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NON-RANDOM EXPOSURE TO EXOGENOUS SHOCKS:
THEORY AND APPLICATIONS

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ABSTRACT

We develop new tools for causal inference in settings where exogenous shocks affect the treatment status of multiple observations jointly, to different extents. In these settings researchers may construct treatments or instruments that combine the shocks with predetermined measures of shock exposure. Examples include measures of spillovers in social and transportation networks, simulated eligibility instruments, and shift-share instruments. We show that leveraging the exogeneity of shocks for identification generally requires a simple but nonstandard recentering, derived from the specification of counterfactual shocks that might as well have been realized. We further show how specification of counterfactual shocks can be used for finite-sample inference and specification tests, and we characterize the recentered instruments that are asymptotically efficient. We use this framework to estimate the employment effects of Chinese market access growth due to high-speed rail construction and the insurance coverage effects of expanded Medicaid eligibility.

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1 Introduction

To estimate causal effects or structural parameters, researchers often use regression procedures that leverage the orthogonality of an unobserved residual ε_ℓ and an observed treatment or instrument z_ℓ . Such orthogonality has a strong justification when z_ℓ is generated by a randomized controlled trial (RCT) or a natural experiment that approximates an RCT, and when an appropriate exclusion restriction holds. In conventional settings the experiment assigns one as-good-as-random shock to each observation ℓ , which is used directly as an exogenous z_ℓ .

In some settings, however, exogenous shocks affect the treatment status of many observations jointly, to different extents. For example, exogenous improvements to a transportation network may differentially increase the market access of many cities, exogenous reforms to an entitlement program may differentially expand the eligibility of many individuals, and exogenous changes in international trade policy may differentially affect import competition in many labor markets. In such settings, researchers may construct a z_ℓ that combines a set of as-good-as-random shocks with some predetermined measures of shock exposure. We will generically refer to such z_ℓ as an instrument, although it could also denote treatment in a reduced-form analysis. Typically the assumption of shock exogeneity is viewed as sufficient for this constructed instrument to be orthogonal with ε_ℓ , perhaps conditional on a set of observed controls.

This paper establishes identification and inference challenges with z_ℓ that combine exogenous shocks and predetermined exposure, and proposes new solutions. We first show that a novel source of omitted variables bias (OVB) may confound conventional regression approaches. The bias arises from different observations receiving systematically different values of z_ℓ because of their exposure to shocks. For example, even when transportation upgrades are randomly selected in an RCT, more central regions may see systematically larger increases in market access if they are closer to any random set of upgrades. Identification of market access effects then fails if the more exposed regions have systematically different unobservables, such as changes in local productivity or amenities.

We propose a general solution to this OVB challenge based on specifying counterfactual shocks that might as well have been realized. With this approach, the set of observed shocks is viewed as one realization of some data-generating process—what we call the shock *assignment process*. A researcher draws counterfactual shocks from this process, recomputes the instrument, and repeats many times. Then, for each observation ℓ , the instrument is averaged across these draws to construct the *expected instrument* μ_ℓ . Finally, μ_ℓ is subtracted from z_ℓ to obtain the *recentered instrument* $\tilde{z}_\ell = z_\ell - \mu_\ell$. We show that this recentering is sufficient to purge the OVB that results from non-random shock exposure: instrumenting with \tilde{z}_ℓ identifies β , given a non-zero first stage. Intuitively, this instrumental variable (IV) regression compares the outcomes of observations with higher-than-expected values of z_ℓ , due to the realized shocks, to those with lower-than-expected values. By construction this comparison is driven only by the exogenous shock variation, and is therefore valid, even though \tilde{z}_ℓ continues to vary

cross-sectionally because of differences in predetermined shock exposure. A closely related solution, which isolates the same exogenous variation, is to include μ_ℓ as a regression control.¹

Adjusting for the expected instrument is generally necessary for identification to follow just from shock exogeneity, and not also from an implicit assumption of exogenous shock exposure. In contrast, conventional controls and fixed effects are only guaranteed to purge OVB (without restricting unobservables) when they span the expected instrument—a condition that is difficult to assess when counterfactual shocks are not specified.

Counterfactual shocks can be specified to compute μ_ℓ in several ways. When the shocks arise from true randomization with a known protocol (whether controlled by the researcher or occurring naturally), counterfactual shocks can be drawn according to this protocol. Otherwise, they can be specified by plausible exchangeability assumptions, similar to those commonly imposed in observational data. For example, one might be comfortable assuming that entitlement program reforms could as well be exchanged across certain states or that different railroad lines had an equal chance of being selected for upgrade from a partially implemented transportation development plan. In these examples counterfactual shocks can be drawn by randomly permuting states’ policies or the construction status of planned lines. Policy discontinuities, as commonly used in regression discontinuity designs, can similarly yield local exchangeability of shocks and a measurable μ_ℓ .

We next show that problems with statistical inference in this setting can also be addressed by specifying counterfactual shocks. Inference may be challenging when both observed and unobserved shocks affect many observations jointly, as this may induce non-standard data dependencies with observations “fuzzily clustered” by their common shock exposure. Conventional asymptotics that rely on independence between most pairs of observations (for example, those from different clusters or located at geographic or network distance above some threshold, as in Conley (1999) and Ogburn et al. (2020)) can then fail. Our solution adapts principles of randomization inference (RI) via shock counterfactuals. RI-based confidence intervals are exact under constant treatment effects, allow for any correlation structure in the residuals, and are robust to weak instruments (Imbens and Rosenbaum 2005). RI is particularly attractive for placebo tests, where heterogeneous treatment effects are not a problem, and yields new specification tests. To address the practical issue of choosing a powerful RI test statistic, we use the fact that each statistic induces an estimator which rationalizes its observed value as typical under the null (Hodges and Lehmann 1963; Rosenbaum 2002). We pick the RI statistic that induces the recentered IV estimator, tightly linking our approach to estimation and inference.

We complement our finite-sample framework for identification and inference with an asymptotic analysis of consistency and efficiency. Recentered instruments yield consistent estimates and RI tests, regardless of the correlation structure of the unobservables, so long as the observed shocks induce

¹This solution can be represented as recentering the instrument and then taking out the μ_ℓ -related variation from the residual. Typically, controlling is therefore is weakly more efficient than just recentering in large samples, as would be including any fixed set of predetermined controls after recentering.

sufficient cross-sectional variation in the instrument and treatment. We further characterize asymptotically efficient instrument constructions by extending the classical analysis of optimal IVs by Chamberlain (1987). The optimal instrument involves finding the best predictor of the endogenous variable from exogenous shocks and endogenous exposure, recentering it, and then adjusting for the structural residual’s heteroskedasticity and dependence on shock exposure. While this instrument is typically infeasible, it can guide the construction of powerful and feasible recentered instruments.²

Our theoretical framework provides practical insights for a large number of empirical strategies. We first illustrate the usefulness of expected instrument adjustment by estimating the local employment effects of recent high-speed rail (HSR) development in China (Zheng and Kahn 2013; Lin 2017) via the market access approach of Donaldson and Hornbeck (2016). Simple regressions of employment growth on market access growth suggest a large and statistically significant effect. However, we find this to be largely driven by systematic differences in a region’s exposure to planned HSR lines, which we construct by permuting lines that connect the same number of regions (a process that passes RI specification tests). Chinese prefectures with higher-than-expected market access growth do not have a significantly higher employment growth. We discuss how our approach relates to a long literature estimating transportation upgrade effects (e.g. Baum-Snow (2007), Donaldson and Hornbeck (2016), Donaldson (2018), Bartelme (2018), Ahlfeldt and Feddersen (2018), and Tsivanidis (2019)). We contrast the challenges of strategically chosen transportation upgrades (Redding and Turner 2015) with the less well-understood problem that regional exposure to exogenous upgrades may be unequal. The latter problem can arise either because the geography of upgrades is not uniform or because the effects of upgrades on measures like market access are heterogeneous.

Second, we show how our framework can be used to boost the power of simulated policy eligibility instruments (e.g., Currie and Gruber (1996a, 1996b), Cohodes et al. (2016), Cullen and Gruber (2000), Gruber and Saez (2002), and East and Kuka (2015)). These instruments are used when individual policy eligibility depends on both exogenous policies, e.g. at the state level, and non-random individual characteristics. Simulated instruments isolate exogenous policy variation by not taking into account individual heterogeneity in policy exposure. We propose to incorporate this endogenous variation but appropriately recenter the instrument, yielding instruments that are valid under the same assumptions but likely more powerful because they better predict the treatment. In an application estimating Medicaid take-up and crowd-out effects from recent Affordable Care Act expansions, recentered IV estimates have 60-70% smaller standard errors than conventional simulated IV estimates; we similarly find dramatic and uniform power gains in calibrated Monte Carlo simulations.

Third, we discuss how instrument recentering can be used to avoid OVB in regression estimates of network spillovers (e.g. Miguel and Kremer (2004), Acemoglu et al. (2015), Jaravel et al. (2018),

²Adão et al. (2020) also follow Chamberlain (1987) in characterizing efficient instruments in a setting with interdependence (specifically, in a model of spatial linkages). Our general characterization of efficient recentered instruments differs from theirs by allowing for a complex data dependence structure, induced by common shocks, as well as the endogeneity of shock exposure.

and Carvalho et al. (2020)) which may arise when nodes have systematically different exposure to exogenous shocks because of their network position. Our solution applies even in settings with complex specifications of spillover effects and shock assignment processes, where conventional regression controls are generally insufficient. We also discuss how RI can be used to overcome difficult inference challenges when spillovers are non-local.

Fourth, we consider shift-share instruments: both traditional ones, which average a common set of shocks with heterogeneous exposure weights (e.g., Bartik (1991), Blanchard and Katz (1992), Card (2001), and Autor et al. (2013)), and a recent class of shift-share constructions that are nonlinear in the shocks (e.g., Boustan et al. (2013), Berman et al. (2015), and Chodorow-Reich and Wieland (2020)). This analysis builds on Borusyak et al. (2019), who first study identification with linear shift-share instruments when shocks are exogenous but exposure is not. While they find that simple controls may suffice to purge OVB, we show that recentering nonlinear shift-share IVs generally requires specifying shock counterfactuals. We also show how this specification can be avoided, at a likely efficiency cost, by replacing the nonlinear instrument with its first-order approximation, a linear shift-share IV. We further illustrate how RI can be a useful mode of inference with shift-share instruments, even in the linear case where asymptotic approaches have been proposed (Adão et al. 2019) but may suffer from size distortions in finite samples. We find favorable properties of RI in Monte Carlo simulations based on the “China shock” setting of Autor et al. (2013). RI has proper coverage even in simulations with substantial treatment effect heterogeneity, where there is no theoretical guarantee. The simulated power of RI tests furthermore dominates that of asymptotic tests which impose the null hypothesis to achieve correct finite-sample size (Adão et al. 2019).

We further discuss the implications of our framework for model-implied instruments (Adão et al. 2020), instruments generated by centralized school assignment mechanisms (Abdulkadiroglu et al. 2017; 2019), and instruments based on weather shocks (e.g. Gomez et al. (2007) and Madestam et al. (2013)). We show how appropriate instrument recentering and randomization inference can be used to relax various assumptions imposed in these settings.

From an econometric perspective, the expected instrument can be seen as a generalization of the propensity score of Rosenbaum and Rubin (1983). Conventional propensity scores are defined in settings with randomly sampled data and a conditionally exogenous binary treatment. Earlier generalizations have considered binary instruments (e.g. Abadie (2003)) and non-binary treatments (e.g. Hirano and Imbens (2004)). Our setting accommodates these extensions but also allows for the kinds of interdependent data that naturally arise when exogenous shocks jointly affect the treatment of many observations. Adjusting for the non-random shock exposure, as captured by the expected instrument, is furthermore relevant even when shocks are exogenous unconditionally.

By formalizing the OVB problem and proposing the recentering solution, our paper relates to

specific settings where some of these issues have been previously discussed.³ In the linear shift-share IV setting, Borusyak et al. (2019) show that instrument validity does not follow from exogeneity of industry shocks when the sum of exposure shares underlying the instrument varies across observations. We develop a general characterization of such OVB and a general solution based on expected instrument adjustment. In the context of network spillovers, Aronow (2012) notes that random selection of treatment units does not imply the randomization of network proximity to them. Aronow and Samii (2017) propose a way to estimate spillover effects from discrete shocks that does not suffer from this problem, via inverse propensity score weighting (see also Gerber and Green (2012, p. 261)).⁴ We consider a broader class of network settings by imposing no restrictions on the support of the shocks, develop regression-based estimators, and consider exact randomization-based inference.⁵

Our use of randomization inference builds on a rich statistical literature dating back to Fisher (1935) and reviewed in Lehmann and Romano (2006, Ch. 15). RI was originally proposed for randomized control trials and has recently been used, for example, to test for network interference and heterogeneous treatment effects (Aronow 2012; Athey et al. 2018b; Puelz et al. 2019; Ding et al. 2016). RI has also been deployed in a range of non-experimental settings by, among others, Rosenbaum (1984), Rosenbaum (2002), Bertrand et al. (2004), Imbens and Rosenbaum (2005), Ho and Imai (2006), Abadie et al. (2010), Cattaneo et al. (2015), Dell and Olken (2018), Ganong and Jäger (2018), Canay and Kamat (2018), and Shaikh and Toulis (2019). We apply RI to a broad class of settings where random or as-good-as-random shocks affect many observations jointly but to different extents.

Finally, our paper contributes to a growing literature on causal inference that focuses on the assignment process of observed exogenous shocks (e.g. Lee (2008), Athey and Imbens (2018), Shaikh and Toulis (2019), De Chaisemartin and Behaghel (2018)). This approach contrasts with alternative identification strategies that adjust for the endogeneity in the expected structural residual, as in the control functions approach of Heckman and Robb (1985) and difference-in-difference strategies (e.g. Angrist and Pischke (2008, Ch. 5), De Chaisemartin and D’haultfœuille (2020), Athey et al. (2018a)). Leveraging the assignment process is most natural when the shocks are truly randomized. However, the clarity of an experimental ideal and the implied specification tests make them appealing with observational data, too. Specification of counterfactuals shocks can be viewed as a formalization of a natural experiment—what DiNardo (2008) defines as a “serendipitous randomized trial”—in terms of

³Our recentering based on simulations is reminiscent of the “dartboard approach” of Ellison and Glaeser (1997), but this connection is superficial as this paper corrects a biased descriptive statistic (a measure of spatial agglomeration) rather than an instrument for causal effects.

