtained with conventional shock-level procedures. Monte-Carlo simulations confirm the accuracy of our asymptotic approximations in moderately-sized samples of shocks, and that the finite-sample properties of SSIV are similar to those of conventional shock-level IV regressions which use the same shocks as instruments.

We illustrate the practical insights from our framework in the setting of Autor et al. (2013), who estimate the effect of increased Chinese import penetration on manufacturing employment across U.S. commuting zones. We find supporting evidence for the interpretation of their SSIV as leveraging quasi-random variation in industry-specific Chinese import shocks. This application uses a new Stata package, *ssaggregate*, which we have developed to help practitioners implement the appropriate shock-level analyses.³

Our quasi-experimental approach is not the only framework for SSIV identification and consis-

³This Stata package creates the shock-level aggregates used in the equivalent regression. Users can install this package with the command `ssc install ssaggregate`. See the associated help file and this paper’s replication archive at <https://github.com/borusyak/shift-share> for more details.

tency. In related work, Goldsmith-Pinkham et al. (2020) formalize a different approach based on the exogeneity of the exposure shares, imposing no explicit assumption of shock exogeneity. This framework is motivated by a different equivalence result: the SSIV coefficient also coincides with a generalized method of moments estimator, with exposure shares as multiple excluded instruments. Though exposure exogeneity is a sufficient condition for SSIV identification (and, as such, implies our shock-level orthogonality condition), we focus on plausible conditions under which it is not necessary.

We delineate two cases where identification via exogenous shocks is attractive. In the first case, the shift-share instrument is based on a set of shocks which can itself be thought of as an instrument. Consider the Autor et al. (2013; hereafter ADH) shift-share instrument, which combines industry-specific changes in Chinese import competition (the shocks) with local exposure given by the lagged industrial composition of U.S. regions (the exposure shares). In such a setting, exogeneity of industry employment shares is difficult to justify *a priori* since unobserved industry shocks (e.g., automation or innovation trends) are likely to affect regional outcomes through the same mixture of exposure shares. Our approach, in contrast, allows researchers to specify a set of shocks that are plausibly uncorrelated with such unobserved factors. Consistent with this general principle, ADH attempt to purge their industry shocks from U.S.-specific confounders by measuring Chinese import growth outside of the United States. Similarly, Hummels et al. (2014) combine country-by-product changes in transportation costs to Denmark (as shocks) with lagged firm-specific composition of intermediate inputs and their sources (as shares). They argue these shocks are “idiosyncratic,” which our approach formalizes as “independent from relevant country-by-product unobservables.” Other recent examples of where our approach may naturally apply are found, for example, in finance (Xu 2019), the immigration literature (Peri et al. 2016), and studies of innovation (Stuen et al. 2012).

In the second case, a researcher can think of quasi-experimental shocks which are not observed directly but are instead estimated in-sample in an initial step, potentially introducing biases. In the canonical estimation of regional labor supply elasticities by Bartik (1991), for example, the shocks are measured as national industry growth rates. Such growth captures national industry labor demand shocks, which one may be willing to assume are as-good-as-randomly assigned across industries; however, industry growth rates also depend on unobserved regional labor supply shocks. We show that our framework can still apply to such settings by casting the industry employment growth rates as noisy estimates of latent quasi-experimental demand shocks and establishing conditions to ensure the supply-driven estimation error is asymptotically ignorable. These conditions are weaker if the latent shocks are estimated as leave-one-out averages. Although leave-one-out shift-share IV estimates do not have a convenient shock-level representation, we provide evidence that in the Bartik (1991) setting this leave-out adjustment is unimportant.

Formally, our approach to SSIV relates to the analysis of IV estimators with many invalid instruments by Kolesar et al. (2015). Consistency in that setting follows when violations of individual instru-

ment exclusion restrictions are uncorrelated with their first-stage effects. For quasi-experimental SSIV, the exposure shares can be thought of as a set of invalid instruments (per the Goldsmith-Pinkham et al. (2020) interpretation), and our orthogonality condition requires their exclusion restriction violations to be uncorrelated with the shocks. Despite this formal similarity, we argue that shift-share identification is better understood through the quasi-random assignment of a single instrument (shocks), rather than through a large set of invalid instruments (exposure shares) that nevertheless produce a consistent estimate. This view is reinforced by our equivalence results, yields a natural shock-level identification condition, and suggests new validations and extensions of SSIV.

Our analysis also relates to other recent methodological studies of shift-share designs, including those of Jaeger et al. (2018) and Broxterman and Larson (2018). The former highlights biases of SSIV due to endogenous local labor market dynamics, and we show how their solution can be implemented in our setting. The latter studies the empirical performance of different shift-share instrument constructions. As discussed above we also draw on the inferential framework of Adão et al. (2019), who derive valid standard errors in shift-share designs with a large number of idiosyncratic shocks. More broadly, our paper adds to a growing literature studying the causal interpretation of common research designs, including work by Borusyak and Jaravel (2017), Goodman-Bacon (2018), Sun and Abraham (Forthcoming), and Callaway and Sant’Anna (Forthcoming) for event study designs; Chaisemartin and D’Haultfoeuille (2019) for two-way fixed effects regressions; Słoczyński (Forthcoming) for regressions with other controls; and Hull (2018) for mover designs.

The remainder of this paper is organized as follows. Section 2 introduces the environment, derives our equivalence results, and motivates our approach to SSIV identification and consistency. Section 3 establishes the baseline quasi-experimental assumptions and Section 4 derives various extensions. Section 5 discusses shock-level procedures for valid SSIV inference and testing. Section 6 summarizes the types of empirical settings where our framework may be applied and illustrates its practical implications in the ADH setting. Section 7 concludes.

2 Setting and Motivation

We begin by presenting the SSIV setting and motivating our approach to identification and consistency with two equivalence results. We first show that population orthogonality of the shift-share instrument can be recast at the shock level, motivating identification by exogenous shocks when exposure shares are endogenous. We then derive a similar shock-level equivalence result for the SSIV estimator, motivating its consistency with many as-good-as-randomly assigned shocks.

2.1 The Shift-Share IV Setting

We observe an outcome y_ℓ , treatment x_ℓ , control vector w_ℓ (which includes a constant) and shift-share instrument z_ℓ for a set of observations $\ell = 1, \dots, L$. We also observe a set of regression weights $e_\ell > 0$ with $\sum_\ell e_\ell = 1$ ($e_\ell = \frac{1}{L}$ covers the unweighted case). The instrument can be written as

$$z_\ell = \sum_n s_{\ell n} g_n, \quad (1)$$

for a set of observed shocks g_n , $n = 1, \dots, N$, and a set of observed shares $s_{\ell n} \geq 0$ defining the exposure of each observation ℓ to each shock n . Initially we assume the sum of these exposure weights is constant across observations, i.e. that $\sum_n s_{\ell n} = 1$; we relax this assumption in Section 4.2.⁴ Although our focus is on shift-share IV, we note that the setup nests shift-share reduced-form regressions, of y_ℓ on z_ℓ and w_ℓ , when $x_\ell = z_\ell$.

We seek to estimate the causal effect or structural parameter β in a linear model of

$$y_\ell = \beta x_\ell + w'_\ell \gamma + \varepsilon_\ell, \quad (2)$$

where the residual ε_ℓ is defined to be orthogonal with the control vector w_ℓ .⁵ For example, we might be interested in estimating a classic model of labor supply which relates observations of log wage growth y_ℓ and log employment growth x_ℓ across local labor markets ℓ by an inverse labor supply elasticity β . The residual ε_ℓ in equation (2) would then contain all local labor supply shocks, such as those arising from demographic, human capital, or migration changes, that are not systematically related to the observed controls in w_ℓ .⁶ To estimate β we require an instrument capturing variation in local labor demand.

We consider a z_ℓ based on the introduction of new import tariffs g_n across different industries n , with $s_{\ell n}$ denoting location ℓ 's lagged shares of industry employment.⁷ In estimating β we may weight observations by the overall lagged regional employment, e_ℓ . We return to this labor supply example at several points to ground the following theoretic discussion.

⁴Note that the shares are defined relative to the total across components n . In practice shift-share instruments are sometimes presented differently, with the shares defined relative to the total across observations ℓ (see footnote 39 for an example in the Autor et al. (2013) setting). We recommend that researchers follow the representation in (1) to apply our theoretical results.

⁵Formally, given a linear causal or structural model of $y_\ell = \beta x_\ell + \varepsilon_\ell$ we define $\gamma = \mathbb{E} [\sum_\ell e_\ell w_\ell w'_\ell]^{-1} \mathbb{E} [\sum_\ell e_\ell w_\ell \varepsilon_\ell]$ and $\varepsilon_\ell = \varepsilon_\ell - w'_\ell \gamma$ as the residual from this population projection, satisfying $\mathbb{E} [\sum_\ell e_\ell w_\ell \varepsilon_\ell] = 0$. Defining a unique γ requires an implicit maintained assumption that $\mathbb{E} [\sum_\ell e_\ell w_\ell w'_\ell]$ is of full rank, which holds when there is no perfect collinearity in the control vector. We consider models with heterogeneous treatment effects in Appendix A.1; see footnote 16 for a summary.

⁶While this simple labor supply equation is only well-defined under certain assumptions (for instance, it rules out wage bargaining and profit sharing between firms and workers), it is a standard modeling tool. We note that it is inconsequential whether wages or employment are on the right-hand side of the second stage regression; we choose wage growth as the outcome following the tradition of Bartik (1991).

⁷Import tariffs affect import prices, consumer demand for domestic products, and in turn labor demand. Recent studies illustrate that one can obtain quasi-random identifying variation in import tariffs in practice. Changes in import tariffs across industries have been used for identification in both industry-level analyses (e.g. Fajgelbaum et al. (2020)) and shift-share analyses of regional outcomes (e.g. Kovak (2013)).

It is worth highlighting that in studying this setting we do not impose a typical assumption of independent and identically-distributed (*iid*) data $\{e_\ell, z_\ell, w_\ell, x_\ell, \varepsilon_\ell\}$, as might arise from random sampling of potential observations. Relaxing the usual *iid* assumption is required for us to treat the g_n as random variables, which generate dependencies of the instrument (1) across observations exposed to the same random shocks. The non-*iid* setting further allows for unobserved common shocks, which may generate dependencies in the residual ε_ℓ .

Given this non-*iid* setting, we consider IV identification of β by the full-data moment condition

$$\mathbb{E} \left[\sum_{\ell} e_{\ell} z_{\ell} \varepsilon_{\ell} \right] = 0. \quad (3)$$

This condition captures the orthogonality of the shift-share instrument with the second-stage residual, in expectation over realizations of $\{e_\ell, z_\ell, \varepsilon_\ell\}$ for all $\ell = 1, \dots, L$. When such orthogonality holds the β parameter is identified: i.e., uniquely recoverable from observable moments, provided the instrument has a first stage.⁸ The full-data orthogonality condition generalizes the conventional condition of $\mathbb{E}[z_\ell \varepsilon_\ell] = 0$, which might be considered in an *iid* setting with fixed e_ℓ .

The moment condition (3) yields a natural estimator of β : the coefficient on x_ℓ in an IV regression of y_ℓ which instruments by z_ℓ , controls for w_ℓ , and weights by e_ℓ . By the Frisch-Waugh-Lovell theorem, this SSIV estimator can be represented as a bivariate IV regression of outcome and treatment residuals, or as the ratio of e_ℓ -weighted sample covariances between the instrument and the residualized outcome and treatment:

$$\hat{\beta} = \frac{\sum_{\ell} e_{\ell} z_{\ell} y_{\ell}^{\perp}}{\sum_{\ell} e_{\ell} z_{\ell} x_{\ell}^{\perp}}, \quad (4)$$

where v_{ℓ}^{\perp} denotes the residual from an e_{ℓ} -weighted sample projection of a variable v_{ℓ} on the control vector w_{ℓ} . Note that by the properties of residualization, it is enough to residualize y_{ℓ} and x_{ℓ} without also residualizing the shift-share instrument z_{ℓ} .

In our non-*iid* setting, we study consistency and other asymptotic properties of $\hat{\beta}$ by considering a sequence of data-generating processes, indexed by L , for the complete data $\{e_{\ell}, s_{\ell n}, g_n, w_{\ell}, x_{\ell}, \varepsilon_{\ell}\}$, for $\ell = 1, \dots, L$, $n = 1 \dots, N$, and $N = N(L)$. Consistency, for example, is defined as $\hat{\beta} \xrightarrow{P} \beta$ as $L \rightarrow \infty$ along this sequence. We do not employ conventional sampling-based asymptotic sequences (and corresponding laws of large numbers) as these are generally inappropriate in a non-*iid* setting where both z_{ℓ} and ε_{ℓ} may exhibit non-standard mutual dependencies. It is worth emphasizing that any assumptions on the data-generating sequence are useful only to approximate the finite-sample distribution of the SSIV estimator, not to define an actual process for realizations of the data. For example, we will consider below a sequence in which the number of shocks N grows with L , recognizing that in reality shift-share instruments are constructed from a fixed set of shocks (e.g. tariffs across all

⁸Formally, when equation (3) holds the moment condition $m(b, c) \equiv \mathbb{E}[\sum_{\ell} e_{\ell}(z_{\ell}, w'_{\ell})(y_{\ell} - bx_{\ell} - w'_{\ell}c)] = 0$ has a unique solution of (β, γ) , provided $\mathbb{E}[\sum_{\ell} e_{\ell}(z_{\ell}, w'_{\ell})(x_{\ell}, w'_{\ell})]$ is of full-rank.

industries) along with a fixed number of observations (e.g. all local labor markets). The assumption of growing N should here be interpreted as a way to capture the presence of a large number of shocks in a given set of observations, such that the asymptotic sequence provides a good approximation to the observed data.⁹

2.2 A Shock-Level Orthogonality Condition

We first build intuition for our approach to satisfying the IV moment condition by showing that the structure of the shift-share instrument allows equation (3) to be rewritten as condition on the orthogonality of shocks g_n . Namely, by exchanging the order of summation across ℓ and n , we obtain

$$\mathbb{E} \left[\sum_{\ell} e_{\ell} z_{\ell} \varepsilon_{\ell} \right] = \mathbb{E} \left[\sum_{\ell} e_{\ell} \sum_n s_{\ell n} g_n \varepsilon_{\ell} \right] = \mathbb{E} \left[\sum_n s_n g_n \bar{\varepsilon}_n \right] = 0, \quad (5)$$

where we define $s_n = \sum_{\ell} e_{\ell} s_{\ell n}$ and $\bar{\varepsilon}_n = \frac{\sum_{\ell} e_{\ell} s_{\ell n} \varepsilon_{\ell}}{\sum_{\ell} e_{\ell} s_{\ell n}}$. Just as the left-hand side of this expression captures the orthogonality of the instrument z_{ℓ} with the residual ε_{ℓ} when weighted by e_{ℓ} , the right-hand side captures the orthogonality of shocks g_n and $\bar{\varepsilon}_n$ when weighted by s_n . Since these two expressions are equivalent, equation (5) shows that such shock orthogonality is necessary and sufficient condition for the orthogonality of the shift-share instrument. As with e_{ℓ} , the shock-level weights are also non-negative and sum to one, since $\sum_n s_n = \sum_{\ell} e_{\ell} (\sum_n s_{\ell n}) = 1$. The shock-level unobservables $\bar{\varepsilon}_n$ represent exposure-weighted averages of the residuals ε_{ℓ} .

The labor supply example is useful for unpacking this first equivalence result. When $s_{\ell n}$ are lagged employment shares and e_{ℓ} are similarly lagged regional employment weights, the s_n weights are proportional to the lagged industry employment.¹⁰ Moreover, with ε_{ℓ} capturing unmeasured supply shocks, $\bar{\varepsilon}_n$ is the average unobserved supply shock among regions ℓ that are the most specialized in industry n , in terms of their lagged employment $e_{\ell} s_{\ell n}$. Equation (5) then shows that for the shift-share instrument z_{ℓ} to identify the labor supply elasticity β , the industry demand shocks g_n must be orthogonal with these industry-level unobservables when weighted by industry size. For example, the industries which experience a rise in import tariffs should not face systematically different unobserved labor supply conditions (e.g., migration patterns) in their primary markets.

Shock orthogonality is a necessary condition for SSIV identification and is satisfied when, as in the preferred interpretation of Goldsmith-Pinkham et al. (2020), the exposure shares are exogenous, the data are *iid*, and the shocks are considered non-random.¹¹ In practice, however, this approach to SSIV identification may be untenable in many settings. In our labor supply example, the Goldsmith-Pinkham et al. (2020) approach to identification requires the (lagged) local employment share of each

⁹This is similar to how Bekker (1994) uses a non-standard asymptotic sequence to analyze IV estimators with many instruments: “The sequence is designed to make the asymptotic distribution fit the finite sample distribution better. It is completely irrelevant whether or not further sampling will lead to samples conforming to this sequence” (p. 658).

¹⁰Without regression weights (i.e. $e_{\ell} = \frac{1}{L}$), s_n is instead the average employment share of industry n across locations.

¹¹Formally, in this framework $\mathbb{E}[e_{\ell} s_{\ell n} \varepsilon_{\ell}] = 0$ for each (ℓ, n) , so $\mathbb{E}[\sum_n s_n g_n \bar{\varepsilon}_n] = \sum_n g_n \sum_{\ell} \mathbb{E}[e_{\ell} s_{\ell n} \varepsilon_{\ell}] = 0$.

industry to be a valid instrument in the labor supply equation, i.e. uncorrelated with all unobserved labor supply shocks. This assumption is unlikely to hold: changes in foreign immigration, for example, are a type of local labor supply shock which is likely related to the local industry composition (e.g., new immigrants may prefer to settle in areas with larger clusters of specific industries, such as high-tech, even conditionally on the prevailing wage). Formally, whenever the second-stage error term has a component with the shift-share structure, $\sum_n s_{\ell n} \nu_n$ for unobserved shocks ν_n , then the exposure shares will be mechanically endogenous even if the ν_n and g_n are uncorrelated (see Appendix A.2 for a proof).¹²

When shares are endogenous, equation (5) suggests that identification may instead follow from the exogeneity of shocks. We formalize this approach in Section 3.1, by specifying a quasi-experimental design in which the g_n are as-good-as-randomly assigned with respect to the other terms in the expression. We show how this simple exogeneity can be relaxed with controls in Section 3.2.

2.3 Estimator Equivalence

We next build intuition for our approach to SSIV consistency by showing that the estimate $\hat{\beta}$ is equivalently obtained as the coefficient from a non-standard shock-level IV procedure, in which g_n directly serves as the instrument. This equivalence result suggests that the large-sample properties of $\hat{\beta}$ can be derived from a law of large numbers for the equivalent shock-level regression. An attractive feature of this approach is that it does not rely on an assumption of *iid* observations, which can be untenable in the presence of observed and unobserved n -level shocks. We instead place assumptions on the assignment of the equivalent IV regression’s instrument g_n , similar to a more standard analysis of a randomized treatment in an experimental settings (Abadie et al. 2019).

Formally, we have the following equivalence result:

Proposition 1 The SSIV estimator $\hat{\beta}$ equals the second-stage coefficient from a s_n -weighted shock-level IV regression that uses the shocks g_n as the instrument in estimating

$$\bar{y}_n^\perp = \alpha + \beta \bar{x}_n^\perp + \bar{\varepsilon}_n^\perp, \quad (6)$$

where $\bar{v}_n = \frac{\sum_\ell e_\ell s_{\ell n} v_\ell}{\sum_\ell e_\ell s_{\ell n}}$ denotes an exposure-weighted average of variable v_ℓ .

PROOF By definition of z_ℓ ,

$$\hat{\beta} = \frac{\sum_\ell e_\ell (\sum_n s_{\ell n} g_n) y_\ell^\perp}{\sum_\ell e_\ell (\sum_n s_{\ell n} g_n) x_\ell^\perp} = \frac{\sum_n g_n (\sum_\ell e_\ell s_{\ell n} y_\ell^\perp)}{\sum_n g_n (\sum_\ell e_\ell s_{\ell n} x_\ell^\perp)} = \frac{\sum_n s_n g_n \bar{y}_n^\perp}{\sum_n s_n g_n \bar{x}_n^\perp}. \quad (7)$$

¹²In the immigration example, ν_n is positive in high-tech industries and negative in industries that do not attract immigrants. Note that the same argument applies to migration flows within the U.S., which can similarly make local labor supply shocks related to local industry composition. Lagging local employment shares does not alleviate these threats to identification in general.

Furthermore $\sum_n s_n \bar{y}_n^\perp = \sum_\ell e_\ell (\sum_n s_{\ell n}) y_\ell^\perp = \sum_\ell e_\ell y_\ell^\perp = 0$, since y_ℓ^\perp is an e_ℓ -weighted regression residual and $\sum_n s_{\ell n} = 1$. This and an analogous equality for \bar{x}_n^\perp imply that (7) is a ratio of s_n -weighted covariances, of \bar{y}_n^\perp and \bar{x}_n^\perp with g_n . Hence it is obtained from the specified IV regression.

As with equation (5), Proposition 1 exploits the structure of the instrument to exchange orders of summation in the expression for the SSIV estimator (4). This exchange shows that SSIV estimates can also be thought to arise from variation across shocks, rather than across observations. The equivalent IV regression uses the shocks g_n directly as the instrument and shock-level aggregates of the original (residualized) outcome and treatment, \bar{y}_n^\perp and \bar{x}_n^\perp . Specifically, \bar{y}_n^\perp reflects the average residualized outcome of the observations most exposed to the n th shock, while \bar{x}_n^\perp is the same weighted average of residualized treatment. The regression is weighted by s_n , representing each shock’s average exposure across the observations.¹³

The fact that shift-share estimates can be equivalently obtained by a shock-level IV procedure suggests a new approach to establishing their consistency. Generally, IV regressions of the form of (7) will be consistent when the instrument (here, g_n) is as-good-as randomly assigned, there is a large the number of observations (here, N), the importance weights are sufficiently dispersed (here, that the s_n are not too skewed), and there is an asymptotic first stage. Consistency is then guaranteed regardless of the correlation structure of the residuals $\bar{\varepsilon}_n^\perp$, and thus in the primitive residuals ε_ℓ and exposure shares $s_{\ell n}$. We formalize this approach below.

Before proceeding, it is worth emphasizing that while the shock-level IV regression from Proposition 1 motivates our approach to *identification* of β , it does not affect the *interpretation* of the coefficient as measuring an ℓ -level relationship. The shock-level equation (6), in which the outcome and treatment are unconventional shock-level objects, does not have independent economic content. For example, in the labor supply setting \bar{y}_n is not industry n ’s wage growth; rather, it measures the average wage growth in regions where industry n employs the most workers. Thus, while $\hat{\beta}$ can be computed at the industry level it estimates the elasticity of regional, rather than industry, labor supply, and could, for example, capture the kinds of local spillovers that a regression of industry wages on industry employment cannot.¹⁴ Furthermore, Proposition 1 holds even for the outcomes and treatments which cannot be naturally computed at the shock level, e.g. when n indexes industries and y_ℓ measures labor force non-participation, as in Autor et al. (2013).

¹³In the special case of reduced-form shift-share regressions, Proposition 1 shows that the equivalent shock-level procedure is still an IV regression, of \bar{y}_n^\perp on the transformed shift-share instrument \bar{z}_n^\perp , again instrumented by g_n and weighted by s_n .

¹⁴In Appendix A.3 we develop a stylized model to illustrate how the SSIV coefficient can differ from a “native” shock-level IV coefficient in the presence of local spillovers or treatment effect heterogeneity, though both parameters may be of interest. Intuitively, in the labor supply case one may estimate a low regional elasticity but a high elasticity of industry labor supply if, for example, migration is constrained but workers are mobile across industries within a region.

3 A Quasi-Experimental SSIV Framework

We now show how SSIV identification and consistency can be satisfied by a quasi-experiment in which shocks are as-good-as-randomly assigned, mutually uncorrelated, large in number, and sufficiently dispersed in terms of their average exposure. Instrument relevance generally holds in such settings when the exposure of individual observations tends to be concentrated in a small number of shocks, and when those shocks affect treatment. We then show how this framework is naturally generalized to settings in which shocks are only conditionally quasi-randomly assigned or exhibit some forms of mutual dependence, such as clustering.

3.1 Quasi-Randomly Assigned and Mutually Uncorrelated Shocks

Our approach to SSIV consistency is based on a thought experiment in which the shocks g_n are as-good-as-randomly assigned conditional on the shock-level unobservables $\bar{\varepsilon}_n$ and exposure weights s_n . As motivated above, placing assumptions on this assignment process (rather than on the sampling properties of observations) has two key advantages. First, we do not rely on conventional assumptions of independent or clustered data which are generally inconsistent with the shift-share data structure when the shocks are considered random variables. Second, in conditioning on $\bar{\varepsilon} = \{\bar{\varepsilon}_n\}_n$ and $s = \{s_n\}_n$ we place no restrictions on the dependence between the $s_{\ell n}$ and ε_ℓ , allowing shock exposure to be endogenous. We first show that such endogeneity need not pose problems for SSIV identification:

Proposition 2 The SSIV moment condition (3) is satisfied by the following condition:

Assumption 1 (*Quasi-random shock assignment*): $\mathbb{E}[g_n | \bar{\varepsilon}, s] = \mu$, for all n .

PROOF By equation (5) and the law of iterated expectations, $\mathbb{E}[\sum_\ell e_\ell z_\ell \varepsilon_\ell] = \mathbb{E}[\sum_n s_n g_n \bar{\varepsilon}_n] = \mu \cdot \mathbb{E}[\sum_n s_n \bar{\varepsilon}_n]$ under Assumption 1. Furthermore, since $\sum_\ell s_{\ell n} = 1$ and $\mathbb{E}[\sum_\ell e_\ell \varepsilon_\ell] = 0$ by construction, $\mathbb{E}[\sum_n s_n \bar{\varepsilon}_n] = \mathbb{E}[(\sum_n s_{\ell n}) (\sum_\ell e_\ell \varepsilon_\ell)] = 0$.

Proposition 2 shows that the shift-share instrument is valid, in that the IV moment condition (3) holds, when the underlying shocks are as-good-as-randomly assigned: each g_n has the same mean μ , regardless of the realizations of the relevant unobservables $\bar{\varepsilon}$ (and average exposures s). In the labor supply example this assumption would mean that import tariffs should not have been chosen strategically, based on labor supply trends, or in a way that is correlated with such trends.¹⁵

It follows from Proposition 2 that β is identified by Assumption 1 provided the instrument is relevant.¹⁶ In practice, the existence of a non-zero first stage can be inferred from the data; we discuss

¹⁵For example, if labor supply trends differ between regions specializing in manufacturing vs. services, the import tariffs should apply to both types of sectors. We discuss in Section 4.2 how to apply our framework in the case where import tariffs only apply to a subsector of the economy, e.g. in manufacturing only.

¹⁶Appendix A.1 shows how SSIV identifies a convex average of heterogeneous treatment effects (varying potentially across both ℓ and n) under a stronger notion of as-good-as-random shock assignment and a first-stage monotonicity condition. This can be seen as generalizing both the IV identification result of Angrist et al. (2000) to shift-share instruments, as well as the reduced-form shift-share identification result in Adão et al. (2019).

appropriate inferential techniques in Section 5. To illustrate how such instrument relevance might hold with quasi-experimental shocks, we consider a simple first-stage model. Consider a setting without controls ($w_\ell = 1$) and where treatment is a share-weighted average of shock-specific components: $x_\ell = \sum_n s_{\ell n} x_{\ell n}$, where $x_{\ell n} = \pi_{\ell n} g_n + \eta_{\ell n}$ with $\pi_{\ell n} \geq \bar{\pi}$ almost surely for some fixed $\bar{\pi} > 0$. In line with Assumption 1, suppose that the shocks are independent mean-zero, given the full set of exposure shares $s_{\ell n}$ and regression weights e_ℓ and the full set of $\pi_{\ell n}$ and $\eta_{\ell n}$, with variances that are bounded below by some fixed $\bar{\sigma}_g^2 > 0$. Then the instrument first stage is positive:

$$\begin{aligned} \mathbb{E} \left[\sum_\ell e_\ell z_\ell x_\ell \right] &= \mathbb{E} \left[\sum_\ell e_\ell \left(\sum_n s_{\ell n} g_n \right) \left(\sum_n s_{\ell n} (\pi_{\ell n} g_n + \eta_{\ell n}) \right) \right] \\ &\geq \bar{\pi} \bar{\sigma}_g^2 \mathbb{E} \left[\sum_\ell e_\ell \sum_n s_{\ell n}^2 \right] > 0. \end{aligned} \quad (8)$$

Given identification, SSIV consistency follows from an appropriate law of large numbers. Motivated by the estimator equivalence in Section 2.3, we consider settings in which the effective sample size of the shock-level IV regression (6) is large and the observations of the effective instrument (shocks) are mutually uncorrelated:

Assumption 2 (*Many uncorrelated shocks*): $\mathbb{E} [\sum_n s_n^2] \rightarrow 0$ and $\text{Cov} [g_n, g_{n'} \mid \bar{\varepsilon}, s] = 0$ for all (n, n') with $n' \neq n$.

The first part of Assumption 2 states that the expected Herfindahl index of average shock exposure, $\mathbb{E} [\sum_n s_n^2]$, converges to zero as $L \rightarrow \infty$. This condition implies that the number of observed shocks grows with the sample (since $\sum_n s_n^2 \geq 1/N$), and can be interpreted as requiring a large effective sample for the equivalent shock-level IV regression. An equivalent condition is that the largest importance weight in this regression, s_n , becomes vanishingly small.¹⁷ The second part of Assumption 2 states that the shocks are mutually uncorrelated given the unobservables and s_n . Both of these conditions, while novel for SSIV, would be standard assumptions to establish the consistency of a conventional shock-level IV estimator with g_n as the instrument and s_n weights.

Assumptions 1 and 2 are the baseline assumptions of our quasi-experimental framework. Given a standard relevance condition and additional regularity conditions listed in Appendix B.1, they are sufficient to establish SSIV consistency:¹⁸

Proposition 3 Suppose Assumptions 1 and 2 hold, $\sum_\ell e_\ell z_\ell x_\ell \stackrel{p}{\rightarrow} \pi$ with $\pi \neq 0$, and Assumptions

¹⁷Goldsmith-Pinkham et al. (2020) propose a different measure of the importance of a given n , termed ‘‘Rotemberg weights.’’ In Appendix A.4 we show the formal connection between s_n and these weights, and that the latter do not carry the sensitivity-to-misspecification interpretation as they do in the exogenous shares view of Goldsmith-Pinkham et al. (2020). Instead, the Rotemberg weight of shock n measures the leverage of n in the equivalent shock-level IV regression from Proposition 1. Shocks may have large leverage either because of large s_n , as would be captured by the Herfindahl index, or because the shocks have a heavy-tailed distribution which is allowed by Assumption 2.

¹⁸One high-level condition used in Proposition 3 (Assumption B2) is that the control coefficient γ is consistently estimated by its sample analog, $\hat{\gamma} = (\sum_\ell e_\ell w_\ell w'_\ell)^{-1} \sum_\ell e_\ell w_\ell \epsilon_\ell$ (see footnote 5). We discuss sufficient conditions for this assumption in Appendix A.5.

B1-B2 hold. Then $\hat{\beta} \xrightarrow{p} \beta$.

PROOF See Appendix B.1.

As before, the relevance condition merits further discussion. In our simple first-stage model, $\sum_{\ell} e_{\ell} z_{\ell} x_{\ell}^{\perp}$ converges to $\mathbb{E} [\sum_{\ell} e_{\ell} z_{\ell} x_{\ell}]$ under appropriate regularity conditions, which is bounded above zero by a term proportional to $\mathbb{E} [\sum_{\ell} e_{\ell} \sum_n s_{\ell n}^2]$. Thus, in this case, SSIV relevance holds when the e_{ℓ} -weighted average of local exposure Herfindahl indices $\sum_n s_{\ell n}^2$ across observations does not vanish in expectation. In our running labor supply example, where $x_{\ell n}$ is industry-by-region employment growth, SSIV relevance generally arises from individual regions ℓ tending to specialize in a small number of industries n , provided import tariffs have a non-vanishing effect on local industry employment.¹⁹ Compare this to the Herfindahl condition in Assumption 2, which instead states that the *average* shares of industries across locations become small. Both conditions may simultaneously hold when most regions specialize in a small number of industries, differentially across a large number of industries.²⁰

3.2 Conditional Shock Assignment and Weak Shock Dependence

Proposition 3 establishes SSIV consistency when shocks have the same expectation across n and are mutually uncorrelated, but both requirements are straightforward to relax. We next provide extensions that allow the shock expectation to depend on observables and for weak mutual dependence (such as clustering or serial correlation) of the residual shock variation.

