Shift-Share Designs in Political Science

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Abstract

Shift-share designs are gaining popularity in political science. This article introduces what shift-share designs are, reviews their application in the literature, synthesizes recent methodological developments, and discusses their potential utility in the field. Although shift-share designs have a long historical use in economics, their causal properties only recently began to be understood. Articles in political science tend to be aware of these developments, but do not fully discuss and test identifying assumptions and sometimes apply the methods incorrectly. Most articles rely on the share exogeneity framework, suggesting that the shifter exogeneity framework is underutilized despite its comparable prevalence in economics. I illustrate shifter exogeneity framework and develop auxiliary theoretical results that are potentially useful in applying the framework in political science settings.

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1 Introduction

Comprehensive introductions to shift-share methods are few despite a growing interest in them within political science. Such an introduction is needed because many shift-share methods are conceptually and mathematically convoluted, and it is time-consuming to understand each of them through original articles alone. Also, since the term *shift-share* has been used in various contexts, navigating the literature can be confusing without some backgrounds. This article focuses on *shift-share designs* as opposed to shift-share methods in general, and aims to fill the gap in the literature by introducing what they are, reviewing their applications in the literature, synthesizing recent methodological developments, and discussing their potential utility in political science.

Shift-share designs refer to research designs that include shift-share measures as independent or instrumental variables. Suppose units are subject to common treatments by varying degrees, and the aggregate level of treatments is calculated as the average of individual treatment values weighted by their respective degrees of exposure. Formally, there are n units and m treatments in total, and the unit i is exposed to the jth treatment D_j ("shifter") by the weight w_{ij} ("share") where $\sum_{j=1}^{m} w_{ij} \leq 1$. The shift-share measure for unit i is defined as

$$X_i = \sum_{j=1}^{m} w_{ij} D_j. \tag{1}$$

When used as independent variables, shift-share measures primarily impute an unobserved quantity in empirical studies. Autor et al. (2020) estimate the effect of Chinese import penetration on regional electoral results. One would directly measure how much local industries were replaced by Chinese imports for the independent variable, but trade data are typically collected at the national level. Shift-share designs impute the level of penetration in the region by interacting the national level of Chinese imports by industry D_j with the local industry employment shares w_{ij}. This composite measure captures whether regions have a large presence of industries that were significantly impacted by Chinese imports at the national level.

When used as instrument variables, they can be used to impute a counterfactual quantity as in "Bartik instruments" (Bartik 1991) or serve as a purely theoretical construct as in the "propensity score" approach (Nunn and Qian 2014). Fouka and Tabellini (2022) ask whether Mexican immigrants to the United States have changed white Americans' perceptions of Black Americans. The actual number of Mexican immigrants in each region is available in this case, but both the number of immigrants and the racial attitudes of local residents would be affected by unobserved regional-level confounders as immigrants can choose where to move in.[1] To address the endogeneity, the authors construct a counterfactual number with the initial share of Mexicans living in the region w_i and the national inflow of Mexicans in subsequent decades D_t: $X_{it} = w_i D_t$.[2] The new number correlates with the actual number of incoming immigrants since new immigrants tend to move in places where other Mexican expats also live, or w_i is high. At the same time, it is free from time-varying regional confounders since shifters are national and regional shares are fixed at the initial year. This shift-share instrument proxies the inflow of Mexican immigrants into each region if there had been no internal changes in the United States in subsequent years, isolating forces that drove them out of Mexico from forces that attracted them into specific US regions.

Shift-share has multiple meanings in the literature. Defining it based on the special structure of variables allows us to study threats to statistical inference that such designs commonly face. Equation (1) may be misspecified if treatments have heterogeneous or non-linear effects or if their values are noisy estimates of some true quantities. If so, the regression estimate may be subject to attenuation bias or weak instrument problems. More importantly, even when equation (1) correctly represents the true aggregate treatment level, shares w_{ij} are rarely exogenous to the dependent variable. In Autor et al. (2020), regions with similar industry portfolios tend to share similar demographic composition and economic interests, making them prone to similar electoral shocks distinct from others due to events unrelated to Chinese imports. In Fouka and Tabellini (2022), regions where Mexican immigrants have historically settled tend to be less Black and more highly educated, resulting in similar distinct attitudinal shocks over time. Endogeneous shares create endogeneity between the shift-share measure and the dependent variable.

Two frameworks have been proposed to address this new endogeneity problem. The share exogeneity framework seeks variables that make shares exogenous to the dependent variable when conditioned on (Goldsmith-Pinkham, Sorkin and Swift 2020). Given that the imputation scheme is correct, the regression compares otherwise identical units that are exposed to the same shifters but to varying degrees, analogously to typical difference-in-difference designs. The shifter exogeneity framework allows individual treatments to be endogenously assigned, and instead seeks to cancel out the resulting biases by using many comparable, exogenous and independent shifters (Adão,

[1] Reverse causality does not matter much here because racial attitudes are unlikely to change on their own. Any endogeneity problem can be attributed to unobserved events that caused the change in racial attitudes.

[2] X_{it} is then converted to a counterfactual share of new Mexicans among residents using the predicted regional population. The difference-in-difference design in the original paper measures how much the change in the fraction of new Mexican immigrants among the local population affects the change in racial attitudes.

Kolesár and Morales 2019; Borusyak, Hull and Jaravel 2022). To achieve the cancellation, the sizes of the biases must be weakly correlated and no single bias can be large enough to influence the estimate alone under assumptions on both shifters and shares. Borusyak and Hull (2023) propose an alternative method that exploits comparable, exogenous, independent but finite shifters using randomized inference under stronger assumptions.

Shift-share designs are increasingly found in political science. From their initial use in trade and immigration, they now range to many other topics such as technology shock, capital movement, foreign aid and more. Despite the growing interest, studies often faultily apply the method. Among the articles published in 2021 or after, only a half of them cite one of the above two frameworks, implying the other half inadequately discuss identifying assumptions in their research design. Among the half that did, most of them rely on the share exogeneity framework but not always with substantive justifications or falsification tests that could have been done. This shows that the shifter exogeneity is being underutilized in political science. To reduce the confusion around shift-share designs, I propose thinking about them in terms of three dimensions: how to define units and shifters, what problem to solve in the research question, and how to think about the source of exogeneity. These dimensions are not necessarily tied in a specific way by topics. Finally, I note that shift-share methods are least represented in American politics and might have ample research potentials there.

Given the low awareness, I illustrate the shifter exogneity framework by replicating the results in Colantone and Stanig (2018). The authors ask whether trade shock led to the rise of nationalism in European countries, hence in common with Autor et al. (2020) in studying political consequences of international trade. While the replication methods generally follow Borusyak, Hull and Jaravel (2022) that replicate Autor, Dorn and Hanson (2013), the first article that introduced the shift-share trade shock measure, I propose an original shifter transformation scheme. This scheme addresses the unique data structure and context of the study by trimming noisy outlier shifters arising from the divide-by-zero problem, thereby better satisfying identifying assumptions. The main findings indicate that the data passes diagnostic tests but the instrument loses most of the first-stage power after residualization. This does not necessarily invalidate the findings of the original paper, because the reconstructed data used in the replication might be too noisy to capture any meaningful variation. However, the reconstructed data produces similar results before transformation and residualization, and I interpret this as the importance of implementing the correct procedures. Furthermore, the shifter transformation increases the first-stage F value at least to the level that makes the shift-share measure a valid weak instrument. This demonstrates that researchers may need to develop methodological innovations when applying existing shift-share designs to their specific contexts and settings.

The paper is organized as follows. Section 2 surveys the historical use of shift-share designs in economics. Section 3 synthesizes two main identification strategies under share exogeneity and shifter exogeneity. Section 4 overviews the use of shift-share designs in political science. Section 5 illustrates the shifter exogeneity framework by replicating Colantone and Stanig (2018). Section 6 concludes the paper.

2 Brief Overview of Shift-Share Methods

This section summarizes the development of shift-share methods for readers interested in their application in the literature. The term "shift-share" has at least three interrelated but distinct meanings. First, shift-share analysis or shift-share decomposition refers to a technique that linearly decomposes outcome variables into several components using initial shifters and shares (Dunn Jr 1960; Esteban-Marquillas 1972; Lemieux 2002). For example, Perloff (1957), one of the pioneers of the shift-share method, calculated the "expected" income per capita for each state using the national average income per industry and the local industry structure. The difference between the expected and observed income per capita can be understood as the effect of idiosyncratic regional shocks. Let D_{ij} be the income per capita of state i and industry j and w_{ij} be the employment share of industry j in state i. The income per capita of state i can be decomposed into

$$X_i = \sum_j w_{ij} D_{ij} = \sum_j w_{ij} \bar{D}_{\cdot j} + \sum_j w_{ij}(D_{ij} - \bar{D}_{\cdot j})$$

where bar denotes the mean. If the share w_{ij} varies over time, so w_{ijt} denotes the share w_{ij} at time $t = 0,\, 1,\, \cdots$, we can express w_{ijt} as $w_{ij0} + (w_{ijt} - w_{ij0})$ and break down X_i into three components:

$$X_{it} = \sum_j w_{ijt} D_{ijt} = \sum_j w_{ij0} \bar{D}_{\cdot jt} + \sum_j w_{ij0}(D_{ijt} - \bar{D}_{\cdot jt}) + \sum_j (w_{ijt} - w_{ij0}) \bar{D}_{\cdot jt}. \tag{2}$$

Equation (2) has two regional components each due to the change in shares and in the idiosyncratic income shocks. This technique is often used to assess the relative explanatory power of multiple possible causes of phenomena (Bound and Johnson 1992; Olivetti and Petrongolo 2016; Burstein, Morales and Vogel 2019; Freeman, Ganguli and Handel 2020). While modern literature has advanced into non-linear or nonparametric decompositions, this linear method remains popular for its simplicity.

Researchers who understand shift-share methods under the second meaning view them as a way to instrument an endogenous independent variable. Bartik (1991) initially formalized the idea to estimate the inverse elasticity of labor supply. In his paper, the independent variable was local labor supply and the dependent variable was local wage. This requires an instrumental variable approach since quantity (observed labor supply) and price are simultaneously determined at the market equilibrium by demand and supply shifters. A valid instrument must be correlated with X_i, but not with local labor supply shifters so that the instrument only captures the effect of labor demand on wage. Equation (2) gives a clue: $\sum_j w_{ij0}\bar{D}_{\cdot jt}$ is correlated with X_{it} via the initial share w_{ij0} but implausibly with any time-varying factors idiosyncratic to unit i, hence isolating labor demand shifters.[3] This type of shift-share instruments is called Bartik instruments. Multiple instruments may exist for the same X_{it} depending on how to decompose the variable (the dimension of j) and which exogenous variations to consider (the nature of D_j). Note that the shift-share measure here is used for causal identification rather than structurally adjudicating between competing hypotheses as in the first meaning.

Bartik instruments subsequently gained popularity in the wider economics literature. As regions and industries are two natural orthogonal levels of analysis, economists have constructed instrumental variables by interacting the same industry shares with various national outcomes, including earnings (Luttmer 2005; Diamond 2016), hours worked (Bound and Holzer 2000), productivity and technology shocks (Gould, Weinberg and Mustard 2002; Acemoglu and Restrepo 2020), and immigration inflow (Card 2001). Regions can be decomposed along other dimensions such as immigration inflow by skill groups (Card 2009), income growth by income groups (Boustan et al. 2013), credit shifts by banks (Greenstone, Mas and Nguyen 2020) or price changes by housing characteristics (Graham and Makridis 2023). In a recent article, Gabriel, Klein and Pessoa (Forthcoming) interact the ratio of regional and national per capita government spending, reflecting how sensitive each region is to government spending, with measures of the national parties' austerity narratives to instrument austerity policy implementation in each region. All of these studies consider shifters D_j similar to the dependent variable of interest but measured at a more aggregated level in order to remove time-varying regional variations.

[3]The assumption here is that labor demand shifters are national and labor supply shifters are local, so the instrument contains no time-varying regional factors that could possibly be related to labor supply shifters.

The last meaning defines shift-share methods from the methodological perspective. Shift-share designs refer to research designs where the independent variable or the instrument variable exhibits inner-product structure between unit-specific variables w_{ij} and shifter-specific variables D_j. Bartik instruments are thus a specific type of shift-share designs. This definition also includes Autor, Dorn and Hanson (2013), a prominent example where a shift-share measure is used as an independent variable. The authors ask how trade with China affected the US local labor market wage. Their dependent variable is the wage differential in each economic zone, and the independent variable is the interaction between regional industry shares and the industrial change in Chinese import exposure per worker. Unlike in the case of Bartik instruments, this shift-share measure is a noisy estimate of local exposure to Chinese imports that is hard to observe directly. The effect of this "China shock" measure has been studied on various macro outcomes such as employment (Acemoglu et al. 2016), mortality (Pierce and Schott 2020), marriage rate (Autor, Dorn and Hanson 2019), public goods provision (Feler and Senses 2017), and polarization (Autor et al. 2020). Researchers also measured regional import penetration in other advanced economies using the same methodology (Dauth, Findeisen and Suedekum 2017; Barone and Kreuter 2021).

Researchers can be creative in their choice of units and shares. Shares can be more sophisticated than plain observed quantities. Kovak (2013) measures regional exposure to trade liberalization by interacting price differentials per industry with composite shares constructed from regional industry labor shares, the elasticity of substitution between production factors and their cost shares, which is justified by the structural model. Units also need not be geographic regions. Hummels et al. (2014) define firms as units and measure their exposure to transportation costs by interacting changes in country-level transportation cost with shares of input source countries for each firm. Acemoglu and Linn (2004) define new drug categories as units and measure their exposure to demographic changes by interacting demographic changes per age group and age profiles of users for each drug category. Xu (2022) defines ports as units and measures their exposure to bank failure by interacting bank failure rates and their credit exposure to each bank. These are examples where shift-share designs are used as a measurement strategy.