⁴Aronow et al. (2020) distinguish between methods to estimate spillover effects that allow all units to interact while imposing parametric structure (e.g., Manski (2013)) and those with unrestricted interactions among a small number of node pairs (e.g., Hudgens and Halloran (2008)). Like Aronow and Samii (2017), we advance the former approach.

⁵There are further differences between the results of our paper and Aronow and Samii (2017). Our regression approach generally captures a convex average of heterogeneous effects of a non-binary treatment under an appropriate monotonicity condition (see Appendix A.4), but not necessarily the average causal effect estimated by Aronow and Samii (2017). We obtain exact randomization-based inference by assuming constant treatment effects but allowing for a general spillover structure, while Aronow and Samii (2017) construct asymptotically conservative confidence intervals with unrestricted heterogeneity by assuming a small fraction of unit pairs are affected by the same shocks.

a particular randomization protocol.⁶

The remainder of this paper is organized as follows. The next section motivates our analysis with a stylized example of the identification and inference challenges in market access regressions. Section 3 develops our general framework and results on identification, inference, and asymptotic efficiency. Section 4 discusses and illustrates practical implications of the framework. Section 5 concludes.

2 A Motivating Experimental Example

We begin with an idealized example where our framework applies most clearly: where exogenous transportation shocks from an RCT are used to estimate the local effects of market access growth. Market access is a statistic which captures the average cost of transportation from a region ℓ to other regions of varying size (the exact formula is unimportant at this point). To estimate its effects we consider a linear structural equation relating the growth of a regional outcome such as land value, $\Delta \log V_\ell$, to the growth of market access $\Delta \log MA_\ell$:

$$\Delta \log V_\ell = \beta \Delta \log MA_\ell + \varepsilon_\ell, \tag{1}$$

where ε_ℓ captures unobserved shocks to local productivity and amenities occurring in region ℓ between two periods. This equation can be derived from standard models of economic geography (e.g. Redding and Venables (2004)), in which β is a structural elasticity. Equation (1) can also be interpreted as a reduced-form causal model, in which β captures the effect of interventions that affect market access (but not the residuals). For these reasons, equation (1), first proposed by Donaldson and Hornbeck (2016), and its variants have become increasingly popular in estimating the regional effects of transportation infrastructure upgrades (e.g. Bartelme (2018) and Tsivanidis (2019)).

To illustrate the key insights of our framework we imagine estimating β by leveraging experimental shocks to market access. Specifically, we imagine an RCT that changes transportation costs by randomly selecting for construction a set of new roads that connect different regions; we assume that other determinants of market access are held fixed. New roads affect $\Delta \log MA_\ell$ for all regions (even those not directly connected by new roads) to different extents, according to the known market access formula. While we are not aware of actual experimental studies of market access, similar RCTs and natural experiments have been previously analyzed. For example, Gonzalez-Navarro and Quintana-Domeque (2016) study an RCT that paved streets in random neighborhoods across Mexico, while Volpe Martincus and Blyde (2013) exploit random road disruptions in various parts of Chile due to an

⁶The term “natural experiment” is used in a variety of ways in social sciences; see Titiunik (2020) for a recent discussion. While Titiunik argues that the key feature of natural experiments is the presence of an external factor that affects treatment assignment, for us they are defined by the specification of counterfactuals for exogenous shocks. Our results show how this definition yields novel methods for causal inference and specification tests, while for Titiunik (2020) the utility of natural experiments is more limited—only to help justify certain identifying assumptions.

earthquake. Our hypothetical RCT can also serve as an experimental ideal for observational studies of market access effects that leverage plausibly exogenous changes to transportation networks.

At first glance, it may seem that the experimental variation in $\Delta \log MA_\ell$ is sufficient to estimate β by a simple linear regression. When new roads are selected at random, their construction is guaranteed to be exogenous: i.e., independent from all local productivity and amenity shocks in ε_ℓ . Exogenous transportation shocks are the only reason that $\Delta \log MA_\ell$ is not identically zero across regions, since market size and other determinants of transportation costs are held fixed in the RCT. We further assume that the linear model (1) is correctly specified, such that the effects of the shocks on the outcome are fully captured by the observed variation in $\Delta \log MA_\ell$.

The first key insight of this paper is that even in this idealized experimental example, non-random exposure to exogenous transportation shocks can generate omitted variable bias (OVB) in regression estimates of β . Intuitively, randomization of roads that affect market access is not the same as randomization of market access. Even when new roads are placed randomly in space, some regions will tend to see systematically higher market access growth because of their relative position in the country’s geography. This tendency can bias regression estimates of market access effects when unobserved productivity and amenity shocks differ systematically in different areas. Formally, the necessary orthogonality between $\Delta \log MA_\ell$ and ε_ℓ can fail, even though the transportation shocks underlying $\Delta \log MA_\ell$ are independent of ε_ℓ .

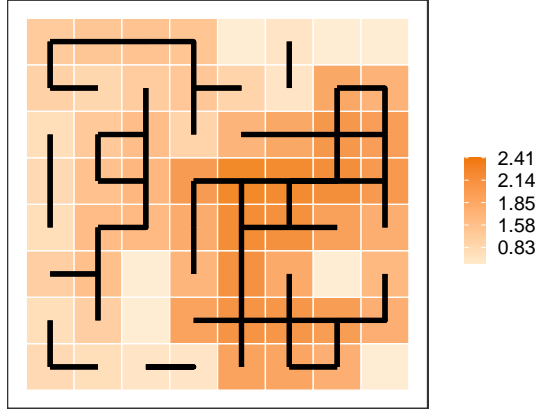
To see this OVB problem simply, consider a square island consisting of 64 equally-sized regions ℓ with no initial connectivity, such that initial market access is identical for all regions. Suppose new roads are constructed between regions completely at random: out of all potential roads connecting adjacent regions, the RCT selects half for construction. One such draw from this experiment is shown in Panel A of Figure 1, along with the resulting growth in market access.⁷ Expectedly, regions that become connected by road tend to have higher $\Delta \log MA_\ell$. However, the figure reveals another tendency: many of the regions with high market access growth are in the center of the island. This concentration is not by chance. Panel B of Figure 1 shows that the average growth of market access in each region, simulated across 1,000 counterfactual road networks drawn randomly from the same assignment process (i.e. experimental protocol), is also higher in the center of the map. We label this statistic μ_ℓ , and it can be thought of as a region’s “expected” market access prior to the realization of exogenous shocks. The spatial pattern of μ_ℓ indicates that more central regions are more exposed to the RCT: no matter where random roads are built, central regions are more likely to be closer to them and thus to see a larger increase in market access.

Systematic differences in shock exposure, as captured by μ_ℓ , can generate bias in ordinary least

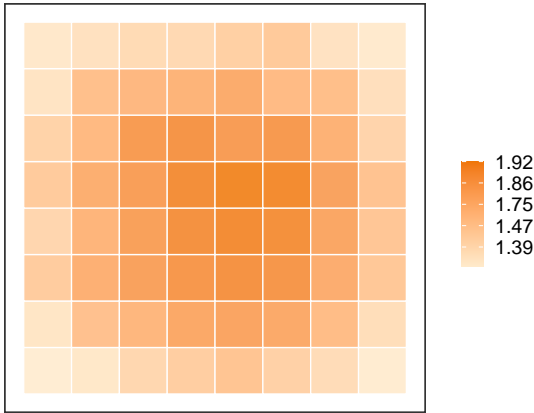
⁷Market access in period $t = 1, 2$ is here given by $MA_{\ell t} = \sum_k \tau_{\ell kt}^{-\theta} P_k$ where $\tau_{\ell kt}$ is a function of distance and connectivity in period t and P_k denotes region k ’s time-invariant market size (e.g., population). In this simplified example $P_{kt} = 1$ is constant across regions and periods, $\theta = 1$, and $\tau_{\ell kt} = 2^{0.1d_{\ell kt}}$ where $d_{\ell kt}$ is the distance by road from ℓ to k at time t (or infinity if there is no path).

Figure 1: Market Access Growth in the Motivating Example

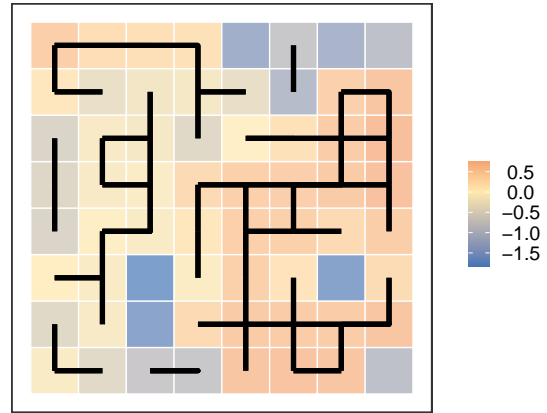
A. Line Construction and Market Access Growth



B. Expected Market Access Growth



C. Recentered Market Access Growth



Notes: This figure illustrates the omitted variables bias problem and the recentering solution in the simple market access example discussed in Section 2. Panel A shows a random draw of the railroad construction experiment, with lines indicating connected regions and shading indicating corresponding market access growth (computed as described in the text). Panel B shows average market access growth over 1,000 such random draws. The shading in Panel C indicates the recentered market access measure which subtracts expected market access in Panel B from realized market access in Panel A, with the lines again indicating realized line construction.

squares (OLS) estimates of β . The OLS estimates come from a comparison of outcome growth between regions with high and low market access growth, which tend to be regions with high and low μ_ℓ , respectively. Expected market access growth is predetermined (with respect to the experimental shocks) but need not be exogenous, and endogeneity of μ_ℓ will generally bias the regression comparisons. In the simple example of Figure 1, OVB arises when unobserved productivity and amenity shocks differ between the center and periphery of the map. For example, if rising sea levels reduce amenity values near the edges of the island then central regions will tend to see both higher market access growth and higher residuals, biasing OLS estimates of β upward.

The second insight of this paper is that this identification challenge has an intuitive but non-standard solution, based on the same knowledge of the shock assignment process that generated Panel B of Figure 1. In this experimental setting, one can simulate market access growth across counterfactual draws of the transportation upgrade RCT in order to compute each region’s expected market access growth μ_ℓ . One can then construct a recentered measure, $\tilde{z}_\ell = \Delta \log MA_\ell - \mu_\ell$, which subtracts each region’s expected growth from its observed growth. This \tilde{z}_ℓ can then be used to instrument for $\Delta \log MA_\ell$ in equation (1). IV estimation with the recentered measure compares regions with market access growth that is higher than expected, because of the realization of exogenous shocks, to regions with lower-than-expected market access growth. By construction, \tilde{z}_ℓ has no tendency to be higher or lower in any predetermined group of regions and is thus uncorrelated with any productivity or amenity shocks in ε_ℓ . In the simple example of Figure 1, observed market access growth is no longer concentrated in the center of the map after recentering by μ_ℓ (see Panel C).

Adjusting for expected market access is generally necessary for identification of β to follow just from the experimental transportation upgrades, and not also from an implicit assumption of exogenous μ_ℓ . We show in the next section that this adjustment can also be performed by controlling for μ_ℓ in OLS estimation of (1). Conventional controls and fixed effects similarly isolate experimental variation in market access when they linearly span μ_ℓ . Otherwise, controls may or may not purge OVB depending on the properties of the unobservable, and there is no general guarantee.⁸ While in our simple example controlling for geographic centrality would suffice, as it spans μ_ℓ , this solution is very fragile. In general μ_ℓ depends in a complex way on the country’s geography and the distribution of market size, as well as the road assignment process. Appendix Figure A1, for instance, reproduces Figure 1 in a scenario with unequal population. In this case expected market access growth exhibits complex patterns: μ_ℓ is high near the more populated regions (because the value of being connected to large markets is high), low in the dense regions themselves (since for them internal market access is most important), and remains low in the geographic periphery. Further complications arise when the map is not square, when there are preexisting transportation networks, or when the potential set of roads selected by the

⁸Controlling for regional geography perfectly is of course not possible, as this would remove all variation in market access growth. OVB is also not generally solved by using panel data: if roads tend to be built rather than destroyed, expected market access grows over time and is thus not captured by region fixed effects. Indeed, equation (1) is already written in differences and can be equivalently estimated in a panel with region fixed effects, with the same OVB problem.

RCT is concentrated in some parts of the country. Our solutions to OVB, based on drawing shock counterfactuals to measure μ_ℓ , continue to apply with these complications.

The third insight of this paper is that problems with classic statistical inference on β can also be overcome by simulating counterfactual transportation upgrades. The recentered market access instrument is inherently correlated across regions because of the common exposure to experimental shocks. Such spatial dependence may generate challenges for conventional asymptotic approaches to inference that specify a geographic distance threshold after which observations of $\tilde{z}_\ell \varepsilon_\ell$ are uncorrelated (e.g., Conley (1999)). For the asymptotic approximation to hold this threshold should be sufficiently small, which may be implausible with all regions exposed to all potential roads.⁹ We show in the next section how classical methods of randomization inference can be applied to address such “exposure clustering.” RI confidence intervals have valid coverage in finite samples, regardless of the dependence structure of $\tilde{z}_\ell \varepsilon_\ell$. In the market access setting this procedure again leverages knowledge of the road assignment process, and is again based on simulations of the experimental transportation upgrades. Challenges of market access effect identification and inference in experimental settings can thus be overcome in this unified way.

In most settings, of course, transportation upgrades are not drawn uniformly on a map, their assignment process is not known, and they may be placed strategically in violation of exogeneity. In the next section we discuss how shock assignment processes may generally be specified, simulated, and validated in observational data, provided the exogeneity of shocks is *ex ante* plausible. In Section 4.1 we apply this approach to a specific market access setting and relate it to existing approaches to estimating transportation effects, with or without exogenous upgrades.

3 Identification, Inference, and Asymptotic Efficiency

3.1 Setting

We now develop a general econometric framework for settings with non-random exposure to exogenous shocks. We suppose an outcome y_ℓ and treatment x_ℓ are observed for units $\ell = 1, \dots, L$. Of interest is a causal or structural parameter β relating treatment to outcomes by

$$y_\ell = \beta x_\ell + \varepsilon_\ell, \tag{2}$$

where ε_ℓ denotes an unobserved residual. Initially we assume y_ℓ and x_ℓ are both scalar and demeaned, and that the effect of interest is linear. We discuss extensions to heterogeneous causal effects, additional control variables, multiple treatments, and nonlinear outcome models in Section 3.6. Although we use

⁹As-good-as-random upgrades to long roads, for example, will tend to cause regions which are far apart in space to “cluster” by their common market access growth. If the unobserved shocks in ε_ℓ also tend to propagate widely then $\tilde{z}_\ell \varepsilon_\ell$ will tend to be correlated across long distances, invalidating spatially clustered standard errors.

a single index ℓ for the observed units, our framework readily accommodates repeated cross-sections and panel data.