We first relax Assumptions 1 and 2 to only hold conditionally on a vector of shock-level observables q_n (that includes a constant). For example, it may be more plausible that shocks are as-good-as-randomly assigned within a set of observed clusters $c(n) \in \{1, \dots, C\}$ with non-random cluster-average shocks, in which case q_n collects $C - 1$ cluster dummies and a constant. In the labor supply example, this may allow import tariffs to vary systematically across groups of industries with similar labor intensity, but be as-good-as-random within each of those groups. In general, with $q = \{q_n\}_n$, we consider the following weakened version of Assumption 1:

Assumption 3 (*Conditional quasi-random shock assignment*): $\mathbb{E} [g_n \mid \bar{\varepsilon}, q, s] = q'_n \mu$, for all n .

Similarly, we consider a weakened version of Assumption 2 which imposes mutual uncorrelatedness on the residual $\tilde{g}_n = g_n - q'_n \mu$:

¹⁹Note that this precludes consideration of an asymptotic sequence where L remains finite as N grows. With L (and also e_1, \dots, e_L) fixed, Assumption 2 implies $\sum_{\ell} e_{\ell}^2 \mathbb{E} [\sum_n s_{\ell n}^2] \rightarrow 0$ and thus $\text{Var} [z_{\ell}] = \text{Var} [\sum_n s_{\ell n} g_n] \rightarrow 0$ for each ℓ if $\text{Var} [g_n]$ is bounded. If the instrument has asymptotically no variation it cannot have a first stage, unless the $\pi_{\ell n}$ grow without bound. This result also highlights the role of picking the shares which reflect the impact of g_n on x_{ℓ} . Here when the shares are misspecified, i.e. when the treatment is constructed from different shares $\tilde{s}_{\ell n}$ as $x_{\ell} = \sum_n \tilde{s}_{\ell n} x_{\ell n}$, the first-stage is bounded by a term proportional to $\mathbb{E} [\sum_{\ell} e_{\ell} \sum_n s_{\ell n} \tilde{s}_{\ell n}]$, which can be arbitrarily small even if $\mathbb{E} [\sum_{\ell} e_{\ell} \sum_n \tilde{s}_{\ell n}^2] \not\rightarrow 0$.

²⁰As an extreme example, suppose each region specializes on one industry only: $s_{\ell n} = \mathbf{1} [n = n(\ell)]$ for some $n(\ell)$. Then the average local concentration index $\sum_{\ell} e_{\ell} \sum_n s_{\ell n}^2$ equals one, while Assumption 2 holds when national industry composition is asymptotically dispersed: for example, when $e_{\ell} = 1/L$ and $n(\ell)$ is drawn *iid* across regions and uniformly over $1, \dots, N$.

Assumption 4 (*Many uncorrelated shock residuals*): $\mathbb{E} [\sum_n s_n^2] \rightarrow 0$ and $\text{Cov} [\tilde{g}_n, \tilde{g}_{n'} \mid \bar{\varepsilon}, q, s] = 0$ for all (n, n') with $n' \neq n$.

In the shock cluster example, Assumption 4 applies with a small number of clusters, each with its own random effect, as in that case a law of large numbers may apply to the within-cluster residuals \tilde{g}_n but not the original shocks g_n .

By a simple extension of the proof to Proposition 3, the SSIV estimator is consistent when these conditions replace Assumptions 1 and 2 and the residual shift-share instrument $\tilde{z}_\ell = \sum_n s_{\ell n} \tilde{g}_n$ replaces z_ℓ . While this instrument is infeasible, since μ is unknown, the following result shows that SSIV regressions that control for the exposure-weighted vector of shock-level controls, $\tilde{w}_\ell = \sum_n s_{\ell n} q_n$, provide a feasible implementation:

Proposition 4 Suppose Assumptions 3 and 4 hold, $\sum_\ell e_\ell z_\ell x_\ell^\perp \xrightarrow{P} \pi$ with $\pi \neq 0$, and Assumptions B1-B2 hold. Then $\hat{\beta} \xrightarrow{P} \beta$ provided \tilde{w}_ℓ is included in w_ℓ .

PROOF See Appendix B.1.

This result highlights a special role of controls with a shift-share structure (i.e. $\sum_n s_{\ell n} q_n$): besides removing confounding variation from the residual (as any w_ℓ would do), they can also be viewed as removing such variation directly from the shocks (i.e. implicitly using \tilde{g}_n in place of g_n). In particular, Proposition 4 shows that controlling for each observation's individual exposure to each cluster $\sum_n s_{\ell n} \mathbf{1}[c(n) = c]$ isolates the within-cluster variation in shocks. This allows for a thought experiment in which shocks are drawn quasi-randomly only within observed clusters, but not across clusters with potentially different shock means. Note that Proposition 3 is obtained as a special case of Proposition 4, which sets $q_n = 1$.

Even conditional on observables, mutual shock uncorrelatedness may be undesirably strong. It is, however, straightforward to further relax this assumption to allow for shock assignment processes with weak mutual dependence, such as further clustering or autocorrelation. In Appendix B.1 we prove extensions of Proposition 4 which replace Assumption 4 with one of the following alternatives:

Assumption 5 (*Many uncorrelated shock clusters*): There exists a partition of shocks into clusters $c(n)$ such that $\mathbb{E} [\sum_c s_c^2] \rightarrow 0$ for $s_c = \sum_{n: c(n)=c} s_n$ and $\text{Cov} [\tilde{g}_n, \tilde{g}_{n'} \mid \bar{\varepsilon}, q, s] = 0$ for all (n, n') with $c(n) \neq c(n')$;

Assumption 6 (*Many weakly correlated shocks*): For some sequence of numbers $B_L \geq 0$ and a fixed function $f(\cdot)$ satisfying $\sum_{r=1}^\infty f(r) < \infty$, $B_L \mathbb{E} [\sum_n s_n^2] \rightarrow 0$ and $|\text{Cov} [\tilde{g}_n, \tilde{g}_{n'} \mid \bar{\varepsilon}, q, s]| \leq B_L \cdot f(|n' - n|)$ for all (n, n') .

Assumption 5 relaxes Assumption 4 by allowing shock residuals to be grouped within mutually mean-independent clusters $c(n)$, while placing no restriction on their within-cluster correlation. At the same

time, the Herfindahl index assumption of Assumption 4 is strengthened to hold for industry clusters, with s_c denoting the average exposure of cluster c . Assumption 6 takes a different approach, allowing all nearby shock residuals to be mutually correlated provided their covariance is bounded by a function $B_L \cdot f(|n' - n|)$. This accommodates, for example, the case of first-order autoregressive time series with the covariance bound declining at a geometric rate, i.e. $f(r) = \delta^r$ for $\delta \in [0, 1)$ and constant B_L . With B_L growing, stronger dependence of nearby shocks is also allowed (see Appendix B.1).

4 Extensions

We now present several other extensions of our quasi-experimental framework. Section 4.1 discusses how our framework applies when the shocks are estimated within the sample, as in the canonical Bartik (1991) study. Section 4.2 explains the need for additional controls when the sum of exposure shares vary across observations. Section 4.3 considers shift-share identification with panel data. Finally, Section 4.4 extends the framework to allow for multiple treatments and shift-share instruments.

4.1 Shift-Share Designs with Estimated Shocks

In some shift-share designs, the shocks are equilibrium objects that can be difficult to view as being quasi-randomly assigned. For example, in the canonical Bartik (1991) estimation of the regional labor supply elasticity, the shocks are national industry employment growth rates. Such growth reflects labor demand shifters, which one may be willing to assume are as-good-as-randomly assigned across industries. However industry growth also aggregates regional labor supply shocks that directly enter the residual ε_ℓ . Here we show how the quasi-experimental SSIV framework can still apply in such cases, by viewing the g_n as noisy estimates of some latent true shocks g_n^* (labor demand shifters, in the Bartik (1991) example) that satisfy Assumption 1. We establish the conditions on estimation noise (aggregated labor supply shocks, in Bartik (1991)) such that a feasible shift-share instrument estimator, perhaps involving a leave-one-out correction as in Autor and Duggan (2003), is asymptotically valid.

We leave a more general treatment of this issue to Appendix A.6 and for concreteness focus on the Bartik (1991) example. The industry growth rates g_n can be written as weighted averages of the growth of each industry in each region: $g_n = \sum_\ell \omega_{\ell n} g_{\ell n}$, where the weights $\omega_{\ell n}$ are the lagged shares of industry employment located in region ℓ , with $\sum_\ell \omega_{\ell n} = 1$ for each n . In a standard model of regional labor markets, $g_{\ell n}$ includes (to first-order approximation) an industry labor demand shock g_n^* and a term that is proportional to the regional supply shock ε_ℓ .²¹ We suppose that the demand shocks are as-good-as-randomly assigned across industries, such that the infeasible SSIV estimator which uses

²¹Appendix A.7 presents such a model, showing that $g_{\ell n}$ also depends on the regional average of g_n^* (via local general equilibrium effects) and on idiosyncratic region-specific demand shocks. Both of these are uncorrelated with the error term in the model and thus do not lead to violations of Assumption 1; we abstract away from this detail here.

$z_\ell^* = \sum_n s_{\ell n} g_n^*$ as an instrument satisfies our quasi-experimental framework. The asymptotic bias of the feasible SSIV estimator which uses $z_\ell = \sum_n s_{\ell n} g_n$ then depends on the large-sample covariance between the labor supply shocks ε_ℓ and an aggregate of the supply shock “estimation error,”

$$\psi_\ell = z_\ell - z_\ell^* \propto \sum_n s_{\ell n} \sum_{\ell'} \omega_{\ell' n} \varepsilon_{\ell'}. \quad (9)$$

Two insights follow from considering the bias term $\sum_\ell e_\ell \psi_\ell \varepsilon_\ell$. First, part of the covariance between ψ_ℓ and ε_ℓ is mechanical, since ε_ℓ enters ψ_ℓ . In fact, if supply shocks are spatially uncorrelated this is the only source of bias from using z_ℓ rather than z_ℓ^* as an instrument. This motivates the use of a leave-one-out (LOO) shock estimator, $g_{n,-\ell} = \sum_{\ell' \neq \ell} \omega_{\ell' n} g_{\ell' n} / \sum_{\ell' \neq \ell} \omega_{\ell' n}$, and the feasible instrument $z_\ell^{LOO} = \sum_n s_{\ell n} g_{n,-\ell}$ to remove this mechanical covariance.²² Conversely, if the regional supply shocks ε_ℓ are spatially correlated a LOO adjustment may not be sufficient to eliminate mechanical bias in the feasible SSIV instrument, though more restrictive split-sample methods (e.g. those estimating shocks from distant regions) may suffice.

Second, in settings where there are many regions contributing to each shock estimate even the mechanical part of $\sum_\ell e_\ell \psi_\ell \varepsilon_\ell$ may be ignorable, such that the conventional non-LOO shift-share instrument z_ℓ (which, unlike z_ℓ^{LOO} , has a convenient shock-level representation per Proposition 1) is asymptotically valid when z_ℓ^{LOO} is.²³ In Appendix A.6, we derive a heuristic for this case, under the assumption of spatially-independent supply shocks. In a special case when each region is specialized in a single industry and there are no importance weights, the key condition is $L/N \rightarrow \infty$, or that the average number of regions specializing in the typical industry is large. With incomplete specialization or weights, the corresponding condition requires the typical industry to be located in a much larger number of regions than the number of industries that a typical region specializes in.

To illustrate the preceding points in the data, Appendix A.6 replicates the setting of Bartik (1991) with and without a LOO estimator, using data from Goldsmith-Pinkham et al. (2020). We find that in practice the LOO correction does not matter for the SSIV estimate, consistent with the findings of Goldsmith-Pinkham et al. (2020) and Adão et al. (2019), and especially so when the regression is estimated without regional employment weights. Our framework provides an explanation for this: the heuristic statistic we derive is much larger without importance weights. These findings imply that if, in the canonical Bartik (1991) setting, one is willing to assume quasi-random assignment of the underlying industry demand shocks and that the regional supply shocks are spatially-uncorrelated, one can interpret the uncorrected SSIV estimator as leveraging demand variation in large samples, as some of the literature has done (e.g. Suárez Serrato and Zidar (2016)).

²²This problem of mechanical bias is similar to that of two-stage least squares with many instruments (Bound et al. 1995), and the solution is similar to the jackknife instrumental variable estimate approach of Angrist et al. (1999).

²³Adão et al. (2019) derive the corrected standard errors for LOO SSIV and find that they are in practice very close to the non-LOO ones, in which case the SSIV standard errors we derive in the next section are approximately valid even when the LOO correction is used.

4.2 SSIVs with Incomplete Shares

While we have so far assumed the sum of exposure shares is constant, in practice this $S_\ell = \sum_n s_{\ell n}$ may vary across observations ℓ . For example, in the labor supply setting, the quasi-experiment in tariffs may only cover manufacturing industries, while the lagged manufacturing employment shares of $s_{\ell n}$ may be measured relative to total employment in region ℓ . In this case S_ℓ equals the lagged total share of manufacturing employment in region ℓ . The Autor et al. (2013) setting is another example of this scenario, as we discuss below.²⁴

Our framework highlights a potential problem with such “incomplete share” settings: even if Assumptions 1 and 2 hold, the SSIV estimator will generally leverage non-experimental variation in S_ℓ in addition to quasi-experimental variation in shocks. To see this formally, note that one can always return to the complete shares setting by rewriting the shift-share instrument with the “missing” (e.g., non-manufacturing) shock included: $z_\ell = s_{\ell 0}g_0 + \sum_{n>0} s_{\ell n}g_n$, where $g_0 = 0$ and $s_{\ell 0} = 1 - S_\ell$, yielding $\sum_{n=0}^N s_{\ell n} = 1$ for all ℓ . The previous quasi-experimental framework then applies to this expanded set of shocks g_0, \dots, g_N . But since $g_0 = 0$, Proposition 3 requires in this case that $\mathbb{E}[g_n | s, \bar{\varepsilon}] = 0$ for $n > 0$ as well; that is, that the expected shock to each manufacturing industry is the same as the “missing” non-manufacturing shock of zero. Otherwise, even when manufacturing shocks are random, regions with higher manufacturing shares S_ℓ will tend to have systematically different values of the instrument z_ℓ , leading to bias when these regions also have different unobservables.²⁵

Cast in this way, the incomplete shares issue has a natural solution via Assumption 3: to control for the sum of exposure shares. Formally, one can allow the missing and non-missing shocks to have different means by conditioning on the indicator $\mathbf{1}[n > 0]$ in the q_n vector. By Proposition 4, the SSIV estimator allows for such conditional quasi-random assignment when the control vector w_ℓ contains the exposure-weighted average of $\mathbf{1}[n > 0]$, which here is $\sum_{n=0}^N s_{\ell n} \mathbf{1}[n > 0] = S_\ell$.²⁶ Thus, in the labor supply example, quasi-experimental variation in manufacturing shocks is isolated provided one controls for a region’s lagged manufacturing share S_ℓ . More generally, the control $\sum_{n=1}^N s_{\ell n} q_n$ which, per Proposition 4, allows the shock mean to depend on observables q_n for $n > 0$ changes the interpretation in the incomplete shares case: it is a exposure-weighted sum, rather than average.

²⁴We note that this scenario applies to quasi-experiments in which shocks are impossible (*ex ante*) for some industries. In contrast, if all industries were equally likely to receive tariffs but only some did *ex post*, the set of n should include all industries, with $S_\ell = 1$, and zero tariffs captured by $g_n = 0$ for some n . In such a case, however, it is unlikely that all manufacturing industries receive the tariffs by chance when no non-manufacturing industries do.

²⁵Formally, if Assumptions 1 and 2 hold for all $n > 0$ we have from the proof to Proposition 3 that $\sum_\ell e_\ell z_\ell \varepsilon_\ell = \sum_{n=0}^N s_n g_n \bar{\varepsilon}_n = \mathbb{E} \left[\sum_{n=0}^N s_n (g_n - \mu) \bar{\varepsilon}_n \right] + o_p(1) = -\mu \mathbb{E}[s_0 \bar{\varepsilon}_0] + o_p(1)$. If $\mu \neq 0$ and the missing industry share is large ($s_0 \not\rightarrow 0$) this can only converge to zero when $\mathbb{E}[s_0 \bar{\varepsilon}_0] = \mathbb{E}[\sum_\ell e_\ell s_{\ell 0} \varepsilon_\ell]$ does, i.e. when S_ℓ is exogenous.

²⁶By effectively “dummying out” the missing industry, SSIV regressions that control for S_ℓ further require a weaker Herfindahl condition: $\mathbb{E} \left[\sum_{n=1}^N s_n^2 \right] \rightarrow 0$, allowing the non-manufacturing industry share s_0 to stay large.

4.3 Panel Data

In practice, SSIV regressions are often estimated with panel data, where the outcome $y_{\ell t}$, treatment $x_{\ell t}$, exposure shares $s_{\ell nt}$, and shocks g_{nt} are additionally indexed by time periods $t = 1, \dots, T$.²⁷ In such settings a time-varying instrument $z_{\ell t} = \sum_n s_{\ell nt} g_{nt}$ is used, and the controls $w_{\ell t}$ may include unit- or period-specific fixed effects.

It is straightforward to apply the preceding quasi-experimental framework to the panel case with a simple relabeling: $\tilde{\ell} = (\ell, t)$ for the LT observations and $\tilde{n} = (n, \tau)$ for the NT shocks (where τ also indexes time periods). With exposure shares redefined as $\tilde{s}_{\tilde{\ell}\tilde{n}} = s_{\ell nt} \mathbf{1}[t = \tau]$ (i.e. by definition zero for $t \neq \tau$), the instrument can be rewritten as $z_{\tilde{\ell}} = \sum_{\tilde{n}} \tilde{s}_{\tilde{\ell}\tilde{n}} g_{\tilde{n}}$, mirroring the cross-sectional case.

With this relabeling, standard intuitions for panel consistency readily translate into shift-share designs. In short panels or repeated cross-sections (i.e. with fixed T) the SSIV estimator can be consistent if $L, N \rightarrow \infty$ and the cross-sectional conditions of Proposition 4 hold.²⁸ Alternatively, consistency of the estimator can follow from a long time series of shocks ($T \rightarrow \infty$) that have weak serial dependence, even if L and N are small. This case accommodates, in particular, shift-share designs that leverage purely time-series shocks ($N = 1, T \rightarrow \infty$), as in Nunn and Qian (2014).²⁹

One subtlety of panel SSIV regressions concerns the role of fixed effect (FE) controls. Unit fixed effects play a dual role in conventional panels (with exogenous shocks varying at the same level as the observations): they purge both time-invariant unobservables ($\frac{1}{T} \sum_{\tau} \varepsilon_{\ell\tau}$) from the residual and the time-invariant component of the shocks ($\frac{1}{T} \sum_{\tau} g_{n\tau}$). While the first role directly extends to the FEs of cross-sectional units ℓ in the shift-share case, the second role only does when exposure shares are fixed across periods, i.e. when $s_{\ell nt} \equiv s_{\ell n0}$.³⁰ Similarly, while period FEs always purge period-specific unobservables ($\frac{1}{L} \sum_{\ell} \varepsilon_{\ell t}$), in SSIV designs they only isolate within-period shock variation when the exposure shares add up to one. With incomplete shares, in contrast, period FEs need to be interacted with the sum of exposure shares S_{ℓ} .³¹

Finally, we note that while fixing exposure shares may have the advantage of isolating cleaner time-varying shock variation, it may also have an efficiency cost: lagging the shares by many periods

²⁷Exposure shares are typically lagged and sometimes fixed in a pre-period. Our subscript t notation indicates that these shares are used to construct the instrument for period t , not that they are measured in that period.

²⁸Arbitrary serial correlation of shocks can be allowed here via Assumption 5, with n defining a cluster. One complication arises when an increasing number of unit fixed effects are included in the control vector, violating Assumption B2 in Proposition 4. In Appendix A.8 we show how this incidental parameter problem can be solved by imposing shock exogeneity with respect to demeaned residuals.

²⁹Nunn and Qian (2014) estimate the impact of U.S. food aid on civil conflict, using variation in U.S. wheat production (a single “shock” per period) over a long time horizon ($T = 36$ years), interacted with a country’s tendency to receive US food aid (the “exposure shares” for $L = 125$ countries). Our approach may also be appropriate in settings where LT and NT are large despite moderate N and T . Berman et al. (2017), for example, leverage price changes for $N = 14$ minerals over $T = 14$ years in a very large cross-section of spatial cells.

³⁰Shift-share IV settings with panel data and time-invariant shares include, for example, Berman et al. (2015), Berman et al. (2017), Hummels et al. (2014), Imbert et al. (2019), and Nunn and Qian (2014).

³¹Both results follow from Proposition 4. The exposure-weighted sums of shock-level unit FEs, which isolate the time-varying component of g_{nt} , are only absorbed by observation-level FEs when the exposure shares are time-invariant. Similarly, if the q_n include shock-level period FEs, the corresponding exposure-weighted sums equal $\sum_{\tilde{n}} \tilde{s}_{\tilde{\ell}\tilde{n}} \mathbf{1}[\tau = \tilde{t}] = S_{\ell} \mathbf{1}[t = \tilde{t}]$, which simplifies to period FEs when $S_{\ell} = 1$.

is likely to make the shift-share instrument less predictive of treatment (see Appendix A.9 for a formal argument). If a researcher wants to update the shares to maximize the first stage, but also isolate the shock variation over time (which is not achieved by controlling for unit ℓ fixed effects with time-varying shares), she may instead use the first-differenced specification. That is, estimate $\Delta y_{\ell t} = \beta \Delta x_{\ell t} + \gamma' \Delta w_{\ell t} + \Delta \varepsilon_{\ell t}$, instrumenting $\Delta x_{\ell t}$ with $z_{\ell t, FD} = \sum_n s_{\ell n, t-1} \Delta g_{nt}$, where Δ is the first-differencing operator for both observations and shocks. This strategy has been employed, for example, by Autor et al. (2013) as we discuss in Section 6.2.³²

4.4 Multiple Shocks and Treatments

In some shift-share designs one may be interested in leveraging several shift-share instruments, corresponding to multiple sets of shocks satisfying Assumptions 3 and 4. For example while Autor et al. (2013) construct an instrument from average Chinese import growth across eight non-U.S. countries, in principle the industry shocks from each individual country may be each thought to be as-good-as-randomly assigned. Jaeger et al. (2018) instrument two treatments—the current and lagged immigration rates—with two shift-share instruments. Bombardini and Li (2019) estimate the reduced-form effects of two shift-share variables: the regional growth of all exports and the regional growth of exports in pollution-intensive sectors. In Appendix A.10 we show that our quasi-experimental framework extends to these settings, in which the exposure shares used to construct the instruments are the same but the shocks differ. The key insight is that SSIV regressions with multiple instruments—with and without multiple endogenous variables—again have an equivalent representation as particular shock-level IV estimators, although the equivalence result is more complex under overidentification.

5 Shock-Level Inference and Testing

A shock-level view also brings new insights to SSIV inference and testing. In this section we first show how a problem with conventional SSIV inference, first studied by Adão et al. (2019), has a convenient solution based on our shock-level equivalence result. We then discuss how other novel shock-level procedures can be used to assess first-stage relevance and to implement valid falsification tests of shock exogeneity. Lastly, we summarize a variety of Monte-Carlo simulations illustrating the finite-sample properties of SSIV.

³²There is another argument for fixing the shares in a pre-period that arises when the current shares are affected by lagged shocks in a way that is correlated with unobservables $\varepsilon_{\ell t}$. In the labor supply example, suppose local labor markets vary in flexibility, with stronger reallocation of employment to industries with larger increases in import tariffs in flexible markets. If import tariffs are random but persistent, industries with growing tariffs will be increasingly concentrated in regions with flexible labor markets and Assumption 1 will be violated if such flexibility is correlated with $\varepsilon_{\ell t}$. This concern is not specific to panel data, but may also arise in cross-sections or repeated cross-sections.

5.1 Exposure-Robust Standard Errors

As with consistency, SSIV inference is complicated by the fact that the observed shocks g_n and any unobserved shocks ν_n induce dependencies in the instrument z_ℓ and residual ε_ℓ across observations with similar exposure shares. This problem can be understood as an extension of the standard clustering concern (Moulton 1986), in which the instrument and residual are correlated across observations within predetermined clusters, with the additional complication that in SSIV every pair of observations with overlapping shares may have correlated $(z_\ell, \varepsilon_\ell)$. Adão et al. (2019) develop a novel approach to conducting valid inference in presence of exposure-based clustering, building on our quasi-experimental framework for identification.

Our equivalence result in Section 2.3 motivates a convenient alternative approach to valid SSIV inference. By estimating SSIV coefficients with an equivalent shock-level IV regression, one directly obtains valid (“exposure-robust”) standard errors under the assumptions in Adão et al. (2019) and an additional condition on the controls that we discuss below.³³

Proposition 5 Consider s_n -weighted IV estimation of the second stage equation

$$\bar{y}_n^\perp = \alpha + \beta \bar{x}_n^\perp + q_n' \delta + \bar{\varepsilon}_n^\perp \quad (10)$$

where $\tilde{w}_\ell = \sum_n s_{\ell n} q_n$ is included in the control vector w_ℓ used to compute \bar{y}_n^\perp and \bar{x}_n^\perp , and \bar{x}_n^\perp is instrumented by g_n . The IV estimate of β is numerically equivalent to the SSIV estimate $\hat{\beta}$. Furthermore, when Assumptions B3–B6 in Appendix B.2 hold and $\sum_\ell e_\ell x_\ell^\perp z_\ell \xrightarrow{p} \pi$ for $\pi \neq 0$, the conventional heteroskedasticity-robust standard error for $\hat{\beta}$ yields asymptotically-valid confidence intervals for β .

PROOF See Appendix B.2.

Equation (10) extends the previous shock-level estimating equation (6) by including a vector of controls q_n which, as in Proposition 4, are included in the SSIV control vector w_ℓ as exposure-weighted averages. The first result in Proposition 5 is that the addition of these controls does not alter the coefficient equivalence established in Proposition 1. The second result states conditions, which strengthen those of Proposition 4, under which conventional shock-level standard errors from estimation of (10) yield valid asymptotic inference on β .³⁴

While our previous results do not restrict the structure of the control vector w_ℓ , Proposition 5 (specifically, Assumption B4) allows for only two types of controls. All sources of shock-level confounding have to be captured by controls with a shift-share structure (i.e. $\sum_n s_{\ell n} q_n$, as in Proposition

³³This solution generalizes a well-known approach to addressing conventional group clustering (Angrist and Pischke 2008, p. 313): by estimating a regression at the level of as-good-as-random variation (here, shocks) one avoids inferential biases due to clustering (here, by shock exposure).

³⁴Appendix B.2 also establishes two related results regarding specifications without controls and an alternative inference procedure to improve finite-sample performance.

4); the other controls should not be asymptotically correlated with the instrument although they may increase the asymptotic efficiency of the estimator. While valid shift-share inference with general control vectors remains an open problem, we show in Appendix B.2 that the standard errors from Proposition 5 are asymptotically conservative under a much weaker assumption, which allows for controls of the form $\sum_n s_{\ell n} p_n + u_\ell$, where p_n are unobserved confounders and u_ℓ is noise.³⁵

Our shock-level approach to estimating exposure-robust standard errors offers three practical features. First, it can be performed with standard statistical software packages given a simple initial transformation of the data (i.e. to obtain \bar{y}_n^\perp , \bar{x}_n^\perp , and s_n), for which we have released a Stata package *ssaggregate* (see footnote 3). Second, it is readily extended to settings where shocks are clustered or autoregressive, as in Assumptions 5 and 6 respectively. Conventional cluster-robust or heteroskedastic-and-autocorrelation-consistent (HAC) standard error calculations applied to equation (10) are then valid. Third, the shock-level inference approach works when $N > L$ or when some exposure shares are collinear.³⁶

5.2 Falsification and Relevance Tests

Our Proposition 5 also provides a practical way to perform valid regression-based tests of shock orthogonality (i.e. falsification tests) and first-stage relevance. As a falsification test of Assumption 3, one may regress an observed proxy r_ℓ for the unobserved residual ε_ℓ on the instrument z_ℓ (controlling for w_ℓ). Examples of such r_ℓ include baseline characteristics realized prior to the shocks or lagged observations of the outcome (yielding a so-called “pre-trend” test). To make the magnitude of the placebo coefficient more interpretable, one may also regress r_ℓ on x_ℓ while instrumenting with z_ℓ . To address the exposure clustering problem that may arise in these regressions, the shock-level regression of Proposition 5 can be used, yielding valid inference on these coefficients. If a researcher instead starts from a shock-level confounder r_n , they can construct its observation-level average $r_\ell = \sum_n s_{\ell n} r_n$ and proceed similarly; a simpler test regresses r_n on g_n directly (weighting by s_n and controlling for q_n).³⁷

Similarly, Proposition 5 yields a convenient way to test first-stage relevance in the OLS regression of x_ℓ on z_ℓ and w_ℓ . We note that the equivalent shock-level regression is IV, not OLS (see footnote 13). The first stage F -statistic, which is a common heuristic for relevance, is then obtained as a squared t -statistic.³⁸

³⁵Adão et al. (2019) provide asymptotically valid standard errors in a special case of this weaker assumption: when the average variance of the noise u_ℓ is asymptotically small for all controls that are necessary for identification. Our standard errors remain asymptotically conservative in this case.

³⁶This is not possible with the standard error calculation of Adão et al. (2019) because their procedure involves projecting z_ℓ^\perp on the vector of shares in order to account for the shock-level confounders underlying the approximate shift-share controls. This issue can be empirically relevant: for instance, employment shares of some industries are collinear in the Autor et al. (2013) setting.

³⁷While pre-trend and other ℓ -level balance tests are also useful in the alternative Goldsmith-Pinkham et al. (2020) framework (albeit with a different approach to inference), this shock-level test is specific to our approach to identification. We emphasize that all tests discussed here are meant to falsify the quasi-random shock assignment assumption made *a priori*, and not to test the two frameworks against each other.

³⁸We generalize this result to the case of multiple shift-share instruments in Appendix A.10 by detailing the appropriate construction of the “effective” first-stage F -statistic of Montiel Olea and Pflueger (2013), again based on an equivalent

5.3 Monte-Carlo Simulations

Though the exposure-robust standard errors obtained from estimating equation (10) are asymptotically valid, it is useful to verify that they offer appropriate coverage with a finite number of observations and shocks. In Appendix A.11 we provide Monte-Carlo simulations confirming that the finite-sample performance of the equivalent regression (10) is comparable to that of more conventional shock-level IV regressions, in which the outcome and instrument are not aggregated from a common set of y_ℓ and x_ℓ . The asymptotic approximation performs well even with a Herfindahl concentration index $\sum_n s_n^2$ of 1/20 (which can be compared to a shock-level regression with 20 equal-sized industries); the conventional rule of thumb for detecting weak instruments based on the appropriate constructed first-stage F -statistic applies equally well to SSIV estimators. These results indicate that a researcher who is comfortable with the finite-sample performance of a shock-level analysis with some set of g_n should also be comfortable using such shocks in SSIV, provided there is sufficient variation in exposure shares to yield a strong SSIV first stage.

6 Shift-Share IV in Practice

We now summarize and illustrate the practical implications of our econometric framework. We first characterize the kinds of empirical settings to which the foregoing framework may be applied. We then apply the framework to the influential setting of Autor et al. (2013).

6.1 A Taxonomy of SSIV Settings

Our framework can be applied to various empirical settings. To characterize these settings, we distinguish between three cases of SSIVs employed in the literature.

In the first case, the shift-share instrument is based on a set of shocks which can itself be thought of as an instrument. For example, the g_n which enter z_ℓ might correspond to a set of observed growth rates that could be plausibly thought of as being randomly assigned to a large number of industries. Our framework shows how the shift-share instrument maps these shocks to the level of observed outcomes and treatments (e.g., geographic regions). A researcher who is comfortable with the identification conditions and finite-sample performance of an industry-level analysis based on g_n should generally also be comfortable applying our framework, provided there is sufficient variation in the exposure shares and treatment to yield a strong first stage. Autor et al. (2013) and the corresponding industry-level analysis conducted by Acemoglu et al. (2016) give a prime example of this case, as we show below.