Autor, Dorn and Hanson (2013) is also notable for their instrument variable strategy.[4] The authors point out that the Chinese import exposure D_j are subject to both Chinese supply shifters and the US demand shifters. The latter affect local labor demand and, in turn, local wage, hence potentially creating omitted variable biases. The authors address the problem by constructing

[4] This strategy has been replicated in many other "China shock" papers including Autor et al. (2020).

a shift-share instrument with counterfactual shifters measured by the change in Chinese import exposure per worker in other advanced economies comparable to the US: $Z_i = \sum w_{ij} D_j^*$ where D_j^* denotes the counterfactual shifters. This amounts to creating a fictitious economic zone in those other countries that has the same industry shares as the economic zone of interest. The new measure will be affected by Chinese supply shifters and demand shifters in those economies, allowing us to isolate the effect of the former when instrumenting the original independent variable that reflects Chinese supply shifters and the US demand shifters. In studying the effect of the supply of foreign doctoral students at US universities on scientific publications, Stuen, Mobarak and Maskus (2012) employ the similar instrumental variable approach to isolate source-country shocks by interacting shares of students from source countries at each university and field with the number of doctoral students in other host countries.

This article focuses on shift-share designs. Not only does it provide the broadest applicability of shift-share measures in causal studies, but also shift-share variables defined in this way share similar statistical properties. These properties extend to inner-product measures that do not have interpretation of typical shares and shifters as will be seen in Section 4.

3 Identification and Inference Strategies

The biggest challenge in shift-share designs is that shares and shifters are rarely both exogenous. One plausibly exogenous variation is already hard to find in observational studies; finding two is much harder. In Bartik (1991), shares are endogenous if initial regional shares predict time-varying labor supply shifters, and shifters are endogenous if national industrial employments vary due to labor supply shifters such as immigration, which also directly affect wage. In Autor, Dorn and Hanson (2013), their instrumental variable strategy addresses endogeneity of shifters, but shares are likely to remain endogenous as local wage and employment are codetermined in market equilibria.

Two distinct statistical problems arise from the endogeneity. First, estimates suffer the omitted variable bias if units systematically select into qualitatitvely different treatments by unobserved characteristics. If initial regional shares correlate with time-varying labor supply shifters, so will the instrument and its exclusion restriction will be compromised. Second, the typical cluster-robust standard error estimator tends to underestimate standard errors even when the point estimate is valid (Adão, Kolesár and Morales 2019). If the error term exhibits an inner-product structure as in Autor, Dorn and Hanson (2013) where errors are also a product of the market equilibria that determine local employment, units with similar shares will have correlated errors. This implies that shares behave as clusters and, if the similarity in the shares cross-cuts geographical boundaries, errors may violate the independence assumption even after accounting for typical geographical clusters. The first is an identification problem, while the second is an inferential problem.

This section provides intuition behind identification strategies when either shares or shifters are exogenous. Readers interested in technical details may refer to Appendix B, or to Borusyak, Hull and Jaravel (2024) who review the same literature from a more generalized shifter exogeneity perspective. Hahn et al. (2024) discuss overidentification tests in shift-share designs while succintly summarizing the two frameworks. Borusyak, Hull and Jaravel (Forthcoming) give intuitions behind shift-share designs and some practical guides.

3.1 Share Exogeneity: Exogenous Shares

Share exogeneity holds when shares in the shift-share measure are not influenced by the realized values of the dependent variable. Even if the distribution of shifters depends on that of the dependent variable, units cannot select into the level of exposure to each shifter when shares are exogeneously determined. Therefore, share exogeneity ensures comparability between units. Typical regression under this framework estimates to what extent the variation in the dependent variable results from the difference in the total exposure to shifters, just as in difference-in-difference.

Suppose that the true model is $Y_i = \alpha + \beta X_i + \varepsilon_i$, and $X_i = w_{i1}D_1 + w_{i2}D_2$. The regression of Y_i on X_i yields $\hat{\beta} = \frac{\hat{\text{cov}}(Y_i, X_i)}{\hat{\mathbb{V}}(X)} = \beta + \frac{\hat{\text{cov}}(X_i, \varepsilon_i)}{\hat{\mathbb{V}}(X)}$, and the covariance between X_i and ε_i is $\text{cov}(w_{i1}D_1,\, \varepsilon_i) + \text{cov}(w_{i2}D_2,\, \varepsilon_i)$. Share exogeneity provides a sufficient condition for consistency by fixing shifters and instead assuming that each share and the error term are uncorrelated: $\text{cov}(w_{ij},\, \varepsilon_i) = 0$ whenever $D_j \neq 0$. Fixing shifters allows for an arbitrary dependence structure among D_j's so that the inference is valid regardless of their nature.[5] The zero covariance condition may be less stricter than mean-independence or independence assumptions required in the other frameworks below, but it remains strong as it must hold for every pair of i and j.

Goldsmith-Pinkham, Sorkin and Swift (2020) propose two ways to relax the unconditional zero covariance condition. First, shares may be exogenous to the error term conditionally on control variables: $\text{cov}(w_{ij},\, \varepsilon_i \mid \Pi_i) = 0$ where Π_i denotes a control vector. This selection-on-observables strategy addresses the omitted variable problem. What about the inferential problem? The authors

[5] This is equivalent to say that inference is conditioned on any realized values of the shifters. All the other variables are sampled *i.i.d.* across units.

show that shift-share regression is equivalent to the generalized method of moments (GMM) with special weights. Returning to the above example, we can view the OLS regression as an IV regression where X_i instruments itself. If we instead use w_{i1} and w_{i2} for instruments, we estimate the following two equations, omitting controls:

$$X_i = \gamma + \delta_1 w_{i1} + \delta_2 w_{i2} + \eta_i$$
$$Y_i = \alpha + \beta X_i + \varepsilon_i.$$

Restricting the first-stage coefficients as $\delta_1/\delta_2 = D_1/D_2$ immediately turns the first stage into regression of X_i on itself, which can be done by assigning an appropriate weighting matrix in the GMM procedure. Therefore, the standard error from shift-share regression is same as that from an overidentified IV regression and is valid as long as shares are valid instruments. The argument is identical if the shift-share measure was an instrument rather than the independent variable in the original regression.

Second, the zero covariance condition might not hold for every pair of i and j even after conditioning. However, if the violation of assumptions does not bias the final estimate equally, it may suffice to show that the exogeneity holds for the most important shares. Let $\hat{\beta}_j$ be the estimate instrumented with the jth shares only. The authors show that for some real constants $\hat{\alpha}_j$ summing to 1,

$$\hat{\beta}_{\text{shift-share}} = \sum_{j=1}^{m} \hat{\alpha}_j \hat{\beta}_j.$$

$\hat{\alpha}_j$ is called Rotemberg weights. This implies that the shift-share estimate is most influenced by shares with the largest absolute weights, and researchers must be ready to defend exgoeneity of these shares more than others. `R` packge `bartik.weight` and `Stata` package `bartik-weight` automatically calculate the weights.

The discussion so far has been cross-sectional. The panel setup warrants a few remarks. First, the model in Goldsmith-Pinkham, Sorkin and Swift (2020) assumes that units may be dependent across time but are independent across units, which implies that the model does not accommodate geographically clustered errors. This is because inference under clustering has not yet been developed in overidentified settings. Second, empirical studies commonly fix shares at their initial values as in Fouka and Tabellini (2022) to avoid potential post-treatment bias. Such bias can occur when past errors affect both current shares and current errors, leading to a correlation between the two. A typical panel model would look like:

$$Y_{it} = \beta X_{it} + \gamma^\top \Pi_{it} + \varepsilon_{it}$$
$$Z_{it} = \sum_{j=1}^{m} w_{ij0} D_{jt}$$

where $t = 0, \cdots, T$ denotes time, Z_{it} instruments X_{it}, and $\{\varepsilon_{i0}, \cdots, \varepsilon_{iT}\}$ is independent across i.

Researchers may assess these modeling assumptions with several diagnostic tests. First, the conditional covariance $\text{cov}(w_{ij}, T_i \mid \Pi_i)$ must be zero for T_i that proxies ε_i. T_i can be any variables that affect the dependent variable not through the independent or instrument variable, such as any labor supply shifters in the settings of Bartik (1991). This falsification test is more so important for the key shares with high Rotemberg weights. Second, the estimate $\hat{\beta}$ should be zero before the onset period of the causal relationship of interest if it exists. This is similar to pre-trend tests in difference-in-difference designs. The immigration regime change of 1965 in Fouka and Tabellini (2022) is an example. Third, since the shift-share instrument is one specific way to combine individual share instruments, the estimate should be robust to alternative models with multiple instruments. Overidentification tests are a formal method to do this.

How would the homogeneous effect model perform if the true treatment effect varies across individuals? It can be shown that the shift-share estimate is guaranteed to be a convex combination of individual unit effects of X_i on Y_i only if (1) all unit-level effects are either uniformly positive or negative, and (2) all Rotemberg weights are positive.[6] Since achieving both conditions is challenging, we conclude that the shift-share regression may not provide a reliable estimate under effect heterogeneity.

3.2 Shifter Exogeneity: Many Exogenous and Independent Shifters

Shifter exogeneity holds when the distribution of shifters are not meaningfully influenced by the realized values of the dependent variable. This framework applies when there are no viable selection-on-observables strategies for shares. It instead identifies conditions where comparable shifters can

[6] Convexity is considered an important property in bridging structural and causal models as in the recent two-way fixed effect literature (De Chaisemartin and d'Haultfoeuille 2020). If we allow for effect heterogeneity among shifters as well, the estimate is a convex combination only when shares are uncorrelated with one another, which is unattainable when their sum is fixed at 1, but only under a stronger condition that shares are independent of errors (Hahn et al. 2024).

be combined into a single 'proper' treatment. The resulting effect is consistent as the individual biases due to share endogeneity cancel out under those conditions.

The key intuition is twofold. First, one should isolate incidental deviations from their systematic variations with demeaning. Suppose there are two shifters whose expectations are always $\mathbb{E}[D_1] = 1$ and $\mathbb{E}[D_2] = 2$ regardless of the realization of other variables, and the true aggregate treatment is $X_i = w_{i1}D_1 + w_{i2}D_2$. X_i can be decomposed into its expectation $w_{i1}\mathbb{E}[D_1] + w_{i2}\mathbb{E}[D_2] = w_{i1} + 2w_{i2}$ and the remainder. The first term captures the variation in the total treatment due to the variation in non-random shares, and the second term captures the variation due to the incidental deviations of the shifters. The first term is always endogenous if shares are endogenously assigned. Therefore, demeaning purges the systematic endogeneity in X_i.

However, this alone does not guarantee the exogeneity of the incidental term since incidental deviations are still endogenously assigned to each unit. This calls for some restrictions on shares to purge the incidental endogeneity in X_i. Adão, Kolesár and Morales (2019) prove that biases due to individual non-random shares cancel out when (1) shifters are independent so that the biases are also independent to one another, and (2) individual shares are small enough that no finite number of biases dominate the rest. The result can be understood as the shifter-level law of large numbers. Note that the share exogneity framework naturally requires many shifters unlike the share exogeneity framework.

An alternative way to understand this framework is inverting the shift-share regression into the shifter level. Consider the same model as in the previous section, $Y_i = \alpha + \beta X_i + \varepsilon_i$ and $X_i = w_{i1}D_1 + w_{i2}D_2$. Borusyak, Hull and Jaravel (2022) show that if shares are complete, *i.e.* $\sum_j w_{ij} = 1$ for all i, the estimate $\hat{\beta}$ from the OLS regression of Y_i on X_i is mechanically equal to the estimate $\hat{\beta}'$ from the below regression with two 'observations':

$$\begin{cases} \frac{\sum_i w_{i1}Y_i}{\sum_i w_{i1}} = \alpha' + \beta' \frac{\sum_i w_{i1}X_i}{\sum_i w_{i1}} + \varepsilon'_1 \\ \frac{\sum_i w_{i2}Y_i}{\sum_i w_{i2}} = \alpha' + \beta' \frac{\sum_i w_{i2}X_i}{\sum_i w_{i2}} + \varepsilon'_2 \end{cases}$$

where the independent variables are instrumented by D_1 and D_2 with particular weights $\sum_i w_{i1}$ and $\sum_i w_{i2}$. Therefore, the shift-share estimate is valid as long as shifter D_i validly instruments some function of X_i, which holds if the shifters are mean-independent and demeaned.[7] This val-

[7]Shifters have to be numerous and independent to apply the central limit theorem to $\hat{\beta}'$ since each observation in the inverted regression corresponds to shifters rather than units. Shares should be asymptotically negligible since the resulting regression weights determine the effective number of observations.

idation strategy resembles Goldsmith-Pinkham, Sorkin and Swift (2020) in that both find an IV regression that is equivalent to the original shift-share regression and justify the latter by verifying the assumptions in the former.

In fact, the inverted regression offers a simple solution to the inferential problem as well. The inverted regression and the original regression produce the same point estimate but different standard errors. Borusyak, Hull and Jaravel (2022) find that the typical heteroskedasticity-robust estimator in the former produces valid standard errors conditionally on all other variables but the shifters, implying that the model can handle any dependence structure among shares and errors. Note that the current version of the share exogeneity framework cannot handle geographical dependence. The dependence structure among shifters can be addressed by clustering errors in the inverted regression. The inversion can be done with `R` and `stata` packages `ssaggregate`. Alternative but likewise asymptotically valid standard error estimators proposed in Adão, Kolesár and Morales (2019) are available in `R` package `ShiftShareSE`.[8] Panel settings naturally follow from this setup. The shares need not be fixed at the initial value since they are allowed to be endogenous. Shifters must be clustered along the time dimension if they are autocorrelated.