Importantly, in writing equation (2) we do not assume that the observations of (y_ℓ, x_ℓ) are independently or identically distributed (*iid*), as when arising from random sampling. This generality allows for complex dependencies across ℓ due to the common exposure to observed and unobserved shocks. The lack of random sampling is also consistent with settings where the L units represent a population—for example, all regions of a country—and conventional asymptotic frameworks are inappropriate (Abadie et al. 2020).

We suppose that to estimate β a researcher has constructed an instrument z_ℓ which incorporates variation from exogenous shocks, summarized by an $N \times 1$ observed vector g . However the instrument also incorporates additional predetermined variables which govern a unit’s exposure to the shocks. Collecting these additional observables in the set w , we write the instrument as

$$z_\ell = f_\ell(g; w), \tag{3}$$

where $\{f_\ell(\cdot)\}_{\ell=1}^L$ is a set of known non-stochastic functions. In the previous motivating example, g contained information on transportation network upgrades and w summarized the regional populations; the $f_\ell(\cdot)$ functions combined g and w to form market access growth for each city ℓ according to a known formula. As another example, linear shift-share instruments set $f_\ell(g; w) = \sum_{n=1}^N w_{\ell n} g_n$ where the $w_{\ell n}$ are non-negative exposure share weights. We note that our framework allows $x_\ell = z_\ell$, in which case β is the reduced-form causal effect of the instrument (as in the motivating example).

Equation (3) is very general, nesting many applied examples as we discuss in Section 4. Any instrument that is measurable from a set of observed shocks g and other variables w can be described in this way.¹⁰ Mapping the shocks into the instrument using some transformation $f_\ell(\cdot; w)$ is generally necessary, for example, when the shocks are defined at a different “level” than the unit of observation (e.g. industry shocks and regional data) or when shocks to one observation have spillover effects to others. In some cases, such as the market access and linear shift-share examples, the instrument specification may follow from a particular model for the treatment variable. In particular, when $x_\ell = \tilde{f}_\ell(g, w, u)$ for a known $\tilde{f}_\ell(\cdot)$ and some (possibly unobserved) endogenous shocks u , an instrument may be specified as the treatment prediction that shuts down these shocks: $f_\ell(g, w) = \tilde{f}_\ell(g, w, 0)$. For now we take the choice of $f_\ell(\cdot)$ as given, addressing the question of which instrument constructions may be more desirable in Section 3.5.

Partitioning the determinants of z_ℓ into a set of shocks g and other variables w allows us to formalize the notion that some (but not all) sources of variation in the instrument are exogenous. In an RCT the exogeneity of shocks can naturally arise from the experimental intervention. With observational data,

¹⁰Note that equation (3) does not contain a residual: it formalizes an algorithm for computing an instrument rather than characterizing an economic relationship.

a researcher may appeal to an experimental ideal in which the shocks in g are as-good-as-randomly assigned given predetermined variables in w , which are not exogenous. For example, in shift-share designs it may be plausible that the industry-level shocks in g arise from a natural experiment but that local industrial composition w is endogenous (Borusyak et al. 2019).

We formalize shock exogeneity by the conditional independence of g from the residual vector $\varepsilon = (\varepsilon_\ell)_{\ell=1}^L$, given the other sources of instrument variation:

Assumption 1. (*Shock exogeneity*): $g \perp\!\!\!\perp \varepsilon \mid w$

This notion of shock exogeneity combines two conceptually distinct conditions. First, it imposes an exclusion restriction: that the realization of shocks only affects the outcome of each unit via its treatment x_ℓ . This condition may be violated when the structural equation (2) is misspecified; for example, when market access inadequately captures the local economic effects of new transportation.¹¹ Second, Assumption 1 requires the as-good-as-random assignment of shocks with respect to the unobserved outcome determinants ε . This condition is satisfied when the shocks are fully randomly assigned, as in an RCT: i.e., $g \perp\!\!\!\perp (\varepsilon, w)$. More generally, Assumption 1 allows w to contain variables that govern the shock assignment process. We discuss how such conditioning is useful for specifying shock counterfactuals in Section 3.3. The exclusion and as-good-as-random assignment assumptions are isolated in Appendix A.4, which places our framework in a general potential outcomes model.

We first consider identification of β from an instrumental variables regression of y_ℓ on x_ℓ , with z_ℓ as an instrument. Identification follows when the instrument z_ℓ is relevant to the treatment and orthogonal to the structural residual. In our *non-iid* setting, we formalize these conditions as $\mathbb{E} \left[\frac{1}{L} \sum_\ell z_\ell x_\ell \right] \neq 0$ and

$$\mathbb{E} \left[\frac{1}{L} \sum_\ell z_\ell \varepsilon_\ell \right] = \mathbb{E} \left[\frac{1}{L} \sum_\ell z_\ell y_\ell \right] - \beta \mathbb{E} \left[\frac{1}{L} \sum_\ell z_\ell x_\ell \right] = 0, \quad (4)$$

implying that β is uniquely recoverable from the observable moments $\mathbb{E} \left[\frac{1}{L} \sum_\ell z_\ell y_\ell \right]$ and $\mathbb{E} \left[\frac{1}{L} \sum_\ell z_\ell x_\ell \right]$. It is worth highlighting that full-data instrument orthogonality (4) combines two dimensions of variation: over the stochastic realizations of g , w , and ε , and across the cross-section of observations $\ell = 1, \dots, L$. In the *iid* case it reduces to the more familiar condition $\mathbb{E} [z_\ell \varepsilon_\ell] = 0$.

When β is identified, a natural estimator is given by the solution to the sample analog of (4):

$$\hat{\beta} = \frac{\frac{1}{L} \sum_\ell z_\ell y_\ell}{\frac{1}{L} \sum_\ell z_\ell x_\ell}. \quad (5)$$

This $\hat{\beta}$ is obtained by the sample IV regression of y_ℓ on x_ℓ , instrumenting by z_ℓ . While our primary

¹¹The shock exclusion restriction may follow from a particular economic model, as in Donaldson and Hornbeck (2016), or be relaxed by including multiple treatments in x_ℓ (e.g. allowing for both direct and spillover effects of the same shocks, as in Miguel and Kremer (2004)).

focus is on identification and finite-sample inference, some of our theoretic results consider the asymptotic properties of such IV estimators. We establish these properties by considering a sequence of data-generating processes, indexed by L , for the complete data (y, x, g, w) . Consistency, for example, is defined as $\hat{\beta} \xrightarrow{P} \beta$ for $L \rightarrow \infty$, while asymptotic efficiency considers large- L approximations to the variance of $\hat{\beta}$. We emphasize that this asymptotic sequence should be viewed as a way to approximate the finite-sample distribution of the IV estimators, rather than as a realistic description of the actual sampling process for the data.¹²

3.2 Identification and Instrument Recentering

Our first result formalizes the omitted variables bias problem motivated in Section 2. We show that the exogeneity of shocks underlying z_ℓ is not enough to ensure identification of β , even if they are fully randomly assigned. We then derive a simple but non-standard recentering of z_ℓ that purges OVB in this setting. We end this section with results on recentered IV consistency.

As in the motivating market access example, identification under Assumption 1 can fail when pre-determined exposure to the natural experiment is endogenous. While this exposure variation is potentially high-dimensional, our first result shows that OVB is governed by a particular one-dimensional confounder μ_ℓ :

Lemma 1. *Under Assumption 1,*

$$\mathbb{E} \left[\frac{1}{L} \sum_{\ell} z_{\ell} \varepsilon_{\ell} \right] = \mathbb{E} \left[\frac{1}{L} \sum_{\ell} \mu_{\ell} \varepsilon_{\ell} \right], \quad (6)$$

where $\mu_\ell = \mathbb{E}[f_\ell(g; w) | w]$ defines the expected instrument. Thus β is not identified by the instrument z_ℓ when $\mathbb{E}[\frac{1}{L} \sum_{\ell} \mu_{\ell} \varepsilon_{\ell}] \neq 0$.

Proof. $\mathbb{E}[\frac{1}{L} \sum_{\ell} z_{\ell} \varepsilon_{\ell}] = \mathbb{E}[\frac{1}{L} \sum_{\ell} \mathbb{E}[f_{\ell}(g; w) \varepsilon_{\ell} | w]] = \mathbb{E}[\frac{1}{L} \sum_{\ell} \mu_{\ell} \mathbb{E}[\varepsilon_{\ell} | w]] = \mathbb{E}[\frac{1}{L} \sum_{\ell} \mu_{\ell} \varepsilon_{\ell}]$. The first and third equality follow from the law of iterated expectations, while the second equality follows by Assumption 1 and the definition of μ_ℓ . \square

Like in Section 2, the expected instrument μ_ℓ captures the average value of z_ℓ across different realizations of the shocks (here, conditional on w). Lemma 1 shows that the exogeneity of shocks makes z_ℓ a valid instrument if and only if μ_ℓ is orthogonal to the residual ε_ℓ . Absent further assumptions on the unobserved error, adjustment for μ_ℓ is thus generally necessary to remove OVB. Note that adjustment is generally necessary even if the shocks are unconditionally as-good-as-randomly assigned, i.e. when $g \perp (\varepsilon, w)$ in Assumption 1.

¹²This is similar to how Bekker (1994) studies IV regressions with many instruments. As he writes, “the [asymptotic] sequence is designed to make the asymptotic distribution fit the finite sample distribution better. It is completely irrelevant whether or not further sampling will lead to samples conforming to this sequence” (p. 658).

As the sole relevant confounder of z_ℓ , our expected instrument generalizes the propensity score of Rosenbaum and Rubin (1983). Propensity scores are typically defined for binary treatments (or occasionally instruments, as in Abadie (2003)), while our regression-based correction applies to arbitrary z_ℓ . A more important distinction is that propensity scores are considered in *iid* settings, as functions of observation-specific confounders: i.e., $\mu_\ell = \tilde{\mu}(w_\ell) = Pr(x_\ell = 1 | w_\ell)$ for w_ℓ *iid* across ℓ . When w_ℓ is low-dimensional, this obviates the need to specify the treatment assignment process explicitly: $\tilde{\mu}(\cdot)$ can be non-parametrically estimated or flexible controls for w_ℓ can simply be included in the estimation equation. These solutions do not apply in our non-*iid* setup, where all components of w may affect μ_ℓ for each ℓ and where the $w \mapsto \mu_\ell$ mapping may be observation-specific. In the market access example, as explained in Section 2, it is not *ex ante* clear which features of the country’s geography, distribution of population, and preexisting transportation networks may suffice to span the cross-sectional variation in μ_ℓ even with fully random placement of transportation.

When shock exposure is endogenous but Assumption 1 holds, Lemma 1 suggests a simple but non-standard recentering of z_ℓ that identifies β . In fact, a weaker notion of shock exogeneity suffices. Consider:

Assumption 2. (*Weak shock exogeneity*):

- (i) $\mathbb{E}[\varepsilon_\ell | g, w] = \mathbb{E}[\varepsilon_\ell | w]$ almost surely for each ℓ .
- (ii) $\mathbb{E}[\varepsilon_\ell \varepsilon_m | g, w] = \mathbb{E}[\varepsilon_\ell \varepsilon_m | w]$ almost surely for each ℓ and m .

Such mean and covariance independence of the residuals from the shocks is implied by Assumption 1 and will also be sufficient for some of our later asymptotic results. Here we use the first condition to show that β is identified by an adjusted \tilde{z}_ℓ , given a non-zero first-stage:

Proposition 1. *Suppose Assumption 2(i) holds and let $\tilde{z}_\ell = z_\ell - \mu_\ell$. Then*

$$\mathbb{E} \left[\frac{1}{L} \sum_{\ell} \tilde{z}_\ell \varepsilon_\ell \right] = 0, \tag{7}$$

such that β is identified by the instrument \tilde{z}_ℓ provided $\mathbb{E} \left[\frac{1}{L} \sum_{\ell} \tilde{z}_\ell x_\ell \right]$ is non-zero.

Proof. See Appendix C.1. □

A recentered IV regression compares units with a higher-than-expected value of z_ℓ , because of the realization of the shocks, to units affected less than expected. The validity of \tilde{z}_ℓ thus stems from the exogeneity of shocks (specifically, Assumption 2(i)), even though it continues to vary cross-sectionally due to heterogeneous shock exposure. First-stage relevance holds when the units with higher-than-expected values of z_ℓ have systematically different values of the treatment x_ℓ .¹³

¹³Whenever the shocks induce some variation in treatment, there exist $f_\ell(\cdot)$ constructions such that the corresponding recentered instrument satisfies the relevance condition. Formally, when $\text{Var}[\mathbb{E}[x_\ell | g, w] | w]$ is not almost-surely zero at least for some ℓ , the recentered instrument constructed as $\tilde{z}_\ell = \mathbb{E}[x_\ell | g, w] - \mathbb{E}[x_\ell | w]$ is relevant.

A closely related regression-based solution to OVB is further implied by Lemma 1: including the expected instrument μ_ℓ as a control while using the original z_ℓ as an instrument. This regression yields the reduced-form and first-stage moments $\mathbb{E} [\frac{1}{L} \sum_\ell z_\ell y_\ell^\perp]$ and $\mathbb{E} [\frac{1}{L} \sum_\ell z_\ell x_\ell^\perp]$, where v_ℓ^\perp denotes the cross-sectional projection of v_ℓ on μ_ℓ . Appendix C.1 shows that these moments also identify β under Assumption 2(i).

This result clarifies the role of conventional controls and fixed effects in purging OVB under our assumptions: shock exogeneity is sufficient to identify β without recentering z_ℓ or restricting unobservables only when the included controls linearly span μ_ℓ . In panel data, for example, unit fixed effects generally purge OVB only when the expected instrument is time-invariant, which generally requires both the exposure w and the conditional distribution of shocks given w to be time-invariant. While plausible in some applications, these conditions (in particular, stationarity of the shock distribution) are quite restrictive. For instance, when roads tend to be built more than destroyed expected market access will tend to grow over time. Similarly, when the average growth in Chinese imports across manufacturing industries changes over time expected shift-share instruments that average those shocks together (as in Autor et al. (2013)) will tend to be time-varying, even if the exposure shares are held fixed.¹⁴

Given identification of β , one may be interested in consistency of the recentered IV estimator which uses \tilde{z}_ℓ as an instrument for x_ℓ . Establishing consistency with our general asymptotic sequence is non-trivial, as we cannot rely on conventional sampling-based arguments for *iid* data. Instead, we show in Appendix A.2 how consistency is achieved given an asymptotic first stage, a substantive assumption on the mutual cross-sectional dependence of \tilde{z}_ℓ , and a weak regularity condition on the residual ε_ℓ . In line with our general approach, we make no restriction on the mutual dependence of residuals. Intuitively, the substantive assumption requires the recentered instrument construction to well-differentiate observations by their exposure to the exogenous shocks, yielding a law of large numbers that brings $\hat{\beta}$ close to β for large L . Lower-level conditions sufficient for this assumption are also given in Appendix A.2.