Empirical settings covered by this first case belong to various fields in economics, with outcomes and shocks defined at levels (ℓ and n , respectively) different than regions and industries. In international shock-level IV regression.

trade, Hummels et al. (2014) estimate the wage effects of offshoring across Danish importing firms ℓ . They leverage a shift-share instrument for offshoring based on shocks to export supply by type of intermediate inputs and origin country; titanium hinges from Japan is an example of an n . While they translate these shocks to the firm level by using the lagged composition of firm imports as the shares, one could imagine an analysis of Danish imports at the input-by-country level directly that would leverage the same supply shocks. In finance, Xu (2019) examines the long-term effects of financial shocks on exports across countries ℓ . Her shift-share instrument is based on a disruption that affected some but not all London-based banks n in 1866, with country-specific exposure shares measuring pre-1866 market shares of those banks in each country. In line with considering bank shocks as-good-as-randomly assigned, she reports that affected and unaffected banks were balanced on various observable characteristics. In the immigration literature, Peri et al. (2016) estimate the effect of immigrant STEM workers on the labor market outcomes of natives across U.S. cities ℓ . They exploit variation in the supply of STEM workers across migration origin countries n and over time that arises from plausibly exogenous shifts to national H1-B policy. Similarly, in a literature on innovation, Stuen et al. (2012) leverage education policy shocks in foreign countries as a supply shock to U.S. doctoral programs.

In the second case, a researcher does not directly observe a large set of quasi-experimental shocks, but can still conceive of an underlying set of g_n which if observed would be a useful instrument. Constructing the instrument then requires an initial step where these shocks are estimated in-sample, potentially introducing mechanical biases. In the canonical setting of Bartik (1991) and Blanchard and Katz (1992), for example, a local labor demand instrument is sought, with the ideal g_n measuring an aggregate change in industry labor demand that may be assumed orthogonal to local labor supply shocks. Aggregate demand changes are however not directly observed and must be estimated from national industry employment growth (often using leave-out corrections, as in Autor and Duggan (2003) and Diamond (2016)). We have discussed how our framework generalizes to this more involved setting in Section 4.1, showing the additional assumptions required for the estimation error to be asymptotically ignorable. While this case differs from the first in terms of the instrument construction, the underlying logic of our framework still applies. This case also covers instruments in the immigration literature, as in Card (2001) and Card (2009), where latent shocks to out-migration from foreign countries can be thought to be as-good-as-randomly assigned but are estimated from aggregate immigration flows in the U.S.

The third case is conceptually distinct, in that the g_n underlying the (perhaps idealized) instrument cannot be naturally viewed as an instrument itself. This could either be because it is not plausible that these shocks are as-good-as-randomly assigned, even conditionally on shock-level observables, or because there are too few shocks. Identification in this case may instead follow from exogeneity of the exposure shares, as suggested by Goldsmith-Pinkham et al. (2020).

Share exogeneity may be a more plausible approach in the third case when the exposure shares are “tailored” to the specific economic question, and to the particular endogenous variable included in the model. In this case, the scenario considered in Section 2.2—that there are unobserved shocks ν_n which enter ε_ℓ through the shares—may be less of a concern. Mohnen (2019), for example, uses the age profile of older workers in local labor markets as the exposure shares of a shift-share instrument for the change in the local elderly employment rate in the following decade. He argues, based on economic intuition, that these tailored shares are uncorrelated with unobserved trends in youth employment rates. This argument notably does not require one to specify the age-specific shocks g_n , which only affect power of the instrument (in fact, the shocks are dispensed with altogether in robustness checks that directly instrument with the shares). Similarly, Algan et al. (2017) use the lagged share of the construction sector in the regional economy as an instrument for unemployment growth during the Great Recession, arguing that it does not predict changes in voting outcomes in other ways. With a single industry the identification assumption reduces to that of conventional difference-in-differences with continuous treatment intensity and our framework cannot be applied.

In contrast, our framework may be more appropriate in settings where shocks are tailored to a specific question while the shares are “generic,” in that they could conceivably measure an observation’s exposure to multiple shocks (both observed and unobserved). Both Autor et al. (2013) and Acemoglu and Restrepo (Forthcoming), for example, build shift-share instruments with similar lagged employment shares but different shocks—rising trade with China and the adoption of industrial robots, respectively. According to the Goldsmith-Pinkham et al. (2020) view, these papers use essentially the same instruments (lagged employment shares) for different endogenous variables (growth of import competition and growth of robot adoption), and are therefore mutually inconsistent. Our framework helps reconcile these identification strategies, provided the variation in each set of shocks can be described as arising from a natural experiment. In principle, shares and shocks may simultaneously provide valid identifying variation, but in practice it would seem unlikely for both sources of variation to be *a priori* plausible in the same setting.

This discussion highlights that plausibility of our framework, as with the alternative framework of Goldsmith-Pinkham et al. (2020), depends on the details of the SSIV application. We encourage practitioners to use our framework only after establishing an *a priori* argument for the plausibility of exogenous shocks. Various diagnostics on the extent of shock variation and falsification of this assumption may then be conducted to assess *ex post* the plausibility of exogenous shocks. We next illustrate this approach in the Autor et al. (2013) setting.

6.2 Application to Autor, Dorn, and Hanson (2013)

Our application to Autor et al. (2013, henceforth ADH) aims to illustrate our theoretical framework only, and not to reassess their substantive findings. In line with this goal, we first describe how the

ADH instrument could be thought to leverage quasi-experimental shocks and discuss potential threats to this identification strategy. We then illustrate the tools and lessons that follow from our framework, demonstrating steps that researchers can emulate in their own SSIV applications. Specifically, we analyze the distribution of shocks to assess the plausibility of Assumption 4 (many conditionally uncorrelated shocks), use balance tests to corroborate the plausibility of Assumption 3 (conditional quasi-random shock assignment), use equivalent shock-level IV regressions to obtain exposure-robust inference, and analyze the sensitivity of the results to the inclusion of different shock-level controls. This analysis shows how our quasi-experimental framework can help understand the identifying variation in the ADH SSIV design.

6.2.1 Setting and Intuition for Identification

ADH use a shift-share IV to estimate the causal effect of rising import penetration from China on U.S. local labor markets. They do so with a repeated cross section of 722 commuting zones ℓ and 397 four-digit SIC manufacturing industries n over two periods t , 1990-2000 and 2000-2007. In these years U.S. commuting zones were exposed to a dramatic rise in import penetration from China, a historic change in trade patterns commonly referred to as the “China shock.” Variation in exposure to this change across commuting zones results from the fact that different areas were initially specialized in different industries which saw different changes in the aggregate U.S. growth of Chinese imports. ADH combine import changes across industries in eight comparable developed economies (as shocks) with lagged industry employment (as exposure shares) to construct their shift-share instrument.

To illustrate our framework in this setting we focus on ADH’s primary outcome of the change in total manufacturing employment as a percentage of working-age population during period t in location ℓ , which we write as $y_{\ell t}$. The treatment variable $x_{\ell t}$ measures local exposure to the growth of imports from China in \$1,000 per worker. The vector of controls $w_{\ell t}$, which comes from the preferred specification of ADH (Column 6 of their Table 3), contains start-of-period measures of labor force demographics, period fixed effects, Census region fixed effects, and the start-of-period total manufacturing share to which we return below. The shift-share instrument is $z_{\ell t} = \sum_n s_{\ell nt} g_{nt}$, where $s_{\ell nt}$ is the share of manufacturing industry n in total employment in location ℓ (measured a decade before each period t begins) and g_{nt} is industry n ’s growth of imports from China in the eight comparable economies over period t (also expressed in \$1,000 per U.S. worker).³⁹ Importantly, the

³⁹To be precise, local exposure to the growth of imports from China is constructed for period t as $x_{\ell t} = \sum_n s_{\ell nt}^{\text{current}} g_{nt}^{\text{US}}$. Here $g_{nt}^{\text{US}} = \frac{\Delta M_{nt}^{\text{US}}}{E_{nt}^{\text{current}}}$ is the growth of U.S. imports from China in thousands of dollars ($\Delta M_{nt}^{\text{US}}$) divided by the industry employment in the U.S. at the beginning of the current period (E_{nt}^{current}) and $s_{\ell nt}^{\text{current}}$ are local employment shares, also measured at the beginning of the period. The instrument, in contrast, is constructed as $z_{\ell t} = \sum_n s_{\ell nt} g_{nt}$ with $g_{nt} = \frac{\Delta M_{nt}^{\text{8 countries}}}{E_{nt}}$, where $\Delta M_{nt}^{\text{8 countries}}$ measures the growth of imports from China in eight comparable economies (in thousands of U.S. dollars) and both local employment shares $s_{\ell nt}$ and U.S. employment E_{nt} are lagged by 10 years. The eight countries are Australia, Denmark, Finland, Germany, Japan, New Zealand, Spain, and Switzerland. Note that Autor et al. (2013) express the same instrument differently, based on employment shares relative to the industry total, rather than the regional total. Our way of writing $z_{\ell t}$ aims to clearly separate the exposure shares from the

sum of lagged manufacturing shares across industries ($S_{\ell t} = \sum_n s_{\ell nt}$) is not constant across locations and periods, placing the ADH instrument in the “incomplete shares” class discussed in Section 4.2. All regressions are weighted by $e_{\ell t}$, which measures the start-of-period population of the commuting zone, and all variables are measured in ten-year equivalents.

To see how the ADH instrument can be viewed as leveraging quasi-experimental shocks, consider an idealized experiment generating random variation in the growth of imports from China across industries. One could imagine, for example, random variation in industry-specific productivities in China affecting import growth both in the U.S. and in comparable economies. This would yield a set of observed productivity changes g_{nt} which would plausibly satisfy our Assumption 1. Assumption 2 would further hold when the productivity shocks are idiosyncratic across many industries, with small average exposure to each shock across commuting zones. Weaker versions of this experimental ideal, in which productivity shocks can be partly predicted by industry observables and are only weakly dependent across industries, are accommodated by the extensions in Section 3.2. For example, in ADH’s repeated cross section one might invoke Assumption 3 in allowing the average shock to vary across periods, in recognition that the 1990s and 2000s were very different trade environments, as China joined the World Trade Organization in 2001. Here q_{nt} would indicate periods.

ADH’s approach can be seen as approximating this idealized experiment by using observed changes in trade patterns between China and a group of developed countries outside the United States. Trade between the U.S. and China depends on changes in U.S. supply and demand conditions, which may have direct effects on employment dynamics in U.S. regions. In contrast, variation in the ADH g_{nt} reflects only Chinese productivity shocks and the various supply and demand shocks in the non-U.S. developed countries. In this way, the ADH strategy can be understood as eliminating bias from shocks that are specific to the United States.

This discussion gives an *a priori* justification for thinking of the ADH instrument as leveraging quasi-experimental shocks within the two periods. Nonetheless, since the ADH shocks are not truly randomized, one may still worry that they are confounded by other unobserved characteristics. For example, China’s factor endowment may imply that it specializes in low-skill industries, which could have been on different employment trends in the U.S. even absent increased trade with China. Similarly, one can imagine a common component of import growth in the U.S. and the group of comparable developed economies due to correlated technological shocks in those countries, which may have a direct effect on U.S. labor markets. Given these potential concerns, it will be important to assess the plausibility of Assumption 3 in this setting by conducting within-period falsification tests of the kind we describe in Section 5.2. It will also be important to assess whether there is sufficient variation in the ADH shocks for Assumption 4 to hold.

Before applying these tests, it is worth highlighting that the assumption of exogenous exposure

 industry shocks, highlighting the shift-share structure of the instrument.

shares, as discussed by Goldsmith-Pinkham et al. (2020), is likely to be *a priori* implausible in the ADH setting. As indicated in Section 2.2, any unobserved shocks ν_{nt} invalidate the share exogeneity assumption if they enter the error $\varepsilon_{\ell t}$ in a manner which is correlated with the shares. Because ADH use generic manufacturing employment shares to instrument for a specific treatment variable, the possibility of other industry shocks entering $\varepsilon_{\ell t}$ looms large. These unobserved shocks could take many forms, for example heterogeneous speeds of automation, secular changes in consumer demand, or changes in factor prices which differentially affect industries based on their skill intensity. This is in contrast to our Assumption 3 which allows for any of these unobserved shocks as long as they are uncorrelated with g_{nt} across industries, conditionally on observables q_{nt} .⁴⁰

With a plausible justification of our framework in hand, we next illustrate its application.

6.2.2 Properties of Industry Shocks and Exposure Shares

Our quasi-experimental view of the ADH research design places particular emphasis on the variation in Chinese import growth rates g_{nt} and their average exposure s_{nt} across industries and periods. With few or insufficiently-variable shocks, or highly concentrated shocks exposure, the large- N asymptotic approximation developed in Section 3 is unlikely to be a useful tool for characterizing the finite-sample behavior of the SSIV estimator. We thus first summarize the distribution of g_{nt} , as well as the industry-level weights from our equivalence result, $s_{nt} \propto \sum_{\ell} e_{\ell t} s_{\ell nt}$ (normalized to add up to one in the entire sample).⁴¹

In summarizing the industry-level variation it is instructive to recall that the ADH instrument is constructed with “incomplete” manufacturing shares. Per the discussion in Section 4.2, this means that absent any regression controls the SSIV estimator uses variation not only in manufacturing industry shocks but also implicitly the variation in the 10-year lagged total manufacturing share $S_{\ell t}$ across commuting zones and periods. In practice, ADH control for the start-of-period manufacturing share, which is highly—though not perfectly—correlated with $S_{\ell t}$. We thus summarize the ADH shocks both with and without the “missing” shock $g_{0t} = 0$, which here represents the lack of a “China shock” in service (i.e. non-manufacturing) industries. Given that trade with China was very different in the 1990s and 2000s, we focus on the within-period variation in manufacturing shocks.

Table 1 reports summary statistics for the ADH shocks g_{nt} computed with importance weights s_{nt} , and characterizes these weights. Column 1 includes the “missing” service industry shock of zero in each period. It is evident that with this shock the distribution of g_{nt} is unusual: for example, its interquartile range is zero. This is because the service industry accounts for a large fraction of total employment (s_{0t} is 71.9% of the period total in the 1990s and 79.5% in the 2000s). As a result

⁴⁰This assumption, which allows one to isolate import competition from other industry shocks, is standard in similar industry-level analyses (e.g. Acemoglu et al. 2016) and can be tested with falsification tests, as we do in Section 6.2.3.

⁴¹Note that s_{nt} would be proportional to lagged industry employment if the ADH regression weights $e_{\ell t}$ were lagged regional employment. ADH however use a slightly different $e_{\ell t}$: the start-of-period commuting zone population.

we see a high concentration of industry exposure as measured by the inverse of its Herfindahl index (HHI), $1/\sum_{n,t} s_{nt}^2$, which corresponds to the effective sample size of our equivalent regression and plays a key role in Assumption 2. With the “missing” shock included, the effective sample size is only 3.5. For an HHI computed at the level of three-digit industry codes $\sum_c s_c^2$, where s_c aggregates exposure across the two periods and industries within the same 3-digit group c , it is even lower, at 1.7. This suggests even less industry-level variation is available when shocks are allowed to be serially correlated or clustered by groups. Furthermore, the mean of manufacturing shocks is significantly different from the zero shock of the missing service industry.⁴² Together, these analyses suggest that the service industry should be excluded from the identifying variation, because it is likely to violate both Assumption 1 ($\mathbb{E}[g_{nt} | \bar{\varepsilon}, s] \neq g_{0t} = 0$) and Assumption 2 ($\sum_{n,t} s_{nt}^2$ is not close to zero).

Column 2 of Table 1 therefore summarizes the sample with the service industry excluded. The distribution of shocks is now much more regular, with an average of 7.4, a standard deviation of 20.9 and an interquartile range of 6.6. The inverse HHI of the s_{nt} is also relatively high: 191.6 across industry-by-period cells and 58.4 when exposure is aggregated by SIC3 group. The largest shock weights in this column are only 3.4% across industry-by-periods and 6.5% across SIC3 groups. This suggests a sizable degree of variation at the industry level, consistent with Assumption 2. In general, we recommend that researchers report the inverse of the HHI of shock-level average exposure as a simple way of describing their effective sample size. A first-stage F -statistic, which we discuss appropriate computation of in Section 5.2, will provide a formal test of the power of the shock variation.

Finally, column 3 of Table 1 summarizes the distribution of within-period manufacturing shocks, which would be leveraged by an assumption of conditional quasi-experimental assignment (Assumption 3). The column confirms that even conditional on period there is sizable residual shock variation. The standard deviation and interquartile range of shock residuals (obtained from regressing shocks on period fixed effects with s_{nt} weights) are only mildly smaller than in Column 2, despite the higher mean shock in the later period, at 12.6 versus 3.6.

Besides the condition on the effective sample size, Assumption 2 (and its clustered version in Assumption 5) requires the shocks to be sufficiently mutually uncorrelated. To assess the plausibility of this assumption and choose the appropriate level of clustering for exposure-robust standard errors, we next analyze the correlation patterns of shocks across manufacturing industries using available industry classifications and the time dimension of the pooled cross section. In particular, we compute intra-class correlation coefficients (ICCs) of shocks within different industry groups, as one might do to correct for conventional clustering parametrically (e.g. Angrist and Pischke (2008, p. 312)).⁴³

⁴²The weighted mean of manufacturing shocks is 7.4, with a standard error clustered at the 3-digit SIC level (as in our analysis below) of 1.3.

⁴³Note that similar ICC calculations could be implemented in a setting that directly regresses industry outcomes on industry shocks, such as Acemoglu et al. (2016). Mutual correlation in the instrument is a generic concern that is not specific to shift-share designs, although one that is rarely tested for. Getting the correlation structure in shocks right is especially important for inference in our framework, since the outcome and treatment in the industry-level regression (\bar{y}_{nt}^\perp and \bar{x}_{nt}^\perp) are by construction correlated across industries.

These ICCs come from a random effects model, providing a hierarchical decomposition of residual within-period shock variation:

$$g_{nt} = \mu_t + a_{\text{ten}(n),t} + b_{\text{sic2}(n),t} + c_{\text{sic3}(n),t} + d_n + e_{nt}, \quad (11)$$

where μ_t are period fixed effects; $a_{\text{ten}(n),t}$, $b_{\text{sic2}(n),t}$, and $c_{\text{sic3}(n),t}$ denote time-varying (and possibly auto-correlated) random effects generated by the ten industry groups in Acemoglu et al. (2016), 20 groups identified by SIC2 codes, and 136 groups corresponding to SIC3 codes, respectively; and d_n is a time-invariant industry random effect (across our 397 four-digit SIC industries). Following convention, we estimate equation (11) as a hierarchical linear model by maximum likelihood, assuming Gaussian residual components.⁴⁴

Table 2 reports estimated ICCs from equation (11), summarizing the share of the overall shock residual variance due to each random effect. These reveal moderate clustering of shock residuals at the industry and SIC3 level (with ICCs of 0.169 and 0.073, respectively). At the same time, there is less evidence for clustering of shocks at a higher SIC2 level and particularly by ten cluster groups (ICCs of 0.047 and 0.016, respectively, with standard errors of comparable magnitude). This supports the assumption that shocks are mean-independent across SIC3 clusters, so it will be sufficient to cluster standard errors at the level of SIC3 groups, as Acemoglu et al. (2016) do in their conventional industry-level IV regressions. The inverse HHI estimates in Table 1 indicate that at this level of shock clustering there is still an adequate effective sample size.

6.2.3 Falsification Tests

We next implement falsification tests of ADH shock orthogonality, which provide a way of assessing the plausibility of Assumption 3. Following Section 5.2, we do this in two ways, both different from conventional falsification tests sometimes run in SSIV settings. First, we regress potential proxies for the unobserved residual (i.e., any unobserved industry labor demand or labor supply shock) on the instrument z_ℓ but use exposure-robust inference that takes into account the inherent dependencies of the data. Second, we regress potential industry-level confounders directly on the shocks (while again clustering by SIC3). While this second type of falsification tests would be standard in industry-level analyses, such as Acemoglu et al. (2016), it has rarely been used to assess the plausibility of SSIV designs (with Xu (2019), mentioned above, being a rare exception).

Choosing the set of potential confounders for these exercises is a context-specific issue, which should be justified separately in every application. To discipline our illustrative exercise, we use the industry-level production controls in Acemoglu et al. (2016) and the regional controls in ADH. Consistent with our *a priori* view of the quasi-experiment, we maintain only the period fixed effects as

⁴⁴In particular we estimate an unweighted mixed-effects regression using Stata’s *mixed* command, imposing an exchangeable variance matrix for $(a_{\text{ten}(n),1}, a_{\text{ten}(n),2})$, $(b_{\text{sic2}(n),1}, b_{\text{sic2}(n),2})$, and $(c_{\text{sic3}(n),1}, c_{\text{sic3}(n),2})$.

controls when evaluating balance on these other observables. For the industry-level balance test this amounts to regressing each potential confounder on the manufacturing shocks (normalized to have a unit variance) and period fixed effects, weighting by average industry employment shares. Regional balance coefficients are obtained by regressing each potential confounder on the shift-share instrument (normalized to have a unit variance) and the share-weighted average of period effects (i.e., the period-interacted sum-of-shares), since ADH is a setting with incomplete shares. To obtain exposure-robust standard errors, we implement these regressions at the shock level, as discussed above.

Panel A of Table 3 reports the results of our industry-level balance tests. The five Acemoglu et al. (2016) production controls are an industry’s share of production workers in employment in 1991, the ratio of its capital to value-added in 1991, its log real wages in 1991, the share of its investment devoted to computers in 1990, and the share of its high-tech equipment in total investment in 1990.⁴⁵ Broadly, these variables reflect the structure of employment and technology across industries. If the ADH shocks are as-good-as-randomly assigned to industries within periods, we expect them to not predict these predetermined variables. Panel A shows that there is indeed no statistically significant correlation within periods, consistent with Assumption 3.

Panel B of Table 3 reports the results of our regional balance tests. The five ADH controls are the fraction of a commuting zone’s population who is college-educated, the fraction of its population who is foreign-born, the fraction of its workers who are female, its fraction of employment in routine occupations, and the average offshorability index of its occupations. Broadly, these variables reflect the composition of a region’s workforce. We again find no statistically significant relationships between these variables and the shift-share instrument within periods, except for the foreign-born population fraction. Locations exposed to a large ADH trade shock tend to have a higher fraction of immigrants, suggesting that they may be subject to different labor supply dynamics. We explore the importance of this imbalance for the SSIV coefficient estimate in sensitivity tests below.

Finally, the last two rows of the same panel conduct a regional “pre-trends” analysis. We regress the pre-trend variables from ADH—manufacturing employment growth in the 1970s and 1980s—on the shift-share instrument, using the same specification as in the previous rows. We find no relationship between the shift-share instrument and manufacturing employment growth in the 1980s, but there is a positive statistically significant relationship with manufacturing employment growth in the 1970s. Both findings are similar to those from ADH’s pre-trend analysis.

Overall, we fail to reject imbalance in ten out of the twelve potential confounders at conventional levels of statistical significance. How to proceed when some balance tests fail is a general issue in quasi-experimental analyses and has to be decided in the context of an application. One might view the balance failures as sufficient evidence against Assumption 3 to seek alternative shocks or more appropriate shock-level controls. Alternatively, one may argue that the observed imbalances

⁴⁵The last two controls are missing for five out of 397 industries. We impute the missing values by the medians in the SIC3 industry group or, when not available, in the SIC2 group.

are unlikely to invalidate the research design. ADH, for example, note that the positive relationship they find between the shift-share instrument and manufacturing employment in the 1970s occurs in the distant past, while the insignificant relationship in the 1980s demonstrates that the relationship between rising China trade exposure and declining manufacturing employment was absent in the decade immediately prior to China’s rise. Similarly the imbalance of the foreign-born share that we observe need not generate a bias in the estimate if it is not strongly correlated with the second-stage residual. To gauge this potential for omitted variable bias one can include such variables as controls in the SSIV specification and check sensitivity of the coefficient; we report results of this exercise next.

6.2.4 Main Estimates and Sensitivity Analyses

We next estimate the effects of import competition on local labor market outcomes, leveraging within-period exogeneity of the industry shocks g_{nt} . We then check sensitivity of results to inclusion of the Autor et al. (2013) regional controls and Acemoglu et al. (2016) industry-level controls.

Table 4 reports SSIV coefficients from regressing regional manufacturing employment growth in the U.S. on the growth of import competition from China, instrumented by predicted Chinese import growth.⁴⁶ Per the results in Section 5.1, we estimate these coefficients with equivalent industry-level regressions in order to obtain valid exposure-robust standard errors. Consistent with the above analysis of shock ICCs, we cluster standard errors at the SIC3 level. We also report first-stage F -statistics with corresponding exposure-robust inference. As discussed in Section 5.2, these come from industry-level IV regressions of the aggregated treatment and instrument (i.e. \bar{x}_{nt}^\perp on \bar{z}_{nt}^\perp), instrumented with shocks and weighting by s_{nt} . The F -statistics are well above the conventional threshold of ten in all columns of the table.

Column 1 first replicates column 6 of Table 3 in Autor et al. (2013) by including in $w_{\ell t}$ period fixed effects, Census division fixed effects, start-of-period conditions (% college educated, % foreign-born, % employment among women, % employment in routine occupations, and the average offshorability index), and the start-of-period manufacturing share. The point estimate is -0.596, with a corrected standard error of 0.114.⁴⁷

As noted, the ADH specification in column 1 does not include the lagged manufacturing share control $S_{\ell t}$, which is necessary to solve the incomplete shares issue in Section 4.2, though it does include a highly correlated control (start-of-period manufacturing share). In column 2 of Table 4

⁴⁶Appendix Table C1 reports estimates for other outcomes in ADH: growth rates of unemployment, labor force non-participation, and average wages, corresponding to columns 3 and 4 of Table 5 and column 1 of Table 6 in ADH.

⁴⁷Appendix Table C2 implements three alternative methods for conducting inference in Table 4, reporting conventional state-clustered standard errors as in ADH (which are not exposure-robust), the Adão et al. (2019) standard errors (which are asymptotically equivalent to ours but differ in finite samples), and null-imposed confidence intervals obtained from shock-level Lagrange multiplier tests (which may have better finite-sample properties). Consistent with the theoretical discussion in Appendix B.2, the conventional standard errors are generally too low, while the Adão et al. (2019) standard errors are slightly larger than those from Table 4 in most columns. Imposing the null widens the confidence interval more substantially, by 30–50%, although more so on the left end, suggesting that much larger effects are not rejected by the data. This last finding is consistent with Adão et al. (2019), except that we use the equivalent industry-level regression to compute the null-imposed confidence interval.

we isolate within-manufacturing variation in shocks by replacing the latter sum-of-share control with the former. The SSIV point estimate remains almost unchanged, at -0.489 (with a standard error of 0.100). Here exposure-robust standard errors are obtained from an industry-level regression that drops the implicit service sector shock of $g_{0t} = 0$.

Isolating the within-period variation in manufacturing shocks requires further controls in the incomplete shares case, as discussed in Section 4.3. Specifically, column 3 controls for lagged manufacturing shares interacted with period indicators, which are the share-weighted sums of period effects in q_{nt} . This is equivalent to the use of period fixed effects in the industry-level analysis of Acemoglu et al. (2016). With these controls the SSIV point estimate is -0.267 with an exposure-robust standard error of 0.099.⁴⁸ While the coefficient remains statistically and economically significant, it is smaller in magnitude than the estimates in columns 1 and 2. The difference stems from the fact that 2000–07 saw both a faster growth in imports from China (e.g., due to its entry to the WTO) and a faster decline in U.S. manufacturing. The earlier columns attribute the faster manufacturing decline to increased trade with China, while the specification in Column 3 controls for any unobserved shocks specific to the manufacturing sector overall in the 2000s (e.g., any demand or supply shock affecting the manufacturing sector, which could include automation, innovation, falling consumer demand due to income effects, etc.). Conventional industry-level IV regressions control for such unobserved shocks with period fixed effects, as in Table 3, column 1 of Acemoglu et al. (2016).⁴⁹ The translation of their industry specification into the regional setup of ADH requires interacting the lagged manufacturing shares with period indicators, a simple but important insight of our framework.

Column 4 implements a simple sensitivity test to assess the stability of the results when the controls from ADH are omitted. This test is motivated by the result of the balance test in panel B of Table 3, which indicated that the shift-share instrument was correlated with the share of foreign born population. It is therefore instructive to see whether the headline regression coefficient is sensitive to the inclusion of this and other controls. In fact, we find that the results remain very similar without controls, with a point estimate of -0.314 and an exposure-robust standard error of 0.134. We proceed by keeping the ADH controls for the remainder of the analysis.

Further columns of Table 4 parallel the specifications of Acemoglu et al. (2016, Table 3) that

⁴⁸Appendix Figure C1 reports binned scatter plots that illustrate the first-stage and reduced-form industry-level relationships corresponding to the column 3 specification. This estimate can be interpreted as a weighted average of two period-specific shift-share IV coefficients. Column 1 of Appendix Table C3 shows the underlying estimates, from a just-identified IV regression where both treatment and the instrument are interacted with period indicators (as well as the manufacturing share control, as in column 3), with exposure-robust standard errors obtained by the equivalent industry-level regression discussed in Section 5. The estimated effect of increased Chinese import competition is negative in both periods (-0.491 and -0.225). Other columns repeat the analysis for other outcomes.

⁴⁹In principle, China could have affected the path of the U.S. manufacturing sector as a whole, and thus the variation in the average China shock across periods may be informative about the effects of interest. However, because of the multiplicity of shocks that may affect the manufacturing sector as a whole in a given period, this variation cannot be viewed as a quasi-experimental source of variation for the impact of trade with China on employment and other outcomes. This is why industry-level studies of the China shock use period fixed effects, possibly reducing power but substantially improving robustness of the estimates. In the ADH application the estimation power is not actually reduced, as the Table 4 column 3 standard error is even slightly smaller than that in the previous columns.

include further industry-level controls. This illustrates how our framework makes it straightforward to introduce more detailed industry-level controls in SSIV, which are commonly used in industry-level studies of the China shock. The validity of these estimates relies on weaker versions of conditional random assignment (Assumption 3), and robustness of the coefficients is therefore reassuring. Specifically, Acemoglu et al. (2016) control for fixed effects of ten broad industry groups (one-digit manufacturing sectors) in column 2 of their Table 3. By Proposition 4, we can exploit shock variation within these industry groups in the SSIV design by controlling for the lagged shares of exposure to these industry groups (and including fixed effects of these groups in the equivalent industry regressions for correct inference, per Section 5.1). The resulting point estimate in column 5 of Table 4 remains very similar to that of column 3, at -0.310 with a standard error of 0.134.

Column 6 instead parallels the specification of Acemoglu et al. (2016) that includes production controls, which we used for the balance tests in Panel A of Table 3. This is done by controlling for the regional share-weighted sums of those controls. The results remain virtually unchanged, with a regression coefficient of -0.293 and an exposure-robust standard error of 0.125.

Finally, column 7 instead introduces industry fixed effects, again following Acemoglu et al. (2016). This specification is more ambitious because it isolates changes in trade with China within each four-digit SIC industry, across the two periods. To translate the industry fixed effects to the location-level setup, we control for the lagged location-specific share of exposure to each industry.⁵⁰ The magnitude of the regression coefficient increases, to -0.432, with an exposure-robust standard error of 0.205. Broadly, these results demonstrate the stability of the SSIV regression coefficient under alternative sets of controls, corresponding to different assumptions of conditional quasi-random shock assignment.

The appendix reports estimates from additional specifications. Appendix Table C4 includes additional controls corresponding to other specifications of Acemoglu et al. (2016), Table 3: for example, controlling for observed changes in employment in the pre-periods or combining multiple sets of controls. The regression coefficients remain stable across all specifications. Appendix Table C5 instead shows robustness of the coefficients to using overidentified SSIV procedures (leveraging variation in eight country-specific Chinese import growth, instead of the ADH total), illustrating the theoretical results of Section 4.4. The table also reports a p -value for the shock-level overidentification test of 0.142, providing further support to the identification assumptions.

6.2.5 Discussion

Taken together, the sensitivity, falsification, and overidentification exercises suggest that the ADH approach can be reasonably viewed as leveraging exogenous shock variation via our framework. This is notably in contrast to the analysis of Goldsmith-Pinkham et al. (2020), who find the ADH exposure

⁵⁰If the shares used to construct the instrument were time-invariant, a more conventional and intuitive way to exploit over-time variation in the shocks would be by including the regional fixed effects in the regression, as Section 4.3 explained. In the ADH setting where the shares vary over time, they need to be controlled for directly.

shares to be implausible instruments via different balance and overidentification tests. This contrast should perhaps come as no surprise. As mentioned, the exogeneity of industry employment shares is an *ex ante* implausible research design, because it is invalidated by any unobserved labor demand or supply shocks across industries (which we view as an inherent feature of the economy).

In contrast, our approach relies on the exogeneity of the specific ADH trade shocks, allowing for endogenous exposure shares. With this view, the potential confounders are a more specific set of unobserved industry shocks (namely, unobserved shocks that would correlate with the ADH shocks), rather than any unobserved shocks. In principle, the conditions for shock orthogonality could still fail because of these specific unobserved shocks. In practice, our balance tests indicate that there is little evidence to suggest that the ADH shocks are confounded.