A challenge with this approach is that the expectations of the shifters are unknown in most observational studies. Parametrizing the expectation of every shifter is infeasible since demeaning will render all shifters zero, leaving no incidental variation in the total treatment. One solution is to model the expectations using a smaller number of latent variables; all shifters having the same expectation is an extreme case with only one latent dimension. However, since latent variables are often unobservable and valid inference under estimated latent variables remains unsolved, the currently available best practice is to theoretically argue which shifters have the same expectation and include dummy variables for each shifter group.[9] These latent variables should be weighted by shares and included as controls. In the inverted regression, the dependent and independent variables must be residualized over the control variables before inversion, and then the latent variables must be added as fixed effects for valid standard error estimation.[10]

Researchers can assess modeling assumptions with similar diagnostic tests. D_j must be exogenous to ε_i in the shift-share regression and ε'_j in the inverted regression. To verify this, we test $\mathrm{cov}\big(D_j,\, \frac{\sum_i w_{ij} T_i}{\sum_i w_{ij}}\big) = \mathrm{cov}(D_j,\, T'_j) = 0$ for unit-level covariates T_i and shifter-level covariates T'_j that

[8] Appendix B.2 discusses how these estimators are different.

[9] The validity of this empirical strategy thus depends on how much we can control for heterogeneous expectations of the shifters.

[10] This is different from inverting the long regression that includes control variables.

proxy ε_i and ε'_j. Parallel trend tests can also be performed in the same way if the causal relationship of interest began at a specific onset period. Hahn et al. (2024) propose overidentification tests under a stronger condition where shifters are not only mean-independent but also independent of other structural terms. Since there is nothing special in shares in this framework, the exclusion restriction of the shift-share instrument will hold under alternative shares. Overidentification tests measure the sensitivity of the estimate to specific choices of these alternative instruments.

Lastly, the shifter exogeneity framework performs slightly better under effect heterogeneity than the share exogeneity framework. Individual effects of the shifters on the dependent variable always have positive weights in the shift-share estimate with the original shares. However, Hahn et al. (2024) find that negative weights can be produced under alternative shares if at least one pair of shifters is negatively correlated, if no restrictions are imposed on the effect heterogeneity and the same stronger condition holds.[11]

3.3 Shifter Exogeneity: Finite Exogenous and *i.i.d.* Shifters

Another way to exploit shifter exogeneity is running a finite-sample randomization test by imposing a more restrictive structure on the shifter distribution. Recall that the major challenge in the previous section was the unknown expectations of the shifters. What if we could group shifters so that those within the same group follow the same distribution? The *i.i.d.* assumption enables estimation of the systematic biases that non-random shares create and removes need for asymptotics.

Borusyak and Hull (2023) propose a two-step testing procedure.[12] The first stage estimates systematic biases for the aggregate shift-share instrument of each unit. The second stage simulates counterfactual shifters, recenters them by subtracting biases from the simulated instruments, and compares test statistics under the null hypothesis. Although the null hypothesis assumes effect homogeneity by default, this approach retains the same robustness to effect heterogeneity described in the previous section. However, despite the test being exact, it must be noted that this method assumes much stringent conditions on the distributions of shifters; while the mean independence requires conditions only on the first moments, the identical distribution imposes conditions on every moment of the shifters. Dependence among shifters may also significantly increase the uncertainty surrounding bias estimation.

[11] Chetverikov et al. (2023) derive a simpler and asymptotically equivalent point and standard error estimators under the shifter independence.

[12] The paper treats shift-share instruments are treated as a special case of formula instruments, which combine random and non-random components in possibly non-linear functions.

3.4 Discussion

Share exogeneity and shifter exogeneity rely on different sets of identifying assumptions. The former derives from the comparability between units, while the latter leverages the comparability among shifters. Empirical designs that researchers have in mind may suggest which framework is more suitable. Share exogeneity is implied when they focus on similarity among units or shocks to specific industries that are key to identification. Shifter exogeneity is relevant when they emphasize the quasi-experimental characteristics of many comparable shocks or finite identical shocks.

Both approaches have weaknesses that researchers must be aware of. Share exogeneity might not be feasible when errors also have a shift-share structure comprising shares identical to or correlated with those in the shift-share variable. It also cannot currently accommodate clustered errors. Shifter exogeneity requires tricky assumptions on control variables under the asymptotic approach, and highly strict assumptions on shifters under the randomization approach. Neither framework provides a fully satisfactory solution under effect heterogeneity, which extends shift-share models to potential outcome models.

We have so far assumed either shifters or shares are exogenous. What if both are endogenous or exogenous? If both are endogenous, one may consider exogenize one of them by fixing shares at their initial values, averaging shifters as in Bartik (1991), or finding instrument shifters as in Autor, Dorn and Hanson (2013). If both are exogenous, either framework will be asymptotically valid but their comparative test power will depend on the number of units and the number of independent shifters.

4 Shift-Share Designs in Political Science

This section surveys the application of shift-share designs in political science, proposes a framework for understanding and developing shift-share designs, identifies the strengths and weaknesses of current studies, and discusses opportunities for future research. The review is based on Table A.1 that compiles thirty-five articles that use shift-share designs published in political science journals. These shift-share articles were identified through keyword searches for “shift-share” and “Bartik” on Google Scholar, as well as by tracking papers that cited milestone shift-share articles such as Card (2001) and Autor, Dorn and Hanson (2013). I note that creating an exhaustive list is challenging as some articles might have used shift-share designs as a measurement strategy without explicitly stating it.

Most studies, knowingly or unknowingly, rely on share exogeneity assumptions, including trade, although trade shock is more associated with shifter exogeneity in economics. Motivations for shift-share variables are more balanced between factual and counterfactual imputation introduced above.

Table 1: Shift-Share Designs in Political Science Journals

	APSR/AJPS/JOP	Others	Total
Before 2015	0	0	0
2015−2019	4	2	6
2020−present	8	21	29

Note: This non-exhaustive list was collected through keyword searches for "shift-share" and "Bartik" on Google Scholar and by tracking papers that cited milestone shift-share articles.

Table 1 presents a breakdown of the articles in Table A.1 by year and journal. The first article was published in 2015, followed by five more before 2020. Twenty-nine articles have been published since then in the last five years, including six forthcomings. This indicates increasing interests in shift-share designs in political science. The table also reveals another pattern: while shift-share designs were initially found in general interest journals such as APSR, AJPS and JOP, there is now a growing number of articles appearing in field-specific journals. Shift-share designs have become a standard method for studying specific topics as will be seen below, and this explains their increasing presence in field journals. Closely follows immigration/migration and capital movement.

The use of shift-share designs in political science largely mirrors their application in economics. Trade shock is the most popular topic, with fifteen studies. The typical research design follows Autor, Dorn and Hanson (2013) and Autor et al. (2020) where shift-share designs are the aggregate treatment imputation strategy. Technology shock and employment are similar to trade shock, replacing industrial import exposures with industry-level technological innovations or employment rates. Immigration/migration and capital movement trail, with six and five studies respectively. The typical research design resembles Card (2001) and Fouka and Tabellini (2022).

Trade shock and immigration/migration are the two topics that shift-share designs are most widely used, which motivated the shifter exogeneity and the share exogeneity frameworks in economics in the first place. These articles are often interested in the effect of similar independent or instrument variables as above on political outcomes. Other topics include technology shock, capital movement, foreign aid, employment, and natural resources. Technology shock is measured analogously to trade shock in the labor market context. If each industry can be mapped to their technology content, then the local labor market is exposed to industry-level innovations accordingly to its local employment shares (Autor, Dorn and Hanson 2015). However, if technology shock of interest is idiosyncratic growth in each industry, the shift-share variable can also have a Bartik instrument interpretation measuring the counterfactual growth rate without some external factor (Finseraas and Nyhus Forthcoming). Employment articles often use shift-share designs for Bartik-style instrumenting as local employment data is more readily available than local trade or technology prevalence.

Most articles on capital movement, foreign aid and natural resources exploit a different kind of shift-share variables first devised by Nunn and Qian (2014), which interacts exogenous instruments with unit-specific propensity scores. The authors asks how US food aid affects the incidence of conflict in recipient countries and instrument food aid with the US wheat production of the year to address endogeneity issues.[13] Since the wheat production generates variation only across time and not across units, the authors interact it with the fraction of years that the recipient country received food aid from the US to gain a higher test power. This instrument does not conform to the conventional shares and shifters framework as shares are defined across time periods of interest while shifters are fixed at a certain year. It therefore lacks an imputation interpretation of other shift-share variables. However, it is a product of a endogenous propensity score and an exogenous wheat production, so the causal claim is valid as long as the shifter exogeneity assumptions are satisfied.[14] This propensity score approach is also used to instrument IMF lending decisions or proxy dependence on natural resources (Carreri and Dube 2017). Ziaja (2020) expand the approach to multiple donors in the context of foreign aid, *i.e.*, $Z_{it} = \sum_j p_{ij} D_{jt}$ where p_{ij} denotes the propensity score of recipient country i with respect to donor j. The shifter exogeneity framework can verify the validity of this seemingly complicated instrument as well.

Table 2: Three Dimensions of Shift-Share Designs

Components	Units	Shifters	Shares
Type	Factual Imputation	Bartik	Propensity Score
Exogeneity	Share	Shifter	Both

[13] The rationale is that wheat aid is institutionally used as a way to handle surplus food production.

[14] The propensity score is endogenous if conflict can reversely determine the decision of food aid and the conflict incidence is highly autocorrelated.

The above overview by no means suggests that topics determine shift-share designs of the research. Table 2 summarizes three dimensions to consider in devising shift-share designs. The first dimension concerns how to define basic components in shift-share designs. Units are given in the research question, but researchers can be creative in the choice of shifters and shares depending on the data availability. Section 2 lists some articles that devised creative breakdowns of units. The propensity score approach was innovative in their choice of shares. The second dimension is the type of shift-share variables. This depends on why the research question calls for shift-share designs in the first place. Counterfactual imputation includes both Bartik instruments and China shock-style instruments that use shifters satisfying exclusion restriction. The third dimension is the source of exogeneity, which determines the identification strategy of the shift-share design. Carreri and Dube (2017) is a rare case where both shifter and share are exogenous, hence able to borrow the difference-in-difference framework without complexities of most shift-share designs. Note that the three dimensions can be orthogonal to one another. Trade shock studies usually have the same components, but Autor, Dorn and Hanson (2013) has two different types of shift-share variables, factual and counterfactual imputation. Scheve and Serlin (2023) is based on share exogeneity even though their independent variable measures trade shock in a conventional way.

As most shift-share articles in political science were recently published, 10 out of 35 articles cited at least one of the three papers reviewed in the previous section. Nine of these made causal claims using the share exogeneity framework, and some of articles that did not reference any papers also justified their research design by drawing parallel to difference-in-difference designs. However, only a few of them spelled out and tested the required identifying assumptions, even among those that were aware of the new methods. Fouka and Tabellini (2022) conduct placebo tests and randomization inference similar to Borusyak and Hull (2023) as one of the exceptions. Most articles also cluster standard errors under share exogeneity, where inference currently depends on the independence among units as clustering under multiple instruments remains an open question (Footnote 14 of Goldsmith-Pinkham, Sorkin and Swift (2020)). Overall, shift-share designs can be more transparently used if articles are more explicit about identifying assumptions and report diagnostic tests as is often done in difference-in-difference designs.

Finally, I note that shift-share designs are underrepresented in American Politics. Only five studies can be classified into American Politics, while the other 21 belong to Comparative Politics or International Relations. This may be because a wider data availability reduces the need for data imputation or because shift-share measures devised in economics are more applicable to other political science fields. However, I believe shift-share designs also have potential in American Politics by helping to develop new exposure designs, proxying conceptual variables that are unobservable by nature, or measuring counterfactuals.

5 Example: China Shock

This section revisits the shift-share design in Colantone and Stanig (2018) using the shifter exogeneity framework. The authors ask how imports from China led to the rise of nationalism and far-right parties in European countries. Borusyak, Hull and Jaravel (2022) have replicated Autor, Dorn and Hanson (2013) who pioneered the shift-share measure of this kind. This paper follows their step, but it also highlights the new shifter transformation scheme that stems from the uniqueness of Colantone and Stanig (2018).

5.1 Research Design

To study the given research question, the authors regress electoral outcomes of electoral districts on the shift-share trade measure. The data spans from 1988 to 2007. The electoral results of interest are the vote share-weighted mean and median nationalism score and the nationalist autarchy score of parties, and vote shares of far-right parties. The independent variable is the interaction between local manufacturing industry employment shares and two-year differences in Chinese imports scaled by total national employment of the industry:

$$X_{rt} = \sum_{j=1}^{m} \frac{L_{rj(t-2)}}{L_{r(t-2)}} \times \frac{\Delta \text{Import}_{cjt}}{L_{cj(t-2)}} \tag{3}$$

where c indexes countries, r regions, j industries, t years. Local employment L_{rjt} is measured at the NUTS-2 level, and industries j are at the two-letter NACE level totaling ten, which is twice more coarse than 2-digit SIC codes.[15]

The main variables exhibit three structural differences from Autor, Dorn and Hanson (2013). First, NUTS-2 regions subsume electoral districts so the dependent variable and the independent variable are measured at different levels, or, $r \neq i$. This matters since the original research design did not weight districts by their population. I choose districts for the unit of analysis since pop-

[15]NUTS is the administrative geographical unit and NACE is the industry code system developed by Eurostat. Autor, Dorn and Hanson (2013) use the Standard Industrial Classification (SIC) at the 4-digit level where shifters are found to be correlated at and below the 3-digit level (Borusyak, Hull and Jaravel 2022).

ulations are institutionally guaranteed to be more uniformly distributed across electoral districts than NUTS-2 regions.[16] Second, import shocks are defined as two-year differences instead of much longer, ten-year differences. This inflates the number of total shocks at the cost of potentially higher dependence among shocks. Third, despite the smaller number of industries, the nominal number of shifters still remains comparable as data covers multiple countries. This raises new concerns about cross-country dependency of the shifters.