3.3 Specifying Shock Counterfactuals

Our solution to the OVB challenge involves measuring the expected instrument, which typically requires specifying counterfactual shocks that may well have occurred. Here we formalize this specification of the shock assignment process, and discuss general ways in which it may be accomplished. We discuss and illustrate specific approaches in the context of various applied settings in Section 4.

Formally, we denote the shock assignment process by the conditional distribution of $g \mid w$, which

¹⁴Borusyak et al. (2019) show that period fixed effects are also not enough in this scenario when the sum of exposure shares (e.g. the lagged share of manufacturing employment in Autor et al. (2013)) varies across regions. When shocks are *iid* within periods, the expected instrument is instead captured by the interaction between this sum of shares and period indicators.

we write as $G(g | w)$. When $G(\cdot)$ is known, the expected instrument $\mu_\ell = \int f_\ell(\gamma; w) dG(\gamma | w)$ is measurable and can be used to purge OVB. To emphasize the importance of such knowledge, we state it as an assumption:

Assumption 3. (*Known assignment process*): $G(g | w)$ is known in the support of w .

Specification of $G(\cdot)$ is straightforward when the shocks are actually determined by a known randomization protocol, either overseen by the researcher herself or occurring naturally. Literal randomization of g given w implies both the exogeneity of shocks (i.e. Assumption 1, given shock exclusion) and Assumption 3; the expected instrument may then be given either analytically or by simulating and averaging across counterfactual randomizations. One might also leverage known discontinuities in the policies that generate shocks by appealing to local randomization (Lee 2008).

Absent true randomization or discontinuities, one might instead satisfy Assumptions 1 and 3 by intuitive specifications of shock exchangeability given appropriate w . Suppose, for example, that one assumes all permutations of g are equally likely to arise, as when the shocks g_n are *iid* across n . In this case $G(g | w)$ is known to be uniform when w includes the permutation class $\Pi(g) = \{\pi(g) | \pi(\cdot) \in \Pi_N\}$, where Π_N denotes the set of permutation operators $\pi(\cdot)$ on vectors of length N (e.g. Lehmann and Romano 2006, p. 634). The marginal distribution of g_n (conditionally on other components of w) then need not be specified; the expected instrument is the average z_ℓ across all permutations of shocks, which serve as counterfactuals:

$$\mu_\ell = \frac{1}{N!} \sum_{\pi(\cdot) \in \Pi_N} f_\ell(\pi(g); w). \quad (8)$$

This μ_ℓ is easy to compute (or approximate with a random set of permutations, when N is large).¹⁵ This scenario highlights the potentially dual role of w in our framework: as a means of satisfying exogeneity (Assumption 1) and as a way to simplify the specification of shock counterfactuals (Assumption 3).¹⁶

Similar expected instrument calculations follow under weaker shock exchangeability conditions. When the g_n are *iid* within, but not across, a set of known clusters, Assumption 3 is satisfied when the class of within-cluster permutations is conditioned on and used to draw counterfactuals. Assignment processes and exchangeability up to some unknown parameters can be accommodated, allowing for example parameterized shock heteroskedasticity (see Section 3.6). Besides exchangeability, other

¹⁵ Approximating μ_ℓ is sufficient for identification because the recentered IV still identifies β in this case: i.e. $\mathbb{E} \left[\frac{1}{T} \sum_\ell (f_\ell(g, w) - f_\ell(\pi(g), w)) \varepsilon_\ell \right] = 0$ under Assumption 2(i), for any fixed or random $\pi(\cdot)$.

¹⁶ A subtlety with Assumption 3 is that it requires the conditional distribution of g to be known in the full support of w . In some cases this is not restrictive, such as with the exchangeability assumption discussed above. It is also not restrictive if $g \perp (\varepsilon, w)$, as when w contains predetermined variables and g arises from a truly random process or natural event, since then $G(g | w)$ does not depend on w . However, in other cases specifying the distribution of $g | w$ for counterfactual w may be challenging. In a transportation network setting where upgrades g are drawn randomly from a predetermined plan w , it may be infeasible to specify the distribution of upgrades given *any* possible plan, or even define the set of such plans, i.e. the support of w . In these situations our framework still directly applies viewing w as non-stochastic.

symmetries in the joint shock distribution can also be used to construct valid counterfactuals (as we illustrate in Section 4.4).

We finally note that in observational data it is imperative to corroborate an *ex ante* argument for Assumptions 1 and 3 by empirical tests. The next section shows that these assumptions yield testable implications and a natural testing procedure.

3.4 Randomization Inference and Testing

We next discuss how specification of the shock assignment process can be used to construct valid statistical tests and confidence intervals for β , following a long tradition of randomization inference (Fisher 1935). Under constant effects the RI approach guarantees correct coverage in finite samples, of both observations and shocks, even when the observations exhibit complex unobserved dependencies. We focus on a particular type of RI tests which is tightly linked to our estimation approach and may have favorable large-sample power. We then discuss how RI can be used to validate Assumptions 1 and 3, through exact falsification and specification tests.

In general, RI tests and confidence intervals for β are based on a scalar test statistic $T = \mathcal{T}(g, y - bx, w)$, where b is a candidate parameter value. Under the null hypothesis of $\beta = b$ and Assumption 1, the distribution of $T = \mathcal{T}(g, \varepsilon, w)$ conditional on ε and w is implied by the shock assignment process $G(g | w)$. One may simulate this distribution, by redrawing (e.g., permuting) the shocks in g and recomputing T . If the original value of T is far in the tails of the simulated distribution, one has grounds to reject the null that $\beta = b$. Appendix A.1 formalizes this logic and explains how inversion of these tests yields confidence interval for β by collecting all b that are not rejected. These intervals have correct size, both conditionally on (ε, w) and unconditionally. Valid RI confidence intervals can be obtained for any test statistic, although the choice of \mathcal{T} generally affects the power against alternative hypotheses.¹⁷

We address the practical issue of choosing a powerful randomization test statistic, and draw a tight link between \mathcal{T} and the recentered IV estimator $\hat{\beta}$, by building on the theory of Hodges and Lehmann (1963). Specifically, we consider a $\mathcal{T}(g, y - bx, w)$ which $\hat{\beta}$ rationalizes as being typical under the null, in the following sense:

Lemma 2. *Let $T = \mathcal{T}(g, y - bx, w)$ and $T^* = \mathcal{T}(g^*, y - bx, w)$, where g^* is distributed according to $G(\cdot | w)$, independently of (g, x, y) , conditionally of w . Define the Hodges-Lehmann estimator as the $b \in \mathbb{R}$ that solve $T = \mathbb{E}[T^* | y, x, w]$. Then the recentered IV estimator is the Hodges-Lehmann estimator associated with the statistic $T = \frac{1}{L} \sum_{\ell} (f_{\ell}(g, w) - \mu_{\ell})(y_{\ell} - bx_{\ell})$.*

Proof. See Appendix C.2. □

¹⁷There are no general results on the relative power of different RI statistics, although good power properties of certain statistics have been established in some special contexts (Lehmann and Romano 2006, Section 15.2.2).

This result shows that the recentered IV estimator of β equates the sample covariance between the recentered instrument \tilde{z}_ℓ and implied residual $y_\ell - bx_\ell$ with the expectation of its randomization distribution, satisfying our definition of a Hodges-Lehmann estimator.¹⁸ Notably, the same randomization tests, confidence intervals, and Hodges-Lehmann estimators are obtained from the statistic based on the non-recentered instrument, $\frac{1}{L} \sum_\ell f_\ell(g, w)(y_\ell - bx_\ell)$.¹⁹ In this sense, the RI approach performs the recentering needed for identification of β automatically.

Statistics chosen on the basis of Hodges-Lehmann estimators can inherit their power properties. While we are not aware of existing general results, in Appendix A.2 we show that randomization tests of Lemma 2 are generally consistent, in the sense of having power that asymptotically increases to one for any fixed alternative, under the conditions which make the recentered IV estimator consistent. This asymptotic result reinforces the tight connection between \mathcal{T} and $\hat{\beta}$.²⁰ We note, however, that as in other settings (e.g. Abadie et al. 2010; Mackinnon and Webb 2020) the finite-sample validity of RI may be most useful when the conditions for consistency are *not* met, such as when there are few shocks with concentrated exposure (we discuss an example of such a setting in Section 4.3 and illustrate good finite-sample power of RI for shift-share instruments with few shocks in Section 4.4).

Randomization inference can also be used to perform falsification tests on our key Assumptions 1 and 3. Recentering implies a testable prediction that \tilde{z}_ℓ is orthogonal to any variable r_ℓ satisfying $g \perp r \mid w$, which holds for r_ℓ that are either functions of w or some other observables thought to be determined prior to (or independent of) the shocks g . To test this restriction, one may check that the sample covariance $\frac{1}{L} \sum_\ell \tilde{z}_\ell r_\ell$ is sufficiently close to zero by re-randomizing shocks and checking that T is not in the tails of its conditional-on- (w, r) distribution. Multiple falsification tests, based on a vector of predetermined variables R_ℓ , can be combined by an appropriate RI procedure, e.g. by taking T to be the sample sum of squared fitted values from regressing \tilde{z}_ℓ on R_ℓ .²¹

Falsification tests are useful in two ways. First, when r_ℓ is a lagged outcome or another variable thought to proxy for ε_ℓ , they provide an RI implementation of conventional placebo and covariate balance tests of Assumption 1. While the use of RI for inference on causal effects may be complicated by treatment effect heterogeneity, the sharp hypothesis of zero placebo effects is a natural null. Second,

¹⁸This definition follows Rosenbaum (2002) and Imbens and Rosenbaum (2005). The original definition in Hodges and Lehmann (1963) is the value of β that maximizes the p-value of the randomization test. For two-sided confidence interval this means equating T to its median, rather than its mean.

¹⁹This follows because recentering shifts both T and T^* by the same value, $\frac{1}{L} \sum_\ell \mu_\ell (y_\ell - bx_\ell)$, which does not depend on g . Appendix C.2 further shows that the μ_ℓ -controlled IV estimator is the Hodges-Lehmann estimator corresponding to the residualized covariance statistic $\frac{1}{L} \sum_\ell z_\ell (y_\ell^\perp - bx_\ell^\perp)$.

²⁰One might instead consider computing confidence intervals from the distribution of the recentered estimator itself with re-randomized shocks g^* . This idea fails in IV since the re-randomized instrument $f_\ell(g^*, w) - \mu_\ell$ has a true first-stage of zero. The distribution of reduced-form coefficients across re-randomized shocks is also not useful, except for testing $\beta = 0$, as that distribution is centered around zero rather than β .

²¹Formally, this $T = \tilde{z}'R(R'R)^{-1}R'\tilde{z}$ can be seen as a quadratic form of the vector-valued statistic $\frac{1}{L} \sum_\ell \tilde{z}_\ell R_\ell$, weighted by $(R'R)^{-1}$, where R is the matrix collecting R_ℓ and \tilde{z} is the vector collecting \tilde{z}_ℓ . In applications we will further exploit the observation that each coefficient in the regression of \tilde{z}_ℓ on R_ℓ can be directly used as an RI test statistic: by the Frisch-Waugh-Lovell theorem, the k th coefficient is proportional to $\frac{1}{L} \sum_\ell \tilde{z}_\ell R_{k\ell}^\perp$, where here R_k^\perp denotes the vector of residuals from projecting the k th column of R on all other columns.

RI tests will generally have power to reject false specifications of the shock assignment process, i.e. violations of Assumption 3, even when r_ℓ does not proxy for ε_ℓ . For $r_\ell = 1$, for example (which is trivially conditionally independent of g), the test verifies that the sample mean of z_ℓ is typical for the realizations of the specified assignment process. Setting $r_\ell = \mu_\ell$ instead checks that the recentered instrument is not correlated with the expected instrument that it is supposed to remove.

3.5 Asymptotic Efficiency

While any instrument $f_\ell(g, w)$ can be made valid by appropriate recentering and used for valid randomization inference, the choice of instrument construction from the set of possible $\{f_\ell(\cdot)\}_{\ell=1}^L$ will generally matter for power. In Appendix A.3 we show that the following instrument minimizes the asymptotic variance of recentered IV and maximizes the local power of RI-based tests, under appropriate regularity conditions:

$$z^* = \mathbb{E}[\varepsilon\varepsilon' | w]^{-1} (\mathbb{E}[x | g, w] - \mathbb{E}[x | w]). \quad (9)$$

This characterization extends the classic result of Chamberlain (1987) to our setting in showing how exogenous shocks can be efficiently leveraged. Constructing such optimal instruments may not be feasible in practice, and typically requires an economic model for both the dependence of treatment on shocks and the endogeneity of exposure: $\mathbb{E}[x | g, w]$ and $\mathbb{E}[\varepsilon\varepsilon' | w]$, respectively. Our characterization nevertheless provides guidance for constructing recentered instruments, by showing what researchers should strive for when choosing between alternative IV estimators.

To build intuition for the optimal instrument, we establish the following Lemma:

Lemma 3. *Let $\tilde{z} = \mathbb{E}[x | g, w] - \mathbb{E}[x | w]$, $\psi = \mathbb{E}[\varepsilon | w]$, and $\Omega = \text{Var}[\varepsilon | w]$. Then*

$$z^* = \Omega^{-1} (\tilde{z} - \hat{\nu}\hat{\rho}\psi), \quad (10)$$

where $\hat{\rho} = \frac{\psi'\Omega^{-1}\tilde{z}}{\psi'\Omega^{-1}\psi}$ is the coefficient from projecting \tilde{z} on ψ (weighted by Ω^{-1}), and $\hat{\nu} = \frac{\psi'\Omega^{-1}\psi}{1+\psi'\Omega^{-1}\psi}$.