Our ADH application therefore illustrates two points. First, the assumptions of our framework are plausible, both *ex ante* and *ex post*, in an influential empirical setting, where an alternative SSIV framework is inapplicable. Second, our framework helps researchers translate shock-level identifying assumptions to appropriate SSIV regression controls, falsify those assumptions with appropriate balance tests, and perform correct inference.

7 Conclusion

Shift-share instruments combine a common set of observed shocks with variation in shock exposure. In this paper, we provide a quasi-experimental framework for the validity of such instruments based on identifying variation in the shocks, allowing the exposure shares to be endogenous. Our framework revolves around novel equivalence results: the orthogonality between a shift-share instrument and an unobserved residual can be represented as the orthogonality between the underlying shocks and a shock-level unobservable, and SSIV regression coefficients can be obtained from a transformed shock-level regression with shocks directly used as an instrument. Shift-share instruments are therefore valid when shocks are idiosyncratic with respect to an exposure-weighted average of the unobserved factors determining the outcome variable, and yield consistent IV estimates when the number of shocks is large and they are sufficiently dispersed in terms of their average exposure.

Through various extensions and illustrations, we show how our quasi-experimental SSIV framework can guide empirical work in practice. By controlling for exposure-weighted averages of shock-level confounders, researchers can isolate more plausibly exogenous variation in shocks, such as over time or within narrow industry groups. By estimating SSIV coefficients, placebo regressions, and first stage F -statistics at the level of shocks, researchers can conveniently perform exposure-robust inference that accounts for the inherent non-standard clustering of observations with common shock exposure. Our shock-level analysis also raises new concerns: SSIV designs with few or insufficiently dispersed shocks may have effectively small samples, despite there being many underlying observations, and

instruments constructed from exposure shares that do not add up to a constant require appropriate controls in order to isolate quasi-random shock variation. We illustrate these practical implications in an application to the influential study of Autor et al. (2013).

In sum, our analysis formalizes the claim that SSIV identification and consistency may arise from the exogeneity of shocks, while providing new guidance for SSIV estimation and inference that may be applied across a number of economic fields, including international trade, labor economics, urban economics, macroeconomics, and public finance. Our shock-level assumptions connect SSIV in these settings to conventional shock-level IV estimation, bringing shift-share instruments to more familiar econometric territory and facilitating the assessment of SSIV credibility in practice.

Figures and Tables

Table 1: Shock Summary Statistics in the Autor et al. (2013) Setting

	(1)	(2)	(3)
Mean	1.79	7.37	0
Standard deviation	10.79	20.92	20.44
Interquartile range	0	6.61	6.11
<u>Specification</u>			
Excluding service industries		✓	✓
Residualizing on period FE			✓
<u>Effective sample size ($1/HHI$ of s_{nt} weights)</u>			
Across industries and periods	3.5	191.6	191.6
Across SIC3 groups	1.7	58.4	58.4
<u>Largest s_{nt} weight</u>			
Across industries and periods	0.398	0.035	0.035
Across SIC3 groups	0.757	0.066	0.066
<u>Observation counts</u>			
# of industry-period shocks	796	794	794
# of industries	398	397	397
# of SIC3 groups	137	136	136

Notes: This table summarizes the distribution of China import shocks g_{nt} across industries n and periods t in the Autor et al. (2013) application. Shocks are measured as the total flow of imports from China in eight developed economics outside of the United States. All statistics are weighted by the average industry exposure shares s_{nt} ; shares are measured from lagged manufacturing employment, as described in Section 6.2.1. Column 1 includes the non-manufacturing industry aggregate in each period with a shock of 0, while columns 2 and 3 restrict the sample to manufacturing industries. Column 3 residualizes manufacturing shocks on period indicators. We report the effective sample size (the inverse renormalized Herfindahl index of the s_{nt} weights, as described in Section 6.2.2) with and without the non-manufacturing industry, at the industry-by-period level and at the level of SIC3 groups (aggregated across periods), along with the largest s_{nt} .

Table 2: Shock Intra-Class Correlations in the Autor et al. (2013) Setting

	Estimate	SE
	(1)	(2)
<u>Shock ICCs</u>		
10 sectors	0.016	(0.022)
SIC2	0.047	(0.052)
SIC3	0.073	(0.057)
Industry	0.169	(0.047)
<u>Period means</u>		
1990s	4.65	(1.38)
2000s	16.87	(3.34)
# of industry-periods	794	

Notes: This table reports intra-class correlation coefficients for the Autor et al. (2013) manufacturing shocks, estimated from the hierarchical model described in Section 6.2.2. Estimates come from a maximum likelihood procedure with an exchangeable covariance structure for each industry and sector random effect and with period fixed effects. Robust standard errors are reported in parentheses.

Table 3: Shock Balance Tests in the Autor et al. (2013) Setting

Panel A: Industry-Level Balance

Balance variable	Coef.	SE
Production workers' share of employment, 1991	-0.011	(0.012)
Ratio of capital to value-added, 1991	-0.007	(0.019)
Log real wage (2007 USD), 1991	-0.005	(0.022)
Computer investment as share of total, 1990	0.750	(0.465)
High-tech equipment as share of total investment, 1990	0.532	(0.296)
# of industry-periods		794

Panel B: Regional Balance

Balance variable	Coef.	SE
Start-of-period % of college-educated population	0.915	(1.196)
Start-of-period % of foreign-born population	2.920	(0.952)
Start-of-period % of employment among women	-0.159	(0.521)
Start-of-period % of employment in routine occupations	-0.302	(0.272)
Start-of-period average offshorability index of occupations	0.087	(0.075)
Manufacturing employment growth, 1970s	0.543	(0.227)
Manufacturing employment growth, 1980s	0.055	(0.187)
# of region-periods		1,444

Notes: Panel A of this table reports coefficients from regressions of the industry-level covariates in Acemoglu et al. (2016) on the Autor et al. (2013) shocks, controlling for period indicators and weighting by average industry exposure shares. Standard errors are reported in parentheses and allow for clustering at the level of three-digit SIC codes. Panel B reports coefficients from regressions of commuting zone-level covariates and pre-trends from Autor et al. (2013) on the shift-share instrument, controlling for period indicators interacted with the lagged manufacturing share. Balance variables (the first five rows of this panel) vary across the two periods, while pre-trends (the last two rows) do not. SIC3-clustered exposure-robust standard errors are reported in parentheses and obtained from equivalent industry-level IV regressions as described in Section 6.2.3. Independent variables in both panels are normalized to have a variance of one in the sample.

Table 4: Shift-Share IV Estimates of the Effect of Chinese Imports on Manufacturing Employment

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Coefficient	-0.596	-0.489	-0.267	-0.314	-0.310	-0.290	-0.432
	(0.114)	(0.100)	(0.099)	(0.107)	(0.134)	(0.129)	(0.205)
<u>Regional controls</u>							
Autor et al. (2013) controls	✓	✓	✓		✓	✓	✓
Start-of-period mfg. share	✓						
Lagged mfg. share		✓	✓	✓	✓	✓	✓
Period-specific lagged mfg. share			✓	✓	✓	✓	✓
Lagged 10-sector shares					✓		✓
Local Acemoglu et al. (2016) controls						✓	
Lagged industry shares							✓
SSIV first stage F -stat.	185.6	166.7	123.6	272.4	64.6	63.3	27.6
# of region-periods	1,444	1,444	1,444	1,444	1,444	1,444	1,444
# of industry-periods	796	794	794	794	794	794	794

Notes: This table reports shift-share IV coefficients from regressions of regional manufacturing employment growth in the U.S. on the growth of import competition from China, instrumented with predicted China import growth as described in Section 6.2.1. Column 1 replicates column 6 of Table 3 in Autor et al. (2013) by controlling for period fixed effects, Census division fixed effects, start-of-period conditions (% college educated, % foreign-born, % employment among women, % employment in routine occupations, and the average offshorability index), and the start-of-period manufacturing share. Column 2 replaces the start-of-period manufacturing shares control with the lagged manufacturing shares underlying the instrument, while column 3 interacts this control with period indicators. Column 4 removes the Census division fixed effects and start-of-period covariates. Columns 5–7 instead add exposure-weighted sums of industry controls from Acemoglu et al. (2016): indicators of 10 industry sectors (column 5), production controls (column 6), and indicators of 397 industries (column 7). Production controls are: employment share of production workers, ratio of capital to value-added, log real wage (all measured in 1991); and computer investment as share of total and high-tech equipment as share of total employment (both measured in 1990). Exposure-robust standard errors (reported in parentheses) and first-stage F -statistics are obtained from equivalent industry-level IV regressions, as described in the text, allowing for clustering of shocks at the level of three-digit SIC codes. For commuting zone controls which have a shift-share structure (all controls starting with the lagged manufacturing share), we include the corresponding q_{nt} controls in the industry-level IV regression. The sample in columns 2–7 includes 722 locations (commuting zones) and 397 industries, each observed in two periods; the estimate in column 1 implicitly includes an additional two observations for the non-manufacturing industry with a shock of zero in each period.

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Appendix to “Quasi-Experimental Shift-Share Research Designs”

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Contents

A Appendix Results	43
A.1 Heterogeneous Treatment Effects	43
A.2 Unobserved n -level Shocks Violate Share Exogeneity	44
A.3 Comparing SSIV and Native Shock-Level Regression Estimands	45
A.4 Connection to Rotemberg Weights	47
A.5 Consistency of Control Coefficients	48
A.6 Estimated Shocks	49
A.7 Equilibrium Industry Growth in a Model of Local Labor Markets	53
A.8 SSIV Consistency in Short Panels	54
A.9 SSIV Relevance with Panel Data	55
A.10 SSIV with Multiple Endogenous Variables or Instruments	56
A.11 Finite-Sample Performance of SSIV: Monte-Carlo Evidence	60
B Appendix Proofs	66
B.1 Proposition 4 and Extensions	66
B.2 Proposition 5 and Related Results	69
B.3 Proposition A1	76
B.4 Proposition A2	77
B.5 Proposition A3	78
B.6 Proposition A4	79
B.7 Proposition A5	80
B.8 Proposition A6	81
C Appendix Figures and Tables	82

A Appendix Results

A.1 Heterogeneous Treatment Effects

In this appendix we consider what a linear SSIV identifies when the structural relationship between y_ℓ and x_ℓ is nonlinear. We show that under a first-stage monotonicity condition the large-sample SSIV coefficient estimates a convexly weighted average of heterogeneous treatment effects. This holds even when the instrument has different effects on the outcome depending on the underlying realization of shocks, for example when $y_\ell = \sum_n s_{\ell n} \tilde{\beta}_{\ell n} x_{\ell n} + \varepsilon_\ell$ with $\tilde{\beta}_{\ell n}$ capturing the effects of (possibly unobserved) observation- and shock-specific treatments $x_{\ell n}$ making up the observed $x_\ell = \sum_n s_{\ell n} x_{\ell n}$.

Consider a general structural outcome model of

$$y_\ell = y(x_{\ell 1}, \dots, x_{\ell R}, \varepsilon_\ell), \quad (12)$$

where the R treatments are given by $x_{\ell r} = x_r(g, \eta_{\ell r})$ with g collecting the vector of shocks g_n and with $\eta_\ell = (\eta_{\ell 1}, \dots, \eta_{\ell R})$ capturing first-stage heterogeneity. We consider an IV regression of y_ℓ on some aggregated treatment $x_\ell = \sum_r \alpha_{\ell r} x_{\ell r}$ with $\alpha_{\ell r} \geq 0$. Note that this nests the case of a single aggregate treatment ($R = 1$ and $\alpha_{\ell 1} = 1$) with arbitrary effect heterogeneity, as well as the special case above ($R = N$ and $\alpha_{\ell r} = s_{\ell n}$). We abstract away from controls w_ℓ and assume each shock is as-good-as-randomly assigned (mean-zero and mutually independent) conditional on the vector of second-stage unobservables ε_ℓ and the matrices of first-stage unobservables $\eta_{\ell r}$, exposure shares $s_{\ell n}$, importance weights e_ℓ , and aggregation weights $\alpha_{\ell r}$, collected in $\mathcal{I} = \{\varepsilon_\ell, e_\ell, \{\eta_{\ell r}, \alpha_{\ell r}\}_r, \{s_{\ell n}\}_n\}_\ell$. This assumption is stronger than Assumption 3 but generally necessary in a non-linear setting while still allowing for the endogeneity of exposure shares. For further notational simplicity we assume that $y(\cdot, \varepsilon_\ell)$ and each $x_r(\cdot, \eta_{\ell r})$ are almost surely continuously differentiable, such that $\beta_{\ell r}(\cdot) = \frac{\partial}{\partial x_r} y(\cdot, \varepsilon_\ell)$ captures the effect, for observation ℓ , of marginally increasing treatment r on the outcome and $\pi_{\ell nr}(\cdot) = \frac{\partial}{\partial g_n} x_r(\cdot, \eta_{\ell r})$ captures the effect of marginally increasing the n th shock on the r th treatment at ℓ .

Under an appropriate law of large numbers, the shift-share IV estimator approximates the IV estimand:

$$\hat{\beta} = \frac{\mathbb{E}[\sum_\ell e_\ell z_\ell y_\ell]}{\mathbb{E}[\sum_\ell e_\ell z_\ell x_\ell]} + o_p(1) = \frac{\sum_\ell \sum_n \mathbb{E}[s_{\ell n} e_\ell g_n y_\ell]}{\sum_\ell \sum_n \sum_r \mathbb{E}[s_{\ell n} e_\ell g_n \alpha_{\ell r} x_{\ell r}]} + o_p(1). \quad (13)$$

Given this, we have the following result:

Proposition A1 When $\pi_{\ell nr}(\check{g}; g_{-n}) \geq 0$ almost surely for all $\check{g} \in \mathbb{R}$, equation (13) can be written

$$\hat{\beta} = \frac{\sum_\ell \sum_n \sum_r \mathbb{E} \left[\int_{-\infty}^{\infty} \tilde{\beta}_{\ell nr}(\check{g}) \omega_{\ell nr}(\check{g}) d\gamma \right]}{\sum_\ell \sum_n \sum_r \mathbb{E} \left[\int_{-\infty}^{\infty} \omega_{\ell nr}(\check{g}) d\gamma \right]} + o_p(1), \quad (14)$$

where $\omega_{\ell nr}(\check{g}) \geq 0$ almost surely and

$$\tilde{\beta}_{\ell nr}(\check{g}) = \frac{\beta_{\ell r}(x_1([\check{g}; g_{-n}], \eta_{\ell 1}), \dots, x_R([\check{g}; g_{-n}], \eta_{\ell R}))}{\alpha_{\ell r}} \quad (15)$$

is a rescaled treatment effect, evaluated at $(x_1([\check{g}; g_{-n}], \eta_{\ell 1}), \dots, x_R([\check{g}; g_{-n}], \eta_{\ell R}))$ for $[\check{g}; g_{-n}] = (g_1, \dots, g_{n-1}, \check{g}, g_{n+1}, \dots, g_N)'$.

PROOF See Appendix B.3.

This shows that in large samples $\hat{\beta}$ estimates a convex average of rescaled treatment effects, $\tilde{\beta}_{\ell nr}(\check{g})$, when the first stage is monotone in each shock. Appendix B.3 shows that the weights $\omega_{\ell nr}(\check{g})$ are proportional to the first-stage effects $\pi_{\ell nr}([\check{g}; g_{-n}])$, exposure shares $s_{\ell n}$, regression weights e_{ℓ} , treatment aggregation weights $\alpha_{\ell r}$, and a function of the shock distribution. In the case without aggregation, i.e. $R = \alpha_{\ell r} = 1$, there is no rescaling in the $\tilde{\beta}_{\ell nr}(\check{g})$. Equation (14) then can be seen as generalizing the result of Angrist et al. (2000), on the identification of heterogeneous effects of continuous treatments, to the continuous shift-share instrument case. Intuition for the $\omega_{\ell nr}(\check{g})$ weights follows similarly from this connection. With aggregation—that is, when the realization of shocks may have heterogeneous effects on y_{ℓ} holding the aggregated x_{ℓ} fixed—equation (14) shows that SSIV captures a convex average of treatment effects per aggregated unit. Thus in the leading example of $y_{\ell} = \sum_n s_{\ell n} \tilde{\beta}_{\ell n} x_{\ell n} + \varepsilon_{\ell}$ and $x_{\ell} = \sum_n s_{\ell n} x_{\ell n}$, this result establishes identification of a convex average of the $\tilde{\beta}_{\ell n}$. In this way the result generalizes Adão et al. (2019), who establish the identification of convex averages of rescaled treatment effects in reduced form shift-share regressions.

A.2 Unobserved n -level Shocks Violate Share Exogeneity

In this appendix, we show that the assumption of SSIV share exogeneity from Goldsmith-Pinkham et al. (2020) is violated when there are unobserved shocks ν_n that affect outcomes via the exposure shares $s_{\ell n}$, i.e. when the residual has the structure

$$\varepsilon_{\ell} = \sum_n s_{\ell n} \nu_n + \check{\varepsilon}_{\ell}. \quad (16)$$

We consider large-sample violations share exogeneity in terms of the asymptotic non-ignorability of the $\bar{\varepsilon}_n$ terms in the equivalent moment condition (5). It is intuitive that the cross-sectional dependence between $s_{\ell n}$ and ε_{ℓ} will not asymptotically vanish when N is fixed (as in Goldsmith-Pinkham et al. (2020)) and each ν_n shock contributes significantly to the residual, causing $\bar{\varepsilon}_n \not\rightarrow 0$ for some or all n . We next prove this result and show that it generalizes to the case of increasing N , where the contribution of each ν_n to the variation in ε_{ℓ} becomes small. The intuition here is that the SSIV relevance condition generally requires individual observations to be sufficiently concentrated in a small

number of shocks (see Section 3.1), and under this condition the share exogeneity violations remain asymptotically non-ignorable even as $N \rightarrow \infty$.

We define share endogeneity as non-vanishing $\text{Var}[\bar{\varepsilon}_n]$ at least for some n . This will tend to make the SSIV estimator inconsistent, unless shocks are as-good-as-randomly assigned (Assumption 1), even if the importance weights of individual shocks, s_n , converge to zero (Assumption 2). Here we treat e_ℓ and $s_{\ell n}$ as non-stochastic to show this result with simple notation.

Proposition A2 Suppose condition (16) holds with the ν_n mean-zero and uncorrelated with the $\tilde{\varepsilon}_\ell$ and with each other, and with $\text{Var}[\nu_n] = \sigma_n^2 \geq \sigma_\nu^2$ for a fixed $\sigma_\nu^2 > 0$. Also assume $H_L = \sum_\ell e_\ell \sum_n s_{\ell n}^2 \rightarrow \bar{H} > 0$ such that first-stage relevance can be satisfied. Then there exists a constant $\delta > 0$ such that $\max_n \text{Var}[\bar{\varepsilon}_n] > \delta$ for sufficiently large L .

PROOF See Appendix B.4.

A.3 Comparing SSIV and Native Shock-Level Regression Estimands

In this appendix we illustrate economic differences between the estimands of two regressions that researchers may consider: SSIV using outcome and treatment observations y_ℓ and x_ℓ (which we show in Proposition 1 are equivalent to certain shock-level IV regressions), and more conventional shock-level IV regressions using “native” y_n and x_n . These outcomes and treatments capture the same economic concepts as the original y_ℓ and x_ℓ , in contrast to the constructed \bar{y}_n and \bar{x}_n discussed in Section 2.3. In line with the labor supply and other key SSIV examples, we will for concreteness refer to the ℓ and n as indexing regions and industries, respectively. We consider the case where both the outcome and treatment can be naturally defined at the level of region-by-industry cells (henceforth, cells)— $y_{\ell n}$ and $x_{\ell n}$, respectively—and thus suitable for aggregation across either dimension with some weights $E_{\ell n}$ (e.g., cell employment growth rates aggregated with lagged cell employment weights): $y_\ell = \sum_n s_{\ell n} y_{\ell n}$ for $s_{\ell n} = \frac{E_{\ell n}}{\sum_{n'} E_{\ell n'}}$ and $y_n = \sum_\ell \omega_{\ell n} y_{\ell n}$ for $\omega_{\ell n} = \frac{E_{\ell n}}{\sum_{\ell'} E_{\ell' n}}$, with analogous expressions for x_ℓ and x_n . We further define $E_\ell = \sum_n E_{\ell n}$ and $E_n = \sum_\ell E_{\ell n}$.⁵¹

We consider the estimands of two regression specifications: β from the regional level model (2), instrumented by z_ℓ and weighted by $e_\ell = E_\ell/E$ for $E = \sum_\ell E_\ell$, and β_{ind} from a simpler industry-level IV regression of

$$y_n = \beta_{\text{ind}} x_n + \varepsilon_n, \tag{17}$$

instrumented by the industry shock g_n and weighted by $s_n = E_n/E$. For simplicity we do not include any controls in either specification and implicitly condition on $\{E_{\ell n}\}_{\ell, n}$ (and some other variables as

⁵¹This formulation nests reduced-form shift-share regressions when $x_{\ell n} = g_n$ for each ℓ . The labor supply example of Section 2.1 fits only partially in this formal setup because the industry or regional wage growth y_n is not equal to a weighted average of wage growth across cells: reallocation of employment affects the average wage growth even in the absence of wage changes in any given cell.

described below), viewing them as non-stochastic.⁵²

We show that β and β_{ind} generally differ when there are within-region spillover effects or when treatment effects are heterogeneous. We study these cases in turn, maintaining several assumptions: (i) a first stage relationship analogous to the one considered in Section 3.1:

$$x_{\ell n} = \pi_{\ell n} g_n + \eta_{\ell n}, \quad (18)$$

for non-stochastic $\pi_{\ell n} \geq \bar{\pi} > 0$, (ii) a stronger version of our Assumption 1 that imposes $\mathbb{E}[g_n] = \mathbb{E}[g_n \varepsilon_{\ell n'}] = \mathbb{E}[g_n \eta_{\ell n'}] = 0$ for all ℓ , n , and n' , with $\varepsilon_{\ell n'}$ denoting the unobserved cell-level residual of each model, (iii) the assumption that g_n is uncorrelated with $g_{n'}$ for all n and n' , and (iv) that all appropriate laws of large numbers hold.

Within-Region Spillover Effects Suppose the structural model at the cell level is given by

$$y_{\ell n} = \beta_0 x_{\ell n} - \beta_1 \sum_{n'} s_{\ell n'} x_{\ell n'} + \varepsilon_{\ell n}. \quad (19)$$

Here β_0 captures the direct effect of the shock on the cell outcome, and β_1 captures a within-region spillover effect. The local employment effects of industry demand shocks from the model in Appendix A.7 fit in this framework, see equation (42).⁵³ The following proposition shows that the SSIV estimand β captures the effect of treatment net of spillovers (i.e. $\beta_0 - \beta_1$), whereas β_{ind} subtracts the spillover only partially; this is intuitive since the spillover effect is fully contained within regions but not within industries.

Proposition A3 Suppose equation (19) holds and the average local concentration index $H_L = \sum_{\ell, n} e_{\ell} s_{\ell n}^2$ is bounded from below by a constant $\bar{H}_L > 0$. Further assume $\pi_{\ell n} = \bar{\pi}$ and $\text{Var}[g_n] = \sigma_g^2$ for all ℓ and n . Then the SSIV estimator satisfies

$$\hat{\beta} = \beta_0 - \beta_1 + o_p(1) \quad (20)$$

while the native industry-level IV estimator satisfies

$$\hat{\beta}_{\text{ind}} = \beta_0 - \beta_1 H_L + o_p(1), \quad (21)$$

If $\beta_1 \neq 0$ (i.e. in presence of within-region spillovers), $\hat{\beta}$ and $\hat{\beta}_{\text{ind}}$ asymptotically coincide if and only if $H_L \xrightarrow{p} 1$, which corresponds to the case where the average region is asymptotically concentrated in one industry.

⁵²Note that we thereby condition on the shares $s_{\ell n}$ and importance weights e_{ℓ} . Yet we still allow for share endogeneity by not restricting $\mathbb{E}[\varepsilon_{\ell n}]$ to be zero.

⁵³In the labor supply example from the main text $y_{\ell n}$ is the cell wage, which is equalized within the region, and $x_{\ell n}$ is cell employment. Equation (19) therefore holds for $\beta_0 = 0$ and $-\beta_1$ being the inverse labor supply elasticity.

PROOF See Appendix B.5.

Treatment Effect Heterogeneity Now consider a different structural model which allows for heterogeneity in treatment effects:

$$y_{\ell n} = \beta_{\ell n} x_{\ell n} + \varepsilon_{\ell n}. \quad (22)$$

We also allow the first-stage coefficients $\pi_{\ell n}$ and shock variance σ_n^2 to vary. The following proposition shows that β and β_{ind} differ in how they average effect $\beta_{\ell n}$ (here treated as non-stochastic) across the (ℓ, n) cells. The weights corresponding to the SSIV estimand β are relatively higher for cells that represent a larger fraction of the regional economy. This follows because in the regional regression $s_{\ell n}$ determines the cell's weight in both the outcome and the shift-share instrument, while in the industry regression only the former argument applies. Heterogeneity in the $\pi_{\ell n}$ and σ_n^2 , in contrast, has equivalent effects on the weighting scheme of both estimands.

Proposition A4 In the casual model (22),

$$\hat{\beta} = \frac{\sum_{\ell, n} E_{\ell n} s_{\ell n} \pi_{\ell n} \sigma_n^2 \cdot \beta_{\ell n}}{\sum_{\ell, n} E_{\ell n} s_{\ell n} \pi_{\ell n} \sigma_n^2} + o_p(1) \quad (23)$$

and

$$\hat{\beta}_{\text{ind}} = \frac{\sum_{\ell, n} E_{\ell n} \pi_{\ell n} \sigma_n^2 \cdot \beta_{\ell n}}{\sum_{\ell, n} E_{\ell n} \pi_{\ell n} \sigma_n^2} + o_p(1), \quad (24)$$

PROOF See Appendix B.6.

A.4 Connection to Rotemberg Weights

In this appendix we rewrite the decomposition of the SSIV coefficient $\hat{\beta}$ from Goldsmith-Pinkham et al. (2020) that gives rise to their ‘‘Rotemberg weight’’ interpretation, and show that these weights measure the leverage of shocks in our equivalent shock-level IV regression. We then show that, in our framework, skewed Rotemberg weights do not measure sensitivity to misspecification (of share exogeneity) and do not pose a problem for SSIV consistency. We finally discuss the implications of high-leverage observations for SSIV inference.

Proposition 1 implies the following decomposition:

$$\hat{\beta} = \frac{\sum_n s_n g_n \bar{y}_n^\perp}{\sum_n s_n g_n \bar{x}_n^\perp} = \sum_n \alpha_n \hat{\beta}_n, \quad (25)$$

where

$$\hat{\beta}_n = \frac{\bar{y}_n^\perp}{\bar{x}_n^\perp} = \frac{\sum_\ell e_\ell s_{\ell n} y_\ell^\perp}{\sum_\ell e_\ell s_{\ell n} x_\ell^\perp} \quad (26)$$

and

$$\alpha_n = \frac{s_n g_n \bar{x}_n^\perp}{\sum_{n'} s_{n'} g_{n'} \bar{x}_{n'}^\perp}. \quad (27)$$

This is a shock-level version of the decomposition discussed in Goldsmith-Pinkham et al. (2020): $\hat{\beta}_n$ is the IV estimate of β that uses share $s_{\ell n}$ as the instrument, and α_n is the so-called Rotemberg weight.

To see the connection with leverage (defined, typically in the context of OLS, as the derivative of each observation’s fitted value with respect to its outcome) in our equivalent IV regression, note that

$$\frac{\partial \left(\bar{x}_n^\perp \hat{\beta} \right)}{\partial \bar{y}_n^\perp} = \bar{x}_n^\perp \frac{s_n g_n}{\sum_{n'} s_{n'} g_{n'} \bar{x}_{n'}^\perp} = \alpha_n. \quad (28)$$

In this way, α_n measures the sensitivity of $\hat{\beta}$ to $\hat{\beta}_n$.

In the preferred interpretation of Goldsmith-Pinkham et al. (2020), exposure to each shock is a valid instrument such that $\hat{\beta}_n \xrightarrow{P} \beta$ for each n . However, in our framework deviations of $\hat{\beta}_n$ from β reflect nonzero $\bar{\varepsilon}_n$ in large samples, and such share endogeneity is not ruled out; thus α_n does not have the same sensitivity-to-misspecification interpretation. Moreover, a high leverage of certain shocks (“skewed Rotemberg weights,” in the language of Goldsmith-Pinkham et al. (2020)) is not a problem for consistency in our framework, provided it results from a heavy-tailed and high-variance distribution of shocks (that still satisfies our regularity conditions, such as finite shock variance), and each s_n is small as required by Assumption 2.

Nevertheless, skewed α_n may cause issues with SSIV inference, as would high leverage observations in any regression. In general, the estimated residuals $\hat{\varepsilon}_n^\perp$ of high-leverage observations will tend to be biased toward zero, which may lead to underestimation of the residual variance and too small standard errors (e.g., Cameron and Miller 2015). This issue can be addressed, for instance, by computing confidence intervals with the null imposed, as Adão et al. (2019) recommend and as we discuss in Section 5.1. In practice our Monte-Carlo simulations in Appendix A.11 find that the coverage of conventional exposure-robust confidence intervals to be satisfactory even with Rotemberg weights as skewed as those reported in the applications of Goldsmith-Pinkham et al. (2020) analysis.

A.5 Consistency of Control Coefficients

This appendix shows how the control coefficient γ , defined in footnote 5, can be consistently estimated as required in Proposition 3 (Assumption B2). We discuss conditions for $\sum_\ell e_\ell w_\ell \varepsilon_\ell \xrightarrow{P} \mathbb{E} [\sum_\ell e_\ell w_\ell \varepsilon_\ell]$, where by definition $\mathbb{E} [\sum_\ell e_\ell w_\ell \varepsilon_\ell] = 0$. Consistency of the estimator $\hat{\gamma} = \gamma + (\sum_\ell e_\ell w_\ell w_\ell')^{-1} \sum_\ell e_\ell w_\ell \varepsilon_\ell$ follows, provided the elements of $(\sum_\ell e_\ell w_\ell w_\ell')^{-1}$ are stochastically bounded (i.e., $O_p(1)$). For simplicity we consider control vectors w_ℓ of fixed length.

The argument for convergence of $\sum_\ell e_\ell w_\ell \varepsilon_\ell$ depends on the source of randomness in w_ℓ and ε_ℓ . We consider two characteristic cases. In the first case, $(e_\ell, w_\ell', \varepsilon_\ell)'$ can be viewed as *iid* or clustered

in a conventional way. For example, w_ℓ and ε_ℓ may contain observed and unobserved local labor supply shocks which are uncorrelated across markets, clusters of markets (e.g. states), or beyond a given distance threshold. In this case conventional laws of large numbers can be used to establish $\sum_\ell e_\ell w_\ell \varepsilon_\ell \xrightarrow{P} 0$. For instance if $(e_\ell, w'_\ell, \varepsilon_\ell)'$ is *iid* then $\sum_\ell e_\ell w_\ell \varepsilon_\ell$ gives a vector of sample averages of mutually uncorrelated mean-zero random variables, which weakly converge to zero when the e_ℓ weights are asymptotically dispersed ($\mathbb{E} [\sum_\ell e_\ell^2] \rightarrow 0$) and when $\mathbb{E} [w_\ell^2 \varepsilon_\ell^2 | e]$ is uniformly bounded.

In the second case, either w_ℓ or ε_ℓ has a shift-share structure like z_ℓ : i.e. $w_\ell = \sum_n s_{\ell n} q_n$ for an observed q_n (in line with our Proposition 3) or $\varepsilon_\ell = \sum_n s_{\ell n} \nu_n$ for an unobserved ν_n (capturing, for example, a set of unobserved industry-level factors averaged with the employment weights $s_{\ell n}$). In this case convergence of $\sum_\ell e_\ell w_\ell \varepsilon_\ell$ can be shown to follow similarly to the convergence of the sample analog of the instrument moment condition (3). If, for instance, $\varepsilon_\ell = \sum_n s_{\ell n} \nu_n$ with $\mathbb{E} [\nu_n | s, w] = 0$ and $\text{Cov} [\nu_n, \nu_m | s, w] = 0$ for $w = \{w_n\}_n$, then for each control $\sum_\ell e_\ell w_{\ell m} \varepsilon_\ell = \sum_n s_n \nu_n \bar{w}_{nm}$ weakly converges when the s_n weights are dispersed ($\mathbb{E} [\sum_n s_n^2] \rightarrow 0$) and both $\text{Var} [\nu_n | s, w]$ and $\mathbb{E} [\bar{w}_{nm}^2 | s]$ are uniformly bounded. This argument can be extended to the case where either w_ℓ or ε_ℓ is formed from different exposure shares $\tilde{s}_{\ell k}$, perhaps defined over a different range of K observed q_k or unobserved ν_k , and when the q_k or ν_k are clustered or otherwise weakly mutually correlated.