The authors introduce a shift-share instrument to address two possible sources of endogeneity. First, politicians may strategically protect certain districts from Chinese imports, in which case electoral outcomes determine the local exposure to trade with China. Second, both the shifters and the dependent variable are affected by idiosyncratic local shocks such as economic fluctuation or political performance of incumbents that are not attributable to China. The shift-share instrument has the same structure as in Autor, Dorn and Hanson (2013); the first-differences in Chinese imports into the European countries, $\Delta\text{Import}_{cjt}$ in equation (3), are replaced with the first-differences in Chinese imports into the United States, $\Delta\text{Import}_{\text{US}jt}$. Exclusion restriction requires that political protectionism or the idiosyncratic local shocks be not globally correlated.

However, Section 3 suggests another source of endogeneity due to endogenous shares. Regions with different industry structures might have different political orientation due to differing demographic composition, economic interests, and historical backgrounds.[17] Standard errors of the estimate may be underestiamted if this share endogeneity is not accounted for. The share exogenity framework exogenizes shares by conditioning them on covariates. This requires identifying the most critical shares and specifying their endogeneity with political outcomes based on substantive knowledge. The shifter exogeneity framework leverages the comparability among many similar shifters and exploits the incidental deviations from their respective expectations. Colantone and Stanig (2018) is more compatible with the latter since they exploit the quasi-random assignment of import shocks by industry. While errors are only clustered by NUTS 2 region-year in the original paper, a valid estimator should cluster errors across different regions with similar industry portfolios. The shifter exogeneity framework handles arbitrary correlation among errors under suitable assumptions, including that due to the shift-share structure of the error term. Such dependence cannot be addressed under the current share exogeneity framework.

Before assessing the identifying assumptions, I note that the subsequent analyses are based on

[16] The population size of NUTS-2 regions ranges from 800,000 to 3,000,000.

[17] As these regions would respond differently to Chinese imports, the endogeneity would not be resolved by simply taking the first differences in the variables as suggested by Goldsmith-Pinkham, Sorkin and Swift (2020).

the reconstructed data spanning from 2001 to 2007 rather than the original data. This is because their replication file only contains the aggregate shift-share variable and not the individual shares and shifters. Details of the reconstruction process are reported in Appendix C. The reconstructed shift-share variable and instrument have the correlation of 0.87 and 0.90 with the original shift-share variable and instrument.[18] Table F.1 replicates their main results using the original variables, the original variables censored from 2001 to 2007, and the imputed and predicted reconstructed variables.[19] Estimates were close enough with either reconstructed variables instead of the censored original variables, especially with respect to the effect on average nationalism scores. This validates the reconstruction process. The rest of the section focuses on the nationalism score for the main dependent variable.

5.2 Identifying Assumptions and Shifter Transformation

Inference under shifter exogeneity requires the mean-independence and identical expectation of shifters, independence among shifter clusters, and asymptotically negligible cluster shares, where shifters and shares refer to those in the instrumental variable. Mean-independence holds if the factors driving the trade shock between China and the U.S. do not affect the domestic politics of European countries. The other three assumptions are not discussed in Colantone and Stanig (2018). The identical expectation and cross-cluster independence assumptions will depend on the way shifters are manipulated. Borusyak, Hull and Jaravel (2022) residualize the scaled import flow over ten-year period fixed effects and cluster shifters at an equivalence of the NUTS-2 level. I assume the following:

$$\frac{\Delta \text{Import}_{\text{US}jt}}{L_{cj(t-2)}} = u_{cj} + v_t + \eta_{cjt} \tag{4}$$

where u_{cj} is the country-industry fixed effect, v_t is the time fixed effect, and η_{cjt} is the mean-zero incidental deviation in the shifter that is independent across industry j and time t. The residualization here is two-way due to the cross-national variation in the employment $L_{cj(t-2)}$. Since changes in the employment are slower than changes in the trade flow, the country-industry fixed effects can reasonably control for the baseline level of each shifter. The incidental term η_{cjt} is

[18] The two do not perfectly coincide mainly because of the share estimates. The original variable uses national employment statistics sourced from each country, while the reconstructed variable used employment statistics from Eurostat, which is noisier and more limited in coverage.

[19] Imputed variables predict missing employments with linear regression, while predicted variables replace all observed values with values predicted by linear regression.

expected to be independent across industry by the findings of Borusyak, Hull and Jaravel (2022), but not across country due to the common term $\Delta\text{Import}_{\text{US}jt}$. Given the industry-year shifter clusters, asymptotic negligibility holds if the aggregate European economy is not dominated by certain industries or if elections have occurred frequently enough during the given period.

In addition to the residualization, I propose shifter replacement to ensure the identical mean assumption. Since individual European economies are much smaller than the U.S., the shifters in this study tend to be more volatile than those in Autor, Dorn and Hanson (2013). Moreover, some countries barely have certain industries, causing the divide-by-zero problem in the shifters. This problem did not arise in the other paper as their sizes of their numerator and the denominator were comparable. I replace shifters with zero if the sum of the regional shares of a certain industry in the country is less than 0.03, or if $w_{cjt} = \sum_{r\in c} w_{rjt}$ is less than 0.03 with zero. 24% of the shifters were replaced, most of which is in the petroleum and nuclear fuel industry. Theoretical justification of shifter replacement is provided in Appendix D. Statistically, the replacement scheme better protects the identical expectation assumption from possible misspecification of shifter-level covariates and the measurement error from data reconstruction. Substantively, the replacement scheme limits the analysis to industries that align more closely with the structural model underlying the shift-share measure. Note that the scheme is not to replace all industry shocks with a national employment share of less than three percent.

Since efficient estimation of η_{cjt} requires more shifters than those included in the shift-share regression, I develop a new method to blend the two components. Details can be found in Appendix E. repeat the procedure. replace. residualize. estimate using an original standard error estimator that is asymptotically equivalent but more efficient in this setting. I use a standard error estimator and the first-stage F-statistics in Propositions E.2 and E.3.

5.3 Empirical Evaluation of Assumptions

To test the shifter independence conditions, I include all shifters from the countries and time period of interest regardless of whether elections occurred in a given country and year.[20] Table 3 reports shifter summary statistics. The odd columns use replaced shifters and the even columns use residualized replaced shifters.[21] Shifters are centered around zero before residualization, and they

[20] This is to secure test power, but might be problematic if election timing is correlated with the shifters. However, even if election timing is a function of trade shocks in parliamentary countries, the assumptions would not be violated as long as the function is not time-varying and does not depend on the past shifters.

[21] Shifters were weighted by their aggregate share in the process of residualization.

Table 3: Shifter Summary Statistics

	(1)	(2)	(3)	(4)
	Imputed US Shifters		**Predicted US Shifters**	
Mean	6.808	0	6.827	0
Standard deviation	141.881	66.99	141.248	64.971
Specification				
Residualized	F	T	F	T
Replaced	T	T	T	T
SSE		0.242		0.23
Autocorrelations				
1-year	0.882	0.609	0.91	0.665
	(0)	(0.019)	(0)	(0.005)
2-year	0.768	0.153	0.815	0.224
	(0)	(0.581)	(0)	(0.383)
Intra-class correlations				
Country	0.117	0.035	0.131	0.054
	(0.03)	(0.018)	(0.039)	(0.025)
Industry	0.394	0.278	0.403	0.284
	(0.056)	(0.053)	(0.046)	(0.046)

Note: Parentheses indicate p-values for autocorrelations and bootstrapped standard errors for intra-class correlations (ICC) defined as the length of the 95% confidence interval divided by 1.96×2. Mean and standard deviation are weighted by shifter shares w_{cjt}. Columns (2) and (4) use residuals of shifters over country-industry and year fixed effects. SSE is the residual variance compared to raw shifters. Autocorrelations report correlations between shifters one or two years apart. ICC report adjusted random effects on country-year or industry-year indicators. Shfiters are replaced by zero if w_{cjt} is smaller than 0.03.

show a high variance even after residualization. The standard deviation of residualized shifters is around three times of that in Autor, Dorn and Hanson (2013), as expected from the relative size of economies of the U.S. and the European countries. Residualization retains around 24 percent of the variation.

The lower panel tests the dependence among shifters. Autocorrelations measure the correlation between shifters at year t and $t-1$, or year t and $t-2$. Intra-class correlation coefficients (ICC) estimate the following unweighted random-effect models:

$$\text{Unresidualized}: \quad D_{cjt} = \alpha + \beta_{ct} \times \mathbb{I}_{ct} + \gamma_{jt} \times \mathbb{I}_{jt} + \varepsilon_{cjt}$$

$$\text{Residualized}: \quad D_{cjt} = \alpha + (\beta_{ct} \times \mathbb{I}_{ct} \text{ or } \gamma_{jt} \times \mathbb{I}_{jt}) + \delta_{cj} \times \mathbb{I}_{cj} + \nu_t \times \mathbb{I}_t + \varepsilon_{cjt}$$

where $\mathbb{I}$ are indicators and all random terms are normal.[22] Large coefficients imply dependence within the groups and suggest the need for clustering. The results align with our specification. Residualized shifters are uncorrelated at least two years apart, and exhibit little ICCs across country-year but high ICCs across industry-year.[23] One-year autocorrelations are significant because ΔUS Import$_{jt}$ and ΔUS Import$_{j(t-1)}$ both contain the import change from year $t-2$ to year $t-1$. Since only one pair of elections in the sample was held one year apart in the same country, 2002 and 2003 in the Netherlands, I drop the 2002 election from the data.

Table 4: Shifter- and Unit-level Placebo Test

Variable	**Estimate**	**SE**	**Obs**
Shifter-level:			
Initial % of national industry employment	−0.03***	(0.012)	1370
Unit-level:			
Initial % of foreign-born population	0.008	(0.015)	321
Initial % of high-skilled workers	0.658	(0.999)	335
Initial % of high-technology workers	0.121	(0.248)	335
Initial % of medium- or low-skilled workers	−0.062	(0.469)	335
Initial % of medium- or low-technology workers	1.181	(1.459)	335
Initial % of workers in primary sectors	−1.265	(1.334)	335
Initial % of service industry workers	2.683	(3.529)	335

Note: The upper panel tests if all shifters in Table 3 predict pre-shock shifter-level variations using OLS in the inverted regression. The lower panel tests if shifters that correspond to elections in the sample predict pre-shock unit-level variations, with shifters instrumenting regional shocks. Covariates for the shifter-level test are industry-country and year fixed effects. Covariates for unit-level tests are regional-level country-year fixed effects following the original paper, plus newly added aggregated shifter-level fixed effects. All independent variables are normalized to variance 1. Standard errors are clustered by industry-year pair. Imputed values are used whenever necessary. *p<0.1; **p<0.05; ***p<0.01

Table 4 tests the shifter mean-independence. The upper panel tests if shifters predict pre-shock shifter-level variations, and the lower panel tests if shifters corresponding to the elections predict pre-shock unit-level variations. Economic placebos being tested are initial national employment shares by industry and initial worker composition by region. Political placebos could also be considered, but they must come from subnational variation due to the residualization at the country-industry level.[24] These variables can affect the election outcomes independently of the trade shock and proxy the error term in the inverted regression that the shifters are intended to instrument. All specifications use residualized shifters, or equivalently control for industry-country and year fixed

[22]Only one of β_{ct} and γ_{jt} is treated random and the other is fixed in the unresidualized model due to the insufficient number of years. The residualized model includes only one of β_{ct} and γ_{jt} due to collinearity, and δ_{cj} and ν_t are fixed.

[23]The apparent statistical significance of country-year ICCs is due to the asymmetric confidence intervals.

[24]Pre-shock political leaning or nationalism score would be some examples.

effects. Results do not reject the null hypotheses that instruments are uncorrelated with pre-shock variables, except in the case of the shifter-level test. The strong significance in the shifter-level test might be an artefact of data availability. The true pre-shock period should be before 1988 when China had hardly entered international trade, but the placebo variable relies on 1999 employment shares.[25] Unit-level placebo tests use pre-1988 variables. The shifter-level test is presented for illustrative purposes and should not be substantively interpreted much. Table F.2 reports similar results with unreplaced shifters.

Table 5: Cluster Share Distribution

Max (share)	**Max (share squared)**	**1/HHI**
0.03	0.07	58.18

Note: Max (share) and max (share squared) are bounded between 0 and 1, and the HHI is bounded above by the number of clusters. Shares matching replaced shifters are included, but the complementary share is excluded. All metrics use imputed shares.

To assess the share negligibility assumption, I focus on regional employment shares that match with the election data and calculate cluster shares. The total number of clusters is 98. Define cluster shares $w_{jt} = \sum_j w_{cjt}$. Table 5 presents share summary statistics. The first and second columns measure the ratio of the largest w_{jt} (or w_{jt}^2) and the sum of w_{jt} (or w_{jt}^2). These two metrics show as how negligible the largest cluster is and should be as close to zero as possible (Adão, Kolesár and Morales 2019). The third column measures the inverse of the sum of each cluster's relative size squared, or the inverse of their Herfindahl Index (HHI). This shows the effect sample size in the inverted regression, so it should be as large as possible (Borusyak, Hull and Jaravel 2022). All metrics indicate that resulting estimates will be consistent and asymptotically valid.

5.4 Replicated Results

Given the absence of strong evidence failing falsification tests, I replicate the main results using the new shift-share estimator. Regression specifications differ from the original paper in two ways. First, election outcomes are averaged by NUTS-2 region for congruence between the unit of analysis and the unit of shifters. Second, shifter-level controls are added to satisfy the identical mean assumption.

Table 6 summarizes the main results. The dependent variables are the median and weighted

[25] The mean and standard deviation of the initial national industry share are 1.09% and 0.84%, so the effect size can be substantially significant as well.