Proof. See Appendix C.3. □

Equation (10) permits an intuitive four-step description of the optimal instrument. First, one takes the best predictor of treatment given by the shocks and predetermined variables, $\mathbb{E}[x | g, w]$. Second, one recenters this predictor by $\mathbb{E}[x | w]$ to remove the potential OVB from non-random shock exposure, obtaining \tilde{z} . Third, one partially residualizes the recentered instrument on the predictable component of the residual ψ .²² Finally, one adjusts for the residual variance Ω , as in generalized least

²²This residualization is partial (i.e. $\hat{\nu} \in [0, 1)$) for the same reason as why, in the conventional panel data context, the random effects estimator demeans the data within each unit only partially (e.g. Wooldridge 2002, p. 286). As with unit-specific means in the panel setting, ψ_ℓ is orthogonal to \tilde{z}_ℓ in expectation and so provides an additional moment for identifying β . We also note that if ψ is completely known, a more efficient but less robust instrument than (9) is available,

squares. While steps 1 and 4 follow the optimal instrument construction in Chamberlain (1987), steps 2 and 3 are new, stemming from the potential endogeneity of w .

Predicting treatment from shocks and exposure (step 1) is trivial in reduced-form studies (since $\mathbb{E}[x_\ell | g, w] = x_\ell$ for $x_\ell = z_\ell = f_\ell(g, w)$); otherwise it may be feasible when an economic model of treatment is available.²³ Specifically, when $x_\ell = \tilde{f}_\ell(g, w, u)$ for a known $\tilde{f}_\ell(\cdot)$ and a set of unobserved shocks u , a reasonable stand-in for $\mathbb{E}[x_\ell | g, w]$ may be obtained by $\tilde{f}_\ell(g, w, 0)$; that is, a treatment prediction which shuts down the role of unobserved shocks. This approach has been taken, for example, by Bartelme (2018) in the market access setting (see also Berry et al. (1999) for the same idea in an entirely different context). One may also approximate $\tilde{f}_\ell(g, w, 0)$ with a first-order approximation, resulting in a linear shift-share instrument we discuss in Section 4.4. Instrument recentering is then generally necessary to isolate exogenous variation in shocks (step 2).

The third and fourth steps in Lemma 3 may be more difficult to implement as they require models of unobservables rather than the observed treatment. Practically, Step 3 calls to control for predetermined variables which may be correlated with the residual, as including these controls may approximate the projection of \tilde{z} on ψ (by the Frisch-Waugh-Lovell theorem). By the logic of Proposition 1 such controls are orthogonal to \tilde{z} in expectation and will not weaken the first stage, while their inclusion will generally improve efficiency by reducing the residual variance. Step 4 is a more standard correction for heteroskedasticity and mutual correlation of residuals which is commonly ignored in practice. We expect that performing the more feasible steps 1 and 2 alone will typically improve power, although there is no guarantee (see Appendix B.3 for a counterexample discussed in the context of the application in Section 4.2).

3.6 Extensions

Appendices A.4–A.8 extend our basic identification and inference results in several ways. Appendix A.4 shows that in the presence of treatment effect heterogeneity the recentered IV estimator captures a convex average of causal effects under an appropriate monotonicity condition, extending the condition of Imbens and Angrist (1994) to this setting. This appendix further shows how a particular reweighting of the recentered instrument—with weights given by the shock assignment process—can yield identification of local average treatment effects in the traditional setting of a binary treatment and instrument. Appendix A.5 shows how recentered IVs can be constructed when the shock assignment process is only partially specified, allowing for a vector of unknown parameters which govern, for example, how shocks vary systematically with observables. Valid finite-sample confidence intervals can be obtained in this case by a two-step RI procedure (Berger and Boos 1994). Appendix A.6

which replaces y with $y - \psi$ and ε with $\varepsilon - \psi$ (by not adjusting x) and uses the original z . Since $\mathbb{E}[\varepsilon - \psi | w] = 0$, instrument recentering that isolates variation in g but reduces power is unnecessary. However, this efficiency gain is obtained at the cost of losing robustness to misspecification of the residual model.

²³Obtaining $\mathbb{E}[x_\ell | g, w]$ without a treatment model is challenging in our general non-*iid* setup, in contrast to other settings where the first stage can be estimated non-parametrically (e.g. Newey (1990)).

shows how predetermined observables can be included as regression controls in our setting to reduce residual variation and potentially increase power. Appendix A.7 discusses identification and inference with multiple treatments or instruments. Finally, Appendix A.8 extends our framework to nonlinear outcome models.

4 Practical Implications and Applications

We now show how our theoretic framework brings new practical insights to a variety of empirical settings: to the estimation of transportation infrastructure effects (Section 4.1), effects of policy eligibility with simulated instruments (Section 4.2), network spillover effects (Section 4.3), to linear and nonlinear shift-share instruments (Section 4.4), and to other designs (Section 4.5). We illustrate some of these insights with empirical applications, estimating market access effects of Chinese high-speed rail and insurance coverage effects of Medicaid expansions, and with Monte Carlo simulations.

4.1 Effects of Transportation Infrastructure

We first apply our framework to estimate the effect of market access growth on Chinese regional employment growth over 2007–2016, leveraging the construction of high-speed rail (HSR). We show how counterfactual HSR networks can be specified, and how correcting for expected market access growth can help purge OVB. We then provide a broader discussion of how our approach to estimating transportation infrastructure effects relates to existing methods.

Application The recent construction of Chinese HSR over 2007–2016 gave China a network longer than in all other countries combined (Lawrence et al. 2019). We first reproduce, with more recent data and a correspondingly larger network, the finding of Lin (2017) that the HSR-driven growth of market access strongly predicts local employment growth. We then propose and validate a specification of counterfactual HSR maps based on planned but unbuilt lines. Recentering observed market access growth by these counterfactuals reduces the estimated employment effect substantially, to the point of statistical insignificance. Employment growth is instead strongly predicted by expected market access: regions more exposed to *potential* HSR expansion tend to see faster employment growth.

The Chinese HSR network mostly consists of dedicated passenger lines and has developed rapidly since 2007. Construction was started by the Medium- and Long-Term Railway Plan in 2004; this plan was later expanded in 2008 (as part of the stimulus package during the financial crisis) and again in 2016 (Lawrence et al. 2019). Construction objectives included freeing up capacity on the existing rail network (including freight lines) and supporting economic development by improved regional connectivity (Lawrence et al. 2019; Ma 2011). While affordable fares make HSR popular for different purposes, business travel is an important component of rail traffic, ranging between 28% and 62%,

depending on the line (Ollivier et al. 2014; Lawrence et al. 2019). The role of HSR extends beyond directly connected regions, as passengers frequently transfer between HSR lines and between HSR and traditional lines. An early analysis of Zheng and Kahn (2013) finds positive effects of HSR on housing prices, while Lin (2017) similarly finds positive effects on regional employment.

We analyze HSR-induced market access effects for 340 sub-province-level administrative divisions in mainland China. We follow Potlogea and Cheng (2017) in referring to these units as prefectures: although most are officially called “prefecture-level cities,” they typically include multiple urban areas. We measure market access in 2007 and 2016 by combining data on the development of the HSR network and each prefecture’s location and population (as measured in the 2000 census). A total of 83 HSR lines opened between these years, with the first in 2008; a further 66 lines (which we refer to as “planned”) were completed or under construction as of April 2019.²⁴ We compute a simple market access measure in each prefecture ℓ and year t based on the formula in Zheng and Kahn (2013): $MA_{\ell t} = \sum_k \exp(-0.02\tau_{\ell kt}) \cdot P_{k,2000}$. The summation is over all prefectures (including $k = \ell$), $P_{k,2000}$ denotes the predetermined population of prefecture k , and $\tau_{\ell kt}$ denotes predicted travel time between regions ℓ and k in year t (in minutes). Travel time predictions are based on the operational speed of each HSR line as well as geographic distance, which proxies for the travel time by car or a low-speed train. We relate market access growth, $z_\ell = \log MA_{\ell,2016} - \log MA_{\ell,2007}$, to the corresponding growth in prefecture’s urban employment y_ℓ from the Chinese City Statistical Yearbooks. This yields a set of 274 prefectures with non-missing outcome data; see Appendix B.1 for details on the sample construction and market access measure. Panel A of Figure 2 shows the Chinese HSR network as of the end of 2016, along with the implied growth of market access relative to 2007.

Column 1 of Table 1 reports the coefficient from a simple regression of employment growth on market access growth; Appendix Figure A3 visualizes this relationship.²⁵ The estimated elasticity of 0.23 is quite large: given an average growth of market access of 0.54 log points, it implies a 12.4% employment growth attributable to the HSR for an average prefecture—almost half of the 26.6% average employment growth over this period. The estimate is also highly statistically significant using the spatially-clustered standard errors of Conley (1999), echoing the findings of Lin (2017) (while not being directly comparable due to our use of later years and a different specification).

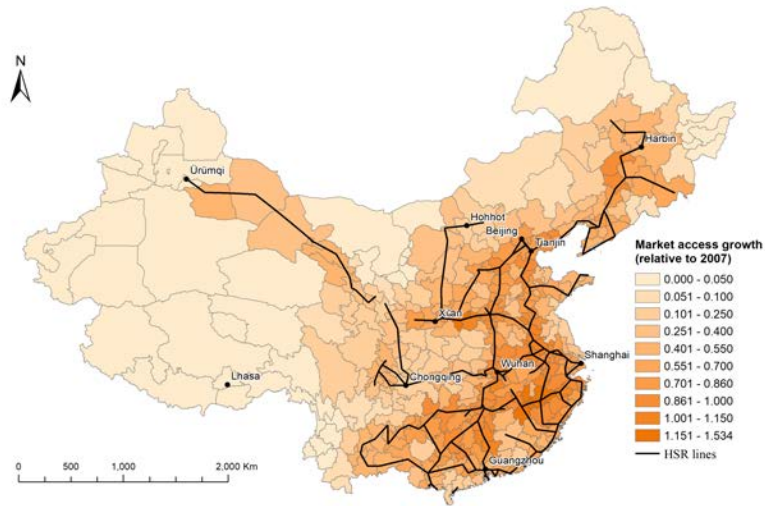
Panel A of Figure 2, however, gives immediate reason for caution against interpreting the OLS coefficient as causal. Prefectures with high market access growth, which serve as the effective treatment group, tend to be clustered in the main economic areas in the southeast of the country where HSR lines are concentrated. Areas near major cities, such as Shanghai and Beijing, also tend to see high

²⁴We define a line by a contiguous set of inter-prefecture HSR links that were proposed together and opened simultaneously. One experimental HSR line between Qinhuangdao and Shenyang opened in 2003. We include it in our market access measure but focus on the majority of HSR-induced changes in the network and employment over 2007–2016.

²⁵This regression can be viewed as a reduced-form of a hypothetical IV regression, in which the treatment variables is a measure of market access that accounts for changing population. We focus on the reduced form here because of data constraints: annual population is measured for all 340 prefectures only in the Census year of 2000. We discuss the potential roles of controls below.

Figure 2: Chinese High Speed Rail and Market Access Growth

A. Completed Lines and Market Access Growth by 2016



B. All Completed and Planned Lines



Notes: Panel A of this figure shows the completed China high-speed rail network by the end of 2016, with shading indicating market access growth relative to 2007. Panel B shows the network of all HSR lines, including those planned but not yet completed as of 2016 (in red).

Table 1: Employment Effects of Market Access: Unadjusted and Recentered Estimates

	Unadjusted OLS (1)	Recentered IV (2)	Controlled OLS (3)
Market Access Growth	0.232 (0.075)	0.036 (0.107) [-0.161, 0.190]	0.078 (0.079) [-0.087, 0.221]
Expected Market Access Growth			0.341 (0.096)
Recentered Prefectures	No 274	Yes 274	Yes 274

Notes: This table reports coefficients from regressions of employment growth on market access growth in Chinese prefectures from 2007–2016. Market access growth is unadjusted in column 1. In column 2 this treatment is instrumented by the market access growth recentered by permuting the opening dates of HSR lines with the same number of links. Column 3 instead estimates an OLS regression with recentered market access growth as treatment and controlling for expected market access growth given by the same HSR counterfactuals. Standard errors which allow for linearly decaying spatial correlation (up to a bandwidth of 500km) are reported in parentheses. 95% confidence intervals based on the same HSR counterfactuals are reported in brackets.

market access growth as they are connected by the HSR network. A comparison between these prefectures and the economic periphery may be confounded by the effects of unobserved policies, both contemporaneous and historic, that differentially affected the economic center.

Our solution is to view certain elements of the HSR network as realizations of a natural experiment. We first formalize this view by specifying a set of counterfactual HSR networks and corroborate it with appropriate falsification tests. We then appropriately recenter market access to ensure that the regression leverages contrasts between actual and counterfactual realizations of the HSR assignment process, and not other sources of cross-sectional variation in market access.

Our specification of counterfactual upgrades exploits the heterogeneous timing of HSR construction. Specifically we permute the 2016 completion status of the built and planned lines, assuming that the timing of line completion is conditionally as-good-as-random. Panel B of Figure 2 compares the built and planned lines which form our counterfactuals. Planned lines tend to be concentrated in the same areas of China as built lines, reinforcing the fact that (unlike in our motivating example in Section 2) construction is not uniformly distributed in space. Although planned lines are of similar length, they tend to connect more regions: the average number of cross-prefecture “links” is 3.31 and 2.45 for built and planned lines, respectively, with a statistically significant difference ($p = 0.029$). To account for this difference we construct counterfactual upgrades by permuting 2016 completion status only among lines with the same number of links. This procedure generates counterfactual HSR maps that are visually similar to the actual 2016 network (see Appendix Figure A2 for an illustrative example) and which isolate more plausibly exogenous variation. For example the main Beijing to

Shanghai HSR line, which has the greatest number of links, is always included in the counterfactuals.

Table 2 validates this specification of the HSR assignment process by the RI tests described in Section 3.4. We regress the resulting recentered market access measure (obtained from 999 counterfactual maps) on a constant and several predetermined controls: a prefecture’s expected market access growth, distance to Beijing, latitude, and longitude. We report the resulting coefficients, along with permutation p-values for individual coefficient significance and their joint significance. . The test passes for all specifications and all but one individual coefficients. The first column, for example, where only a constant is included, indicates that the average market access growth is very similar in the actual and simulated samples; this test would fail if the government prioritized early construction of the lines that generate more market access, even among those with the same number of links. We note that while these results are suggestive of correct specification (i.e. we cannot reject Assumption 3) they do not provide direct support of the exogeneity of HSR construction to the unobserved determinants of employment (i.e. Assumption 1).²⁶

Figure 3 plots expected and recentered market access growth given by the conditional permutations of built and unbuilt lines. The effect of recentering is apparent by contrasting the dark- and light-shaded regions in Panel A of Figure 2 (indicating high and low market access growth) with the solid and striped regions in Panel B of Figure 3 (indicating high and low recentered market access growth). Recentering effectively removes western prefectures by setting $\tilde{z}_\ell \approx 0$, reflecting the fact that expected market access growth in Panel A of 3 is similarly low. Recentering further removes some prefectures in the east (such as Tianjin) which saw an as-expected increase in market access that would otherwise be used to estimate the effect. At the same time, recentering provides a justification for retaining other regional contrasts. Hohhot, for example, had a higher expected market access growth than Harbin, due to the planned direct connection to Beijing. This connection was still under construction in 2016, however, resulting in lower observed market access growth in Hohhot than in Harbin.