More generally, the two cases can be combined to settings where $w_\ell = \sum_k \tilde{s}_{\ell k} q_k + \check{w}_\ell$ and $\varepsilon_\ell = \sum_{k'} \tilde{s}_{\ell k'} \nu_{k'} + \check{\varepsilon}_\ell$ where $(e_\ell, \check{w}'_\ell, \check{\varepsilon}_\ell)'$ is *iid* or conventionally clustered and where q_k and $\nu_{k'}$ are many weakly correlated random shocks or, even more generally, allowing for multiple shift-share terms with different exposure shares.

A.6 Estimated Shocks

This appendix establishes the formal conditions for the SSIV estimator, with or without a leave-one-out correction, to be consistent when shocks g_n are noisy estimates of some latent g_n^* satisfying Assumptions 1 and 2. We also propose a heuristic measure that indicates whether the leave-one-out correction is likely to be important and compute it for the Bartik (1991) setting. Straightforward extensions to other split-sample estimators follow.

Suppose a researcher estimates shocks via a weighted average of variables $g_{\ell n}$. That is, given weights $\omega_{\ell n} \geq 0$ such that $\sum_\ell \omega_{\ell n} = 1$ for all n , she computes

$$g_n = \sum_\ell \omega_{\ell n} g_{\ell n}. \quad (29)$$

A leave-one-out (LOO) version of the shock estimator is instead

$$g_{n,-\ell} = \frac{\sum_{\ell' \neq \ell} \omega_{\ell' n} g_{\ell' n}}{\sum_{\ell' \neq \ell} \omega_{\ell' n}}. \quad (30)$$

We assume that each $g_{\ell n}$ is a noisy version of the same latent shock g_n^* :

$$g_{\ell n} = g_n^* + \psi_{\ell n}, \quad (31)$$

where g_n^* satisfies Assumptions 1 and 2 and $\psi_{\ell n}$ is estimation error (in Section 4.1 we considered the special case of $\psi_{\ell n} \propto \varepsilon_{\ell}$). This implies a feasible shift-share instrument of $z_{\ell} = z_{\ell}^* + \psi_{\ell}$ and its LOO version $z_{\ell}^{LOO} = z_{\ell}^* + \psi_{\ell}^{LOO}$, where $z_{\ell}^* = \sum_n s_{\ell n} g_n^*$, $\psi_{\ell} = \sum_n s_{\ell n} \sum_{\ell'} \omega_{\ell' n} \psi_{\ell' n}$, and $\psi_{\ell}^{LOO} = \sum_n s_{\ell n} \frac{\sum_{\ell' \neq \ell} \omega_{\ell' n} \psi_{\ell' n}}{\sum_{\ell' \neq \ell} \omega_{\ell' n}}$. Consistency with these instruments, given a first stage, requires that $\sum_{\ell} e_{\ell} \varepsilon_{\ell} \psi_{\ell} \xrightarrow{p} 0$ and $\sum_{\ell} e_{\ell} \varepsilon_{\ell} \psi_{\ell}^{LOO} \xrightarrow{p} 0$ respectively.

We now present three sets of results. First, we establish a simple sufficient condition under which the LOO instrument satisfies $\sum_{\ell} e_{\ell} \varepsilon_{\ell} \psi_{\ell}^{LOO} \xrightarrow{p} 0$. We also propose stronger conditions that guarantee consistency of LOO-SSIV. Second, we explore the conditions under which the covariance between ε_{ℓ} and $\psi_{\ell n}$ is ignorable, i.e. asymptotically does not lead to a ‘‘mechanical’’ bias of the conventional non-leave-one-out estimator. We propose a heuristic measure that is large when the bias is likely to be small. Lastly, we apply these ideas to the setting of Bartik (1991) using the data from Goldsmith-Pinkham et al. (2020). In line with previous appendices, we condition on $s_{\ell n}$, $\omega_{\ell n}$, and e_{ℓ} and treat them as non-stochastic for notational convenience. We also assume the SSIV regressions are estimated without controls w_{ℓ} .

LOO Identification and Consistency The following proposition establishes three results. The first is the most important one, providing the condition for orthogonality to hold. The second strengthens this condition so that the estimator converges, which naturally requires that most shocks are estimated with sufficient amount of data. A tractable case of complete specialization is considered in last part, where there should be many more observations than shocks.

Proposition A5

1. If $\mathbb{E}[\varepsilon_{\ell} \psi_{\ell' n}] = 0$ for all $\ell \neq \ell'$ and n , then $\mathbb{E}[\sum_{\ell} e_{\ell} \varepsilon_{\ell} \psi_{\ell, LOO}] = 0$.
2. If $\mathbb{E}\left[(\varepsilon_{\ell}, \psi_{\ell n}) \mid \{(\varepsilon_{\ell'}, \psi_{\ell' n'})\}_{\ell' \neq \ell, n'}\right] = 0$ for all ℓ and n , then the LOO estimator is consistent, provided it has a first stage and two regularity conditions hold: $\mathbb{E}\left[|\varepsilon_{\ell_1} \varepsilon_{\ell_2} \psi_{\ell'_1 n_1} \psi_{\ell'_2 n_2}|\right] \leq B$ for a constant B and all $(\ell_1, \ell_2, \ell'_1, \ell'_2, n_1, n_2)$ and

$$\sum_{\substack{(\ell_1, \ell_2, \ell'_1, \ell'_2) \in \mathcal{J}, \\ n_1, n_2}} e_{\ell_1} e_{\ell_2} s_{\ell_1 n_1} s_{\ell_2 n_2} \frac{\omega_{\ell'_1 n_1}}{\sum_{\ell' \neq \ell_1} \omega_{\ell' n_1}} \frac{\omega_{\ell'_2 n_2}}{\sum_{\ell' \neq \ell_2} \omega_{\ell' n_2}} \rightarrow 0, \quad (32)$$

with \mathcal{J} denoting the set of tuples $(\ell_1, \ell_2, \ell'_1, \ell'_2)$ for which one of the two conditions hold: (i) $\ell_1 = \ell_2$ and $\ell'_1 = \ell'_2 \neq \ell_1$, (ii) $\ell_1 = \ell'_2$ and $\ell_2 = \ell'_1 \neq \ell_1$.

3. Condition (32) is satisfied if $\frac{N}{L} \rightarrow 0$ in the special case where each region is specialized in

one industry, i.e. $s_{\ell n} = \mathbf{1}[n = n(\ell)]$ for some $n(\cdot)$, there are no importance weights ($e_\ell = \frac{1}{L}$), and shocks estimated by simple LOO averaging among observations exposed to a given shock ($\omega_{\ell n} = \frac{1}{L_n}$ for $L_n = \sum_\ell \mathbf{1}[n(\ell) = n]$), assuming further that $L_n \geq 2$ for each n so that the LOO estimator is well-defined.

PROOF See Appendix B.7.

The condition in the first part of Proposition A5 would be quite innocuous in random samples of ℓ – the environment in which leave-one-out adjustments are often considered (e.g. Angrist et al. (1999)) – but is strong without random sampling. It requires ε_ℓ and $\psi_{\ell' n}$ to be uncorrelated for $\ell' \neq \ell$, which may easily be violated when both ℓ and ℓ' are exposed to the same shocks—a situation in which excluding own observation is not sufficient. Moreover, since we have conditioned on the exposure shares throughout, $\mathbb{E}[\varepsilon_\ell \psi_{\ell' n}] = 0$ generally requires either ε_ℓ or $\psi_{\ell' n}$ to have a zero *conditional* mean—the share exogeneity assumption applied to either the residuals or the estimation error. At the same time, this condition does not require $\mathbb{E}[\varepsilon_\ell \psi_{\ell' n}] = 0$ for $\ell = \ell'$, which reflects the benefit of LOO: eliminating the mechanical bias from the residual directly entering shock estimates.

Heuristic for Importance of LOO Correction We now return to the non-LOO SSIV estimator. As in Proposition A5, we assume that $\mathbb{E}[\varepsilon_\ell \psi_{\ell' n}] = 0$ for $\ell' \neq \ell$ and all n , so the LOO estimator is consistent under the additional regularity conditions. We also assume, without loss of generality, that z_ℓ is mean-zero. Then the “mechanical bias” mentioned in Section 4.1 is the only potential problem: under appropriate regularity conditions (similar to those in part 2 of Proposition A5),

$$\begin{aligned} \hat{\beta} - \beta &= \frac{\mathbb{E}[\sum_\ell e_\ell \varepsilon_\ell \psi_\ell]}{\mathbb{E}[\sum_\ell e_\ell z_\ell x_\ell]} + o_p(1) \\ &= \frac{\sum_{\ell, n} e_\ell s_{\ell n} \omega_{\ell n} \mathbb{E}[\varepsilon_\ell \psi_{\ell n}]}{\mathbb{E}[\sum_\ell e_\ell z_\ell x_\ell]} + o_p(1). \end{aligned} \quad (33)$$

With $|\mathbb{E}[\varepsilon_\ell \psi_{\ell n}]|$ bounded by some $B_1 > 0$ for all ℓ and n , the numerator of (33) is bounded by $H_N B_1$, for an observable composite of the relevant shares $H_N = \sum_{\ell, n} e_\ell s_{\ell n} \omega_{\ell n}$. The structure of the shares also influences the strength of the first stage in the denominator. Imposing our standard model of the first stage from Section 3.1 (but specified based on the latent shock g_n^*), i.e. $x_\ell = \sum_n s_{\ell n} x_{\ell n}$ for $x_{\ell n} = \pi_{\ell n} g_n^* + \eta_{\ell n}$, $\eta_{\ell n}$ mean-zero and uncorrelated with g_n^* for all ℓ, n, n' , $\text{Var}[g_n^*] \geq \bar{\sigma}_g^2 > 0$ and $\pi_{\ell n} \geq \bar{\pi} > 0$, yields:

$$\begin{aligned} \mathbb{E}\left[\sum_\ell e_\ell z_\ell x_\ell\right] &= \sum_\ell e_\ell \mathbb{E}\left[\left(\sum_n s_{\ell n} (g_n^* + \psi_{\ell n})\right) \left(\sum_{n'} s_{\ell n'} (\pi_{\ell n} g_{n'}^* + \eta_{\ell n'})\right)\right] \\ &= \sum_{\ell, n} e_\ell s_{\ell n}^2 \cdot \pi_{\ell n} \text{Var}[g_n^*] + \sum_\ell e_\ell \sum_{n, n'} s_{\ell n} s_{\ell n'} \mathbb{E}[\psi_{\ell n} (\pi_{\ell n} g_{n'}^* + \eta_{\ell n'})]. \end{aligned} \quad (34)$$

Excepting knife-edge cases where the two terms in (34) cancel out, $\mathbb{E}[\sum_{\ell} e_{\ell} z_{\ell}^{\perp} x_{\ell}] \not\rightarrow 0$ provided $H_L = \sum_{\ell,n} e_{\ell} s_{\ell n}^2 \geq \bar{H}$ for some fixed $\bar{H} > 0$.

We thus define the following heuristic:

$$H = \frac{H_L}{H_N} = \frac{\sum_{\ell,n} e_{\ell} s_{\ell n}^2}{\sum_{\ell,n} e_{\ell} s_{\ell n} \omega_{\ell n}}. \quad (35)$$

When H is large, we expect the non-LOO SSIV estimator to be relatively insensitive to the mechanical bias generated by the average covariance between $\psi_{\ell n}$ and ε_{ℓ} , and thus similar to the LOO estimator.

We note an important special case. Suppose all weights are derived from variable $E_{\ell n}$ (e.g. lagged employment level in region ℓ and industry n) as $s_{\ell n} = \frac{E_{\ell n}}{E_{\ell}}$, $\omega_{\ell n} = \frac{E_{\ell n}}{E_n}$, and $e_{\ell} = \frac{E_{\ell}}{E}$, for $E_{\ell} = \sum_n E_{\ell n}$, $E_n = \sum_{\ell} E_{\ell n}$, and $E = \sum_{\ell} E_{\ell}$. Then

$$H_N = \sum_{\ell,n} \frac{E_{\ell}}{E} \frac{E_{\ell n}}{E_{\ell}} \frac{E_{\ell n}}{E_n} = \sum_{\ell,n} \frac{E_n}{E} \left(\frac{E_{\ell n}}{E_n} \right)^2 = \sum_n s_n \sum_{\ell} \omega_{\ell n}^2, \quad (36)$$

where $s_n = \frac{E_n}{E}$ is the weight in our equivalent shock-level regression. Therefore, H_N is the weighted average across n of n -specific Herfindahl concentration indices, while H_L is the weighted average across ℓ of ℓ -specific Herfindahl indices. With $E_{\ell n}$ denoting lagged employment, H is high (and thus we expect the LOO correction to be unnecessary) when employment is much more concentrated across industries in a typical region than it is concentrated across regions for a typical industry.

The formula simplifies further with $E_{\ell n} = \mathbf{1}[n = n(\ell)]$ for all ℓ, n , corresponding to the case of complete specialization of observations in shocks with no regression or shock estimation weights, as in part 3 of Proposition A5. In that case,

$$H = \frac{1}{\sum_{\ell} \frac{1}{L} \frac{1}{L_{n(\ell)}}} = \frac{1}{\frac{1}{L} \sum_n \sum_{\ell: n(\ell)=n} \frac{1}{L_n}} = \frac{L}{N}. \quad (37)$$

Our heuristic is therefore large when there are many observations per estimated shock.⁵⁴

Application to Bartik (1991) We finally apply our insights to the Bartik (1991) setting, using the Goldsmith-Pinkham et al. (2020) replication code and data. Table C6 reports the results. Column 1 shows the estimates of the inverse local labor supply elasticity using SSIV estimators with and without the LOO correction and using population weights, replicating Table 3, column 2, of Goldsmith-Pinkham et al. (2020) except with employment on the left-hand side and wages on the right-hand side.⁵⁵ Column 2 repeats the analysis without the population weights.⁵⁶ We find all estimates to

⁵⁴Here $1/H = N/L$ is proportional to the “bias” of the non-LOO estimator, which is similar to how the finite-sample bias of conventional 2SLS is proportional to the number of instruments over the sample size (Nagar 1959).

⁵⁵Goldsmith-Pinkham et al. (2020) estimate the inverse labor supply elasticity. By properties of IV estimation, our coefficient is the inverse of theirs.

⁵⁶Industry growth shocks in this column are the same as in Column 1, again estimated with employment weights.

range between 1.2 and 1.3, showing that in practice for Bartik (1991) the LOO correction does not play a substantial role.

This is however especially true without weights, where the LOO and conventional SSIV estimators are 1.30 and 1.29, respectively. Our heuristic provides an explanation: H is almost 8 times bigger when computed without weights. The intuition is that large commuting zones, such as Los Angeles and New York, may constitute a substantial fraction of employment in industries of their comparative advantage. This generates a potential for the mechanical bias: labor supply shocks in those regions affect shock estimates; this bias is avoided by LOO estimators. However, the role of the largest commuting zones is only significant in weighted regressions (by employment or, as in Goldsmith-Pinkham et al. (2020), population).

A.7 Equilibrium Industry Growth in a Model of Local Labor Markets

This appendix develops a simple model of regional labor supply and demand, similar to the model in Adão et al. (2020). Our goal is to show how the national growth rate of industry employment can be viewed as a noisy version of the national industry-specific labor demand shocks, and how regional labor supply shocks (along with some other terms) generate the “estimation error.”

Consider an economy that consists of a set of L regions. In each region ℓ there is a prevailing wage W_ℓ , and labor supply has constant elasticity ϕ :

$$E_\ell = M_\ell W_\ell^\phi, \quad (38)$$

where E_ℓ is total regional employment and M_ℓ is the supply shifter that depends on the working-age population, the outside option, and other factors. Labor demand in each industry n is given by a constant-elasticity function

$$E_{\ell n} = A_n \xi_{\ell n} W_\ell^{-\sigma}, \quad (39)$$

where $E_{\ell n}$ is employment, A_n is the national industry demand shifter, $\xi_{\ell n}$ is its idiosyncratic component, and σ is the elasticity of labor demand. The equilibrium is given by

$$\sum_n E_{\ell n} = E_\ell. \quad (40)$$

Now consider small changes in fundamentals A_n , $\xi_{\ell n}$ and M_ℓ . We use log-linearization around the observed equilibrium and employ the Jones (1965) hat algebra notation, with \hat{v} denoting the relative change in v between the equilibria. We then establish:

Proposition A6 After a set of small changes to fundamentals, the national industry employment

growth is characterized by

$$g_n = \sum_{\ell} \omega_{\ell n} g_{\ell n}, \quad (41)$$

for $\omega_{\ell n} = E_{\ell n} / \sum_{\ell'} E_{\ell' n}$ denoting the share of region ℓ in industry employment, and the change in region-by-industry employment $g_{\ell n}$ is characterized by

$$g_{\ell n} = g_n^* + \frac{\sigma}{\sigma + \phi} \varepsilon_{\ell} + \hat{\xi}_{\ell n} - \frac{\sigma}{\sigma + \phi} \sum_n s_{\ell n} (g_n^* + \hat{\xi}_{\ell n}), \quad (42)$$

where $g_n^* = \hat{A}_n$ is the national industry labor demand shock, $\varepsilon_{\ell} = \hat{M}_{\ell}$ is the regional labor supply shock, and $s_{\ell n} = E_{\ell n} / \sum_{n'} E_{\ell n'}$.

PROOF See Appendix B.8.

The first term in (42) justifies our interpretation of the observed industry employment growth as a noisy estimate of the latent labor demand shock g_n^* . The other terms constitute the “estimation error.” The first of them is proportional to the residual of the labor supply equation, ε_{ℓ} ; we have previously established the conditions under which it may or may not confound SSIV estimation. The other terms, that we abstracted away from in Section 4.1, include the idiosyncratic demand shock $\hat{\xi}_{\ell n}$ and shift-share averages of both national and idiosyncratic demand shocks. If the model is correct, all of these are uncorrelated with ε_{ℓ} , thus not affecting Assumption 1.

A.8 SSIV Consistency in Short Panels

This appendix shows how alternative shock exogeneity assumptions imply the consistency of panel SSIV regressions with many fixed effect coefficients. We consider the incidental parameters problem in “short” panels, with fixed T and $L \rightarrow \infty$ and with unit fixed effects, in which case the control coefficient γ cannot be consistently estimated with the fixed effects included in $w_{\ell t}$. We show how an analog of Assumption 3 can be instead applied to a demeaned shock-level unobservable that partials out the fixed effect nuisance coefficients. A similar argument applies to period fixed effects in the fixed L and $T \rightarrow \infty$ asymptotic.

Suppose for the linear causal model $y_{\ell t} = \beta x_{\ell t} + \epsilon_{\ell t}$ and control vector $w_{\ell t}$ (which includes unit FEs), we define $\gamma = \mathbb{E} [\sum_{\ell} e_{\ell t} w_{\ell t}^{\Delta} w_{\ell t}^{\Delta'}]^{-1} \mathbb{E} [\sum_{\ell} e_{\ell t} w_{\ell t}^{\Delta} \epsilon_{\ell t}^{\Delta}]$ where $v_{\ell t}^{\Delta}$ is a subvector of the (weighted) unit-demeaned observation of variable $v_{\ell t}$, $v_{\ell t} - \frac{\sum_{\tau} e_{\ell \tau} v_{\ell \tau}}{\sum_{\tau} e_{\ell \tau}}$, that drops any elements that are identically zero (e.g. those corresponding to the unit FEs in $w_{\ell t}$). Note we have assumed no perfect multicollinearity in the remaining elements such that $\mathbb{E} [\sum_{\ell} e_{\ell t} w_{\ell t}^{\Delta} w_{\ell t}^{\Delta'}]$ is invertible. We can then write $y_{\ell t}^{\Delta} = \beta x_{\ell t}^{\Delta} + w_{\ell t}^{\Delta'} \gamma + \varepsilon_{\ell t}^{\Delta}$. Suppose also that $\sum_{\ell} e_{\ell t} z_{\ell t} x_{\ell t}^{\perp} \xrightarrow{p} \pi$ for some $\pi \neq 0$ and the analog of Assumption B2 for unit-demeaned controls holds. Then, following the proof to Proposition 3, $\hat{\beta}$ is

consistent if and only if

$$\sum_{n,t} s_{nt} g_{nt} \bar{\varepsilon}_{nt}^{\Delta} \xrightarrow{p} 0, \quad (43)$$

where $s_{nt} = \sum_{\ell} e_{\ell t} s_{\ell n t}$ and $\bar{\varepsilon}_{nt}^{\Delta} = \frac{\sum_{\ell} e_{\ell t} s_{\ell n t} \varepsilon_{\ell t}^{\Delta}}{\sum_{\ell} e_{\ell t} s_{\ell n t}}$. This condition is satisfied when analogs of Assumptions 1,2, and B1 hold, or under the various extensions discussed in Section 3. In particular when $w_{\ell t}$ contains t -specific FE the key assumption of quasi-experimental shock assignment is $\mathbb{E}[g_{nt} | \bar{\varepsilon}^{\Delta}, s] = \mu_t$, for all n and t , allowing endogenous period-specific shock means μ_t via Proposition 4. This assumption avoids the incidental parameters problem by considering shocks as-good-as-randomly assigned given the set of unobserved $\bar{\varepsilon}_{nt}^{\Delta}$, each of which is a function of the time-varying $\varepsilon_{\ell p}$ across all periods p .

An intuitive special case is when the exposure shares and importance weights are time-invariant: $s_{\ell n t} = s_{\ell n 0}$ and $e_{\ell t} = e_{\ell 0}$. Then the weights in (43) are also time-invariant, $s_{nt} = s_{n0}$, and

$$\begin{aligned} \bar{\varepsilon}_{nt}^{\Delta} &= \frac{\sum_{\ell} e_{\ell 0} s_{\ell n 0} \varepsilon_{\ell t}^{\Delta}}{\sum_{\ell} e_{\ell} s_{\ell n 0}} \\ &= \frac{\sum_{\ell} e_{\ell 0} s_{\ell n 0} (\varepsilon_{\ell t} - \frac{1}{T} \sum_{\tau} \varepsilon_{\ell \tau})}{\sum_{\ell} e_{\ell 0} s_{\ell n 0}} \\ &= \bar{\varepsilon}_{nt} - \frac{1}{T} \sum_{\tau} \bar{\varepsilon}_{n\tau}, \end{aligned} \quad (44)$$

where $\bar{\varepsilon}_{nt} = \frac{\sum_{\ell} e_{\ell 0} s_{\ell n 0} \varepsilon_{\ell t}}{\sum_{\ell} e_{\ell 0} s_{\ell n 0}}$ is an aggregate of period-specific unobservables $\varepsilon_{\ell t}$. It is then straightforward to extend Propositions 3 and 4 under a shock-level assumption of strong exogeneity, i.e. that $\mathbb{E}[g_{nt} | \bar{\varepsilon}, s] = \mu_n + \zeta_t$ for all n and t . Here endogenous n -specific shock means are permitted by the observation in Section 4.3, that share-weighted n -specific FEs at the shock level are subsumed by ℓ -specific FEs in the SSIV regression when shares and weights are time-invariant.

A.9 SSIV Relevance with Panel Data

This appendix shows that holding the exposure shares fixed in a pre-period is likely to weaken the SSIV first-stage in panel regressions. Consider a panel extension of the first stage model used in Section 3.1, where $x_{\ell t} = \sum_n s_{\ell n t} x_{\ell n t}$ with $x_{\ell n t} = \pi_{\ell n t} g_{nt} + \eta_{\ell n t}$, $\pi_{\ell n} \geq \bar{\pi}$ for some fixed $\bar{\pi} > 0$, and the g_{nt} are mutually independent and mean-zero with variance $\sigma_{nt}^2 \geq \bar{\sigma}_g^2$ for fixed $\sigma_g^2 > 0$, independently of $\{\eta_{\ell n t}\}_{\ell, n, t}$. As in other appendices, we here treat $s_{\ell n t}$, $e_{\ell t}$, and $\pi_{\ell n t}$ as non-stochastic. Then an SSIV regression with $z_{\ell t} = \sum_n s_{\ell n t}^* g_{nt}$ as an instrument, where $s_{\ell n t}^*$ is either $s_{\ell n t}$ (updated shares) or $s_{\ell n 0}$ (fixed shares), yields a first-stage of

$$\mathbb{E} \left[\sum_{\ell} \sum_t e_{\ell t} z_{\ell t} x_{\ell t} \right] \geq \bar{\sigma}_g^2 \bar{\pi} \sum_{\ell} \sum_t e_{\ell t} \sum_n s_{\ell n t}^* s_{\ell n t}. \quad (45)$$

For panel SSIV relevance we require the $e_{\ell t}$ -weighted average of $\sum_n s_{\ell nt}^* s_{\ell nt}$ to not vanish asymptotically. With updated shares this is satisfied when the Herfindahl index of an average observation-period (across shocks) is non-vanishing, while in the fixed shares case the overlap of shares in periods 0 and t , $\sum_n s_{\ell n0} s_{\ell nt}$, may become weak or even vanish as $T \rightarrow \infty$, on average across observations.

A.10 SSIV with Multiple Endogenous Variables or Instruments

This appendix first generalizes our equivalence result to SSIV regressions with multiple endogenous variables and instruments, and discusses corresponding extensions of our quasi-experimental framework via the setting of Jaeger et al. (2018). We also describe how to construct the effective first-stage F -statistic of Montiel Olea and Pflueger (2013) for SSIV with one endogenous variable but multiple instruments. We then consider new shock-level IV procedures in this framework, which can be used for efficient estimation and specification testing. Finally, we illustrate these new procedures in the Autor et al. (2013) setting.

Generalized Equivalence and SSIV Consistency We consider a class of SSIV estimators of an outcome model with multiple treatment channels,

$$y_{\ell} = \beta' x_{\ell} + \gamma' w_{\ell} + \varepsilon_{\ell}, \quad (46)$$

where $x_{\ell} = (x_{1\ell}, \dots, x_{K\ell})'$ is instrumented by $z_{\ell} = (z_{1\ell}, \dots, z_{J\ell})'$, for $z_{j\ell} = \sum_n s_{\ell n} g_{jn}$ and $J \geq K$, and observations are weighted by e_{ℓ} . Members of this class are parameterized by a (possibly stochastic) full-rank $K \times J$ matrix \mathbf{c} , which is used to combine the instruments into a vector of length J , $\mathbf{c}z_{\ell}$. For example the two-stage least squares (2SLS) estimator sets $\mathbf{c} = \mathbf{x}^{\perp'} \mathbf{e} \mathbf{z} (\mathbf{z}^{\perp'} \mathbf{e} \mathbf{z}^{\perp})^{-1}$, where \mathbf{z}^{\perp} stacks observations of the residualized z_{ℓ}^{\perp} . IV estimates using a given combination are written as

$$\hat{\beta} = (\mathbf{c} \mathbf{z}' \mathbf{e} \mathbf{x}^{\perp})^{-1} \mathbf{c} \mathbf{z}' \mathbf{e} \mathbf{y}^{\perp}, \quad (47)$$

where \mathbf{y}^{\perp} and \mathbf{x}^{\perp} stack observations of the residualized y_{ℓ}^{\perp} and x_{ℓ}^{\perp} , \mathbf{z} stacks observations of z_{ℓ}' , and \mathbf{e} is an $L \times L$ diagonal matrix of e_{ℓ} weights. In just-identified IV models (i.e. $J = K$) the two \mathbf{c} 's cancel in this expression and all IV estimators are equivalent. Note that while the shocks g_{jn} are different across the multiple instruments, we assume here that the exposure shares $s_{\ell n}$ are all the same.

As in Proposition 1, $\hat{\beta}$ can be equivalently obtained by a particular shock-level IV regression. Intuitively, when the shares are the same, $\mathbf{c}z_{\ell}$ also has a shift-share structure based on a linear combination of shocks $\mathbf{c}g_n$, and thus Proposition 1 extends. Formally, write $\mathbf{z} = \mathbf{s}g$ where \mathbf{s} is an

$L \times N$ matrix of exposure shares and \mathbf{g} stacks observations of the shock vector \mathbf{g}'_n ; then,

$$\begin{aligned}\hat{\beta} &= (\mathbf{c}\mathbf{g}'\mathbf{s}'\mathbf{e}\mathbf{x}^\perp)^{-1}\mathbf{c}\mathbf{g}'\mathbf{s}'\mathbf{e}\mathbf{y}^\perp \\ &= (\mathbf{c}\mathbf{g}'\mathbf{S}\bar{\mathbf{x}}^\perp)^{-1}(\mathbf{c}\mathbf{g}'\mathbf{S}\bar{\mathbf{y}}^\perp),\end{aligned}\tag{48}$$

where \mathbf{S} is an $N \times N$ diagonal matrix with elements s_n , $\bar{\mathbf{x}}^\perp$ is an $N \times K$ matrix with elements \bar{x}_{kn}^\perp , and $\bar{\mathbf{y}}^\perp$ is an $N \times 1$ vector of \bar{y}_n^\perp . This is the formula for an s_n -weighted IV regression of \bar{y}_n^\perp on $\bar{x}_{1n}^\perp, \dots, \bar{x}_{Kn}^\perp$ with shocks as instruments, no constant, and the same \mathbf{c} matrix. Furthermore, as in Proposition 1,

$$\iota'\mathbf{S}\bar{\mathbf{y}}^\perp = \sum_n s_n \bar{y}_n^\perp = \sum_\ell e_\ell \left(\sum_n s_{\ell n} \right) y_\ell^\perp = \sum_\ell e_\ell y_\ell^\perp = 0,\tag{49}$$

and similarly for $\iota'\mathbf{S}\bar{\mathbf{x}}'$, where ι is a $N \times 1$ vector of ones. Therefore, the same estimate is obtained by including a constant in this IV procedure (and the same result holds including a shock-level control vector q_n provided $\sum_n s_{\ell n}$ has been included in w_ℓ , as in Proposition 5). The \mathbf{c} matrix is again redundant in the just-identified case.

A natural generalization of the quasi-experimental framework of Section 3 follows. Rather than rederiving all of these results, we discuss them intuitively in the setting of Jaeger et al. (2018). Here y_ℓ denotes the growth rate of wages in region ℓ in a given period (residualized on Mincerian controls), $x_{1\ell}$ is the immigrant inflow rate in that period, and $x_{2\ell}$ is the previous period's immigration rate. The residual ε_ℓ captures changes to local productivity and other regional unobservables. Jaeger et al. (2018, Table 5) estimate this model with two ‘‘past settlement’’ instruments $z_{1\ell} = \sum_n s_{\ell n} g_{1n}$ and $z_{2\ell} = \sum_n s_{\ell n} g_{2n}$, where $s_{\ell n}$ is the share of immigrants from country of origin n in location ℓ at a previous reference date and $\mathbf{g}_n = (g_{1n}, g_{2n})'$ gives the current and previous period's national immigration rate from n . When this path of immigration shocks is as-good-as-randomly assigned with respect to the aggregated productivity shocks $\bar{\varepsilon}_n$ (satisfying a generalized Assumption 1), the \mathbf{g}_n are uncorrelated across countries and $\mathbb{E}[\sum_n s_n^2] \rightarrow 0$ (satisfying a generalized Assumption 2), and appropriately generalized regularity conditions hold, the multiple-treatment shock orthogonality condition is satisfied: $\sum_n s_n g_{kn} \bar{\varepsilon}_n \xrightarrow{p} 0$ for each k . Then under the relevance condition from Proposition 2, again appropriately generalized, the SSIV estimates are consistent: $\hat{\beta} \xrightarrow{p} \beta$.

Effective First-Stage F -statistics With one endogenous variable and multiple instruments, the Montiel Olea and Pflueger (2013) effective first-stage F -statistic provides a state-of-art heuristic for detecting a weak first-stage. Here we describe a correction to it for SSIV that generalizes the F -statistic in the single instrument case discussed in Section 5.2. The Stata command *weakssivtest*, provided with our replication archive, implements this correction.⁵⁷

⁵⁷Our package extends the *weakivtest* command developed by Pflueger and Wang (2015).