Table 6: Effects of Chinese Imports on Party Nationalism

	(1)	(2)	(3)	(4)	(5)	(6)
		Median			**Center of Gravity**	
Cluster	0.765***	0.646**	3.636	0.311***	0.317***	−0.735
	(0.277)	(0.264)	(7.227)	(0.113)	(0.117)	(2.134)
Obs	3006	295	295	3006	295	295
F	1528.07	174.28	1.82	1528.07	174.28	1.82
BHJ	-	0.646***	3.636	-	0.317***	−0.735
	-	(0.207)	(10.981)	-	(0.098)	(2.544)
Obs	-	335	335	-	335	335
F	-	67.46	0.13	-	67.46	0.13
Unit of Analysis	District	Region	Region	District	Region	Region
Shifter controls	F	F	T	F	F	T
Country-Year FE	T	T	T	T	T	T

Note: Cluster reports 2SLS estimates with standard errors clustered by NUTS-2 region-year pair. BHJ reports inverted regression estimates with standard errors clustered by industry-year pair per Borusyak, Hull and Jaravel (2022). Column (1) and (4) estimate the electoral district-level regression as in the original paper with 2001-2007 election data. Column (2) and (5) convert the election data to the NUTS-2 region level by taking the simple average of electoral outcomes. Column (3) and (6) add shifter-level controls aggregated to the regional level. All specifications use transformed shifters and imputed shares. *p<0.1; **p<0.05; ***p<0.01

average of nationalism scores in each electoral district. Estimates measure how much an extra unit of Chinese import drove parties towards nationalism. The cluster row uses the same 2SLS estimators as in the original paper where standard errors are clustered by NUTS-2 region-year pair. The BHJ row uses inverted regression estimators following Borusyak, Hull and Jaravel (2022), with standard errors clustered by industry-year pair. A comparison between columns (1) and (2) and between columns (4) and (5) suggests that the results are not much affected by aggregation to the district level. However, BHJ estimator reports much smaller effects and larger standard errors. The difference in points estimates are due to the effect heterogeneity. While the other estimator by Adão, Kolesár and Morales (2019) would preserve the point estimate, it is not applicable in this example since the number of shifters is larger than the number of units. Larger standard errors reflect the endogeneity problem between shares and electoral outcomes. This suggests that regions with the similar industrial composition have correlated electoral outcomes unaccounted by trade shocks, and NUTS-2 region-year clusters fail to capture this correlation by treating them as independent units.

Columns (3) and (6) control for shifter-level fixed effects aggregated to the unit level, in addition to the unit-level region-year fixed effects in the original specification. Our residualization strategy specifies that shifters are only comparable when partialled out by country-industry and time fixed effects. These controls reflect the initial share of each industry and the total manufacturing share in the region when aggregated.[26] A comparison between columns (2) and (3) and between columns (5) and (6) indicates that the results are not robust to the addition of these new control variables either.[27] In the shifter exogeneity framework, the shift-share estimate balances out the error terms in the inverted regression (B.4) under the assumption that shifters are independent and have the identical mean. The inconsistent results with respect to residualization suggest that the original results might have been driven by a group of large, correlated shifters. The shift-share variable does not capture the correct impact of shifters if different means are not adjusted for. Also, there is no way to account for dependence among shifters in conventional regression methods. Raw shifters are expected to have different means across country due to varying economic sizes, and would be highly correlated within industry as they share the same US imports as a component. Table F.3 shows that the results are similar when predicted shares or raw shifters are used instead of imputed shares or transformed shifters.

5.5 Discussion

This section finds that the replication data meets assumptions required in the shifter exogeneity framework but the results are not robust either to the asymptotically correct standard error estimator or shifter residualization. The only methodologically valid estimates are those in columns (3) and (6) and BHJ row, and they suggest that the original findings are not well supported from the causal perspective. This replication exercise makes two original contributions. First, the residualization and clustering strategy proposed here is more sophisticated compared to Autor, Dorn and Hanson (2013) that has an almost identical shift-share design. This is due to the complexity of the data structure that has three-way correlations compared to two-way in the original China shock paper. Second, I propose a shifter transformation scheme that trims noisy outlier shifters that do not much affect the shift-share estimate. Shifters in this example involve division by small numbers that are not precisely observed. The scheme protects shifters from misspecifications, hence from erroneously rejecting the null hypothesis regarding the identical mean and independence assumption.

[26]The controls are not collinear by design. Each aggregated country-industry fixed effect contains initial shares of the industry in the country across all years, while each aggregated time fixed effect contains all total manufacturing shares in the year across all countries and industries.

[27]Note that this comparison is not affected by measurement errors caused by share imputation.

6 Conclusion

Having started as an imputation scheme, shift-share designs are now understood more generally as designs that involve inner-product variables between endogenous and exogenous variables. This article reviews how shift-share designs have been used in economics and political science from a methodological standpoint. Shift-share variables can measure factual unobservables using shares and observable shifters, or counterfactual unobservables using initial shares and counterfactual shifters such as the average value of the sample units or values from external units. Shift-share variables based on the propensity score are a statistical construct to enhance the relevance of instruments to the independent variable, but share the same statistical properties as other shift-share variables. This article synthesizes those statistical properties with unified notation and framework. Share exogeneity assumes units are comparable and independent similarly, but not entirely identical, to units in difference-in-differences. Shifter exogeneity assumes shifters are comparable and independent so that errors balance out as in the law of large numbers.

Political science articles are increasingly adopting shift-share designs, but they rarely discuss or are fully aware of the required identifying assumptions. This article exemplifies how to assess those assumptions and how the new methods can affect results with a political science example. In the replication exercise, this article further proposes new methodological techniques that help to verify strict identifying assumptions that are otherwise not possible. Findings were compromised by either newly suggested standard error estimators or shifter residualization, each indicating different identification issues underlying the original design. This article finds that despite complexities in shift-share designs, they still have significant potential in political science, particularly in American Politics where they are underutilized.

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A Shift-share Articles in Political Science

Table A.1: List of shift-share articles in political science journals

Paper	Topic	Cited	Type	Shift-share variable
Feigenbaum and Hall (2015)	trade shock	N		"Specifically, following Autor et al. (2013a), we define import exposure per worker as..."
Colantone and Stanig (2018*a*)	trade shock	N		"...Autor, Dorn, and Hanson (2013) derive an empirical measure of regional exposure to the Chinese import shock from a supply perspective... We employ the same empirical approach."
Colantone and Stanig (2018)	trade shock	N		"To this purpose, we build a region-specific indicator for the exposure to Chinese imports following the methodology introduced by Autor, Dorn, and Hanson (2013)."
Thewissen and Van Vliet (2019)	trade shock	N		"For our measure of exposure to Chinese import competition, we... measure this as the value of the total imported goods as a share of the value added for sector i in country j in year t."
Ballard-Rosa et al. (2021)	trade shock	N		"We then constructed measures of local labor market exposure to import competition equal to the change in Chinese import exposure per worker in a TTWA with imports weighted in the TTWA by its share of national employment in a given industry (Autor et al., 2013)."
Kim and Pelc (2021)	trade shock	N		"By leveraging geographical variation in industry specialization and national-level variation in Chinese imports in the industry, they capture the exogenous shock from China to the local economy."
Milner (2021)	trade shock	N		"I follow Autor et al. (2013), Colantone and Stanig (2018b), and others in defining the globalization shocks as..."
Ballard-Rosa, Jensen and Scheve (2022)	trade shock	N		"[W]e define local labor market shocks as the average change in Chinese import penetration in the commuting zone's industries, weighted by each industry's share in the commuting zones's initial employment."
Ferrara (2023)	trade shock	Y	share	"[W]e replicate the methodology developed by Autor et al. (2013)... we construct an index of exposure to import competition by US commuting zone."

Hosek, Peritz et al. (2022)	trade shock	N		“To estimate the trade stakes of a district, we used imports and exports across manufacturing and other commodity industries weighted by employment in each industry at the district level.”
Scheve and Serlin (2023)	trade shock	Y	share	“Although our measure of imports per worker is computed according to a shift-share formula, our identification strategy does not rely on the use of exogenous variation in the form of exports from Germany to a third party.”
Vall-Prat (2023)	trade shock	N		“To account for economic grievances, I measure constituencies’ exposure to the colonial trade shock using an indicator similar to the shift-share instrument developed by Autor et al. (2013).”
Dür, Huber and Stiller (2024*a*)	trade shock	N		“Furthermore, they aggregated the weighted competitiveness of all industries to obtain a single value that expresses the overall trade competitiveness of this region.”
Dür, Huber and Stiller (2024*b*)	trade shock	N		To measure subnational trade competitiveness, following the approach outlined in detail in Huber, Stiller and Dür (2023), we first calculate a country’s comparative advantage at the industry group level.”
Meyerrose and Watson (Forthcoming)	trade shock	N		“The intuitive idea behind this approach is that local labour markets are differentially affected by the growth in imports from low-wage countries depending on their prior industry specialization.”
Schöll and Kurer (2024)	technology shock	N		“...[I]dentification stems from a shift-share approach, where we use pre-sample-period local employment composition to estimate the exposure to new technologies in a time-varying fashion.”
Finseraas and Nyhus (Forthcoming)	technology shock	N		“The idea is that industry growth would happen in those municipalities that already had investments in the industry. This constructed growth of the industry can be used as an instrument for actual growth.”
Finseraas, Røed and Schøne (2020)	immigration/ migration	N	share	“[W]e construct a predicted immigrant inflow by distributing all incoming immigrants to the BaC industry as if the initial licensing share of each trade completely determines the allocation of the incoming immigrants.”

Fouka and Tabellini (2022)	immigration/ migration	Y	share	"The instrument assigns decadal immigration flows from Mexico between 1970 and 2010 to destinations within the US proportionally to the shares of Mexican immigrants who had settled there in 1960, prior to the change in the immigration regime introduced in 1965."
Lim (2023)	immigration/ migration	N		"I construct the instrument for regional emigration rates in Poland by interacting the unemployment rates in the United Kingdom (the exogenous pull factor) and the past emigration rates of each region in Poland before the EU accession."
Dipoppa (Forthcoming)	immigration/ migration	Y	share	"I instrument migration using a shift-share instrument and I exogenously predict the shift component by leveraging droughts in the south of Italy as push factor for migration to the north."
Smoldt, Mueller and Thies (Forthcoming)	immigration/ migration	N		"We substitute a Bartik (1991) shift-share instrument for ours. In particular, we hold exposure to vote shares as constant either in a single pre-treatment year or for a portion of the pre-treatment period."
Xu (Forthcoming)	immigration/ migration	Y	share	"[D]rawing on the literature for estimating the effect of the Great Migration of Blacks to northern cities in the United States, I develop a shift-share instrumental variable of predicted migration of the rural poor to cities in Brazil and to neighborhoods in São Paulo."
Stubbs et al. (2020)	capital movement	N	share	"Specifically, our instrument is the interaction of the within-country average of the number of conditions across the period of interest with the year-on-year IMF's budget constraint."
Brännlund (2022)	capital movement	Y	share	"I define market-risk exposure as the total value of risky assets in district i during the year 1999, divided by the value of total assets in the same district, multiplied by the change in the VIX index in period t."
Gavin and Manger (2023)	capital movement	Y		"Capital flows could be endogenous... We calculate the country's share of global net capital flows and exclude the country's immediate neighbors... again scaled to country GDP."
Kern, Reinsberg and Lee (Forthcoming)	capital movement	Y	shifter	"Our shift-share instrument is the multiplicative interaction between the number of countries under programs and the long-run probability of a country being under IMF programs."

Raess and Wagner (Forthcoming)	capital movement	N		"We leverage the share of inward FDI from HICs in each DC in our sample in the year prior to the start of our panel (i.e., 2000) interacted with the difference between the GDP growth rate of each country and the average GDP growth rate in Europe."
Ahmed (2016)	foreign aid	N		"The instrument interacts the legislative fragmentation of the U.S. House of Representatives with the probability a country receives U.S. aid in any year."
Ziaja (2020)	foreign aid	N		"I follow recent suggestions to interact exogenous variables on the donor side with endogenous recipient properties in order to increase cross-sectional variation."
Baccini and Weymouth (2021)	employment	Y	share	"Since layoffs are not randomly assigned, we develop an instrumental variables strategy using shift-share methodology (Bartik 1991) derived from national layoff shocks, weighted by initial county-level employment."
Dehdari (2022)	employment	Y	share	"I supplement the OLS analysis with an instrumental variable (IV) approach using a Bartik instrument that predicts the number of layoff notices by the national trends in notices within each industry, and the sectoral composition in each election precinct."
Bisbee and Rosendorff (2024)	employment	Y	share	"I supplement the OLS analysis with an instrumental variable (IV) approach using a Bartik instrument that predicts the number of layoff notices by the national trends in notices within each industry, and the sectoral composition in each election precinct."
Baccini and Sattler (2023)	austerity	N		"The key coefficient of interest is β, which estimates the interaction term between the two main independent variables. It reflects how the impact of national-level austerity measures varies across districts with different degrees of economic vulnerability."
Carreri and Dube (2017)	natural resources		share	"We assess whether changes in the international oil price exert differential impacts among municipalities that produce more oil. Our cross-sectional variation is oil dependence, defined as the value of oil produced in per capita terms in 1993."

B Technical Details of Section 3

B.1 Share Exogeneity: Exogenous Shares

Goldsmith-Pinkham, Sorkin and Swift (2020) consider the following panel setup where X_{it} has a constant linear effect on the dependent variable Y_{it}, and the instrument variable Z_{it} has endogenous shifters and exogenous shares:

$$\begin{aligned} Y_{it} &= \beta X_{it} + \gamma^\top \Pi_{it} + \varepsilon_{it} \\ Z_{it} &= \sum_{j=1}^{m} w_{ij0} D_{jt} \end{aligned} \tag{B.1}$$

where t denotes time period $t = 1, 2, \cdots, T$ and Π_{it} is a vector of control variables.[1] With shifters D_{jt} being fixed, unit-specific variables are *i.i.d.* across units, but not across time within the same unit. Therefore, the model accounts for any correlation structure in shifters D_{jt} and any temporal dependence in ε_{it} but no geographical dependence. Note that Z_{it} uses pre-treated shares w_{ij0} to ensure exogeneity as shifters might affect shares over time.[2] The authors' core observation is that instrumenting X_{it} with Z_{it} is algebraically equivalent to instrumenting X_{it} with $w_{i10}, \cdots, w_{im0}$ using a particular weights.[3] This implies that the shift-share design is valid if shares meet the conditions typically required for instrument variables. Before moving on, I point out that post-treated shares are unlikely to be exogenous in the panel OLS regression. This is because if X_{it} comprises time-varying shifters and shares, the shares w_{ijt} would be affected by previous shifters $D_{j0}, \cdots, D_{j(t-1)}$. However, in case where shares happen to be exogenous or the data contains a single period, setting $Z_{it} = X_{it} = \sum_{j=1}^{m} w_{ijt} D_{jt}$ and instrumenting X_{it} with Z_{it} is algebraically equivalent to running an OLS regression of Y_{it} on X_{it}.