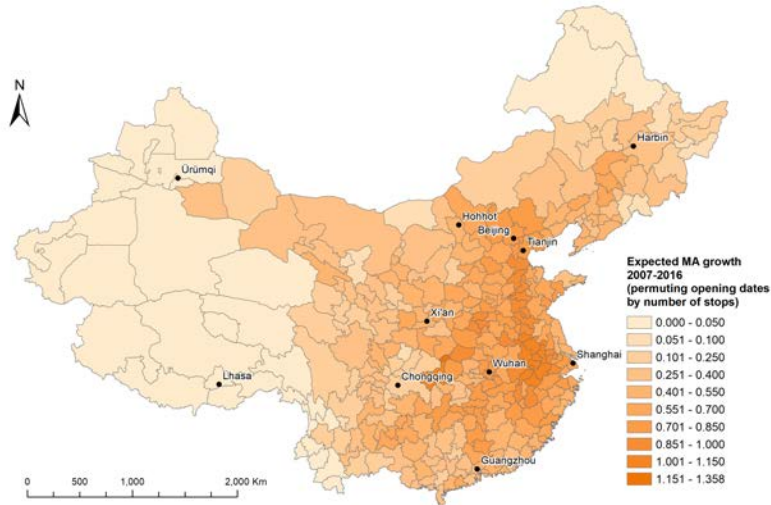
Columns 2 and 3 of Table 1 show that adjusting for expected market access growth reduces the estimated employment elasticity substantially, from 0.23 to 0.04 when we instrument by \tilde{z}_ℓ or to 0.08 when we control for μ_ℓ . This difference is explained by the fact that employment growth is strongly predicted by expected market access growth: in column 3 we find a large coefficient on μ_ℓ , of 0.34.²⁷ Appendix Figure A4 illustrates these findings. Notably, neither of the two adjusted estimates is statistically distinguishable from zero according to either Conley (1999) spatial-clustered standard errors and permutation-based inference (which broadly agree in this setting). The apparently large market access effect estimate in column 1 can thus be viewed as an artifact of non-random exposure

²⁶While our specification tests pass for the 2007–2016 long difference, and are robust to using long differences ending in 2014 or 2015, we have verified in unreported results that the same assignment process is rejected in specifications which focus on earlier years of HSR development. Specifying a dynamic network formation process is a challenging issue in general. We thus focus on the long difference for estimation.

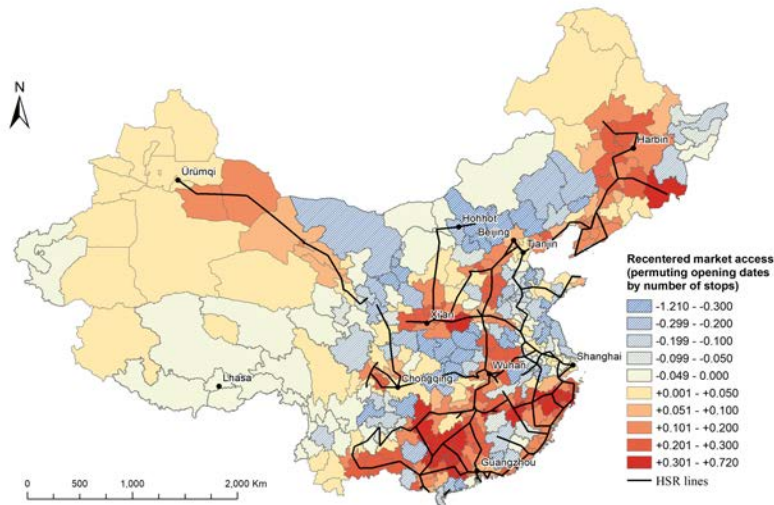
²⁷We use recentered (rather than unadjusted) market access growth as the treatment in column 3. This does not change the estimate of β , but makes the coefficient on the expected instrument control more interpretable: the unadjusted effect in Column 1 is then a weighted average of the two coefficients in column 3.

Figure 3: Expected and Recentered Market Access Growth from Chinese HSR

A. Expected Market Access Growth



B. Recentered Market Access Growth



Notes: Panel A of this figure shows the variation in expected market access growth across Chinese prefectures from 2007 to 2016, simulated by permuting the opening dates of lines with the same number of links. Panel B plots the variation in recentered market access growth: the difference between the market access growth shown in Panel A of Figure 2 and expected market access growth. The HSR network as of 2016 is also shown in this panel.

Table 2: HSR Assignment Process Specification Tests: Regressions of Recentered Market Access

	(1)	(2)	(3)	(4)	(5)
Constant	0.017 {0.460}	0.017 {0.460}	0.017 {0.460}	0.017 {0.460}	0.017 {0.460}
Expected MA Growth		-0.101 {0.260}	-0.030 {0.797}	-0.193 {0.032}	-0.154 {0.094}
Distance to Beijing			0.081 {0.278}		0.036 {0.595}
Latitude/100				-1.019 {0.068}	-0.800 {0.134}
Longitude/100				0.739 {0.074}	0.748 {0.070}
Joint p-value	{0.461}	{0.382}	{0.352}	{0.189}	{0.227}
Prefectures	274	274	274	274	274

Notes: This table reports regression coefficients and permutation p-values for tests of correct specification of the high-speed rail assignment process in the Chinese market access application. Coefficients are from regressions of recentered market access growth (2007–2016) on different sets of predetermined controls, where recentering is done by permuting the opening dates of lines with the same number of links. All regressors are demeaned such that the constant in each regression captures average recentered market access growth. Distance from Beijing is measured in 1,000km. Separate permutation-based p-values for each regression coefficient are reported in curly brackets. Joint permutation p-values are based on each regression’s sum-of-square fitted values, as described in the text.

to the HSR natural experiment, as formalized by our assignment process.²⁸

Discussion Our theoretical results provide a new general approach for estimating the effects of transportation infrastructure upgrades, which remains challenging despite a long history in economics (Redding and Turner 2015). Traditionally, these studies specified as treatment an indicator that region ℓ is connected to the network (e.g. Chandra and Thompson 2000; Michaels 2008) or a measure of local connection intensity (e.g. Baum-Snow 2007; Duranton and Turner 2012). Modern approaches often use more elaborate model-based market access measures (Donaldson and Hornbeck 2016), in recognition of the fact that infrastructure upgrades can impact regions not directly connected to the network. While many analyses of transportation shocks study local outcomes of individual regions, some estimate the effects on bilateral outcomes, such as trade or migration between pairs of regions or firms (Allen et al. 2019; Volpe Martincus and Blyde 2013).

Our framework formalizes three distinct challenges with identifying such effects. The first is strategic placement of infrastructure upgrades in anticipation of regional productivity or amenity growth, a concern that is well-recognized in the literature (Redding and Turner 2015). When viewing upgrades as our shocks g , strategic placement can be formalized by a dependence of g on ε which

²⁸Appendix Table A1 shows that these findings are robust to several alternative measures of employment. It also reports the estimates of the effects of market access on total rail ridership (which may include HSR, traditional intercity rail, and intracity lines). The coefficient decline due to recentering is not as large for this outcome, although the estimates are too noisy to make conclusive claims.

violates shock exogeneity (i.e. Assumption 1). The transportation effects literature has devised several remedies to this challenge, in particular by excluding major cities or other regions which directly affect the placement of infrastructure (the “inconsequential place approach”) or by using planned or historic routes to instrument for the constructed railways. Our framework accommodates these solutions by either limiting attention to inconsequential places (which changes the sample, and therefore ε) or by viewing planned or historic routes as the exogenous shocks (which changes g).²⁹

When no assumption of upgrade exogeneity seems *ex ante* plausible, one can study market access effects by leveraging the exogeneity of its other determinants. For example, Bartelme (2018) estimates market access effects by leveraging exogenous shifters to market size and not transportation upgrades. Our framework also applies to this strategy, with market size shifters collected in g . It is generally difficult to obtain causal estimates without any exogeneity assumption placed on any shock to market access.³⁰ We therefore suppose that Assumption 1 holds for some g and ε , with the other determinants of market access collected in w . For concreteness we suppose g captures some features of transportation upgrades, as the alternative assumption of Bartelme (2018) is less standard.

A second challenge that is less discussed in the transportation upgrade literature is that a cross-sectional correlation between regional connectivity and unobservables is likely to arise from their common dependence on regional geography. Even when upgrades are exogenous in the sense of Assumption 1, they are unlikely to be uniformly assigned across regions. For example, if upgrades are concentrated in more economically developed areas (as in our HSR application above), which differ in their unobservables, an OVB problem may arise. Formally, $\mathbb{E}[\varepsilon_\ell | w]$ and expected network connectivity may covary across regions ℓ .

A further third challenge arises with measures like market access, which respond to non-local upgrade shocks and are defined by a non-linear formula. As discussed in Section 2, this can lead to complex variation in expected market access growth even when exogenous upgrades are uniformly distributed in space. This challenge highlights a novel tradeoff between traditional connectivity regressions and the modern approach of Donaldson and Hornbeck (2016): although market access regressions may better capture the non-local effects of transportation upgrades (and, as such, satisfy the exclusion restriction in Assumption 1), they may also lead to more intricate OVB challenges.

These two new challenges are not specific to connectivity and market access regressions. For example Allen et al. (2019) study how migration between locations in the U.S. and Mexico depends on a measure of difficulty of traveling between them, as affected by the wall constructed in some parts

²⁹Other weakened versions of Assumption 1 are possible too, e.g. that ε_ℓ is conditionally independent of g_n for all potential lines n that do not cross region ℓ ; the market access instrument should then be constructed excluding those lines (Lin 2017). We also note that these approaches may not suffice to remove all statistical dependence between g and ε , for instance if productivity growth is correlated between the major and “inconsequential” regions and historic routes affect current productivity growth in unobserved ways.

³⁰Appendix A.9 illustrates this issue in a scenario where strategic choice of upgrades is the only problem (because there are no other shocks and all relevant shock exposure is exogenous). Consistency of the estimator is obtained under the “approximate” exogeneity of line placement—a nontrivial additional assumption.

of the border between the two countries. Even when new wall placement is not strategic, it is unlikely to be uniformly distributed along a border (the second challenge). The effects of wall upgrades are furthermore non-local, complicating OVB (the third challenge). One may expect, for instance, that wall-induced changes in travel difficulty are correlated with the distance between locations (and thus potentially with the error term): regardless of which sections of the wall are built, places far away from the border will be affected less. This is because, when traveling between them, it is easier to substitute away from newly blocked routes.

Both of the new challenges raised by our framework are solved by specifying and simulating counterfactual transportation network upgrades, which can be achieved in several ways. A first approach, which we illustrated above, is to use predetermined upgrade plans and appeal to the randomness of which subset of the plan materializes as of some date. This approach contrasts with the typical use of upgrade plans in the literature, as instruments themselves (Redding and Turner 2015), which can relax Assumption 1 but does not help with specifying shock counterfactuals.³¹ Our strategy for specifying counterfactuals is instead similar to the use of accepted and rejected plans for new plant construction in Greenstone et al. (2010), but in the transportation setting. Donaldson (2018) and Berger and Enflo (2017) take a step in this direction, using planned but unbuilt railroads in a placebo exercise, as does Lin (2017) by exploiting engineering problems that slowed down construction of certain lines in a robustness check.

A second approach to specifying counterfactuals is to model upgrades in terms of their engineering, economic or political requirements and find alternative upgrades that satisfy these criteria. One of the placebo analyses by Ahlfeldt and Feddersen (2018) follows this logic: they note that the new railway line connected two major cities in Germany at distance around 160km and had three intermediate stops. They then construct 1,000 random placebo lines that satisfy the same description. One could also obtain engineering estimates of viable alternative routes for lines connecting major cities, augmenting the inconsequential places approach with valid counterfactuals.³²

A third approach leverages known discontinuities in the policies determining network links, as in a simpler regression discontinuity analysis. Campante and Yanagizawa-Drott (2018), for example, note that cities just under 6,000 miles apart are distinctly more likely to have direct air links, relative to cities just above that threshold. A local randomization view of such discontinuities (e.g., Lee (2008)) might motivate counterfactual networks that perturb links around the threshold.

Finally, the assignment process for the shocks may sometimes be given by external (e.g. institu-

³¹Historical routes can be used in place of the plan if the researcher is willing to assume that some of them decayed for random reasons while others continued to be in use. This again contrasts with the typical use of historical routes (e.g. Duranton et al. (2013)). We note that if historic routes or plans are instead used to satisfy Assumption 1 then counterfactual routes and plans need to be simulated to apply our approach.

³²These approaches can be viewed as special cases of specifying a stochastic network formation model and simulating it to construct counterfactuals. Other models of network formation (e.g., Chandrasekhar and Jackson 2014; Acemoglu and Azar 2020) could similarly be used to study causal effects of network centrality in appropriate economic contexts (see footnote 40). To our knowledge, this would be a novel use of such models.

tional or scientific) knowledge. For example, Volpe Martincus and Blyde (2013) leverage an earthquake that blocked a large number of roads in Chile to estimate the effect of infrastructure downgrades on trade patterns. Geological knowledge could be used in this case to specify the disruption locations of counterfactual earthquakes and construct the expected instrument.

Without specifying counterfactuals (or even assuming upgrade exogeneity) one may remove some sources of OVB by including control variables, such as province fixed effects, geographic coordinates, and pre-period characteristics of the local economy. The unobservables ε_ℓ which are not captured by those controls should then be assumed orthogonal to the treatment variable. The case for such orthogonality can, however, be challenging to make for market access (or similar treatments) constructed from many sources of variation. It may be untenable to assume that *all* of the treatment determinants are orthogonal to the remaining variation in ε_ℓ , while the weaker assumption that such orthogonality holds despite the endogeneity of some determinants of treatment appears hard to justify *a priori*.