Consider a structural first stage with multiple instruments and one endogenous variable:

$$x_\ell = \pi' z_\ell + \rho w_\ell + \eta_\ell. \quad (50)$$

Suppose each of the shocks satisfies Assumption 3, i.e. $\mathbb{E}[g_{jn} | \bar{\varepsilon}, q, s] = \mu'_j q_n$, where $\sum_n s_{\ell n} q_n$ is included in w_ℓ , and the residual shocks $g_{jn}^* = g_{jn} - \mu'_j q_n$ are independent from $\{\eta_\ell\}_\ell$. The Montiel Olea and Pflueger (2013) effective F -statistic for the 2SLS regression of y_ℓ on x_ℓ , instrumenting with $z_{1\ell}, \dots, z_{J\ell}$, controlling for w_ℓ , and weighting by e_ℓ , is given by

$$F_{\text{eff}} = \frac{(\sum_\ell e_\ell x_\ell^\perp z_\ell^\perp)' (\sum_\ell e_\ell x_\ell^\perp z_\ell^\perp)}{\text{tr}(\hat{V})}, \quad (51)$$

where \hat{V} estimates $V = \text{Var}[\sum_\ell e_\ell z_\ell^\perp \eta_\ell]$. Note that, as before, the first-stage covariance of the original SSIV regression equals that of the equivalent shock-level one from Proposition 5:

$$\sum_\ell e_\ell x_\ell^\perp z_\ell^\perp = \sum_\ell e_\ell x_\ell^\perp z_\ell = \sum_n s_n g_n \bar{x}_n^\perp = \sum_n s_n g_{n\perp} \bar{x}_n^\perp, \quad (52)$$

where $g_{n\perp}$ is the residuals from an s_n -weighted projection of g_n on q_n , which consistently estimates g_n^* . A natural extension of Proposition 5 to many mutually-uncorrelated shocks further implies that V is well-approximated by

$$\hat{V} = \sum_n s_n^2 g_{n\perp} g'_{n\perp} \bar{\eta}_n^2, \quad (53)$$

where, per the discussion in Section 5.2, $\bar{\eta}_n$ denotes the residuals from an IV regression of \bar{x}_n^\perp on $\bar{z}_{1n}^\perp, \dots, \bar{z}_{Jn}^\perp$, instrumented with g_{1n}, \dots, g_{Jn} , weighted by s_n and controlling for q_n . Plugging this \hat{V} into (51) yields the corrected effective first-stage F -statistic.

Efficient Shift-Share GMM In overidentified settings ($J > K$), it is natural to consider which estimators are most efficient; for quasi-experimental SSIV, this can be answered by combining the asymptotic results of Adão et al. (2019) with the classic generalized methods of moments (GMM) theory of Hansen (1982). Here we show how standard shock-level IV procedures (such as 2SLS) may yield efficient coefficient estimates $\hat{\beta}^*$, depending on the variance structure of multiple quasi-randomly assigned shocks.

We first note that the equivalence result (48) applies to SSIV-GMM estimators as well:

$$\begin{aligned} \hat{\beta} &= \arg \min_b (\mathbf{y}^\perp - \mathbf{x}^\perp b)' \mathbf{e} \mathbf{z} \mathbf{W} \mathbf{z}' \mathbf{e} (\mathbf{y}^\perp - \mathbf{x}^\perp b) \\ &= \arg \min_b (\bar{\mathbf{y}}^\perp - \bar{\mathbf{x}}^\perp b)' \mathbf{S} \mathbf{g} \mathbf{W} \mathbf{g}' \mathbf{S} (\bar{\mathbf{y}}^\perp - \bar{\mathbf{x}}^\perp b), \end{aligned} \quad (54)$$

where \mathbf{W} is an $J \times J$ moment-weighting matrix. This leads to an IV estimator with $\mathbf{c} = \bar{\mathbf{x}}^\perp \mathbf{S} \mathbf{g} \mathbf{W}$.

For 2SLS estimation, for example, $\mathbf{W} = (\mathbf{z}^{\perp'} \mathbf{e} \mathbf{z}^{\perp})^{-1}$. Under appropriate regularity conditions, the efficient choice of \mathbf{W}^* consistently estimates the inverse asymptotic variance of $\mathbf{z}' \mathbf{e} (\mathbf{y}^{\perp} - \mathbf{x}^{\perp} \beta) = \mathbf{g}' S \bar{\varepsilon} + o_p(1)$. Generalizations of results in Adão et al. (2019) can then be used to characterize this \mathbf{W}^* when shocks are as-good-as-randomly assigned with respect to $\bar{\varepsilon}$. Given an estimate $\hat{\mathbf{W}}^*$, an efficient coefficient estimate $\hat{\beta}^*$ is given by shock-level IV regressions (48) that set $\mathbf{c}^* = \bar{\mathbf{x}}^{\perp'} \mathbf{S} \mathbf{g} \hat{\mathbf{W}}^*$. A χ^2_{J-K} test statistic based on the minimized objective in (54) can be used for specification testing.

As an example, suppose shocks are conditionally homoskedastic with the same variance-covariance matrix across n , $\text{Var}[\mathbf{g}_n \mid \bar{\varepsilon}, s] = \mathbf{G}$ for a constant $J \times J$ matrix \mathbf{G} . Then the optimal $\hat{\beta}^*$ is obtained by a shock-level 2SLS regression of \bar{y}_n^{\perp} on all \bar{x}_{kn}^{\perp} (instrumented by g_{jn} and weighted by s_n). We show this in the case of no controls (and mean-zero shocks) for notational simplicity. Then,

$$\begin{aligned} \text{Var}[\mathbf{g}' S (\bar{\mathbf{y}}^{\perp} - \bar{\mathbf{x}}^{\perp} \beta)] &= \mathbb{E}[\bar{\varepsilon}' \mathbf{S} \mathbf{g} \mathbf{g}' \mathbf{S} \bar{\varepsilon}] \\ &= \text{tr}(\mathbb{E}[\bar{\varepsilon}' \mathbf{S} \mathbf{G} \mathbf{S} \bar{\varepsilon}]) \\ &= k \mathbf{G} \end{aligned} \tag{55}$$

for $k = \text{tr}(\mathbb{E}[\mathbf{S} \bar{\varepsilon} \bar{\varepsilon}' \mathbf{S}])$. The optimal weighting matrix thus should consistently estimate \mathbf{G} , which is satisfied by $\hat{\mathbf{G}} = \mathbf{g}' \mathbf{S} \mathbf{g}$. Under appropriate regularity conditions, a feasible optimal GMM estimate is thus given by

$$\begin{aligned} \hat{\beta}^* &= (\bar{\mathbf{x}}^{\perp'} \mathbf{S} \mathbf{g} \hat{\mathbf{G}}^{-1} \mathbf{g}' \mathbf{S} \bar{\mathbf{x}}^{\perp})^{-1} (\bar{\mathbf{x}}^{\perp'} \mathbf{S} \mathbf{g} \hat{\mathbf{G}}^{-1} \mathbf{g}' \mathbf{S} \bar{\mathbf{y}}^{\perp}) \\ &= \left((P_{\mathbf{g}} \bar{\mathbf{x}}^{\perp})' \mathbf{S} \bar{\mathbf{x}}^{\perp} \right)^{-1} (P_{\mathbf{g}} \bar{\mathbf{x}}^{\perp})' \mathbf{S} \bar{\mathbf{y}}^{\perp}, \end{aligned} \tag{56}$$

where $P_{\mathbf{g}} = \mathbf{g}(\mathbf{g}' \mathbf{S} \mathbf{g})^{-1} \mathbf{g}' \mathbf{S}$ is an s_n -weighted shock projection matrix. This is the formula for an s_n -weighted IV regression of \bar{y}_n^{\perp} on the fitted values from projecting the \bar{x}_{kn}^{\perp} on the shocks, corresponding to the 2SLS regression above. Straightforward extensions of this equivalence between optimally-weighted estimates of β and shock-level overidentified IV procedures follow in the case of heteroskedastic or clustered shocks, in which case the 2SLS estimator (56) is replaced by the estimator of White (1982). We emphasize that these shock-level estimators are generally different than 2SLS or White (1982) estimators at the level of original observations, which are optimal under conditional homoskedasticity and independence assumptions placed on the residual ε_{ℓ} (assumptions which are generally violated in our quasi-experimental framework).

Many Shocks in Autor et al. (2013) Appendix Table C5 illustrates different shock-level overidentified IV estimators in the setting of Autor et al. (2013), introduced in Section 6.2.1. ADH construct their shift-share instrument based on the growth of Chinese imports in eight economies comparable to the U.S., together. We separate them to produce eight sets of industry shocks g_{jn} , $j = 1, \dots, 8$, each

reflecting the growth of Chinese imports in one of those countries. As in Section 6.2, the outcome of interest is a commuting zone’s growth in total manufacturing employment with the single treatment variable measuring a commuting zone’s local exposure to the growth of imports from China (see footnote 39 for precise variable definitions). The vector of controls coincides with that of column 3 of Table 4, isolating within-period variation in manufacturing shocks. Per Section 5.1, exposure-robust standard errors are obtained by controlling for period main effects in the shock-level IV procedures, and we report corrected first stage F -statistics constructed as detailed above.

Column 1 reports estimates of the ADH coefficient β using the industry-level two-stage least squares procedure (56). At -0.238, this estimate it is very similar to the just-identified estimate in column 3 of Table 4. Column 2 shows that we also obtain a very similar coefficient of -0.247 with an industry-level limited information maximum likelihood (LIML) estimator. Finally, in column 3 we report a two-step optimal IV estimate of β using an industry-level implementation of the White (1982) estimator. Both the coefficient and standard error fall somewhat, with the latter consistent with the theoretical improvement in efficiency relative to columns 1 and 2. From this efficient estimate we obtain an omnibus overidentification test statistic of 10.92, distributed as chi-squared with seven degrees of freedom under the null of correct specification. This yields a p -value for the test of joint orthogonality of all eight ADH shocks of 0.142. Table C5 also reports the corrected effective first-stage F -statistic which measures the strength of the relationship between the endogenous variable and the eight shift-share instruments across regions. At 15.10 it is substantially lower than with one instrument in column 3 of Table 4 but still above the conventional heuristic threshold of 10.

A.11 Finite-Sample Performance of SSIV: Monte-Carlo Evidence

In this appendix we study the finite-sample performance of the SSIV estimator via Monte-Carlo simulation. We base this simulation on the data of Autor et al. (2013), as described in Section 6.2. For comparison, we also simulate more conventional shock-level IV estimators, similar to those used in Acemoglu et al. (2016), which also estimate the effects of import competition with China on U.S. employment. We begin by describing the design of these simulations and the benchmark Monte-Carlo results. We then explore how the simulation results change with various deviations from the benchmark: with different levels of industry concentration, different numbers of industries and regions, and with many shock instruments. Besides showing the general robustness of our framework, these extensions allow us to see how informative some conventional rules of thumb are on the finite-sample performance of shift-share estimators.⁵⁸

⁵⁸Naturally, these simulation results may be specific to the data-generating process we consider here, modeled after the “China shock” setting of Autor et al. (2013). In practice, we recommend that researchers perform similar simulations based on their data if they are concerned with the quality of asymptotic approximation—a suggestion that of course applies to conventional shock-level IV analyses as well.

Simulation design We base our benchmark data-generating process for SSIV on the specification in column 3 of Table 4. The outcome variable $y_{\ell t}$ corresponds to the change in manufacturing employment as a fraction of working-age population of region ℓ in period t , treatment $x_{\ell t}$ is a measure of regional import competition with China, and the shift-share instrument is constructed by combining the industry-level growth of China imports in eight developed economies, g_{nt} , with lagged regional employment weights of different industries $s_{\ell nt}$. We also include pre-treatment controls $w_{\ell t}$ as in column 3 of Table 4 and estimate regressions with regional employment weights $e_{\ell t}$; see Section 6.2.1 for more detail on the Autor et al. (2013) setting.

In a first step we obtain an estimated SSIV second and first stage of

$$y_{\ell t} = \hat{\beta}x_{\ell t} + \hat{\gamma}'w_{\ell t} + \hat{\varepsilon}_{\ell t}, \quad (57)$$

$$x_{\ell t} = \hat{\pi}z_{\ell t} + \hat{\rho}'w_{\ell t} + \hat{u}_{\ell t}. \quad (58)$$

We then generate 10,000 simulated samples by drawing shocks g_{nt}^* , as detailed below, and constructing the simulated shift-share instrument $z_{\ell t}^* = \sum_n s_{\ell nt}g_{nt}^*$ and treatment $x_{\ell t}^* = \hat{\pi}z_{\ell t}^* + \hat{u}_{\ell t}$. Imposing a true causal effect of $\beta^* = 0$, we use the same $y_{\ell t}^* \equiv \hat{\varepsilon}_{\ell t}$ as the outcome in each simulation (note that it is immaterial whether we include $\hat{\pi}'w_{\ell t}$ and $\hat{\rho}'w_{\ell t}$, since all our specifications control for $w_{\ell t}$). By keeping $\hat{\varepsilon}_{\ell t}$ and $\hat{u}_{\ell t}$ fixed, we study the finite sample properties of the estimator that arises from the randomness of shocks, which is the basis of the inferential framework of Adão et al. (2019); we also avoid having to take a stand on the joint data generating process of $(\varepsilon_{\ell t}, u_{\ell t})$, which this inference framework does not restrict.

We estimate SSIV specifications that parallel (57)-(58) from the simulated data

$$y_{\ell t}^* = \beta^*x_{\ell t}^* + \gamma^{*'}w_{\ell t} + \varepsilon_{\ell t}^*, \quad (59)$$

$$x_{\ell t}^* = \pi^*z_{\ell t}^* + \rho^{*'}w_{\ell t} + u_{\ell t}^*. \quad (60)$$

using the original weights $e_{\ell t}$ and controls $w_{\ell t}$. We then test the (true) hypothesis $\beta^* = 0$ using either the heteroskedasticity-robust standard errors from the equivalent industry-level regression or their version with the null imposed, as in Section 5.1.⁵⁹ As in column 3 of Table 4, we control for period indicators as q_{nt} in the industry-level regression.

Our comparison estimator is a conventional industry-level IV inspired by Acemoglu et al. (2016). However, we try to keep the IV regression as similar to the SSIV as possible, thus diverging from Acemoglu et al. (2016) in some details. Specifically, the outcome y_{nt} is the industry employment growth as measured by these authors. It is defined for 392 out of the 397 industries in Autor et

⁵⁹Note that there is no need for clustering since we generate the shocks independently across industries in all simulations. We have verified, however, that allowing for correlation in shocks within industry groups and using clustered standard errors yields similar results.

al. (2013), so we drop the remaining five industries in each period. The endogenous regressor $x_{nt} \equiv g_{nt}^{US}$ (growth of U.S. imports from China per worker) and the instrument g_{nt} (growth of China imports into eight developed economies) are those from which we built the shift-share endogenous regressor and treatment, respectively (see footnote 39). Construction of those variables differ from Acemoglu et al. (2016) who measure imports relative to domestic absorption rather than employment. We also follow our SSIV analysis in using period indicators as the only industry-level control variables q_{nt} and taking identical regression importance weights s_{nt} .

The Monte-Carlo strategy for the conventional shock-level IV parallels the one for SSIV; we obtain an estimated industry-level second and first stage of

$$y_{nt} = \hat{\beta}_{\text{ind}} x_{nt} + \hat{\gamma}' q_{nt} + \hat{\varepsilon}_{nt}, \quad (61)$$

$$x_{nt} = \hat{\pi}_{\text{ind}} g_{nt} + \hat{\rho}' q_{nt} + \hat{u}_{nt}. \quad (62)$$

using the s_{nt} importance weights. We then perform 10,000 simulations where we regenerate shocks g_{nt}^* and regress $y_{nt}^* = \hat{\varepsilon}_{nt}$ (consistent with a true causal effect of $\beta^{\text{ind}} = 0$, given that we control for q_{nt}) on $x_{nt}^* = \hat{\pi}_{\text{ind}} g_{nt}^* + \hat{u}_{nt}$, instrumenting by g_{nt}^* , controlling for q_{nt} , and weighting by s_{nt} . We test $\beta_{\text{ind}} = 0$ by using robust standard errors in this IV regression or the version with the null imposed, which corresponds to a standard Lagrange Multiplier test for this true null hypothesis.

In both simulations we report the rejection rate of nominal 5% level tests for $\beta = 0$ and $\beta_{\text{ind}} = 0$ to gauge the quality of each asymptotic approximation. We do not report the bias of the estimators because they are all approximately unbiased (more precisely, the simulated median bias is at most 1% of the estimator's standard deviation). However we return to the question of bias at the end of the section, where we extend the analysis to having many instruments with a weak first stage.

Main results Table C7 reports the rejection rates for shift-share IV (columns 1 and 2) and conventional industry-level IV (columns 3 and 4) in various simulations. Specifically, column 1 corresponds to using exposure-robust standard errors from the equivalent industry-level IV, and column 2 implements the version with the null hypothesis imposed. Columns 3 and 4 parallel columns 1 and 2 when applied to conventional IV: the former uses heteroskedasticity-robust standard errors and the latter tests $\beta_{\text{ind}} = 0$ with the null imposed, which amounts to using the Lagrange multiplier test.

The simulations in Panel A vary the data-generating process of the shocks. Following Adão et al. (2019) in row (a) we draw the shocks *iid* from a normal distribution with the variance matched to the sample variance of the shocks in the data after de-meaning by year. The rejection rate is close to the nominal rate of 5% for both SSIV and conventional IV (7.6% and 6.8%, respectively), and in both cases it becomes even closer when the null is imposed (5.2% and 5.0%).

This simulation may not approximate the data-generating process well because of heteroskedastic-

ity: smaller industries have more volatile shocks.⁶⁰ To match unrestricted heteroskedasticity, in row (b) we use wild bootstrap, generating $g_{nt}^* = g_{nt}\nu_{nt}^*$ by multiplying the year-demeaned observed shocks g_{nt} by $\nu_{nt}^* \stackrel{iid}{\sim} \mathcal{N}(0, 1)$ (Liu 1988). This approach also provides a better approximation for the marginal distribution of shocks than the normality assumption. Here the relative performance of SSIV is even better: the rejection rate is 8.0% vs. 14.2% for conventional IV.

We now depart from the row (b) simulation in several directions, as a case study for the sensitivity of the asymptotic approximation to different features of the SSIV setup. Specifically, we study the role of the Herfindahl concentration index across industries, the number of regions and industries, and the many weak instrument bias. We uniformly find that the performance of the SSIV estimator is similar to that of industry-level IV. Our results also suggest that the Herfindahl index is a useful statistic for measuring the effective number of industries in SSIV, and the first-stage F -statistic is informative about the weak instrument bias, as usual.

The Role of Industry Concentration Since Assumption 2 requires small concentration of industry importance weights, measured using the Herfindahl index $\sum_{n,t} s_{nt}^2 / \left(\sum_{n,t} s_{nt}\right)^2$, Panel B of Table C7 studies how increasing the skewness of s_{nt} towards the bigger industries affects coverage of the tests.⁶¹ For conventional IV this simply amounts to reweighting the regression. Specifically, for a parameter $\alpha > 1$, we use weights

$$\tilde{s}_{nt} = s_{nt}^\alpha \cdot \frac{\sum_{n',t'} s_{n't'}}{\sum_{n',t'} s_{n't'}^\alpha}.$$

We choose the unique α to match the target level of \widetilde{HHI} by solving, numerically,

$$\frac{\sum_{n,t} (\tilde{s}_{nt})^2}{\left(\sum_{n,t} \tilde{s}_{nt}\right)^2} = \widetilde{HHI}. \quad (63)$$

Matching the Herfindahl index in SSIV is more complicated since we need to choose how exactly to amend shares $\tilde{s}_{\ell nt}$ and regional weights $\tilde{e}_{\ell t}$ that would yield \tilde{s}_{nt} from (63). We proceed as follows: we consider the lagged level of manufacturing employment by industry $E_{\ell nt} = e_{\ell t}s_{\ell nt}$ and the total regional non-manufacturing employment $E_{\ell 0t} = e_{\ell t}(1 - \sum_n s_{\ell nt})$.⁶² We then define $\tilde{E}_{\ell nt} = E_{\ell nt} \cdot \frac{\tilde{s}_{nt}}{s_{nt}}$ for manufacturing industries (and leave non-manufacturing employment unchanged, $\tilde{E}_{\ell 0t} = E_{\ell 0t}$). This increases employment in large manufacturing industries proportionately in all regions, while

⁶⁰This is established by unreported regressions of $|g_{nt}|$ on s_{nt} , for year-demeaned g_{nt} from ADH, with or without weights. The negative relationship is significant at conventional levels.

⁶¹Note that in ADH $\sum_n s_{\ell nt}$ equals the lagged share of regional manufacturing employment, which is below one. We thus renormalize the shares when computing the Herfindahl.

⁶²The interpretation of $E_{\ell nt}$ as the lagged level is approximate since $e_{\ell t}$ is measured at the beginning of period in ADH, while $s_{\ell nt}$ is lagged.

reducing it in smaller ones. We then recompute shares $\tilde{s}_{\ell nt}$ and weights $\tilde{e}_{\ell t}$ accordingly:

$$\tilde{e}_{\ell t} = \sum_{n=0}^N \sum_t \tilde{E}_{\ell nt},$$

$$\tilde{s}_{\ell nt} = \frac{\tilde{E}_{\ell nt}}{\tilde{e}_{\ell t}}.$$

Rows (c)–(e) of Table C7 Panel B implement this procedure for target Herfindahl levels of 1/50, 1/20, and 1/10, respectively. For comparison, the Herfindahl in the actual ADH data is 1/191.6 (Table 1, column 2). The table finds that even with the Herfindahl index of 1/20 (corresponding to the “effective” number of shocks of 20 in both periods total) the rejection rate is still around 7%, a level that may be considered satisfactory. It also shows that the rejection rate grows when the Herfindahl is even higher, at 1/10, suggesting that the Herfindahl can be used as an indicative rule of thumb. More importantly, the rejection rates are similar for SSIV and conventional industry-level IV, as before.

Varying the Number of Industries and Regions The asymptotic sequence we consider in Section 3.1 relies on both N and L growing. Here we study how the quality of the asymptotic approximation depends on these parameters.

First, to consider the case of small N , we aggregate industries in a natural way: from 397 four-digit manufacturing SIC industries into 136 three-digit ones and further into 20 two-digit ones and reconstruct the endogenous right-hand side variable and the instrument using aggregated data.⁶³ Rows (f) and (g) of Table C7 Panel C report simulation results based on the aggregated data. They show that rejection rates are similar to the case of detailed industries, and between SSIV and conventional IV. This does not mean that disaggregated data are not useful: the dispersion of the simulated distribution (not reported) increases with industry aggregation, reducing test power. However, standard errors correctly reflect this variability, resulting in largely unchanged test coverage rates.

Second, to study the implications of having fewer regions L , we select a random subset of them in each simulation. The results are presented in Rows (h) and (i) of Panel C for $L = 100$ and 25, compared to the original $L = 722$, respectively.⁶⁴ They show once again that rejection rates are not significantly affected (even though unreported standard errors expectedly increase).

Many Weak Instruments In this final simulation we return to the question of SSIV bias. Since our previous simulations confirm that just-identified SSIV is median-unbiased, we turn to the case of

⁶³Specifically, we aggregate imports from China to the U.S. and either developed economies as well as the number of U.S. workers by manufacturing industry to construct the new g_{nt} and g_{nt}^{US} . We then aggregate the shares $s_{\ell nt}$ and $s_{\ell nt}^{\text{current}}$ to construct $x_{\ell t}$ and $z_{\ell t}$ (see footnote 39 for formulas). We do not change the regional outcome, controls, or importance weights. For conventional IV, we additionally reconstruct the outcome (industry employment growth) by aggregating employment levels by year in the Acemoglu et al. (2016) data and measuring growth according to their formulas.

⁶⁴When we select regions, we always keep observations from both periods for each selected region. We keep the second- and first-stage coefficients from the full sample to focus on the noise that arises from shock randomness.

multiple instruments. We show that the problem of many weak instruments is similar between SSIV and conventional IV, and that first-stage F -statistics, when properly constructed, can serve as useful heuristics.

For clarity, we begin by describing the procedure for the conventional shock-level IV that is a small departure from Column 3 of Table C7. For a given number of instruments $J \geq 1$, in each simulation we generate g_{jnt}^* , $j = 1, \dots, J$, independently across j using wild bootstrap (as in Table C7 Row (b)).⁶⁵ We make only the first instrument relevant by setting $x_{nt}^* = \hat{\pi}_{\text{ind}} g_{1nt}^* + \sum_{j=2}^J 0 \cdot g_{jnt}^* + \hat{u}_{nt}$. We then estimate the IV regression of $y_{nt}^* \equiv \hat{\varepsilon}_{nt}$ on x_{nt}^* , instrumenting with $g_{1nt}^*, \dots, g_{Jnt}^*$, controlling for q_{nt} , and weighting by s_{nt} . We use robust standard errors and compute the effective first-stage F -statistic using the Montiel Olea and Pflueger (2013) method.

The procedure for SSIV is more complex but as usual parallels the one for the conventional shock-level IV as much as possible. Given simulated shocks g_{jnt}^* , we construct shift-share instruments $z_{j\ell t}^* = \sum_{\ell} s_{\ell nt} g_{jnt}^*$ and make only the first of them relevant, $x_{\ell t}^* = \hat{\pi} z_{1\ell t}^* + \sum_{j=2}^J 0 \cdot z_{j\ell t}^* + \hat{u}_{\ell t}$. Since the equivalence result from Section 2.3 need not hold for overidentified SSIV, we rely on the results in Appendix A.10: we estimate β^* from the industry-level regression of $\bar{y}_{nt}^{*\perp}$ (based on $y_{\ell t}^* = \hat{\varepsilon}_{\ell t}$ as before) on $\bar{x}_{nt}^{*\perp}$ by 2SLS, instrumenting by $g_{1nt}^*, \dots, g_{Jnt}^*$, controlling for q_{nt} and weighting by s_{nt} . We compute robust standard errors from this regression to test $\beta^* = 0$. For effective first-stage F -statistics, we follow the procedure described in Appendix A.10 and implemented via our *weakssivtest* command in Stata.

Table C8 reports the result for $J = 1, 5, 10, 25$, and 50, presenting the rejection rate corresponding to the 5% nominal, the median bias as a percentage of the simulated standard deviation, and the median first-stage F -statistic. Panel A corresponds to SSIV and Panel B to the conventional shock-level IV. For higher comparability, we adjust the first-stage coefficient $\hat{\pi}_{\text{ind}}$ in the latter in order to make the F -statistics approximately match between the two panels. We find that the median bias is now non-trivial and grows with J , at the same time as the F -statistic declines. However, the level of bias is similar for the two estimators. The rejection rates tend to be higher for conventional IV than SSIV, although they converge as J grows.

⁶⁵For computational reasons we perform only $15,000/J$ simulations when $J > 1$ (but 10,000 for $J = 1$ as before).

B Appendix Proofs

B.1 Proposition 4 and Extensions

This section proves Proposition 4 and extensions that allow for certain forms of mutual shock dependence (Assumptions 5 and 6). Proposition 3 is obtained as a special case, where $q_n = 1$. In addition to Assumptions 3 and 4 and the relevance condition of $\sum_{\ell} e_{\ell} z_{\ell} x_{\ell}^{\perp} \xrightarrow{p} \pi$ with $\pi \neq 0$, the proof of Proposition 4 uses two regularity conditions:

Assumption B1: $\mathbb{E}[\tilde{g}_n^2 \mid \bar{\varepsilon}, q, s]$ and $\mathbb{E}[\bar{\varepsilon}_n^2 \mid s]$ are uniformly bounded by some fixed B_g and B_{ε} .

Assumption B2: $\|\sum_{\ell} e_{\ell} w_{\ell} \varepsilon_{\ell}\|_1 = o_p(1)$, $\max |(\sum_{\ell} e_{\ell} w_{\ell} w'_{\ell})^{-1}| = O_p(1)$, and $\max |\sum_{\ell} e_{\ell} w_{\ell} z_{\ell}| = O_p(1)$.

The first of these is a weak condition on the second moments of shocks and shock-level unobservables which we show below permits a shock-level law of large numbers. The second condition ensures the consistency of the IV estimate of the control coefficient, $\hat{\gamma} = (\sum_{\ell} e_{\ell} w_{\ell} w'_{\ell})^{-1} \sum_{\ell} e_{\ell} w_{\ell} \varepsilon_{\ell} = \gamma + (\sum_{\ell} e_{\ell} w_{\ell} w'_{\ell})^{-1} \sum_{\ell} e_{\ell} w_{\ell} \varepsilon_{\ell}$ (see footnote 5), and stochastic boundedness of the weighted average $\sum_{\ell} e_{\ell} w_{\ell} z_{\ell}$, while generally allowing the length of the control vector to increase with L . We discuss low-level conditions for the consistency of $\hat{\gamma}$ in Appendix A.5.

To prove Proposition 4, we first note that under Assumption B2,

$$\begin{aligned} \sum_n s_n g_n \bar{\varepsilon}_n - \sum_n s_n g_n \bar{\varepsilon}_n^{\perp} &= \sum_{\ell} e_{\ell} z_{\ell} (\varepsilon_{\ell} - \varepsilon_{\ell}^{\perp}) \\ &= \left(\sum_{\ell} e_{\ell} z_{\ell} w'_{\ell} \right) (\hat{\gamma} - \gamma) \\ &= \left(\sum_{\ell} e_{\ell} z_{\ell} w'_{\ell} \right) \left(\sum_{\ell} e_{\ell} w_{\ell} w'_{\ell} \right)^{-1} \sum_{\ell} e_{\ell} w_{\ell} \varepsilon_{\ell} \xrightarrow{p} 0, \end{aligned} \quad (64)$$

so that, when the relevance condition holds,

$$\begin{aligned} \hat{\beta} - \beta &= \frac{\sum_n s_n g_n \bar{\varepsilon}_n^{\perp}}{\sum_n s_n g_n \bar{x}_n^{\perp}} \\ &= \pi^{-1} \sum_n s_n g_n \bar{\varepsilon}_n (1 + o_p(1)). \end{aligned} \quad (65)$$

Furthermore, since $\sum_n s_{\ell n} = 1$, we also have under Assumption B2 that

$$\sum_n s_n q'_n \mu \bar{\varepsilon}_n = \left(\sum_{\ell} e_{\ell} \tilde{w}_{\ell} \varepsilon_{\ell} \right)' \mu \xrightarrow{p} 0. \quad (66)$$

Thus

$$\sum_n s_n g_n \bar{\varepsilon}_n = \sum_n s_n \tilde{g}_n \bar{\varepsilon}_n + o_p(1), \quad (67)$$

with

$$\mathbb{E} \left[\sum_n s_n \tilde{g}_n \bar{\varepsilon}_n \right] = 0 \quad (68)$$

under Assumption 3.