The equivalence result is illustrated in Section I of the original paper. I build intuition through a simple example. Suppose that there are two industries and one time period and the model has

[1] The original paper builds on Bartik regression in which shocks D_{ijt} are decomposed into common components D_{jt} and idiosyncratic components $\tilde{D}_{ijt}$ (*e.g.* $\mathbb{E}[\tilde{D}_{ijt}] = 0$, so $D_{jt} = \bar{D}_{\cdot jt}$) and $X_{it} = \sum_{j=1}^{m} w_{ijt} D_{ijt}$, but here we discuss general shift-share designs.

[2] Borusyak, Hull and Jaravel (2022) note that fixing shares at their initial values can potentially weaken the instrument strength as $T \to \infty$.

[3] Since the first-stage relationship between X_{it} and initial shares w_{ij0} will vary over time, we need $m \times T$ instruments (initial shares interacted with time periods) in total where T denotes the total number of periods.

no control variables. Then, the first-stage equation is

$$X_i = \delta Z_i + \eta_i = \delta D_1 w_{i1} + \delta D_2 w_{i2} + \eta_i.$$

Instrumenting X_i on Z_i is equivalent to instrumenting X_i on w_{i1} and w_{i2} if the ratio of coefficients of w_{i1} and w_{i2} is restricted to D_1/D_2. If we take shares for instruments, the moment condition is $\hat{\mathbb{E}}[w_{i1}\varepsilon_i] = \hat{\mathbb{E}}[w_{i2}\varepsilon_i] = 0$, or

$$\sum_{i=1}^{n} w_{i1}(Y_i - \hat{\beta}X_i) = \sum_{i=1}^{n} w_{i2}(Y_i - \hat{\beta}X_i) = 0.$$

Since the number of equations is greater than the number of variables, we minimize the weighted average of moment functions. If shifters D_1 and D_2 are given as weights,

$$\begin{aligned}&\operatorname*{argmin}_{\beta}\left[D_1\sum_{i=1}^{n} w_{i1}(Y_i - \beta X_i) + D_2\sum_{i=1}^{n} w_{i2}(Y_i - \beta X_i)\right]^2\\ =&\operatorname*{argmin}_{\beta}\left[\sum_{i=1}^{n}(\underbrace{D_1 w_{i1} + D_2 w_{i2}}_{Z_i})(Y_i - \beta X_i)\right]^2,\end{aligned}$$

which is the moment condition when Z_i instruments. This shows that choosing shifters for GMM weights can restrict the ratio of coefficients of share instruments and recover the shift-share instrument estimate, though those weights will be generally suboptimal.[4]

Consistency of the shift-share estimate follows if the share instruments are valid. Two conditions are required. First, they must have non-zero correlation with the independent variables X_{it} conditional on controls Π_{it} for each t. Second, they must be exogenous to the dependent variable, *i.e.* $\mathbb{E}[w_{ij0}\varepsilon_{it} \mid \Pi_{it}] = 0$ for all i, j, t whenever the shifter D_j is non-zero. This exclusion restriction is analogous to the random assignment (or parallel trends) in difference-in-difference designs where control units and treated units are identical except for the treatment status. Here, common shocks D_j affect all units but units cannot select into their degree of exposure conditional on control variables.[5] This allows us to attribute any difference in outcomes to the effect of the varying degrees of exposure to shocks and not to anything else.

One concern is that shares are often equilibrium outcomes in which the dependent variable

[4]The resulting estimate will exhibit a higher variance than when using optimal weights determined by the data.

[5]Researchers might want to consider controlling for aggregate shares. For example, if w_{ij0} denote initial 4-digit code industry shares, initial 1- or 2-digit code industry shares of the location are likely to predict both 4-digit code industry shares and the dependent variable.

is simultaneously determined as in the case of Autor, Dorn and Hanson (2013), so they would not be exogenous in many cases. The authors recommend using first differences in the outcome of interest instead of its levels to address the problem. According to Adão, Kolesár and Morales (2019), however, $\mathbb{E}[w_{ij0}\varepsilon_{it} \,|\, \Pi_{it}]$ might not be zero even when first differences are used. This shows that identifying assumptions always must be justified in light of theories. In response, the authors identify several study designs that implicitly use the share exogeneity framework. Researchers likely invoke share exogeneity when they highlight the similarity among units apart from their differential exposure to common shocks. Alternatively, they do so when the emphasis is not on the multiplicity of industries but on a two-industry example or shocks to specific industries. This is because identification under the assumption of shock exogeneity requires a large number of shifters.

The authors further propose diagnostic tests to assess the share exogeneity assumption. First, researchers can examine if variables thought to affect the dependent variable via ε_i in equation (B.1) also predict share instruments. These variables would be correlates of local supply shifters in the settings of Bartik (1991). A significant association between the variables and shares conditional on controls indicates imbalance of shares between different groups of units, suggesting endogeneity between the shift-share variable and the dependent variable. Second, researchers can test whether the data exhibit pre-trends if variations in shifters started affecting the dependent variable from a certain time period onward. The dependent variable in such cases should have little correlation with individual share instruments as well as the shift-share variable. Third, researchers can exploit multiple instruments. As the shift-share instrument is one specific way to combine individual share instruments, the estimate should not differ significantly when shares are individually instrumented using 2SLS or alternative methods if the model is well-specified. Overidentification tests are a formal way to do this, but researchers must be aware of various assumptions underlying the different alternative methods (Section V.C).

When there are multiple instruments, not all of them influence the final estimate equally. Let $\hat{\beta}_j$ be the estimate instrumented with only the jth shares. It is known that for some constants $\hat{\alpha}_j \in \mathbb{R}$ that sum to 1,

$$\hat{\beta}_{\text{shift-share}} = \sum_{j=1}^{m} \hat{\alpha}_j \hat{\beta}_j.$$

$\hat{\alpha}_j$ are called Rotemberg weights. This implies that the shift-share estimate is most influenced by shares with the largest absolute weights, and researchers must be ready to defend exgoeneity of these shares more than others. The authors suggest presenting diagnostic test results with respect to these key shares along with results with respect to the shift-share instrument.[6]

Finally, the above decomposition also provides insight into performance of the shift-share estimate in the presence of effect heterogeneity. Suppose the true model is

$$Y_{it} = \beta_i X_{it} + \gamma^\top \Pi_{it} + \varepsilon_{it}$$

so that the independent variable has unit-specific effects β_i. We can show that $\hat{\beta}_j$ converges in probability to a convex combination of β_i if the true effect of the jth share instrument w_{ij0} on the independent variable X_{it} is positive across i or negative across i. However, this does not guarantee that $\hat{\beta}_{\text{shift-share}}$ converges in probability to a convex combination of β_i as well since Rotemberg weights can be negative. How can researchers tell that $\hat{\beta}_{\text{shift-share}}$ is a convex combination of β_i when Rotemberg weights are not consistently positive? We cannot estimate individual weights on β_i since weights in $\hat{\beta}_j$ are not estimable. Variation in $\hat{\beta}_j$ instead provides some suggestive evidence. $\hat{\beta}_j$ assign different weights on β_i, so little variation in $\hat{\beta}_j$ suggests similar weights across units and negative Rotemberg weights in this case do not cause the negative weight problem. If $\hat{\beta}_j$ vary a lot, however, they can check if the patterns accord with researchers' substantive knowledge and then probe how shares with large negative Rotemberg weights might create negative weights in the final estimate. Regarding diagnostic tests under effect heterogeneity, overidentification tests might fail since $\hat{\beta}_j$ can vary even when shares are exogenous. The other two tests remain valid.

B.2 Shifter Exogeneity: Many Exogenous and Independent Shifters

Adão, Kolesár and Morales (2019) and Borusyak, Hull and Jaravel (2022) take the exact opposite approach by treating shifters D_j random and everything else non-random conditional on shifters. The baseline models of the two papers differ in whether the shift-share variable appear as the independent variable or the instrumental variable, but this does not impact identifying assumptions much as pointed out in the previous section. We consider the following shift-share OLS regression

$$\begin{aligned} Y_i &= \beta X_i + \gamma^\top \Pi_i + \varepsilon_i \\ X_i &= \sum_{j=1}^{m} w_{ij} D_j, \quad \sum_{j=1}^{m} w_{ij} = 1. \end{aligned} \tag{B.2}$$

[6] `R` and `Stata` packages for Rotemberg weights can be found at https://github.com/paulgp/bartik-weight.

We create a fictitious share $w_{i(j+1)} = 1 - \sum_{j=1}^{m} w_{ij}$ and a shifter $D_{i(j+1)} = 0$ for each i if the original sum is smaller than one. The goal is to study the asymptotic properties of $\hat{\beta}$ as the number of shifters m grows when D_j is repeatedly sampled with Π_i, ε_i, w_{ij} held constant. Studying conditional limiting distributions allows not only shares to be endogenous to the dependent variable but units also to have geographical dependence.[7] The authors find that the shift-share estimate is consistent if shocks are as-good-as-randomly assigned and small enough as the number of shocks grows, but the conventional robust and clustered standard error estimators can vastly underperform.

Adão, Kolesár and Morales (2019) observe that shares w_{ij} can introduce a dependence between the independent variable X_i and the error term ε_i that is not captured by typical clusters (Section III). Under the settings of Autor, Dorn and Hanson (2013), this means that economic zones with similar industry portfolio will have correlated errors even if they belong to different states. Shares likely create dependence when the dependent and independent variables are jointly determined by shares, causing the error term to contain shares as well. Taking first differences of the dependent variable does not solve the problem since first differences of shift-share variables are still shift-share variables. Fixing ε_i allows us to work around the complex dependence structure that arises in shift-share equilibria.[8]

Borusyak, Hull and Jaravel (2022) find a similar equivalence result as above under shifter exogeneity. Ignoring control variables Π_i for now, the regression model (B.2) is consistent if $\sum_{i=1}^{n} X_i \varepsilon_i / n$ converges to zero in probability. Observe that

$$\frac{1}{n}\sum_{i=1}^{n} X_i \varepsilon_i = \frac{1}{n}\sum_{i=1}^{n}\sum_{j=1}^{m} w_{ij} D_j \varepsilon_i = \sum_{j=1}^{m} \frac{w_j}{n} D_j \frac{\sum_{i=1}^{n} w_{ij}\varepsilon_i}{w_j} \tag{B.3}$$

where $w_j = \sum_{i=1}^{n} w_{ij}$, so $\sum_{j=1}^{m} w_j = n$.[9] If we rearrange equation (B.2) into the shifter level as

$$\frac{\sum_i w_{ij} Y_i}{w_j} = \beta \frac{\sum_i w_{ij} X_i}{w_j} + \frac{\sum_i w_{ij}\varepsilon_i}{w_j}, \tag{B.4}$$

the last sum in equation (B.3) is the moment condition of the regression model (B.4) instrumented

[7] Temporal dependence is covered too when extended to panel settings, but spillovers are not.

[8] The authors introduce two possibly correlated but distinct shares for labor demand and labor supply each, further complicating the dependence structure in error terms.

[9] Goldsmith-Pinkham, Sorkin and Swift (2020) view $\sum_{i,j} w_{ij} D_j \varepsilon_i = 0$ as shares w_{ij} being invalid instruments, each of which has a non-zero covariance with the error term but those covariances average out (Kolesár et al. 2015).

with D_j and weighted by w_j/n:

$$\sum_{j=1}^{m} \underbrace{\frac{w_j}{n}}_{\text{weight}} \times \underbrace{D_j}_{\text{instrument}} \times \underbrace{\frac{\sum_{i=1}^{n} w_{ij}\varepsilon_i}{w_j}}_{\text{error}} \xrightarrow{p} 0.$$

When does the above moment condition hold? The authors provide sufficient conditions. First, shifters are mean-independent and have the same mean, or $\mathbb{E}[D_j] = \mu$ for all j conditional on other fixed parameters. Mean-independence means that shifters are exogenous to the model in practice, not necessarily identically distributed, but the same mean might be too strict a condition. This can be relaxed by introducing control variables as will be discussed below. Mean-independence implies

$$\mathbb{E}\left[\sum_{j=1}^{m} \frac{w_j}{n} D_j \frac{\sum_{i=1}^{n} w_{ij}\varepsilon_i}{w_j}\right] = \mathbb{E}\left[D_j\right] \times \sum_{j=1}^{m} \frac{w_j}{n} \frac{\sum_{i=1}^{n} w_{ij}\varepsilon_i}{w_j} = 0.$$

Second, shifters are independent to one another and shares w_j become negligible as their number grows.[10] Intuitively, the variance of the moment condition is the sum of the variance of each term if shifters are independent, and each variance should be asymptotically negligible if so are weights:

$$\mathbb{V}\left[\sum_{j=1}^{m} \frac{w_j}{n} D_j \frac{\sum_{i=1}^{n} w_{ij}\varepsilon_i}{w_j}\right] = \sum_{j=1}^{m} \left(\frac{w_j}{n} \frac{\sum_{i=1}^{n} w_{ij}\varepsilon_i}{w_j}\right)^2 \mathbb{V}\left[D_j\right] \xrightarrow{p} 0.$$

This equivalence result shows that the shift-share point estimate equals the IV point estimate instrumented with shifters using particular weights. However, only the IV regression produces a valid standard error under the heteroskedasticity-robust estimator since unlike in the previous section, shifters no longer instrument the original regression model. Adão, Kolesár and Morales (2019) derive the consistency and asymptotic distribution of the OLS estimate under almost identical assumptions directly from equation (B.2).[11] For them, the asymptotic negligibility of weights w_j is an extension of the identifying assumption in the clustering literature that the size of each cluster must be asymptotically negligible. Both approaches yield the same estimator if the model contains no control variables.