Relative to a conventional controlling approach, our framework for estimating transportation upgrade effects offers two important advantages. First, by clarifying an experimental ideal it makes the argument for market access exogeneity explicit and transparent and allows for a substantive debate on whether the institutional features of the setting make such argument *ex ante* plausible. Second, it yields additional tests: while pre-trend and balance tests are useful with any identification approach, our framework offers new specification tests (as illustrated in the HSR setting above), with a new mode of inference. Our approach also yields new robustness checks for a conventional controlling strategy. If market access effects are identified without recentering, because the included controls perfectly capture either the expected instrument or the endogenous features of shock exposure, then the estimates should be robust to further controlling for expected market access growth constructed with some reasonable counterfactuals.³³

4.2 Simulated Eligibility Instruments

We next show how our framework yields more efficient instruments for estimating the effects of policy eligibility, relative to the commonly employed simulated instrument approach of Currie and Gruber (1996a, 1996b). Validity of our novel instruments arises from the same policy exogeneity assumptions as for simulated instruments, but they attain power gains by incorporating endogenous (yet predictive) variation in policy exposure and applying appropriate recentering. We first describe the general approach, drawing on the optimal IV results of Section 3.5. We then illustrate the power gains in an application estimating the take-up and crowd-out effects of Medicaid eligibility.

³³Formally, if either $\mathbb{E} \left[\frac{1}{L} \sum_\ell \tilde{z}_\ell \mid w \right] = 0$ or $\mathbb{E} \left[\frac{1}{L} \sum_\ell \check{\varepsilon}_\ell \mid w \right] = 0$, where \check{v}_ℓ denotes the in-sample residualization of variable v_ℓ on some functions of w used as controls, then $\mathbb{E} \left[\frac{1}{L} \tilde{z}_\ell \check{\varepsilon}_\ell \right] = 0$ where $\tilde{z}_\ell = z_\ell - m_\ell(w)$ for a possibly incorrectly specified expected instrument $m(w)$.

General Approach We suppose that β captures the causal effect of eligibility $x_\ell \in \{0, 1\}$ of individual ℓ for a public program (such as Medicaid or unemployment insurance) on some outcome y_ℓ (such as program takeup, health status, or educational attainment). Eligibility may be a complicated function of regional, e.g. state-level, government policy and individual characteristics such as income or family structure. While individual characteristics are likely correlated with the residual, making x_ℓ endogenous, we suppose that the state policies may be plausibly viewed as exogenous.

In such settings Currie and Gruber (1996a, 1996b; henceforth CG) propose the use of simulated instruments to isolate the exogenous policy variation.³⁴ The CG instrument simulates, for each state, the average eligibility of a nationally representative sample of individuals if they were to reside in that state and be subject to its policies. These simulated eligibilities are then assigned to individuals on the basis of their actual state of residence. In viewing the state policies as plausibly exogenous shocks, we write $g = (g_1, \dots, g_{50})'$ with g_n representing the eligibility policy of state n . To define the CG instrument, we write $h(g_n)$ for the simulated eligibility of the representative sample if they were subject to policy g_n .³⁵ The CG instrument can then be written $z_\ell^{CG} = h(g_{s_\ell})$, where s_ℓ denotes the state of residence of individual ℓ .

Since the CG instrument is a function of state policies only, it is valid when those policies are exogenous. Here we formalize the policy natural experiment by the row-exchangeability of g , conditional on the collections of state residencies s and both observed individual demographics v and unobserved ε . This implies Assumption 1 and a conditionally uniform distribution of g_n given $w = \{s, v, \Pi(g)\}$, where $\Pi(g)$ again denotes the permutation class of g . The expected CG instrument $\mu_\ell^{CG} = \mathbb{E}[z_\ell^{CG} | w] = \frac{1}{50} \sum_n h(g_n)$ is thus constant across individuals, so there is no OVB from using z_ℓ^{CG} . Statistical inference with z_ℓ^{CG} is also straightforward: when eligibility policies are *iid* across states, conventional state-clustered standard errors are likely to suffice. Thus, the above framework does not bring new insight to the use of conventional simulated instruments.

Embedding the CG instrument in the framework of Section 3 instead shows how to construct other valid instruments, based on the same natural experiment but leveraging additional sources of variation. When all individual characteristics relevant to eligibility determination—such as income, family structure, or employment status—are observed and contained in v_ℓ , eligibility itself may be written as a function of shocks and exposure measures: $x_\ell = f(g_{s_\ell}; v_\ell)$, where $f(\cdot)$ is a known mapping. By simulating the average x_ℓ across permutations of state policies, one can measure each individual’s expected eligibility $\mu_\ell = \mathbb{E}[x_\ell | w] = \frac{1}{50} \sum_n f(g_n; v_\ell)$ and either use it to form a recentered IV or control for it in an OLS regression of y_ℓ on x_ℓ to purge OVB. This contrasts with the usual simulation in CG’s approach: rather than applying ℓ ’s state policy to random individuals to construct an instrument

³⁴For example, Currie and Gruber (1996a) write that their aim is “to achieve identification using only legislative variation in Medicaid policy” (p. 445). We interpret this as positing exogenous variation in policies across states.

³⁵For simplicity we view the $h(\cdot)$ function as non-stochastic, ignoring randomness of the representative sample. In principle simulated instruments can be constructed from the entire national population.

for x_ℓ , our approach applies random state policies to individual ℓ to construct a control μ_ℓ .³⁶

Alternative recentered IVs are likely to provide higher asymptotic power, per the discussion in Section 3.5. Since $\mathbb{E}[x_\ell | g, w] = x_\ell$, the recentered IV $\tilde{z}_\ell = x_\ell - \mu_\ell = \mathbb{E}[x_\ell | g, w] - \mathbb{E}[x_\ell | w]$ is precisely the inner term of the optimal instrument in equation (9). To see intuitively why this \tilde{z}_ℓ is likely to yield power gains, consider the set of individuals who have the same eligibility under every state’s policy such that $x_\ell = \mu_\ell$. The presence of such individuals is likely to weaken the first-stage of the CG instrument, since they are unaffected by variation in z_ℓ^{CG} . The recentered IV estimator effectively removes these inframarginal individuals, for whom $\tilde{z}_\ell = 0$.

In Appendix A.10 we extend these insights by showing how more efficient instruments can be constructed when some individual determinants of eligibility are unobserved (as in Cohodes et al. (2016)) or endogenously respond to the state policies (as in East and Kuka (2015)). The results similarly apply in settings where only some policy variation is exogenous, as our application next illustrates. We further discuss in Appendix A.10 the advantages of our recentered IV relative to controlling for individual characteristics flexibly, as is common in the related literature on the eligibility effects of unemployment insurance (e.g., Cullen and Gruber 2000).

Application We illustrate our approach to simulated instruments by estimating the insurance coverage effects of a partial expansion of U.S. Medicaid eligibility in 2014. We first estimate Medicaid take-up and private insurance crowd-out effects from this expansion with a conventional simulated instrument. We then show how significantly more precise estimates are obtained by a recentered IV which leverages both state-wide shocks to eligibility policy and heterogeneity in individual exposure.

Medicaid is the largest health insurance program in the U.S., covering around 29 million poor, non-disabled adults. One of the goals of the 2009 Affordable Care Act (ACA) was to extend Medicaid eligibility to all U.S. citizens and legal residents earning below 138% of the federal poverty line (FPL), replacing older eligibility rules that were mostly stricter and varied widely across U.S. states. The constitutionality of such an expansion was challenged (broadly along partisan lines), leading to a 2012 Supreme Court decision that made expansion at the discretion of individual state governors (National Federation of Independent Business v. Sebelius, 567 U.S. 519). Subsequently, when most of the principal pieces of the ACA took effect in January 2014, only 19 states enacted the federal Medicaid expansion, among those 43 states that had not expanded under the ACA or had a universal 138% FPL cutoff prior to 2014. The divide was partially along the party line: a minority (8 out of 30) of states with Republican governors but a majority (11 out of 13) Democratic-controlled states eligible for expansion in fact did so. We refer to the former 19 eligible states as having expanded Medicaid under the ACA, with the remaining 24 denoted non-expansion states.³⁷ Exact Medicaid eligibility

³⁶Inference with the recentered IV $\tilde{z}_\ell = x_\ell - \mu_\ell$ remains straightforward: $\tilde{z}_\ell \varepsilon_\ell$ and $\tilde{z}_m \varepsilon_m$ are uncorrelated for ℓ and m in different states when policy shocks are independent, so conventional state-clustered standard errors remain valid.

³⁷We follow Frean et al. (2017) in using the Kaiser Family Foundation State Action database to determine which states adopted Medicaid expansions in each year; see <https://web.archive.org/web/20150110162937/https://www.kff.org>.

criteria continued to vary across states in 2014, with some expansion states raising eligibility beyond the ACA’s 138% FPL threshold and some non-expansion states partially raising eligibility though not fully to the ACA threshold.

A large and growing literature uses the 2014 partial Medicaid expansion to study effects of Medicaid eligibility on a number of individual outcomes. Guth et al. (2020) summarizes this literature, which overall suggests that more widespread eligibility increased access to and utilization of care, led to local economic gains, and improved health outcomes. Some papers in this literature explicitly characterize the state expansions as a natural experiment (e.g. Ghosh et al. (2019), Black et al. (2019)); many describe them as exogenous and leverage difference-in-differences specifications comparing the outcome trends of individuals in expansion and non-expansion states before and after 2014 (e.g. Hu et al. (2018), Averett et al. (2019)). Notably, Frean et al. (2017) use simulated eligibility instruments in this context, finding large Medicaid take-up effects and little evidence of private insurance crowd-out, although their analysis exploits ACA policy variation beyond Medicaid expansions. Overall there is mixed evidence on crowd-out effects from the ACA expansions *per se*, with some studies finding relatively large crowd-out (Leung and Mas 2018) and others finding small or inconclusive results (Kaestner et al. (2017), Maclean and Saloner (2019)). Frean et al. (2017) also note a complication in interpreting crowd-out effects during this period: the increased availability of direct-purchase private insurance through new ACA marketplaces, which contrasts with the traditionally studied margin of crowd-out from employer-sponsored plans (e.g. Cutler and Gruber 1996).

Applying the Section 3 framework to this setting requires explicitly specifying counterfactual 2014 Medicaid expansions. Our baseline assumption is that a state’s decision to expand is exchangeable among Republican and Democratic-controlled states, while allowing states with different party control to have different propensities to expand. Thus, all scenarios in which any 8 Republican and 11 Democratic states expanded are viewed as valid counterfactuals. We consider alternative assumptions on the expansion assignment process in robustness checks below.

Our data are drawn from the 2013 and 2014 American Community Surveys and capture a representative 1% sample of non-disabled U.S. adults (ages 21-64) residing in the 43 states eligible for expansion in 2014. This repeated cross-section includes information on insurance coverage (by Medicaid, ACA marketplaces, and employer-sponsored plans), household income and other demographics potentially determining Medicaid eligibility, such as employment status and family structure. We combine this main estimation sample with data from 2012 for falsification exercises; Appendix B.2 describes the sample construction in detail.

org/health-reform/state-indicator/state-activity-around-expanding-medicaid-under-the-affordable-care-act/ . States which expanded coverage under the ACA or which had a universal 138% FPL cutoff prior to 2014 (and which are excluded from our analysis) include California, Connecticut, the District of Columbia, Massachusetts, Minnesota, New Jersey, Washington, and Vermont.

We estimate take-up and crowd-out effects from second-stage specifications of the form

$$y_{\ell t} = \beta x_{\ell t} + \alpha_{s_{\ell t}} + \tau_t + c'_{\ell t} \gamma + \varepsilon_{\ell t}, \quad (11)$$

where ℓ indexes individuals, t indexes years (either 2013 or 2014), and $s_{\ell t}$ specifies an individual's state of residence in year t . The outcome $y_{\ell t}$ is an indicator for a particular type of health insurance coverage (Medicaid or private insurance), the treatment $x_{\ell t}$ is an indicator for Medicaid eligibility under the year- t eligibility rules of ℓ 's state of residence, α_s and τ_t denote state and year fixed effects, and the vector $c_{\ell t}$ includes time-varying controls (discussed below). Recognizing that eligibility is likely endogenous in equation (11), we instrument it with two alternative IVs.

We construct the simulated eligibility instrument consistently with our stance that only a state's decision to expand Medicaid in 2014 as exogenous and not, for example, its prior level of generosity. Hence this $z_{\ell t}^{CG}$ is constant in 2013 and in 2014 is solely determined by the expansion decision $g_{s_{\ell t}} \in \{0, 1\}$ of an individual's state of residence $s_{\ell t}$. Since the expansion shock is binary, the mapping from $g_{s_{\ell t}}$ to $z_{\ell t}^{CG}$ is inconsequential for its use as an IV; we nevertheless construct $z_{\ell t}^{CG}$ in a manner consistent with the original logic of Currie and Gruber (1996a); see Appendix B.2 for details. Because of the state and year fixed effects in (11), this instrument produces the same estimate of β as would the interaction of residing in an expansion state with the 2014 indicator. We include in the control vector $c_{\ell t}$ an indicator for residing in a Republican-controlled state, interacted with year, to match our assumption of conditional exogeneity of expansion decisions within each governor's party.

The alternative recentered IV also leverages conditionally exogenous variation in state Medicaid expansion decisions while also incorporating individual heterogeneity to better predict Medicaid eligibility. We construct eligibility predictions $z_{\ell t} = f(g_{s_{\ell t}}; v_{\ell t})$ by including in $v_{\ell t}$ all individual demographics that affect eligibility (household income, parental and employment status) as well as the precise eligibility rules of the individual's state in 2013. This construction allows for a perfect prediction of $z_{\ell t} = x_{\ell t}$ in 2013; in 2014 we predict eligibility from state expansion decisions and prior eligibility policy (again see Appendix B.2 for details).

The expected instrument which corresponds to this $z_{\ell t}$ is obtained by permuting expansion decisions within Republican- and Democratic-controlled states. It defines a sample of "non-exposed" individuals whose demographics and state of residence make them always or never eligible for Medicaid in 2014 despite the expansion decision and a set of "exposed" individuals for whom the expansion shock is relevant. Per the discussion above, we drop non-exposed individuals from the 2014 sample and, in keeping with the difference-in-difference structure, also drop individuals in 2013 whose individual characteristics would make them non-exposed in 2014. The remaining variation in $\mu_{\ell t}$ is absorbed by the year-interacted state party indicator in $c_{\ell t}$, making instrument recentering unnecessary.