To prove consistency of $\hat{\beta}$, it remains to show that $\text{Var} [\sum_n s_n \tilde{g}_n \bar{\varepsilon}_n] \rightarrow 0$ such that $\sum_n s_n g_n \bar{\varepsilon}_n \xrightarrow{p} 0$. Since

$$\mathbb{E} [\tilde{g}_n \tilde{g}_{n'} \mid \bar{\varepsilon}, q, s] = \text{Cov} [\tilde{g}_n, \tilde{g}_{n'} \mid \bar{\varepsilon}, q, s] = 0 \quad (69)$$

under Assumptions 3 and 4,

$$\begin{aligned} \text{Var} \left[\sum_n s_n \tilde{g}_n \bar{\varepsilon}_n \right] &= \mathbb{E} \left[\left(\sum_n s_n \tilde{g}_n \bar{\varepsilon}_n \right)^2 \right] \\ &= \sum_n \sum_{n'} \mathbb{E} [s_n s_{n'} \tilde{g}_n \tilde{g}_{n'} \bar{\varepsilon}_n \bar{\varepsilon}_{n'}] \\ &= \sum_n \mathbb{E} [s_n^2 \mathbb{E} [\tilde{g}_n^2 \mid \bar{\varepsilon}, q, s] \bar{\varepsilon}_n^2 \mid s] . \end{aligned} \quad (70)$$

Then, by Assumption B1 and the Cauchy-Schwartz inequality:

$$\text{Var} \left[\sum_n s_n \tilde{g}_n \bar{\varepsilon}_n \right] \leq B_g B_\varepsilon \mathbb{E} \left[\sum_n s_n^2 \right] \rightarrow 0. \quad (71)$$

Extensions Similar steps establish equation (71) when Assumption 4 is replaced by either Assumption 5 or 6. Under Assumption 5 we have, for $N(c) = \{n: c(n) = c\}$,

$$\begin{aligned}
\text{Var} \left[\sum_n s_n \tilde{g}_n \bar{\varepsilon}_n \right] &= \mathbb{E} \left[\left(\sum_c \sum_{n \in N(c)} s_n \tilde{g}_n \bar{\varepsilon}_n \right)^2 \right] \\
&= \mathbb{E} \left[\sum_c s_c^2 \mathbb{E} \left[\left(\sum_{n \in N(c)} \frac{s_n}{s_c} \tilde{g}_n \bar{\varepsilon}_n \right)^2 \mid s \right] \right] \\
&= \mathbb{E} \left[\sum_c s_c^2 \sum_{n, n' \in N(c)} \frac{s_n}{s_c} \frac{s_{n'}}{s_c} \mathbb{E} [\tilde{g}_n \tilde{g}_{n'} \bar{\varepsilon}_n \bar{\varepsilon}_{n'} \mid s] \right] \\
&\leq B_g B_\varepsilon \mathbb{E} \left[\sum_c s_c^2 \right] \rightarrow 0.
\end{aligned} \tag{72}$$

Here the last line used Assumption B1 and the Cauchy-Schwartz inequality twice: to establish, for $n, n' \in N(c)$,

$$\begin{aligned}
\mathbb{E} [\tilde{g}_n \tilde{g}_{n'} \mid \bar{\varepsilon}, q, s] &\leq \sqrt{\mathbb{E} [\tilde{g}_n^2 \mid \bar{\varepsilon}, q, s] \mathbb{E} [\tilde{g}_{n'}^2 \mid \bar{\varepsilon}, q, s]} \\
&\leq B_g
\end{aligned} \tag{73}$$

and

$$\begin{aligned}
\mathbb{E} [|\bar{\varepsilon}_n| |\bar{\varepsilon}_{n'}| \mid s_c] &\leq \sqrt{\mathbb{E} [\bar{\varepsilon}_n^2 \mid s] \mathbb{E} [\bar{\varepsilon}_{n'}^2 \mid s]} \\
&\leq B_\varepsilon.
\end{aligned} \tag{74}$$

If we instead replace Assumption 4 with Assumption 6, we have

$$\begin{aligned}
\text{Var} \left[\sum_n s_n \tilde{g}_n \bar{\varepsilon}_n \right] &= \mathbb{E} \left[\left(\sum_n s_n \tilde{g}_n \bar{\varepsilon}_n \right)^2 \right] \\
&= \sum_n \sum_{n'} \mathbb{E} [s_n s_{n'} \mathbb{E} [\tilde{g}_n \tilde{g}_{n'} \mid \bar{\varepsilon}, q, s] \bar{\varepsilon}_n \bar{\varepsilon}_{n'}] \\
&\leq B_L \sum_n \sum_{n'} f(|n' - n|) \mathbb{E} [|s_n \bar{\varepsilon}_n s_{n'} \bar{\varepsilon}_{n'}|] \\
&= B_L \left(\sum_n \mathbb{E} [(s_n \bar{\varepsilon}_n)^2] f(0) + 2 \sum_{r=1}^{N-1} \sum_{n=1}^{N-r} \mathbb{E} [|s_{n+r} \bar{\varepsilon}_{n+r}| \cdot |s_n \bar{\varepsilon}_n|] f(r) \right) \\
&\leq \left(B_L \sum_n \mathbb{E} [s_n^2 \mathbb{E} [\bar{\varepsilon}_n^2 \mid s]] \right) \left(f(0) + 2 \sum_{r=1}^{N-1} f(r) \right) \\
&\leq B_\varepsilon \left(f(0) + 2 \sum_{r=1}^{N-1} f(r) \right) \left(B_L \mathbb{E} \left[\sum_n s_n^2 \right] \right) \rightarrow 0,
\end{aligned} \tag{75}$$

using $\mathbb{E}[\bar{\varepsilon}_n^2 | s_n] < B_\varepsilon$ in the last line. Here the second-to-last line follows because for any sequence of numbers a_1, \dots, a_N and any $r > 0$,

$$\begin{aligned} \sum_n a_n^2 &\geq \frac{1}{2} \left(\sum_{n=1}^{N-r} a_n^2 + \sum_{n=1}^{N-r} a_{n+r}^2 \right) \\ &= \frac{1}{2} \sum_{n=1}^{N-r} (a_n - a_{n+r})^2 + \sum_{n=1}^{N-r} a_n a_{n+r} \\ &\geq \sum_{n=1}^{N-r} a_n a_{n+r}, \end{aligned} \tag{76}$$

and the same is true in expectation if $a_n = |s_n \bar{\varepsilon}_n|$ are random variables. We note that allowing B_L to grow in the asymptotic sequence imposes much weaker conditions on the correlation structure of shocks. For example, with shock importance weights s_n approximately equal, i.e. $\sum_n s_n^2 = O_p(1/N)$, it is enough to have $|\text{Cov}[\tilde{g}_n, \tilde{g}_{n'} | \bar{\varepsilon}, q, s]| \leq B_1/N^\alpha$ for any $\alpha > 0$: in this case one can satisfy Assumption 6 by setting $B_L = B_1 N^{1-\alpha/2}$ and $f(r) = r^{-1-\alpha/2}$.

B.2 Proposition 5 and Related Results

This section proves Proposition 5 and then establishes several additional results mentioned in Section 5.1. First, we show the heteroskedasticity-robust standard error from estimating equation (10) is numerically equivalent to the baseline IV standard error of Adão et al. (2019) when w_ℓ contains only a constant. Second, we show that when Assumption B4 on the structure of controls is relaxed, the standard errors from Proposition 5 are conservative. We also discuss the likely difference between our standard error estimates and those of Adão et al. (2019) when Assumption B4 holds. Finally, we show how the alternative null-imposed inference procedure of Adão et al. (2019) is also conveniently obtained from our equivalent shock-level regression.

We prove Proposition 5 under additional assumptions that largely follow Adão et al. (2019):

Assumption B3: The first stage satisfies $x_\ell = \sum_n s_{\ell n} \pi_{\ell n} g_n + \eta_\ell$, for all ℓ .

Assumption B4: The control vector can be partitioned as $w_\ell = [\tilde{w}'_\ell, u'_\ell]'$, for $\tilde{w}_\ell = \sum_n s_{\ell n} q_n$.

The vector q_n captures all sources of shock confounding: $\mathbb{E}[g_n | \mathcal{I}_L] = q'_n \mu$, for all n and $\mathcal{I}_L = \{\{q_n\}_n, \{u_\ell, \epsilon_\ell, \eta_\ell, \{s_{\ell n}, \pi_{\ell n}\}_n, \epsilon_\ell\}_\ell\}$.

Assumption B5: The g_n are mutually independent given \mathcal{I}_L , $\max_n s_n \rightarrow 0$, and $\max_n \frac{s_n^2}{\sum_{n'} s_{n'}^2} \rightarrow 0$.

Assumption B6: $\mathbb{E}[|g_n|^{4+v} | \mathcal{I}_L]$ is uniformly bounded for some $v > 0$ and $\sum_\ell e_\ell \sum_n s_{\ell n}^2 \text{Var}[g_n | \mathcal{I}_L] \pi_{\ell n} \neq 0$ almost surely. The support of $\pi_{\ell n}$ is bounded, the fourth moments of $\epsilon_\ell, \eta_\ell, u_\ell, q_n$, and \tilde{g}_n exist and are uniformly bounded, $\sum_\ell e_\ell w_\ell w'_\ell \xrightarrow{P} \Omega_{ww}$ for positive definite Ω_{ww} , and $\sum_n s_n q_n q'_n \xrightarrow{P} \Omega_{qq}$ for positive definite Ω_{qq} . The control vector γ is consistently estimated by $\hat{\gamma} = (\sum_\ell e_\ell w_\ell w'_\ell)^{-1} \sum_\ell e_\ell w_\ell \epsilon_\ell$.

We note that Assumption B5 both strengthens our baseline Herfindahl index condition in Assumption 4 and implicitly treats the set of s_n as non-stochastic, following Assumption 2 of Adão et al. (2019). The regularity condition B6 includes the relevant conditions from Assumptions 4 and A.3 of Adão et al. (2019). These assumptions strengthen those of Proposition 4: Assumptions B3–B6 imply our Assumptions 3, 4, B1, and B2. Relative to Adão et al. (2019), we do not impose that $L > N$ or that the shares are non-collinear.

To establish the equivalence of IV coefficients in Proposition 5, note that when $\sum_n s_{\ell n} q_n$ is included in w_ℓ

$$\sum_n s_n q_n \bar{y}_n^\perp = \sum_\ell e_\ell y_\ell^\perp \left(\sum_n s_{\ell n} q_n \right) = 0 \quad (77)$$

and similarly for $\sum_n s_n q_n \bar{x}_n^\perp$. The s_n -weighted regression of \bar{y}_n^\perp and \bar{x}_n^\perp on q_n thus produces a coefficient vector that is numerically zero, implying the s_n -weighted and g_n -instrumented regression of \bar{y}_n^\perp on \bar{x}_n^\perp is unchanged with the addition of q_n controls. Proposition 1 shows that the IV coefficient from this regression is equivalent to the SSIV estimate $\hat{\beta}$.

To establish validity of the standard errors, note that the conventional heteroskedasticity-robust standard error from for the s_n -weighted shock-level IV regression of \bar{y}_n^\perp on \bar{x}_n^\perp and q_n , instrumented by g_n , is given by

$$\widehat{se}_{\text{equiv}} = \frac{\sqrt{\sum_n s_n^2 \hat{\varepsilon}_n^2 \hat{g}_n^2}}{|\sum_n s_n \bar{x}_n^\perp g_n|}, \quad (78)$$

where $\hat{\varepsilon}_n = \bar{y}_n^\perp - \hat{\beta} \bar{x}_n^\perp$ is the estimated shock-level regression residual (where we used the fact that the estimated coefficients on q_n in that regression are numerically zero) and $\hat{g}_n = g_n - \hat{\mu} q_n$, where $\hat{\mu} = (\sum_n s_n q_n q_n')^{-1} \sum_n s_n q_n g_n$, is the residual from a projection of the instrument in equation (10) on the control vector q_n . By Proposition 1, $\hat{\varepsilon}_n$ coincides with the share-weighted aggregate of the SSIV estimated residuals $\hat{\varepsilon}_\ell = y_\ell^\perp - \hat{\beta} x_\ell^\perp$:

$$\hat{\varepsilon}_n = \frac{\sum_\ell e_\ell s_{\ell n} y_\ell^\perp}{\sum_\ell e_\ell s_{\ell n}} - \hat{\beta} \cdot \frac{\sum_\ell e_\ell s_{\ell n} x_\ell^\perp}{\sum_\ell e_\ell s_{\ell n}} = \frac{\sum_\ell e_\ell s_{\ell n} \hat{\varepsilon}_\ell}{\sum_\ell e_\ell s_{\ell n}}. \quad (79)$$

The squared numerator of (78) can thus be rewritten

$$\sum_n s_n^2 \hat{\varepsilon}_n^2 \hat{g}_n^2 = \sum_n \left(\sum_\ell e_\ell s_{\ell n} \hat{\varepsilon}_\ell \right)^2 \hat{g}_n^2. \quad (80)$$

The expression in the denominator of (78) estimates the magnitude of the shock-level first-stage covariance, which matches the e_ℓ -weighted sample covariance of x_ℓ and z_ℓ :

$$\sum_n s_n \bar{x}_n^\perp g_n = \sum_n \left(\sum_\ell e_\ell s_{\ell n} x_\ell^\perp \right) g_n = \sum_\ell e_\ell x_\ell^\perp z_\ell. \quad (81)$$

Thus

$$\widehat{se}_{\text{equiv}} = \frac{\sqrt{\sum_n (\sum_\ell e_\ell s_{\ell n} \hat{\varepsilon}_\ell)^2 \hat{g}_n^2}}{|\sum_\ell e_\ell x_\ell^\perp z_\ell|}. \quad (82)$$

We now compare this expression to the standard error formula from Adão et al. (2019), incorporating the e_ℓ importance weights. Equation (39) in their paper yields

$$\widehat{se}_{\text{AKM}} = \frac{\sqrt{\sum_n (\sum_\ell e_\ell s_{\ell n} \hat{\varepsilon}_\ell)^2 \ddot{g}_n^2}}{|\sum_\ell e_\ell x_\ell^\perp z_\ell|}, \quad (83)$$

where \ddot{g}_n denotes the coefficients from regressing the residualized instrument z_ℓ^\perp on all shares $s_{\ell n}$, without a constant; note that to compute this requires $L > N$ and that the matrix of exposure shares $s_{\ell n}$ is full rank. The formulas for $\widehat{se}_{\text{equiv}}$ and $\widehat{se}_{\text{AKM}}$ therefore differ only in the construction of shock residuals, \hat{g}_n versus \ddot{g}_n .

We establish the general asymptotic equivalence of $\widehat{se}_{\text{equiv}}^2$ and $\widehat{se}_{\text{AKM}}^2$, and thus the asymptotic validity of $\widehat{se}_{\text{equiv}}$, by showing that both capture the conditional asymptotic variance of $\hat{\beta}$ given \mathcal{I}_L under Assumptions B3-B6. Both of the resulting confidence intervals are then asymptotically valid unconditionally, since if $Pr(\beta \in \widehat{CI} \mid \mathcal{I}_L) = \alpha$ then $Pr(\beta \in \widehat{CI}) = \mathbb{E} \left[\mathbb{E} \left[\mathbf{1}[\beta \in \widehat{CI} \mid \mathcal{I}_L] \right] \right] = \alpha$ by the law of iterated expectations. Under Assumptions B3-B6, Proposition A.1 of Adão et al. (2019) applies and shows that

$$\sqrt{r_L}(\hat{\beta} - \beta) \xrightarrow{d} \mathcal{N} \left(0, \frac{\mathcal{V}}{\pi^2} \right) \quad (84)$$

for $\mathcal{V} = \text{plim}_{L \rightarrow \infty} r_L \mathcal{V}_L$, where $r_L = 1 / (\sum_n s_n^2)$ and $\mathcal{V}_L = \sum_n (\sum_\ell e_\ell s_{\ell n} \varepsilon_\ell)^2 \text{Var}[g_n \mid \mathcal{I}_L]$, provided such a limit exists. To establish the asymptotic validity of $\widehat{se}_{\text{AKM}}$, i.e. that $r_L \left(\sum_n (\sum_\ell e_\ell s_{\ell n} \hat{\varepsilon}_\ell)^2 \hat{g}_n^2 - \mathcal{V}_L \right) \xrightarrow{p} 0$, Adão et al. (2019) further assume that $L \geq N$, the matrix of $s_{\ell n}$ is always full rank, and additional regularity conditions (see their Proposition 5). We establish $r_L \left(\sum_n (\sum_\ell e_\ell s_{\ell n} \hat{\varepsilon}_\ell)^2 \hat{g}_n^2 - \mathcal{V}_L \right) \xrightarrow{p} 0$, and thus $r_L \sum_n (\sum_\ell e_\ell s_{\ell n} \hat{\varepsilon}_\ell)^2 \hat{g}_n^2 \xrightarrow{p} \mathcal{V}$, without imposing those assumptions.

To start, we write $\tilde{g}_n = g_n - q_n' \mu$ and decompose

$$\begin{aligned} r_L \left(\sum_n \left(\sum_\ell e_\ell s_{\ell n} \hat{\varepsilon}_\ell \right)^2 \hat{g}_n^2 - \mathcal{V}_L \right) &= r_L \left(\sum_n \left(\sum_\ell e_\ell s_{\ell n} \varepsilon_\ell \right)^2 \tilde{g}_n^2 - \mathcal{V}_L \right) \\ &\quad + r_L \sum_n \left(\left(\sum_\ell e_\ell s_{\ell n} \hat{\varepsilon}_\ell \right)^2 - \left(\sum_\ell e_\ell s_{\ell n} \varepsilon_\ell \right)^2 \right) \tilde{g}_n^2 \\ &\quad + r_L \sum_n \left(\sum_\ell e_\ell s_{\ell n} \hat{\varepsilon}_\ell \right)^2 (\hat{g}_n^2 - \tilde{g}_n^2). \end{aligned} \quad (85)$$

Adão et al. (2019) show that the second term of this expression is $o_p(1)$ under our assumptions, using the fact (their Lemma A.3, again generalized to include importance weights) that for a triangular array

$\{A_{L1}, \dots, A_{LL}, B_{L1}, \dots, B_{LL}, C_{L1}, \dots, C_{LN}\}_{L=1}^{\infty}$ with $\mathbb{E}[A_{L\ell}^4 | \{\{s_{\ell'n}\}_n, e_{\ell'}\}_{\ell'}]$, $\mathbb{E}[B_{L\ell}^4 | \{\{s_{\ell'n}\}_n, e_{\ell'}\}_{\ell'}]$, and $\mathbb{E}[C_{Ln}^2 | \{\{s_{\ell'n'}\}_n, e_{\ell}\}_{\ell}]$ uniformly bounded,

$$r_L \sum_{\ell} \sum_{\ell'} \sum_n e_{\ell} e_{\ell'} s_{\ell n} s_{\ell' n} A_{L\ell} B_{L\ell'} C_{Ln} = O_p(1). \quad (86)$$

Here with $D_{\ell} = (z_{\ell}, w'_{\ell})'$, $\theta = (\beta, \gamma)'$, and $\hat{\theta} = (\hat{\beta}, \hat{\gamma})'$ we can write

$$\begin{aligned} \left(\sum_{\ell} e_{\ell} s_{\ell n} \hat{\varepsilon}_{\ell} \right)^2 &= \left(\sum_{\ell} e_{\ell} s_{\ell n} \varepsilon_{\ell} \right)^2 + 2 \sum_{\ell} \sum_{\ell'} e_{\ell} e_{\ell'} s_{\ell n} s_{\ell' n} D'_{\ell} (\theta - \hat{\theta}) \varepsilon_{\ell'} \\ &\quad + \sum_{\ell} \sum_{\ell'} e_{\ell} e_{\ell'} D'_{\ell} (\theta - \hat{\theta}) D'_{\ell'} (\theta - \hat{\theta}), \end{aligned} \quad (87)$$

and both D_{ℓ} and ε_{ℓ} have bounded fourth moments by the assumption of bounded fourth moments of ε_{ℓ} , η_{ℓ} , u_{ℓ} , and q_n , and g_n in Assumption B6. Thus by the lemma

$$\begin{aligned} r_L \sum_n \left(\left(\sum_{\ell} e_{\ell} s_{\ell n} \hat{\varepsilon}_{\ell} \right)^2 - \left(\sum_{\ell} e_{\ell} s_{\ell n} \varepsilon_{\ell} \right)^2 \right) \tilde{g}_n^2 &= 2 (\theta - \hat{\theta})' \left(r_L \sum_{\ell} \sum_{\ell'} \sum_n e_{\ell} e_{\ell'} s_{\ell n} s_{\ell' n} \tilde{g}_n^2 D_{\ell} \varepsilon_{\ell'} \right) \\ &\quad + (\theta - \hat{\theta})' \left(r_L \sum_{\ell} \sum_{\ell'} \sum_n e_{\ell} e_{\ell'} s_{\ell n} s_{\ell' n} \tilde{g}_n^2 D_{\ell} D'_{\ell'} \right) (\theta - \hat{\theta}) \\ &= (\theta - \hat{\theta})' O_p(1) + (\theta - \hat{\theta})' O_p(1) (\theta - \hat{\theta}), \end{aligned} \quad (88)$$

which is $o_p(1)$ by the consistency of $\hat{\theta}$ (implied by Assumptions B3-B6). Adão et al. (2019) further show the first term of equation (85) is $o_p(1)$, without using the additional regularity conditions of their Proposition 5.

It thus remains for us to show the third term of (85) is also $o_p(1)$. Note that

$$\hat{g}_n^2 = (g_n - q'_n \hat{\mu})^2 = \tilde{g}_n^2 + (q'_n (\hat{\mu} - \mu))^2 - 2\tilde{g}_n q'_n (\hat{\mu} - \mu), \quad (89)$$

so that

$$\begin{aligned} &r_L \sum_n \left(\sum_{\ell} e_{\ell} s_{\ell n} \hat{\varepsilon}_{\ell} \right)^2 (\hat{g}_n^2 - \tilde{g}_n^2) \\ &= r_L \sum_n \left(\sum_{\ell} e_{\ell} s_{\ell n} \hat{\varepsilon}_{\ell} \right)^2 (q'_n (\hat{\mu} - \mu) - 2\tilde{g}_n) q'_n (\hat{\mu} - \mu) \\ &= r_L \sum_n \left(\sum_{\ell} e_{\ell} s_{\ell n} \varepsilon_{\ell} \right)^2 (q'_n (\hat{\mu} - \mu) - 2\tilde{g}_n) q'_n (\hat{\mu} - \mu) \\ &\quad + r_L \sum_n \left(\left(\sum_{\ell} e_{\ell} s_{\ell n} \hat{\varepsilon}_{\ell} \right)^2 - \left(\sum_{\ell} e_{\ell} s_{\ell n} \varepsilon_{\ell} \right)^2 \right) (q'_n (\hat{\mu} - \mu) - 2\tilde{g}_n) q'_n (\hat{\mu} - \mu). \end{aligned} \quad (90)$$

Using the previous lemma, the first term of this expression is $O_p(1) (\hat{\mu} - \mu)$ since ε_ℓ , q_n , and \tilde{g}_n have bounded fourth moments under Assumption B6. The second term is similarly $O_p(1) (\hat{\mu} - \mu)$ by the lemma and the decomposition used in equation (88). Noting that $\hat{\mu} - \mu = (\sum_n s_n q_n q_n')^{-1} \sum_n s_n q_n \tilde{g}_n \xrightarrow{p} 0$ under the assumptions completes the proof.

The Case of No Controls We show that when there are no controls besides a constant, i.e. $w_\ell = g_n = 1$, the standard errors are numerically the same. To prove this, it suffices to show that $\hat{g}_n = \ddot{g}_n$. Absent controls, $\hat{g}_n = g_n - \sum_n s_n g_n$ is the s_n -weighted demeaned shock. The \ddot{g}_n are obtained as the projection coefficient of $z_\ell^\perp = z_\ell - \sum_\ell e_\ell z_\ell$ on the N shares. Note that

$$\sum_\ell e_\ell z_\ell = \sum_\ell e_\ell \sum_n s_{\ell n} g_n = \sum_n s_n g_n, \quad (91)$$

so that, with $\sum_n s_{\ell n} = 1$,

$$z_\ell - \sum_\ell e_\ell z_\ell = \sum_n s_{\ell n} g_n - \sum_n s_n g_n = \sum_\ell s_{\ell n} \hat{g}_n. \quad (92)$$

This means that the projection in Adão et al. (2019) has exact fit and produces $\ddot{g}_n = \hat{g}_n$.

Relaxing Assumption B4 We now show that the standard errors from our equivalent regression in Proposition 5 are asymptotically conservative under a weaker assumption on the structure of controls than Assumption B4:

Assumption B4': There exists a K -dimensional vector p_n , with uniformly bounded fourth moments, such that $w_\ell = \sum_n s_{\ell n} p_n + u_\ell$ for some K -dimensional vector u_ℓ and $\mathbb{E}[g_n | \mathcal{I}_L] = p_n' \mu$ for all n and for $\mathcal{I}_L = \{\{p_n\}_n, \{u_\ell, \varepsilon_\ell, \eta_\ell, \{s_{\ell n}, \pi_{\ell n}\}_n, e_\ell\}_\ell\}$.

This assumption requires that the controls w_ℓ can be represented as noisy versions of some latent shift-share confounding variables $\sum_n s_{\ell n} p_n$. Since the variance of u_ℓ is unrestricted, this assumption relaxes not only our Assumption B4 but also the assumption of approximate shift-share controls in Adão et al. (2019).

Specifically, we show that under Assumptions B3, B4', B5, and B6 the shock-level regression from Proposition 5 that controls for a subvector of confounders $q_n \subseteq p_n$ yields asymptotically conservative standard errors. Consider

$$\hat{\Delta}_L = r_L \sum_n \left(\sum_\ell e_\ell s_{\ell n} \hat{\varepsilon}_\ell \right)^2 \hat{g}_n^2 - r_L \mathcal{V}_L \quad (93)$$

with \hat{g}_n still denoting the s_n -weighted projection of g_n on q_n (only), and $\mathcal{V}_L = \sum_n (\sum_\ell e_\ell s_{\ell n} \varepsilon_\ell)^2 \text{Var}[g_n | \mathcal{I}_L]$

where \mathcal{I}_L is the expanded set from Assumption B4'. Write

$$\begin{aligned}\hat{g}_n &= g_n - p'_n \hat{\mu} \\ &= \tilde{g}_n + p'_n (\mu - \hat{\mu}),\end{aligned}\tag{94}$$

where the non-zero elements of $\hat{\mu}$ correspond to the q_n subvector. We show that

$$\hat{\Delta}_L - \Delta_L \xrightarrow{p} 0\tag{95}$$

for the non-negative

$$\Delta_L = r_L \sum_n \left(\sum_\ell e_\ell s_{\ell n} \hat{\varepsilon}_\ell \right)^2 (p'_n (\mu - \hat{\mu}))^2,\tag{96}$$

when $\hat{\mu} = O_p(1)$.⁶⁶

First note by equation (94) that, for $\tilde{g}_n = g_n - p'_n \mu$,

$$\hat{g}_n^2 = \tilde{g}_n^2 + 2\tilde{g}_n p'_n (\mu - \hat{\mu}) + (p'_n (\mu - \hat{\mu}))^2.$$

Thus

$$\begin{aligned}\hat{\Delta}_L - \Delta_L &= r_L \sum_n \left(\sum_\ell e_\ell s_{\ell n} \hat{\varepsilon}_\ell \right)^2 \tilde{g}_n^2 - r_L \mathcal{V}_L \\ &\quad + 2r_L \sum_n \left(\sum_\ell e_\ell s_{\ell n} \hat{\varepsilon}_\ell \right)^2 \tilde{g}_n p'_n (\mu - \hat{\mu}).\end{aligned}\tag{97}$$

We showed that the first term is $o_p(1)$ in the proof of Proposition 5. It remains to show the second term is also $o_p(1)$. To see this, write

$$r_L \sum_n \left(\sum_\ell e_\ell s_{\ell n} \hat{\varepsilon}_\ell \right)^2 \tilde{g}_n p_n = r_L \sum_n \left(\sum_\ell e_\ell s_{\ell n} \varepsilon_\ell \right)^2 \tilde{g}_n p_n\tag{98}$$

$$+ r_L \sum_n \left(\left(\sum_\ell e_\ell s_{\ell n} \hat{\varepsilon}_\ell \right)^2 - \left(\sum_\ell e_\ell s_{\ell n} \varepsilon_\ell \right)^2 \right) \tilde{g}_n p_n.\tag{99}$$

We have

$$\mathbb{E} \left[r_L \sum_n \left(\sum_\ell e_\ell s_{\ell n} \hat{\varepsilon}_\ell \right)^2 \tilde{g}_n p_n \right] = \mathbb{E} \left[r_L \sum_n \left(\sum_\ell e_\ell s_{\ell n} \varepsilon_\ell \right)^2 \mathbb{E} [\tilde{g}_n | \mathcal{I}_L] p_n \right] = 0\tag{100}$$

⁶⁶We note this is a weaker condition than convergence of the incorrect shock-level projection (i.e. that $\hat{\mu} = \bar{\mu} + o_p(1)$ for some $\bar{\mu}$).

since $\mathbb{E}[\tilde{g}_n | \mathcal{I}_L]$. Furthermore,

$$\text{Var} \left[r_L \sum_n \left(\sum_\ell e_\ell s_{\ell n} \varepsilon_\ell \right)^2 \tilde{g}_n p_n \right] = r_L^2 \sum_n \mathbb{E} \left[\left(\sum_\ell e_\ell s_{\ell n} \varepsilon_\ell \right)^4 \text{Var}[\tilde{g}_n | \mathcal{I}_L] p_n p'_n \right] \rightarrow 0, \quad (101)$$

implying the first term of (98) is $o_p(1)$. The second term of this expression can also be shown to be $o_p(1)$ by applying the lemma from Adão et al. (2019) and the representation used in equation (88). Thus

$$2r_L \sum_n \left(\sum_\ell e_\ell s_{\ell n} \hat{\varepsilon}_\ell \right)^2 \tilde{g}_n p'_n (\mu - \hat{\mu}) = o_p(1)' O_p(1), \quad (102)$$

completing the proof.

Comparison of Standard Errors under Assumption B4 The characterization of the standard errors in equations (82)–(83) also offers insights into how these standard errors may differ in presence of controls, when both standard error calculations are asymptotically valid. We argue that under the conditions of Proposition 5, our standard errors are likely smaller in finite samples. More precisely, we show that the homoskedastic version of (82) is smaller than the homoskedastic version of (83). This is suggestive of the comparison under heteroskedasticity, but is not a proof.

To see this, consider versions of the two standard error formulas obtained under shock homoskedasticity (i.e. $\text{Var}[g_n | \mathcal{I}_L] = \sigma_g^2$):

$$\hat{s}_{\text{equiv}}^{\text{homo}} = \frac{\sqrt{(\sum_n s_n^2 \hat{\varepsilon}_n^2) (\sum_n s_n \hat{g}_n^2)}}{|\sum_n s_n \bar{x}_n^\perp g_n|} \quad (103)$$

$$\hat{s}_{\text{AKM}}^{\text{homo}} = \frac{\sqrt{(\sum_n s_n^2 \hat{\varepsilon}_n^2) (\sum_n s_n \check{g}_n^2)}}{|\sum_\ell e_\ell x_\ell^\perp z_\ell|}, \quad (104)$$

which differ by a factor of $\sqrt{\sum_n s_n \hat{g}_n^2 / \sum_n s_n \check{g}_n^2}$.

When the SSIV controls have an exact shift-share structure, $w_\ell = \sum_n s_{\ell n} q_n$, the share projection producing \check{g}_n has exact fit such that one can represent $\check{g}_n = g_n - q'_n \hat{\mu}_{\text{AKM}}$ for some $\hat{\mu}_{\text{AKM}}$. In this case the s_n -weighted sum of squares of shock residuals is lower in our equivalent regression by construction of $\hat{\mu}$: $\sum_n s_n \hat{g}_n^2 \leq \sum_n s_n \check{g}_n^2$ (with strict inequality when $\hat{\mu}_{\text{AKM}} \neq \hat{\mu}$). Similarly, when w_ℓ instead contains controls that are included for efficiency only and are independent of the shocks, projection of z_ℓ on the shares produces a noisy estimate of $g_n - \sum_n s_n g_n$, which again has a higher weighted sum of squares.

Null-Imposed Inference Procedure Finally, our shock-level equivalence provides a convenient implementation for the alternative inference procedure that may have superior finite-sample performance. Adão et al. (2019) show how standard errors that impose a given null hypothesis $\beta = \beta_0$ in

estimating the residual ε_ℓ can generate confidence intervals with better coverage in situations with few shocks (and a similar argument can be made in the case of shocks with a heavy-tailed distribution).⁶⁷ Building on Proposition 5, such confidence intervals can be constructed in the same way as in any regular shock-level IV regression. To test $\beta = \beta_0$, one regresses $\bar{y}_n^\perp - \beta_0 \bar{x}_n^\perp$ on the shocks g_n (weighting by s_n and including any relevant shock-level controls q_n) and uses a null-imposed residual variance estimate. This procedure corresponds to the standard shock-level Lagrange multiplier test for $\beta = \beta_0$ that can be implemented by standard statistical software.⁶⁸ The confidence interval for β is constructed by collecting all candidate β_0 that are not rejected.