Including controls under shifter exogeneity is a bit complicated since mean-independence must

[10] Consistency of the shift-share estimate holds when shifters are uncorrelated or weakly correlated with one another, but the asymptotic requires independence (Assumption B5). I focus on inference rather than consistency as most political science works are interested in inference beyond identification.

[11] $\max w_j^2/(\sum_j w_j^2) \to 0$ is assumed in both papers, but these authors assume $\max w_j/(\sum_j w_j) \to 0$ instead of $\max w_j \to 0$. The fictitious share is excluded in these share assumptions.

hold at the shifter level while many controls would be avilable at the unit level. We define a latent variable vector η_j and a fixed vector μ, and $\mathbb{E}[D_j \mid \eta_j] = \eta_j^\top \mu$ for all j conditional on other fixed parameters. However, the two papers differ in how the latent variables η_j are mapped to unit-level control variables. Borusyak, Hull and Jaravel (2022) assume that the control variable set Π_i contain the exact average of the latent variables weighted by shares: if we denote the kth element in η_j by η_{jk} and the kth control by Π_{ik}, $\sum_{j=1}^m w_{ij}\eta_{jk} = \Pi_{ik}$ for all i. Adão, Kolesár and Morales (2019) allow asymptotically negligible noises in the controls: for small noises u_{ik}, $\sum_{j=1}^m w_{ij}\eta_{jk} + u_{ik} = \Pi_{ik}$ for all i.[12] The two papers reach slightly different asymptotic distributions due to a difference in estimating procedures, but both are asymptotically valid under the assumptions in each paper.[13] Their setups imply that the sum of real shares has to be controlled for when shares are incomplete. Let us denote the fictitious share and shifter by index $m+1$, and define $\eta_j = 1$ for $1 \leq j \leq m$ and $\eta_j = 0$ for $j = m+1$. Since $\mathbb{E}[D_1], \cdots, \mathbb{E}[D_m]$ would not equal $\mathbb{E}[D_{m+1}] = 0$ in general, η_j distinguishes the fictitious shifter from the real shares. The corresponding control $\sum_{j=1}^{m+1} w_{ij}\eta_j = \sum_{j=1}^m w_{ij}$ is the sum of real shares for each unit. Further heterogeneity existing among the real shifters can be controlled with additional latent variables that take a value of zero for the fictitious share.

Nevertheless, control variables might not be able to capture all dependence among shifters, especially in panel settings where shifters may be serially correlated. We can cluster shifters in such a case. Analogously to the random effect model, Adão, Kolesár and Morales (2019) derive asymptotics under the assumptions that shifters are independent only across different clusters and the largest cluster size is asymptotically negligible. Clusters can be defined over both time and shifters in their model. Meanwhile, Borusyak, Hull and Jaravel (2022) cluster shifters by imposing temporal or cross-sectional dependence on the error terms of the shifter-level IV regression.[14] Another notable issue that might arise in panel settings is fixed effects. Any relevant control variables in addition to those that control for latent variables, including two-way fixed effects, are innocuous to identification and increase efficiency if the model is correct. However, unit-fixed effects in the unit-level regression cannot remove time-invariant components in shifters unless shares are invariant across time.

Researchers can run similar diagnostic tests to assess the shifter exogeneity assumption. Since

[12] $\sigma_k^2/n \to 0$ and $\sigma_k^2/\sqrt{\sum w_j^2} \to 0$ where $\sigma_k^2 = \sum_i \mathbb{E}[u_{ik}]^2$. The authors derive exact asymptotics under these assumptions, while Borusyak, Hull and Jaravel (2022) show that their standard errors are asymptotically conservative in the presence of any form of errors although consistency might not be guaranteed.

[13] `R` package `ShiftShareSE` and `R/stata` package `ssaggregate` compute these standard errors. `ShiftShareSE` also provides null-imposed standard errors, which are known to perform better in finite samples, or in the IV regression, when the shift-share instrument is weak.

[14] As a reminder, this procedure has nothing to do with the dependence structure in the error terms of the original OLS regression as the entire analysis has already been conditioned on all parameters but shfiters.

units and shifters are at the different level, balance tests can be done at both the unit level and the shifter level. Unit-level balance tests ask if variables that are thought to affect the dependent variable via ε_i in equation (B.2) predict the shift-share variable X_i. Shifter-level balance tests ask if variables that are thought to affect the shift-level dependent variable via the error term in equation (B.4) predict shifter instruments D_j, where shocks must be weighted by w_j. Clustering can be applied likewise when shifters have remaining dependence after conditioned on latent variables. Pre-trend tests are also available if shifters started affecting the dependent variable from a certain time period onward. On the other hand, overidentification test is not possible unless the model contains two or more shift-share instruments as the equivalence result shows the same number of endogenous variables and instruments.

The authors lastly discuss three additional issues. First, consider data generated by the following heterogeneous effect model:

$$Y_i = \sum_{j=1}^{m} \beta_{ij} w_{ij} D_j + \gamma^\top \Pi_i + \varepsilon_i.$$

If we use the homogeneous effect model (B.2) and (B.4) for estimation, $\hat{\beta}$ in each model converges to different convex combinations of β_{ij}. While standard errors will be conservative, these estimates might not be quantities of interest as neither is a convex combination of treatment effects, which are $w_{ij}\beta_{ij}$, not β_{ij}. Different estimates under the two estimators might suggest effect heterogeneity and hence a threat to inference. Second, the above arguments hold when we consider shift-share IV regression instead of shift-share OLS regression. The equivalence result holds if we replace X_i with the instrumental variable Z_i in equation (B.3) so that the shifters in the instrument variable are as-if random. If we do not use the equivalence result, the estimation procedures and inference parallel those of the regular IV estimator.

Third, in the shift-share IV settings, sometimes the shifter D_j has to be estimated instead of directly observed. For example, Bartik (1991) assumes that the local employment share is the sum of the national labor demand shocks and the local labor supply shocks, and the national employment share proxies the national labor demand shocks since the local labor supply shocks would average out. This can be formally presented as

$$D_{ij} = D_j + v_{ij}$$
$$\hat{D}_j = \sum_{j=1}^{m} w_{ij} D_{ij}$$

where D_{ij} is the observed local employment share that contains a national shock D_j and local shocks v_{ij}. One concern is that since D_{ij} is endogenous to local shocks, the instrument might be endogenous to the jth independent variable X_j if it contains D_{ij} in it. The authors find that consistency holds if the number of units is much larger than the number of shifters, but if it is not the case, the leave-one-out estimator ($\hat{D}_{j,-k} = \sum_{j \neq k} w_{ij} D_{ij}$ and the instrument $Z_k = \sum_j w_{kj} \hat{D}_{j,-k}$ for unit k) performs better.[15]

B.3 Discussion

Researchers likely invoke share exogeneity when they highlight the similarity among units apart from their differential exposure to common shocks. Alternatively, they do so when the emphasis is not on the multiplicity of industries but on a two-industry example or shocks to specific industries. This is because identification under the assumption of shock exogeneity requires a large number of shifters.

The previous sections discussed how shift-share variables identify the underlying parameter when they consist of one endogenous variable and one exogenous variable. Identification strategies depend on whether shares are exogenous or shifters are exogenous. When shares are exogenous, shift-share designs can be interpreted as difference-in-difference where otherwise identical units are treated with the same set of shifters but to exogenously determined degrees. Shifters can be endogenous in the sense that their expectation may depend on the share or error term distribution among units. The 2SLS estimator cam be shown to yield valid estimate and standard errors by converting the shift-share design into IV regression. The share exogeneity assumption need to be defended harder for some shares than others accordingly to their respective influence on the final estimate.

When shifters are exogenous, shift-share designs gather many negligibly small shocks and estimate the underlying parameter by averaging their effect. Shares may be endogenous as long as

[15] More shifters may need to be dropped if v_{ij} has geographical dependence.

shocks are not seemingly affected by any other unit-level characteristics including shares or outcomes. The OLS or 2SLS estimator yield a valid point estimate if effects are homogeneous but tends to vastly underestimate standard errors. This is because shares create dependence among unit-level outcomes that are not accounted in typical clustering procedures. The papers suggest two alternative estimators that work under slightly different assumptions, but they should not differ much under the condition where both sets of assumptions hold.[16] In addition to shifter exogeneity, researchers must establish that there are an enough number of independent clusters of shifters so that their shares can be treated asymptotically negligible.

One concern is that shares are often equilibrium outcomes in which the dependent variable is simultaneously determined as in the case of Autor, Dorn and Hanson (2013), so they would not be exogenous in many cases. The authors recommend using first differences in the outcome of interest instead of its levels to address the problem. According to Adão, Kolesár and Morales (2019), however, $\mathbb{E}[w_{ij0}\varepsilon_{it} \mid \Pi_{it}]$ might not be zero even when first differences are used. This shows that identifying assumptions always must be justified in light of theories. In response, the authors identify several study designs that implicitly use the share exogeneity framework. Researchers likely invoke share exogeneity when they highlight the similarity among units apart from their differential exposure to common shocks. Alternatively, they do so when the emphasis is not on the multiplicity of industries but on a two-industry example or shocks to specific industries. This is because identification under the assumption of shock exogeneity requires a large number of shifters.

Both schemes come with restrictions in their models that researchers have to be mindful of. The first scheme ignores spatial correlation, a factor typically considered in panel analyses through clustering.[17] The second scheme introduces strict assumptions on a large number of shifters and non-standard assumptions regarding control variables. Neither of them performs effectively if shift-share variables exhibit heterogeneous effects. Also, shift-share designs inherit common problems in OLS or IV regression designs such as outliers and weak instruments.

structural model vs. potential outcome

What if both components of a shift-share variable are endogenous or exogenous? If both are endogenous, one may consider exogenize either of them by fixinig shares at their initial values or averaging shifters as in Bartik (1991). If both are exogenous, the share exogeneity scheme suffices

[16]One important difference not mentioned above is that Adão, Kolesár and Morales (2019) require more units than shifters. Hahn et al. (2024) bypasses this problem by estimating the ridge regression rather than the OLS.

[17]Since clustering under many instruments has not been studied, we do not know if clustered standard errors under many share instruments would be equal to clustered standard errors under a single shift-share instruments.

to justify the design. It remains an open question if there is a more efficient way to take advantage of exogeneity of both components, although this would not be the case in most applications of shift-share designs.

C Data Reconstruction in Section 5

Table C.1: Data source

Variable	Source	Comment
Trade Flow	Eurostat Comext	
Trade Flow (US, Norway)	UN Comtrade	Needs Comtrade API to download
Product Crosswalk	Eurostat Comext	The first two digits of CPA2002.txt were used for NACE Rev. 1.1 division.
CPI	OECD	Consumer price indices (CPIs, HICPs) (COICOP 1999)
Regional Employment	Eurostat SBS	SBS data by NUTS 2 region and NACE Rev. 1.1 (sbs_r_nuts03)
Total Regional Employment	Eurostat LFS	Employment by sex, age and NUTS 2 region (lfst_r_lfe2emp)
Election Outcomes	Replication data	Analysis_Dataset_District_Level.dta

Data compatibility About 4 percent of the HS-6 product codes in UN comtrade data belong to more than one NACE industries. Volumes for these products were equally divided into each industry. Different data sources from Eurostat contain different versions of the NUTS-2 system. Those regions were matched by their names in such cases as most codes inherited the older names. `UKI` codes were refined into smaller regions over time. I use the older, larger regions for analysis to minimize confusion. Variables for these regions were simple averaged.

Imputation method Some regional industry employment statistics are missing in the data, which precludes the full reconstruction of the replication data. I use the following linear regression for imputation:

$$L_{rjt} = \alpha + \sum_{c} \beta_c \mathbb{I}_c + \sum_{j} \beta_j \mathbb{I}_j + \sum_{c,j} \beta_{cj} \mathbb{I}_{cj} + \sum_{t} \gamma_t \sigma_r \mathbb{I}_t + \sum_{j} \gamma_j \sigma_r \mathbb{I}_j + \sum_{t,j} \gamma_{tj} \sigma_r \mathbb{I}_{tj} + \varepsilon_{rjt}$$

where $\mathbb{I}$ denotes indicator and γ_r is the standard deviation of all observed employments in region r. Imputed shares use the imputation scheme only for missing values, while predicted shares replace observed values with the imputed ones. Adjusted R^2 is 0.99. National employments by industry are the sum of the regional employments: $L_{cjt} = \sum_{r \in c} L_{rjt}$.

D Shifter Replacement

D.1 Structural justification

D.2 Statistical justification

E Shifter Residualization

This section develops a standard error estimator for the inverted regression that directly utilizes residualized shifters. We begin with an instrumental variable shift-share regression where $Z_i = \sum_j w_{ij} D_j$ instruments X_i and p_i is a control vector. Suppose that shifters have a latent variable representation $\mathbb{E}[D_j \mid \mathscr{I}_m] = q_j^\top \mu$ for a known q and an unknown μ where $\mathscr{I}_m$ denotes suitable parameters to be conditioned on in the triangular array of populations. Let $\eta_j = D_j - \mathbb{E}[D_j \mid \mathscr{I}_m]$. We make the following assumptions inherited from Proposition 5 of Borusyak, Hull and Jaravel (2022).

Assumption E.1. *The first stage satisfies* $X_i = \sum_j w_{ij} \pi_{ln} D_j + \nu_i$.

Assumption E.2. *The vector* q_j *captures all sources of shock confounding:* $\mathbb{E}[D_j \mid \mathscr{I}_m] = q_j^\top \mu$ *where* $\mathscr{I}_m = ((q_j)_j, (\varepsilon_i, \nu_i, p_i, (w_{ij}, \pi_{ij})_j)_i)$, *and* $\sum_j w_{ij} q_j \subset p_i$.