B.3 Proposition A1

We consider each expectation in equation (13) in turn. For each n , write

$$\kappa_n(g_{-n}, \varepsilon_\ell, \eta_\ell) = \lim_{g_n \rightarrow -\infty} y(x_1([g_n; g_{-n}], \eta_{\ell 1}), \dots, x_R([g_n; g_{-n}], \eta_{\ell R}), \varepsilon_\ell) \quad (105)$$

such that

$$\begin{aligned} s_{\ell n} e_\ell g_n y_\ell &= s_{\ell n} e_\ell g_n \kappa_n(g_{-n}, \varepsilon_\ell, \eta_\ell) \\ &+ s_{\ell n} e_\ell g_n \int_{-\infty}^{g_n} \frac{\partial}{\partial g_n} y(x_1([\gamma; g_{-n}], \eta_{\ell 1}), \dots, x_R([\gamma; g_{-n}], \eta_{\ell R}), \varepsilon_\ell) d\gamma. \end{aligned} \quad (106)$$

By as-good-as-random shock assignment, the expectation of the first term is

$$\mathbb{E} [s_{\ell n} e_\ell g_n \kappa_n(g_{-n}, \varepsilon_\ell, \eta_\ell)] = \mathbb{E} [s_{\ell n} e_\ell \mathbb{E} [g_n \mid s, e, g_{-n}, \varepsilon, \eta_\ell] \kappa_n(g_{-n}, \varepsilon_\ell, \eta_\ell)] = 0, \quad (107)$$

while the expectation of the second is

$$\begin{aligned} &\mathbb{E} \left[s_{\ell n} e_\ell g_n \int_{-\infty}^{g_n} \frac{\partial}{\partial g_n} y(x_1([\gamma; g_{-n}], \eta_{\ell 1}), \dots, x_R([\gamma; g_{-n}], \eta_{\ell R}), \varepsilon_\ell) d\gamma \right] \\ &= \mathbb{E} \left[s_{\ell n} e_\ell \int_{-\infty}^{\infty} \int_{-\infty}^{g_n} g_n \frac{\partial}{\partial g_n} y(x_1([\gamma; g_{-n}], \eta_{\ell 1}), \dots, x_R([\gamma; g_{-n}], \eta_{\ell R}), \varepsilon_\ell) d\gamma dF_n(g_n \mid \mathcal{I}) \right] \\ &= \mathbb{E} \left[s_{\ell n} e_\ell \int_{-\infty}^{\infty} \frac{\partial}{\partial g_n} y(x_1([\gamma; g_{-n}], \eta_{\ell 1}), \dots, x_R([\gamma; g_{-n}], \eta_{\ell R}), \varepsilon_\ell) \int_{\gamma}^{\infty} g_n dF_n(g_n \mid \mathcal{I}) d\gamma \right] \end{aligned} \quad (108)$$

⁶⁷As explained by Adão et al. (2019), the problem that this “AKM0” confidence interval addresses generalizes the standard finite-sample bias of cluster-robust standard errors with few clusters (Cameron and Miller 2015). With few or heavy-tailed shocks, estimates of the residual variance will tend to be biased downwards, leading to undercoverage of confidence intervals based on standard errors that do not impose the null.

⁶⁸For example in Stata one can use the *ivreg2* overidentification test statistic from regressing $\bar{y}_n^\perp - \beta_0 \bar{x}_n^\perp$ on q_n with no endogenous variables and with g_n specified as the instrument (again with s_n weights).

where $F_n(\cdot | \mathcal{I})$ denotes the conditional distribution of g_n . Thus

$$\begin{aligned}\mathbb{E}[s_{\ell n} e_{\ell} g_n y_{\ell}] &= \mathbb{E} \left[s_{\ell n} e_{\ell} \int_{-\infty}^{\infty} \frac{\partial}{\partial g_n} y(x_1([\gamma; g_{-n}], \eta_{\ell}), \dots, x_R([\gamma; g_{-n}], \eta_{\ell}), \varepsilon_{\ell}) \mu_n(\gamma | \mathcal{I}) d\gamma \right] \\ &= \sum_r \mathbb{E} \left[\int_{-\infty}^{\infty} s_{\ell n} e_{\ell} \alpha_{\ell r} \pi_{\ell r n}([\gamma; g_{-n}]) \mu_n(\gamma | \mathcal{I}) \tilde{\beta}_{\ell r n}(\gamma) d\gamma \right]\end{aligned}\quad (109)$$

where

$$\begin{aligned}\mu_n(\gamma | \mathcal{I}) &\equiv \int_{\gamma}^{\infty} g_n dF_n(g_n | \mathcal{I}). \\ &= (\mathbb{E}[g_n | g_n \geq \gamma, \mathcal{I}] - \mathbb{E}[g_n | g_n < \gamma, \mathcal{I}]) Pr(g_n \geq \gamma | \mathcal{I}) (1 - Pr(g_n \geq \gamma | \mathcal{I})) \geq 0 \text{ a.s.}\end{aligned}\quad (110)$$

Similarly

$$\mathbb{E}[s_{\ell n} e_{\ell} g_n x_{\ell}] = \sum_r \mathbb{E} \left[\int_{-\infty}^{\infty} s_{\ell n} e_{\ell} \alpha_{\ell r} \pi_{\ell r n}([\gamma; g_{-n}]) \mu_n(\gamma | \mathcal{I}) d\gamma \right]. \quad (111)$$

Combining equations (109) and (111) completes the proof, with

$$\omega_{\ell r n}(\gamma) = s_{\ell n} e_{\ell} \alpha_{\ell r} \mu_n(\gamma | \mathcal{I}) \pi_{\ell r n}([\gamma; g_{-n}]) \geq 0 \text{ a.s.} \quad (112)$$

B.4 Proposition A2

By definition of $\bar{\varepsilon}_n$,

$$\begin{aligned}\bar{\varepsilon}_n &= \frac{\sum_{\ell} e_{\ell} s_{\ell n} (\sum_{n'} s_{\ell n'} \nu_{n'} + \check{\varepsilon}_{\ell})}{\sum_{\ell} e_{\ell} s_{\ell n}} \\ &\equiv \sum_{n'} \alpha_{nn'} \nu_{n'} + \check{\bar{\varepsilon}}_n,\end{aligned}\quad (113)$$

for $\alpha_{nn'} = \frac{\sum_{\ell} e_{\ell} s_{\ell n} s_{\ell n'}}{\sum_{\ell} e_{\ell} s_{\ell n}}$ and $\check{\bar{\varepsilon}}_n = \frac{\sum_{\ell} e_{\ell} s_{\ell n} \check{\varepsilon}_{\ell}}{\sum_{\ell} e_{\ell} s_{\ell n}}$. Therefore,

$$\begin{aligned}\text{Var}[\bar{\varepsilon}_n] &= \sum_{n'} \sigma_{n'}^2 \alpha_{nn'}^2 + \text{Var}[\check{\bar{\varepsilon}}_n] \\ &\geq \sigma_{\nu}^2 \alpha_{nn}^2,\end{aligned}\quad (114)$$

and

$$\max_n \text{Var}[\bar{\varepsilon}_n] \geq \sigma_{\nu}^2 \max_n \alpha_{nn}^2. \quad (115)$$

To establish a lower bound on this quantity, observe that the s_n -weighted average of α_{nn} satisfies:

$$\begin{aligned}\sum_n s_n \alpha_{nn} &= \sum_n s_n \frac{\sum_\ell e_\ell s_{\ell n}^2}{s_n} \\ &= H_L.\end{aligned}\tag{116}$$

Since $\sum_n s_n = 1$, it follows that $\max_n \alpha_{nn} \geq H_L$ and therefore $\max_n \text{Var}[\bar{\varepsilon}_n] \geq \sigma_\nu^2 H_L^2$. Since $H_L \rightarrow \bar{H} > 0$, we conclude that, for sufficiently large L , $\max_n \text{Var}[\bar{\varepsilon}_n]$ is bounded from below by any positive $\delta < \sigma_\nu^2 \bar{H}^2$.

B.5 Proposition A3

To prove (20), we aggregate (19) across industries within a region using $E_{\ell n}$ weights:

$$y_\ell = (\beta_0 - \beta_1) x_\ell + \varepsilon_\ell,\tag{117}$$

where $\varepsilon_\ell = \sum_n s_{\ell n} \varepsilon_{\ell n}$. The shift-share instrument z_ℓ is relevant because

$$\begin{aligned}\mathbb{E}\left[\sum_\ell e_\ell x_\ell z_\ell\right] &= \sum_\ell e_\ell \mathbb{E}\left[\sum_n s_{\ell n} (\bar{\pi} g_n + \eta_{\ell n}) \cdot \sum_{n'} s_{\ell n'} g_{n'}\right] \\ &= \sum_{\ell, n} e_\ell s_{\ell n}^2 \bar{\pi} \sigma_g^2 \\ &\geq \bar{H}_L \bar{\pi} \sigma_g^2,\end{aligned}\tag{118}$$

while exclusion holds because

$$\begin{aligned}\mathbb{E}\left[\sum_\ell e_\ell z_\ell \varepsilon_\ell\right] &= \sum_\ell e_\ell \mathbb{E}\left[\sum_n s_{\ell n} \varepsilon_{\ell n} \cdot \sum_{n'} s_{\ell n'} g_{n'}\right] \\ &= 0.\end{aligned}\tag{119}$$

Thus by an appropriate law of large numbers, $\hat{\beta} = \beta_0 - \beta_1 + o_p(1)$.

To study $\hat{\beta}_{\text{ind}}$, we aggregate (19) across regions (again with $E_{\ell n}$ weights):

$$y_n = \beta_0 x_n - \beta_1 \sum_\ell \omega_{\ell n} \sum_{n'} s_{\ell n'} x_{\ell n'} + \varepsilon_n,\tag{120}$$

for $\varepsilon_n = \sum_\ell \omega_{\ell n} \varepsilon_{\ell n}$. The resulting IV estimate yields

$$\begin{aligned}\hat{\beta}_{\text{ind}} - \beta_0 &= \frac{\sum_n s_n y_n g_n}{\sum_n s_n x_n g_n} - \beta_0 \\ &= \frac{\sum_n s_n (-\beta_1 \sum_\ell \omega_{\ell n} \sum_{n'} s_{\ell n'} x_{\ell n'} + \varepsilon_n) g_n}{\sum_n s_n x_n g_n}.\end{aligned}\tag{121}$$

The expected denominator of $\hat{\beta}_{\text{ind}}$ is non-zero:

$$\begin{aligned}
\mathbb{E} \left[\sum_n s_n x_n g_n \right] &= \sum_n s_n \mathbb{E} \left[\sum_\ell \omega_{\ell n} (\bar{\pi} g_n + \eta_{\ell n}) g_n \right] \\
&= \sum_n s_n \omega_{\ell n} \bar{\pi} \sigma^2 \\
&= \sum_n \frac{E_n}{E} \cdot \frac{E_{\ell n}}{E} \bar{\pi} \sigma^2 \\
&= \bar{\pi} \sigma^2,
\end{aligned} \tag{122}$$

while the expected numerator is

$$\begin{aligned}
\mathbb{E} \left[\sum_n s_n \left(-\beta_1 \sum_\ell \omega_{\ell n} \sum_{n'} s_{\ell n'} x_{\ell n'} + \varepsilon_n \right) g_n \right] &= -\beta_1 \sum_{n,\ell} s_n \omega_{\ell n} s_{\ell n} \bar{\pi} \sigma^2 \\
&= -\beta_1 H_L \bar{\pi} \sigma^2,
\end{aligned} \tag{123}$$

where the last equality follows because

$$\begin{aligned}
\sum_{n,\ell} s_n \omega_{\ell n} s_{\ell n} &= \sum_{n,\ell} \frac{E_n}{E} \frac{E_{\ell n}}{E_n} \frac{E_{\ell n}}{E_\ell} \\
&= \sum_{n,\ell} \frac{E_\ell}{E} \left(\frac{E_{\ell n}}{E_\ell} \right)^2 \\
&= \sum_{n,\ell} e_\ell s_{\ell n}^2 \\
&= H_L.
\end{aligned} \tag{124}$$

Thus by an appropriate law of large numbers,

$$\hat{\beta}_{\text{ind}} = \beta_0 - \beta_1 H_L + o_p(1). \tag{125}$$

B.6 Proposition A4

By appropriate laws of large numbers,

$$\begin{aligned}
\hat{\beta} &= \frac{\mathbb{E} [\sum_\ell E_\ell (\sum_n s_{\ell n} y_{\ell n}) (\sum_{n'} s_{\ell n'} g_{n'})]}{\mathbb{E} [\sum_\ell E_\ell (\sum_n s_{\ell n} x_{\ell n}) (\sum_{n'} s_{\ell n'} g_{n'})]} + o_p(1) \\
&= \frac{\sum_{\ell,n} E_\ell s_{\ell n}^2 \pi_{\ell n} \sigma_n^2 \beta_{\ell n}}{\sum_{\ell,n} E_\ell s_{\ell n}^2 \pi_{\ell n} \sigma_n^2} + o_p(1) \\
&= \frac{\sum_{\ell,n} E_{\ell n} s_{\ell n} \pi_{\ell n} \sigma_n^2 \beta_{\ell n}}{\sum_{\ell,n} E_{\ell n} s_{\ell n} \pi_{\ell n} \sigma_n^2} + o_p(1)
\end{aligned} \tag{126}$$

while

$$\begin{aligned}
\hat{\beta}_{\text{ind}} &= \frac{\sum_n E_n y_n g_n}{\sum_n E_n x_n g_n} \\
&= \frac{\mathbb{E} [\sum_n E_n (\sum_\ell \omega_{\ell n} y_{\ell n}) g_n]}{\mathbb{E} [\sum_n E_n (\sum_\ell \omega_{\ell n} x_{\ell n}) g_n]} + o_p(1) \\
&= \frac{\sum_{\ell, n} E_n \omega_{\ell n} \pi_{\ell n} \sigma_n^2 \beta_{\ell n}}{\sum_{\ell, n} E_n \omega_{\ell n} \pi_{\ell n} \sigma_n^2} + o_p(1) \\
&= \frac{\sum_{\ell, n} E_{\ell n} \pi_{\ell n} \sigma_n^2 \beta_{\ell n}}{\sum_{\ell, n} E_{\ell n} \pi_{\ell n} \sigma_n^2} + o_p(1). \tag{127}
\end{aligned}$$

B.7 Proposition A5

We prove each part of this proposition in turn.

1. Expanding the moment condition yields:

$$\begin{aligned}
\mathbb{E} \left[\sum_\ell e_{\ell} \varepsilon_{\ell} \psi_{\ell, LOO} \right] &= \sum_\ell \mathbb{E} \left[e_{\ell} \varepsilon_{\ell} \sum_n s_{\ell n} \frac{\sum_{\ell' \neq \ell} \omega_{\ell' n} \psi_{\ell' n}}{\sum_{\ell' \neq \ell} \omega_{\ell' n}} \right] \\
&= \sum_\ell e_{\ell} \sum_n s_{\ell n} \frac{\sum_{\ell' \neq \ell} \omega_{\ell' n} \mathbb{E} [\varepsilon_{\ell} \psi_{\ell' n}]}{\sum_{\ell' \neq \ell} \omega_{\ell' n}} \\
&= 0. \tag{128}
\end{aligned}$$

2. The assumption of part (1) is satisfied here, so $\mathbb{E} [\sum_\ell e_{\ell} \varepsilon_{\ell} \psi_{\ell, LOO}] = 0$. We now establish that $\mathbb{E} \left[(\sum_\ell e_{\ell} \varepsilon_{\ell} \psi_{\ell, LOO})^2 \right] \rightarrow 0$, which implies $\sum_\ell e_{\ell} \varepsilon_{\ell} \psi_{\ell, LOO} \xrightarrow{p} 0$ and thus consistency of the LOO SSIV estimator provided it has a first stage:

$$\begin{aligned}
\mathbb{E} \left[\left(\sum_\ell e_{\ell} \varepsilon_{\ell} \psi_{\ell, LOO} \right)^2 \right] &= \sum_{\substack{\ell_1, \ell_2, n_1, n_2, \\ \ell'_1 \neq \ell_1, \ell'_2 \neq \ell_2}} e_{\ell_1} e_{\ell_2} s_{\ell_1 n_1} s_{\ell_2 n_2} \frac{\omega_{\ell'_1 n_1}}{\sum_{\ell \neq \ell_1} \omega_{\ell n_1}} \frac{\omega_{\ell'_2 n_2}}{\sum_{\ell \neq \ell_2} \omega_{\ell n_2}} \mathbb{E} [\varepsilon_{\ell_1} \varepsilon_{\ell_2} \psi_{\ell'_1 n_1} \psi_{\ell'_2 n_2}] \\
&\leq \sum_{\substack{(\ell_1, \ell_2, \ell'_1, \ell'_2) \in \mathcal{J}, \\ n_1, n_2}} e_{\ell_1} e_{\ell_2} s_{\ell_1 n_1} s_{\ell_2 n_2} \frac{\omega_{\ell'_1 n_1}}{\sum_{\ell \neq \ell_1} \omega_{\ell n_1}} \frac{\omega_{\ell'_2 n_2}}{\sum_{\ell \neq \ell_2} \omega_{\ell n_2}} \cdot B \rightarrow 0. \tag{129}
\end{aligned}$$

Here the second line used the first regularity condition, which implies that $\mathbb{E} [\varepsilon_{\ell_1} \varepsilon_{\ell_2} \psi_{\ell'_1 n_1} \psi_{\ell'_2 n_2}] = 0$ whenever there is at least one index among $\{\ell_1, \ell_2, \ell'_1, \ell'_2\}$ which is not equal to any of the others, i.e. for all $(\ell_1, \ell_2, \ell'_1, \ell'_2) \notin \mathcal{J}$.

3. We show that under the given assumptions on $s_{\ell n}$, e_{ℓ} , and $\omega_{\ell n}$, the expression in (32) is bounded

by $4N/L$:

$$\begin{aligned}
& \sum_{\substack{(\ell_1, \ell_2, \ell'_1, \ell'_2) \in \mathcal{J}, \\ n_1, n_2}} e_{\ell_1} e_{\ell_2} s_{\ell_1 n_1} s_{\ell_2 n_2} \frac{\omega_{\ell'_1 n_1}}{\sum_{\ell \neq \ell_1} \omega_{\ell n_1}} \frac{\omega_{\ell'_2 n_2}}{\sum_{\ell \neq \ell_2} \omega_{\ell n_2}} \\
&= \sum_{(\ell_1, \ell_2, \ell'_1, \ell'_2) \in \mathcal{J}} \frac{1}{L^2} \frac{\omega_{\ell'_1 n(\ell_1)}}{\sum_{\ell \neq \ell_1} \omega_{\ell n(\ell_1)}} \frac{\omega_{\ell'_2 n(\ell_2)}}{\sum_{\ell \neq \ell_2} \omega_{\ell n(\ell_2)}} \\
&= \frac{1}{L^2} \sum_{\substack{(\ell_1, \ell_2, \ell'_1, \ell'_2) \in \mathcal{J} \\ n(\ell'_1) = n(\ell_1), \\ n(\ell'_2) = n(\ell_2)}} \frac{1}{L_{n(\ell_1)} - 1} \frac{1}{L_{n(\ell_2)} - 1} \\
&= \frac{1}{L^2} \sum_n \frac{2L_n (L_n - 1)}{(L_n - 1)^2} \leq 4 \frac{N}{L}. \tag{130}
\end{aligned}$$

Here the second line plugs in the expressions for $s_{\ell n}$ and e_{ℓ} , and the third line plugs in $\omega_{\ell n}$. The last line uses the fact that any tuple $(\ell_1, \ell_2, \ell'_1, \ell'_2) \in \mathcal{J}$ such that $n(\ell'_1) = n(\ell_1)$ and $n(\ell'_2) = n(\ell_2)$ has all four elements exposed to the same shock n . Moreover, it is easily verified that all of these tuples have a structure $(\ell_A, \ell_B, \ell_A, \ell_B)$ or $(\ell_A, \ell_B, \ell_B, \ell_A)$ for any $\ell_A \neq \ell_B$ exposed to the same shock. Therefore, there are $2L_n (L_n - 1)$ of them for each n . Finally, $\frac{L_n}{L_n - 1} \leq 2$ as $L_n \geq 2$.

B.8 Proposition A6

National industry employment satisfies $E_n = \sum_{\ell} E_{\ell n}$; log-linearizing this immediately implies (41).

To solve for $g_{\ell n}$, log-linearize (38), (39), and (40):

$$\hat{E}_{\ell} = \phi \hat{W}_{\ell} + \varepsilon_{\ell}, \tag{131}$$

$$g_{\ell n} = g_n^* + \hat{\xi}_{\ell n} - \sigma \hat{W}_{\ell}, \tag{132}$$

$$\hat{E}_{\ell} = \sum_n s_{\ell n} g_{\ell n}. \tag{133}$$

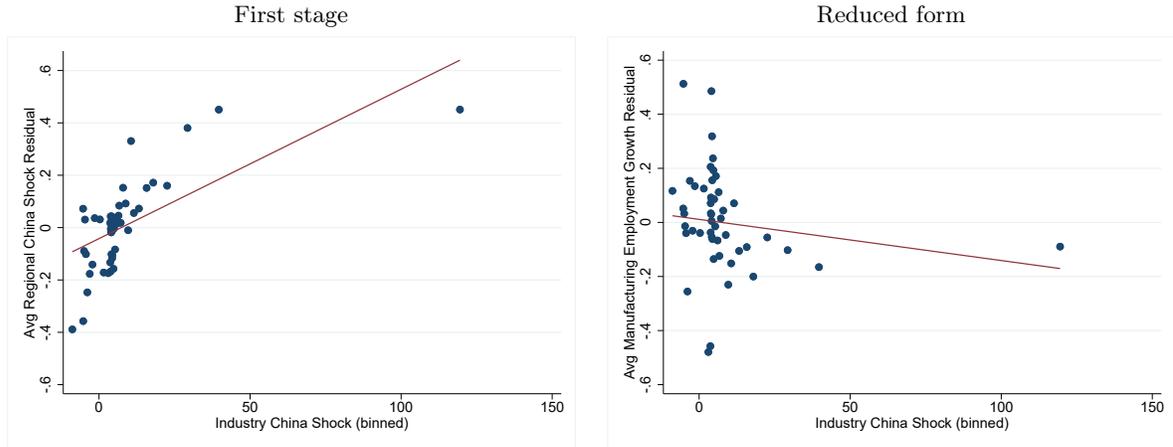
Solving this system of equations yields

$$\hat{W}_{\ell} = \frac{1}{\sigma + \phi} \left(\sum_n s_{\ell n} (g_n^* + \hat{\xi}_{\ell n}) - \varepsilon_{\ell} \right) \tag{134}$$

and expression (42).

C Appendix Figures and Tables

Figure C1: Industry-Level Variation in the Autor et al. (2013) Setting



Notes: This figure shows binned scatterplots of shock-level outcome and treatment residuals, \bar{y}_{nt}^{\perp} and \bar{x}_{nt}^{\perp} , corresponding to the SSIV specification in column 3 of Table 4. The manufacturing industry shocks, g_{nt} , are residualized on period indicators (with the full-sample mean added back) and grouped into fifty weighted bins, with each bin representing around 2% of total share weight s_{nt} . Lines of best fit, indicated in red, are weighted by the same s_{nt} . The slope coefficients equal 5.71×10^{-3} and -1.52×10^{-3} , respectively, with the ratio (-0.267) equaling the SSIV coefficient in column 3 of Table 4.

Table C1: Shift-Share IV Estimates of the Effect of Chinese Imports on Other Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Unemployment growth	0.221 (0.049)	0.217 (0.046)	0.063 (0.060)	-0.014 (0.079)	0.104 (0.079)	0.107 (0.083)	0.235 (0.178)
NILF growth	0.553 (0.185)	0.534 (0.183)	0.098 (0.133)	0.149 (0.083)	0.142 (0.155)	0.117 (0.161)	0.187 (0.297)
Log weekly wage growth	-0.759 (0.258)	-0.607 (0.226)	0.227 (0.242)	0.320 (0.209)	0.145 (0.264)	0.063 (0.260)	-0.211 (0.651)
# of industry-periods	796	794	794	794	794	794	794
# of region-periods	1,444	1,444	1,444	1,444	1,444	1,444	1,444

Notes: This table extends the analysis of Table 4 to different regional outcomes in Autor et al. (2013): unemployment growth, labor force non-participation (NILF) growth, and log average weekly wage growth. The specifications are otherwise the same as in the corresponding columns of Table 4. SIC3-clustered exposure-robust standard errors are computed using equivalent industry-level IV regressions and reported in parentheses.

Table C2: Alternative Standard Errors in the Autor et al. (2013) Setting

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Coefficient	-0.596	-0.489	-0.267	-0.314	-0.310	-0.290	-0.432
Table 4 SE	(0.114)	(0.100)	(0.099)	(0.107)	(0.134)	(0.129)	(0.205)
State-clustered SE	(0.099)	(0.086)	(0.086)	(0.097)	(0.104)	(0.101)	(0.193)
Adão et al. (2019) SE	(0.126)	(0.116)	(0.113)	(0.107)	(0.143)	(0.140)	(0.192)
Confidence interval with the null imposed	[-1.059, -0.396]	[-0.832, -0.309]	[-0.568, -0.028]	[-0.637, -0.018]	[-0.705, -0.002]	[-0.699, 0.002]	[-1.207, 0.122]

Notes: This table extends the analysis of Table 4 by reporting conventional state-clustered standard errors, the Adão et al. (2019) SIC3-clustered standard errors, and confidence intervals based on the equivalent industry-level IV regression with the null imposed, as discussed in Section 5.1. The specifications are the same as those in the corresponding columns of Table 4; for comparison we repeat the coefficient estimates and exposure-robust standard errors from that table.

Table C3: Period-Specific Effects in the Autor et al. (2013) Setting

	(1)	(2)	(3)	(4)
	Mfg. emp.	Unemp.	NILF	Wages
Coefficient (1990s)	-0.491	0.329	1.209	-0.649
	(0.266)	(0.155)	(0.347)	(0.571)
Coefficient (2000s)	-0.225	0.014	-0.109	0.391
	(0.103)	(0.083)	(0.123)	(0.288)

Notes: This table reports coefficient estimates for versions of the shift-share IV specification in column 3 of Tables 4 and C1, allowing the treatment coefficient to vary by period. This specification uses two endogenous treatment variables (treatment interacted with period indicators) and two corresponding shift-share instruments. The controls are the same as in column 3 of Table 4. SIC3-clustered exposure-robust standard errors are obtained by the equivalent shock-level regressions and reported in parentheses.

Table C4: Robustness to Acemoglu et al. (2016) Controls in the Autor et al. (2013) Setting

	(1)	(2)	(3)	(4)
Coefficient	-0.200	-0.293	-0.241	-0.232
	(0.093)	(0.125)	(0.115)	(0.122)
<u>Regional controls (w_{lt})</u>				
Autor et al. (2013) controls	✓	✓	✓	✓
Period-specific lagged mfg. share	✓	✓	✓	✓
Lagged 10-sector shares		✓	✓	✓
Local Acemoglu et al. (2016) controls		✓		✓
Local Acemoglu et al. (2016) pre-trends	✓		✓	✓
SSIV first stage F -stat.	118.9	53.3	65.9	56.6
# of region-periods		1,444		
# of industry-periods		794		

Notes: This table extends Table 4 by adding exposure-weighted sums of the other industry-level controls in Table 3 of Acemoglu et al. (2016). Pre-trends controls refer to the changes in industry log average wages and in the industry share of total U.S. employment over 1976–91; see the notes to Table 4 notes for details on the other controls and calculation of the SIC3-clustered exposure-robust standard errors (in parentheses) and first-stage F -statistics.

Table C5: Overidentified Shift-Share IV Estimates of the Effect of Chinese Imports on Manufacturing Employment

	(1)	(2)	(3)
Coefficient	-0.238 (0.099)	-0.247 (0.105)	-0.158 (0.078)
Shock-level estimator	2SLS	LIML	GMM
Effective first stage F -statistic		15.10	
$\chi^2(7)$ overid. test stat. [p -value]		10.92 [0.142]	

Notes: Column 1 of this table reports an overidentified estimate of the coefficient corresponding to column 3 of Table 4, obtained from a two-stage least squares regression of shock-level average manufacturing employment growth residuals \bar{y}_{nt}^\perp on shock-level average Chinese import competition growth residuals \bar{x}_{nt}^\perp , instrumenting by the growth of imports (per U.S. worker) in each of the eight non-U.S. countries from ADH, g_{nk} for $k = 1, \dots, 8$, controlling for period fixed effects q_{nt} , and weighting by average industry exposure s_{nt} . Column 2 reports the corresponding limited information maximum likelihood estimate, while column 3 reports a two-step optimal generalized method of moments estimate. Standard errors, the optimal weight matrix, and the Hansen (1982) χ^2 test of overidentifying restrictions all allow for clustering of shocks at the SIC3 industry group level. The first-stage F -statistic is computed by a shift-share version of the Montiel Olea and Pflueger (2013) method described in Appendix A.10.

Table C6: Bartik (1991) Application

	(1)	(2)
Leave-one-out estimator	1.277 (0.150)	1.300 (0.124)
Conventional estimator	1.215 (0.139)	1.286 (0.121)
H heuristic	1.32	10.50
Population weights	✓	
# of region-periods		2,166

Notes: Column 1 replicates column 2 of Table 3 from Goldsmith-Pinkham et al. (2020), reporting two SSIV estimators of the inverse labor supply elasticity, with and without the leave-one-out adjustment. Regions are U.S. commuting zones; periods are 1980s, 1990s, and 2000s; all specifications include controls for 1980 regional characteristics interacted with period indicators (see Goldsmith-Pinkham et al. (2020) for more details). Standard errors allow for clustering by commuting zones. Column 1 uses 1980 population weights, while column 2 repeats the same analysis without population weights. The table also reports the H heuristic for the importance of the leave-one-out adjustment proposed in Appendix A.6 (equation (35)).

Table C7: Simulated 5% Rejection Rates for Shift-Share and Conventional Shock-Level IV

		SSIV		Shock-level IV	
		Exposure-Robust SE		Robust SE	
		Null not	Null	Null not	Null
		Imposed	Imposed	Imposed	Imposed
		(1)	(2)	(3)	(4)
Panel A: Benchmark Monte-Carlo Simulation					
(a)	Normal shocks	7.6%	5.2%	6.8%	5.0%
(b)	Wild bootstrap (benchmark)	8.0%	4.9%	14.2%	4.0%
Panel B: Higher Industry Concentration					
(c)	$1/HHI = 50$	5.6%	4.9%	8.4%	6.1%
(d)	$1/HHI = 20$	7.3%	5.5%	7.0%	10.7%
(e)	$1/HHI = 10$	9.0%	8.2%	14.8%	23.8%
Panel C: Smaller Numbers of Industries or Regions					
(f)	$N = 136$ (SIC3 industries)	5.4%	4.5%	7.7%	4.3%
(g)	$N = 20$ (SIC2 industries)	7.7%	3.7%	7.9%	3.2%
(h)	$L = 100$ (random regions)	9.7%	4.5%	N/A	
(i)	$L = 25$ (random regions)	10.4%	4.3%	N/A	

Notes: This table summarizes the results of the Monte-Carlo analysis described in Appendix A.11, reporting the rejection rates for a nominal 5% level test of the true null that $\beta^* = 0$. In all panels, columns 1 and 2 are simulated from the SSIV design based on Autor et al. (2013), as in column 3 of Table 4, while columns 3 and 4 are based on the conventional industry-level IV in Acemoglu et al. (2016). Column 1 uses exposure-robust standard errors from the equivalent industry-level IV and column 2 implements the version with the null hypothesis imposed. Columns 3 and 4 parallel columns 1 and 2 when applied to conventional IV. In Panel A, the simulations approximate the data-generating process using a normal distribution in row (a), with the variance matched to the sample variance of the shocks in the data after de-meaning by year, while wild bootstrap is used in row (b), following Liu (1988). Panel B documents the role of the Herfindahl concentration index across industries, varying $1/HHI$ from 50 to 10 in rows (c) to (e), compared with 191.6 for shift-share IV and 189.7 for conventional IV. Panel C documents the role of the number of regions and industries. We aggregate industries from 397 four-digit manufacturing SIC industries into 136 three-digit industries in row (f) and further into 20 two-digit industries in row (g). In rows (h) and (i), we select a random subset of region in each simulation. See Appendix A.11 for a complete discussion.

Table C8: First Stage F -statistics as a Rule of Thumb: Monte-Carlo Evidence

	Number of Instruments				
	1	5	10	25	50
	(1)	(2)	(3)	(4)	(5)
Panel A: SSIV					
5% rejection rate	8.0%	8.9%	11.5%	15.0%	23.0%
Median bias, % of std. dev.	0.3%	14.6%	28.3%	43.2%	72.1%
Median first-stage F	54.3	14.8	9.1	6.4	7.7
Panel B: Conventional Shock-Level IV					
5% rejection rate	13.6%	13.9%	14.9%	17.7%	22.0%
Median bias, % of std. dev.	-0.3%	10.1%	27.1%	57.0%	80.2%
Median first-stage F	59.4	19.4	13.2	10.0	11.2
Number of simulations	10,000	3,000	1,500	500	300

Notes: This table reports the results of the Monte-Carlo analysis with many weak instruments, described in Appendix A.11. Panel A is simulated from the SSIV design based on Autor et al. (2013), as in column 3 of Table 4, while Panel B is based on the conventional industry-level IV in Acemoglu et al. (2016). The five columns increase the number of shocks $J = 1, 5, 10, 25$, and 50, with only one shock relevant to treatment. The table reports the rejection rates corresponding to a nominal 5% level test of the true null that $\beta^* = 0$, the median bias of the estimator as a percentage of the simulated standard deviation, and the median first-stage F -statistic obtained via the Montiel Olea and Pflueger (2013) method (extended to shift-share IV in Panel A, following Appendix A.10). See Appendix A.11 for a complete discussion.

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