Assumption E.3. *The* D_j *are mutually independent given* $\mathscr{I}_m$, $\max_j w_j \to 0$, *and* $\max_j \frac{w_j^2}{\sum_{j'} w_{j'}^2} \to 0$.

Assumption E.4. $\mathbb{E}[|D_j|^{4+v} \mid \mathscr{I}_m]$ *is uniformly bounded for some* $v > 0$ *and* $\sum_{i,j} w_{ij}^2 \mathbb{V}[D_j \mid \mathscr{I}_m] \pi_{ij} \neq 0$ *for almost surely. The support of* π_{ij} *is bounded, the fourth moments of* ε_i, ν_i, q_j, η_j *exist and are uniformly bounded, and* $\sum_i p_i p_i^\top \xrightarrow{p} \Omega_{pp}$ *and* $\sum w_j q_j q_j^\top \xrightarrow{p} \Omega_{qq}$ *for positive definite* Ω_{pp} *and* Ω_{qq}.

We further assume that the random component in the shifters can be reasonably estimated.

Assumption E.5. $(\hat{\eta}_j)_j$ *is a consistent estimator of* $(\eta_j)_j$: $(\hat{\eta}_j)_j \xrightarrow{p} (\eta_j)_j$.

The following proposition establishes an asymptotically valid standard error estimator.

Proposition E.1. *Under Assumptions E.1-E.5,*

$$\frac{\sqrt{\sum_j \left(\sum_i w_{ij} \hat{\varepsilon}_i\right)^2 \hat{\eta}_j^2}}{\left|\sum_i X_i^\perp Z_i\right|}$$

is an asymptotically valid standard error estimator where $X_i^\perp$ *is the residual of the regression of* X_i *on the controls* p_i *and* $\hat{\varepsilon}$ *is the residuals of the shift-share regression.*

Proof. Repeat the proof of Proposition 5 in Borusyak, Hull and Jaravel (2022) to the point of equation (B22): under Assumptions E.1-E.4,

$$\sqrt{r_m}(\hat{\beta} - \beta) \xrightarrow{d} \mathrm{N}\left(0, \frac{\mathcal{V}}{\left(\sum_i X_i^\perp Z_i\right)^2}\right)$$

for $\mathcal{V} = \operatorname*{plim}_{m\to\infty} r_m \mathcal{V}_m$ where $\hat{\beta}$ is also from the shift-share regression, $r_m = 1/(\sum_j w_j^2)$ and $\mathcal{V}_m = \sum_j \big(\sum_i w_{ij}\varepsilon_i\big)^2 \mathbb{V}[D_j \mid \mathscr{I}_m]$. Then,

$$\begin{aligned} r_m\left(\sum_j\left(\sum_i w_{ij}\varepsilon_i\right)^2\hat{\eta}_j^2 - \mathcal{V}_m\right) =& r_m\left(\sum_j\left(\sum_i w_{ij}\varepsilon_i\right)^2\eta_j^2 - \mathcal{V}_m\right) \\ &+ r_m\left(\sum_j\left(\sum_i w_{ij}\hat{\varepsilon}_i\right)^2 - \left(\sum_i w_{ij}\varepsilon_i\right)^2\right)\eta_j^2 \\ &+ r_m\sum_j\left(\sum_i w_{ij}\hat{\varepsilon}_i\right)^2(\hat{\eta}_j^2 - \eta_j^2). \end{aligned}$$

The first two terms are $o_p(1)$ by Lemma A.3 of Adão, Kolesár and Morales (2019), as pointed out by Borusyak, Hull and Jaravel (2022). The third term is also $o_p(1)$ since $\sum_j \big(\sum_i w_{ij}\hat{\varepsilon}_i\big)^2 = O_p(1)$ by equation (B23) and $\hat{\eta}_j^2 - \eta_j^2 \xrightarrow{p} 0$ by Assumption E.5. ■

Setting the residual estimator $\hat{\eta}_j$ as that from the OLS regression on q_j weighted by shifter shares w_j gives the heteroskedasticity-robust standard error in the reverted regression advocated in Borusyak, Hull and Jaravel (2022). Hahn et al. (2024) discuss multiple options for $\hat{\eta}_j$ in Remark 4.2 and construct an overidentification test using residuals from the ridge regression. The above proposition shows that the choice of estimator does not matter asymptotically as long as consistent. However, the choice of estimator may matter in finite samples. In Colantone and Stanig (2018), shifters are included in the shift-share regression only when there was an election in the country in the given year. If we believe that relation (4) holds regardless of the election history, then $\hat{\eta}_j$ can be much more reliably estimated by including all shifters with no corresponding elections that took place. I use the same weighted OLS regression specification with the extended shifter population.

The following is the clustered counterpart of the above proposition. Borrowing the notations of Adão, Kolesár and Morales (2019), let $c(j) \in \{1, \cdots, C\}$ denote the cluster that shifter j belongs to and $\tilde{w}_c = \sum_j \mathbb{I}[c(j) = c] \cdot w_j$ be the total share of cluster c.

Assumption E.6. *$D_j \perp D_j'$ if $c(j) \neq c(j')$ given $\mathcal{I}_m$, $\max_c \tilde{w}_c \to 0$, and $\max_c \frac{\tilde{w}_c^2}{\sum_{c'} \tilde{w}_{c'}^2} \to 0$.*

Proposition E.2. *Under Assumptions E.1-E.2 and E.4-E.6,*

$$\frac{\sqrt{\sum_c \sum_{j,j'} \mathbb{I}[c(j) = c(j') = c] \cdot \big(\sum_i w_{ij}\hat{\varepsilon}_i\big)\big(\sum_i w_{ij'}\hat{\varepsilon}_i\big)\hat{\eta}_j\hat{\eta}_{j'}}}{\left|\sum_i X_i^{\perp} Z_i\right|} = \frac{\sqrt{\sum_c \big(\sum_{i,\,c(j)=c} w_{ij}\hat{\varepsilon}_i\hat{\eta}_j\big)^2}}{\left|\sum_i X_i^{\perp} Z_i\right|}$$

is an asymptotically valid standard error estimator with $\bar{X}_i^{\perp}$ and $\hat{\varepsilon}_i$ as defined in Proposition E.1.

For the first-stage F-statistics, note that the true standard error that the estimators in the above Propositions target does not depend on the choice of η_j since it concerns estimation of the conditional variance $\mathbb{V}[D_j \mid \mathscr{I}_m]$ in the asymptotic variance $\mathcal{V}$ and nothing else. Therefore, I propose using the asymptotic effective F-statistics in the inverted regression for the estimand, and estimating it with $\hat{\eta}_j$ where appropriate. Let us write the true first-stage regression as

$$\bar{X}_j^{\perp} = \alpha + \beta D_j + \gamma q_j + \bar{\nu}_j$$

where $\bar{v}_j$ denotes the inverted variable defined as $\sum_i w_{ij} v_i / \sum_i w_{ij}$. $\gamma = 0$ by equation (B14) due to the residualization. The effective F-statistics by Olea and Pflueger (2013) is asymptotically equivalent to

$$\frac{\mathbb{E}_w[(\alpha + \beta D_j)^2]}{\mathrm{tr}(\Sigma_{\nu\nu} \times \Sigma_{zz})}$$

where $\Sigma_{\nu\nu}$ is the variance-covariance matrix of the coefficients (α, β), $\mathbb{E}_w$ is the expectation weighted by the shifter share w_j, and $\Sigma_{zz} = \mathbb{E}_w\left[\begin{smallmatrix} 1 & D_j \\ D_j & D_j^2 \end{smallmatrix}\right]$, with a suitable scaling factor. From the Frisch-Waugh-Lovell theorem with the null $\gamma = 0$ imposed, $\Sigma_{\nu\nu}$ can be estimated with the variance-covariance matrix of the regression

$$\bar{X}_j^{\perp} = \alpha + \beta \hat{\eta}_j + \bar{\nu}_j \tag{E.1}$$

weighted by w_j.

Proposition E.3. *The first-stage F-statistics can be estimated with*

$$\frac{\sum_j w_j (\hat{\bar{X}}_j^{\perp})^2}{\hat{\sigma}_{\beta\beta} \sum_j w_j D_j^2 + 2\hat{\sigma}_{\alpha\beta} \sum_j w_j D_j + \hat{\sigma}_{\alpha\alpha} \sum_j w_j}$$

where $\hat{\bar{X}}_j^{\perp}$ is the fitted first-stage value and $\hat{\sigma}_{ij}$ is the estimated covariance between i and j in regression (E.1).

Note that $\hat{\eta}_j = D_j - (\sum_j w_j q_j q_j^{\top})^{-1}(\sum_j w_j q_j D_j)$ gives the first-stage F-statistics in Borusyak, Hull and Jaravel (2022).

F Additional Results

Table F.1: Effect of China Shock on Electoral Outcomes per Colantone and Stanig (2018)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Nationalism				**Nationalist Autarchy**				**Radical Right**	
Outcome:	**Median**		**COG**		**Median**		**COG**		**Share**	
Import	0.78**	1.31**	0.4**	0.75***	0.63**	1.3**	0.38**	0.9***	0.04*	0.13**
Shock	(0.33)	(0.47)	(0.15)	(0.22)	(0.26)	(0.47)	(0.14)	(0.25)	(0.02)	(0.05)
Obs	8181	7782	8181	7782	8181	7782	8181	7782	8181	7782
Censored	0.43*	0.53	0.28**	0.36**	0.43*	0.65**	0.32**	0.67***	0.01	0.03**
Shock	(0.24)	(0.33)	(0.13)	(0.15)	(0.22)	(0.28)	(0.13)	(0.2)	(0.01)	(0.01)
Obs	2757	2757	2757	2757	2757	2757	2757	2757	2757	2757
Imputed	0.63**	0.77**	0.31**	0.33***	0.32	0.51***	0.26**	0.42***	0	0.01
Shock	(0.24)	(0.28)	(0.11)	(0.11)	(0.22)	(0.17)	(0.09)	(0.1)	(0.01)	(0.01)
Obs	2761	2761	2761	2761	2761	2761	2761	2761	2761	2761
Predicted	0.67**	0.76**	0.34***	0.33***	0.32	0.49**	0.26**	0.41***	0	0.01
Shock	(0.24)	(0.29)	(0.11)	(0.12)	(0.23)	(0.17)	(0.09)	(0.1)	(0.01)	(0.01)
Obs	2761	2761	2761	2761	2761	2761	2761	2761	2761	2761
Estimator	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS

Note: The first row replicates the results from the original table. The second row restricts the temporal scope to 2001 through 2007 for which reconstructed variables are available. The third row uses reconstructed variables, with missing employment shares imputed using linear regression. The fourth row replaces all observed values with predictions from linear regression. COG stands for the center of gravity, or the weighted average described in the section. All specifications have country-year fixed effects, and parentheses are standard errors clustered by NUTS-2 region-year. *p<0.1; **p<0.05; ***p<0.01

Table F.2: Shifter- and Unit-level Placebo Test With Raw Shifters

Variable	**Estimate**	**SE**	**Obs**
Shifter-level:			
Initial % of national industry employment	−0.227**	(0.114)	1370
Unit-level:			
Initial % of foreign-born population	0.008	(0.015)	321
Initial % of high-skilled workers	0.658	(0.999)	335
Initial % of high-technology workers	0.121	(0.248)	335
Initial % of medium- or low-skilled workers	−0.062	(0.469)	335
Initial % of medium- or low-technology workers	1.181	(1.459)	335
Initial % of workers in primary sectors	−1.265	(1.334)	335
Initial % of service industry workers	2.683	(3.529)	335

Note: Specifications are all the same with Table 4 except that raw shifters were used instead of transformed shifters. The transformation only affects the shifter-level test as most transformed shifters have too small shares to affect the instrument variable at the regional level. *p<0.1; **p<0.05; ***p<0.01

Table F.3: Table 6 with Predicted Shares and Untransformed Shifters

	(1)	(2)	(3)	(4)	(5)	(6)
		Median			**Center of Gravity**	
Predicted Shares:						
Cluster	0.745**	0.683**	−0.455	0.301***	0.321***	−0.224
	(0.292)	(0.281)	(0.349)	(0.115)	(0.119)	(0.147)
Obs	3006	307	307	3006	307	307
F	1375.02	174.08	33.42	1375.02	174.08	33.42
BHJ	-	0.683***	−0.455	-	0.321***	−0.224
	-	(0.205)	(0.588)	-	(0.094)	(0.49)
Obs	-	349	349	-	349	349
F	-	68.17	1.55	-	68.17	1.55
Untransformed shifters:						
Cluster	0.77***	0.629***	−0.023	0.334***	0.312***	−0.15
	(0.28)	(0.242)	(0.659)	(0.113)	(0.108)	(0.209)
Obs	3006	307	307	3006	307	307
F	1546.76	174.15	125.87	1546.76	174.15	125.87
BHJ	-	0.629***	−0.023	-	0.312***	−0.15
	-	(0.177)	(0.892)	-	(0.084)	(0.21)
Obs	-	349	349	-	349	349
F	-	46.82	1	-	46.82	1
Predicted Shares and untransformed shifters:						
Cluster	0.756***	0.667**	−0.442	0.327***	0.323***	−0.233*
	(0.291)	(0.265)	(0.302)	(0.115)	(0.114)	(0.134)
Obs	3006	307	307	3006	307	307
F	1414.12	172.93	220.19	1414.12	172.93	220.19
BHJ	-	0.667***	−0.442	-	0.323***	−0.233
	-	(0.179)	(0.494)	-	(0.083)	(0.416)
Obs	-	349	349	-	349	349
F	-	39.63	1.01	-	39.63	1.01
Unit of Analysis	District	Region	Region	District	Region	Region
Shifter controls	F	F	T	F	F	T
Country-Year FE	T	T	T	T	T	T

Note: All specifications are the same with Table 6 besides shifters and shares. *p<0.1; **p<0.05; ***p<0.01

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