Table 3 contrasts the predictiveness of the two instruments in first-stage regressions of Medicaid

Table 3: Medicaid Eligibility Effects: First-Stage Estimates

	(1)	(2)	(3)
Simulated IV	0.851 (0.113) [0.567,1.115]	0.032 (0.140) [-0.254,0.503]	
Recentered IV		0.817 (0.171) [0.397,1.162]	0.972 (0.015) [0.941,1.014]
Partial R-Squared	0.022	0.113	0.894
Exposed Sample	N	N	Y
States	43	43	43
Individuals	2,397,313	2,397,313	421,042

Notes: This table reports coefficients from first-stage regressions of Medicaid eligibility on the two instruments described in the text: a simulated eligibility instrument and a recentered prediction of Medicaid eligibility. Columns 1 and 2 estimate regressions in the full sample of individuals in 2013 or 2014, while column 3 restricts to the sample of individuals whose individual characteristics make them exposed to the partial ACA Medicaid expansion in 2014. All regressions control for state and year fixed effects and an indicator for Republican-controlled states interacted with year. State-clustered SEs are reported in parentheses; 95% confidence intervals, obtained by a wild score bootstrap, are reported in brackets. R-squared statistics partial out the fixed effects and controls.

eligibility $x_{\ell t}$ on either $z_{\ell t}^{CG}$ or $z_{\ell t}$, controlling for state and year fixed effects and the year-interacted state party control. We report OLS coefficient estimates, state-clustered standard errors, and partial R-squared statistics which measure the incremental predictiveness of the instruments given the fixed effects and controls. To address finite-sample concerns with a relatively small number of state clusters, we also report confidence intervals by a wild score bootstrap as suggested by Kline and Santos (2012).³⁸

The table shows that the recentered IV is much more predictive of actual Medicaid eligibility than the simulated instrument. In column 1 we find that a one percentage point increase in simulated Medicaid eligibility predicts a 0.85 percentage point increase in actual Medicaid eligibility. The partial R-squared in this first-stage regression instrument is quite small, at 2.2 percent. Including the recentered IV in column 2 increases the R-squared dramatically, to 11.3 percent; the coefficient on the simulated instrument falls to an insignificant 0.03 while the coefficient on the recentered IV is 0.82. In Column 3 we restrict estimation to the “exposed” sample of individuals whose demographics and state of residence make them marginal for the potential expansion of Medicaid eligibility in 2014. Here we find that a one percentage point increase in the recentered IV predicts a 0.97 percentage point increase in actual Medicaid eligibility. We also find a high partial R-squared, of 89.4 percent, reflecting the fact that we have removed the part of the sample where eligibility is unaffected by the state expansion decision.

Table 4 shows that estimates of Medicaid take-up and private insurance crowd-out effects are

³⁸This computationally efficient approach requires inverting bootstrapped test statistics, which generally makes confidence intervals asymmetric around the IV point estimate.

Table 4: Medicaid Eligibility Effects: Simulated and Recentered IV Estimates

	Has Medicaid		Has Private Insurance		Has Employer-Sponsored Insurance	
	Simulated IV (1)	Recentered IV (2)	Simulated IV (3)	Recentered IV (4)	Simulated IV (5)	Recentered IV (6)
	A. Baseline Controls					
Eligibility	0.132 (0.028) [0.080,0.218]	0.072 (0.010) [0.051,0.094]	-0.048 (0.023) [-0.109,0.010]	-0.023 (0.007) [-0.039,-0.008]	0.009 (0.014) [-0.035,0.053]	-0.009 (0.005) [-0.021,0.004]
	B. With Demographics x Post					
Eligibility	0.135 (0.029) [0.082,0.223]	0.073 (0.010) [0.051,0.096]	-0.050 (0.022) [-0.114,-0.002]	-0.024 (0.007) [-0.041,-0.008]	0.003 (0.013) [-0.038,0.036]	-0.008 (0.005) [-0.020,0.005]
Exposed Sample	N	Y	N	Y	N	Y
States	43	43	43	43	43	43
Individuals	2,397,313	421,042	2,397,313	421,042	2,397,313	421,042

Notes: This table reports coefficients from IV regressions of different measures of health insurance coverage on Medicaid eligibility, instrumented by one of the two instruments described in the text: a simulated eligibility instrument and a recentered prediction of Medicaid eligibility. Columns 1, 3, and 5 estimate regressions in the full sample of individuals in 2013–2014, while columns 2, 4, and 6 restrict to the sample of individuals whose individual characteristics make them exposed to the partial ACA Medicaid expansion in 2014. All regressions control for state and year fixed effects and an indicator for Republican-controlled states interacted with year; the regressions in Panel B additionally control for deciles of household income, interacted with indicators for parental and work status and year. Conventional state-clustered SEs are reported in parentheses; 95% confidence intervals, obtained by a wild score bootstrap, are reported in brackets.

correspondingly much more precise when obtained with the recentered IV. Associated standard errors and confidence interval lengths fall by around 60-70 percent when we replace $z_{\ell t}^{CG}$ with $z_{\ell t}$ and restrict estimation to the exposed sample. In columns 1 and 2 of Panel A we obtain a recentered IV confidence interval of [0.05,0.09] for the take-up effect, relative to much wider a simulated instrument confidence interval of [0.08,0.22]. For private insurance crowd-out, the respective confidence intervals in columns 3 and 4 are [-0.04,-0.01] and [-0.10,0.01]. Thus we can only reject the null hypothesis of no crowd-out with 95% confidence when using the recentered IV. Importantly these columns include both the conventional crowd-out margin of employer-sponsored insurance as well as the novel form of private marketplace insurance introduced by the ACA. In columns 5 and 6 we focus on crowd-out of employer-sponsored plans. Neither the simulated nor recentered IV yields statistically significant estimates at the 95% level, though the latter is again much more precise.

In economic terms, the recentered IV estimates in Panel A of Table 4 suggest a total private insurance crowd-out rate of 30%, with a 7.2 percentage point increase in overall coverage offset by a 2.3 percentage point decrease in private insurance coverage. This overall effect is similar to the 42% crowd-out that Leung and Mas (2018) find in applying a difference-in-differences specification to the 2014 Medicaid expansion. However, like Frean et al. (2017), we find no statistically significant evidence of employer-sponsored insurance crowd-out even with our more powerful recentered IV.

Panel B of Table 4 shows that these substantive findings and apparent power gains are not driven by the relatively simple regression specification. Adding flexible controls for the individual characteristics which drive exposure to different eligibility policies (deciles of household income, interacted with indicators for parental and work status and year) in $c_{\ell t}$ leaves both the point estimates and the difference in simulated IV and recentered IV standard errors and confidence interval lengths unchanged.

We further analyze the robustness of this analysis in Appendix B.3. First, we validate our assumption of expansion exogeneity with a placebo test that replaces 2014 outcomes with a comparable cross-section from 2012. Although with the increased precision from the recentered IV we are able to reject the null hypothesis of no pre-trends, Appendix Table A2 shows that the magnitude of the placebo coefficient is small (around 0.01-0.02) regardless of the outcome and the instrument we use. Second, we relax the key exogeneity assumption by allowing a state’s decision to expand to depend not only on the political party of their governor, but also on the state’s median household income and previous rate of Medicaid coverage. Appendix Table A3 shows that the estimated effects of eligibility remain very similar across specifications. Third, we explore robustness to another implementation of our approach: namely, using the recentered $z_{\ell t}$ as the instrument without restricting to the exposed sample. Appendix Table A4 shows that this approach only yields power gains when the additional demographic controls (or an indicator for being in the exposed sample) are included in $c_{\ell t}$; we discuss the combination of factors that drives this finding in Appendix B.3 and relate it to our general efficiency theory of Section 3.5.

Finally, we confirm large and uniform power gains from using the recentered IV in a Monte Carlo study based on our baseline estimates. In this controlled environment the true causal effect and the shock assignment process are known, allowing us to verify that recentered IV estimator is both close to unbiased and significantly more efficient than the traditional simulated instrument approach. We find, for example, that the minimum detectable effects of simulated IV (the smallest null hypotheses which are rejected by a 0.05-size test with probability 0.8) are roughly three times larger than those of the recentered IV (see Appendix Figures A5 and A6).

4.3 Network Spillovers

We now discuss the implications of our framework for identification and inference in spillover regressions. In such settings the units ℓ represent nodes in a network (of people, firms, regions, etc.), and g captures shocks that are as-good-as-randomly assigned to them.³⁹ The target parameter β denotes the causal effect of a node-specific treatment x_ℓ which captures the spillovers from g at node ℓ . For example, one may be interested in the effects of an inventor’s death on the future productivity of her co-inventors (Jaravel et al. 2018), having a direct supplier or supplier’s supplier hit by a natural disaster on a firm’s growth (Carvalho et al. 2020), or having more “dewormed” students at neighboring schools on a student’s test scores (Miguel and Kremer 2004).⁴⁰ In bringing our framework to such settings we maintain the assumption that the spillover treatment has been well-specified, in that it captures all relevant channels by which the shocks affect a given node.⁴¹ Spillover regressions are typically reduced-form, such that x_ℓ is a function of the predetermined network structure in w and the shocks g and $z_\ell = x_\ell$, but we also allow for IV regressions where treatment is affected by both g and other shocks.

Such spillover regressions may suffer from OVB when different nodes face systematically different exposure to the exogenous shocks because of their network position. This exposure, summarized by the expected instrument, depends on how the shocks are assigned and how z_ℓ is constructed (e.g., whether it is nonlinear in the shocks). We first illustrate how these factors determine whether the OVB can arise and whether it can be solved by conventional regression controls.

For concreteness we center this discussion of OVB around a stylized version of Miguel and Kremer (2004, henceforth MK), where $z_\ell = x_\ell$ counts the number of dewormed students at schools within a fixed radius from student ℓ ’s school (excluding itself) and y_ℓ is some health outcome. The deworming shocks are generated by an RCT which as-good-as-randomly selected half of all schools for deworming.

³⁹It is unimportant that the shocks are assigned to and the outcomes are observed for *all* nodes. Our framework applies directly when, for instance, the shocks and the observations correspond to non-overlapping subsets of nodes.

⁴⁰One can also consider settings in which x_ℓ measures node ℓ ’s network centrality itself, after a shock to the network links. The market access measure we discuss in Sections 2 and 4.1 can be viewed as an example, capturing a region’s centrality in a transportation network.

⁴¹This exclusion restriction may follow from a particular model for peer effects, as in Manski (1993) and Manski (2013), or a general equilibrium gravity model in the market access case. See Angrist (2014) for a discussion of potential biases from misspecifying social interaction models in simpler settings.

Thus z_ℓ can be written as a linear function of which students are experimentally dewormed (collected in a binary vector g) with coefficients determined by the pre-existing network of students and schools (summarized in w). To simplify the discussion we abstract from other features of the actual MK setting, including the fact that they estimate direct effects of deworming together with the spillover effect; our framework is extended to such specifications with multiple treatments in Appendix A.7.

The OVB issue is easy to see in this setting: students who live in denser areas will have systematically more dewormed neighbors, and dense areas may also have different unobserved determinants of health. MK address this threat by controlling for the total number of eligible students in neighboring schools, n_ℓ . This is indeed what our approach recommends, provided all students have an equal chance of being dewormed (a probability we denote by q). The expected number of dewormed neighbors $\mu_\ell = \mathbb{E}[z_\ell | w] = qn_\ell$ is then proportional to n_ℓ , so including this control purges OVB.⁴²

We now consider four deviations from the benchmark setup, in which conventional regression controls are no longer sufficient to span the expected spillover treatment and purge OVB. First, there may be a more complex shock assignment process. For example, stratified random assignment which makes deworming more likely for some students or schools (depending on, for example, student gender or school size) would make μ_ℓ no longer proportional to n_ℓ .

Second, spillover effects may involve network weights. Suppose MK had instead specified the spillover treatment as $z_\ell = \sum_{n \in \mathcal{N}_\ell} w_{\ell n} g_n$, where \mathcal{N}_ℓ is the set of ℓ 's neighbors and $w_{\ell n}$ measures the strength of the spillover from neighbor n 's deworming status g_n , for instance determined by how frequently ℓ and n interact.⁴³ Then even in simple experimental designs, the expected instrument equals $\mu_\ell = \sum_{n \in \mathcal{N}_\ell} w_{\ell n}$ which need not be collinear with n_ℓ : a person who interacts with neighbors more will tend to be more affected by the deworming experiment even conditionally on the number of neighbors. An example of this issue can be found in Acemoglu et al. (2015) where ℓ and n are municipalities in Colombia and $w_{\ell n}$ are inverse distance weights; here μ_ℓ reflects the geographic centrality of the region.

Third, spillover treatments may be nonlinear in the network shocks. If, for example, MK had studied the effects of having at least one treated neighbor, the appropriate μ_ℓ control would have been the student-specific probability of this event. Under simple randomization of deworming, this μ_ℓ is a nonlinear function of n_ℓ and OVB may be purged by flexibly controlling for n_ℓ . However this is no longer the case with school-level randomization or more complex experimental protocols.

Finally, spillovers may arise from shocks across the entire network, and not just from immediate neighbors. Suppose MK had instead studied the effect of deworming spillovers given by the geographic distance from the nearest dewormed school (which may be large for some schools). This non-local specification of network spillovers makes μ_ℓ inherently more complex: while in expectation the distance

⁴²The n_ℓ control would not be necessary if spillovers were instead specified as driven by the fraction of dewormed neighbors. Under simple randomization all students would then have the same expected spillover treatment, $\mu_\ell = q$.

⁴³One can view such z_ℓ as a special case of a shift-share instrument further discussed in Section 4.4 below. The benchmark MK case is obtained with $w_{\ell n} \equiv \mathbf{1}[n \in \mathcal{N}_\ell]$.

is smaller in dense areas, there is no simple measure that fully captures the expected distance to the nearest dewormed school. This example is inspired by Carvalho et al. (2020) who study the effects of network distance between firm ℓ and the nearest firm located in the geographic area of a natural disaster (specifically, the 2011 Tōhoku earthquake) in the firm-to-firm supplier network.⁴⁴

In RCT settings like MK’s the more intricate OVB challenges raised in each of these scenarios are easily solved. A researcher can simply compute the expected spillover treatment by redrawing shocks according to the randomization protocol, and appropriately adjust for it. With natural experiments like that of Carvalho et al. (2020) institutional or scientific knowledge can help specify counterfactual shocks, for instance by drawing on appropriate geological models of earthquake probabilities across regions. With observational data counterfactual shocks can be specified by the partial exchangeability of shocks, perhaps conditional on node-level observables. The approach of Jaravel et al. (2018), for example, in which deceased and non-deceased co-inventors are matched based on age and other characteristics, can be viewed as leveraging such an exchangeability assumption.

Besides providing a solution to the OVB problem, our framework helps address well-known challenges of inference in network regressions. A conventional approach in the literature is to assume that $z_{\ell \in \ell}$ is uncorrelated beyond a small geographical or network distance (Conley (1999); see Acemoglu et al. (2015) for a network example). This may work well when z_{ℓ} captures local spillovers. However, when both observed and unobserved shocks propagate further through the network conventional standard errors may be distorted. As usual, randomization inference is valid regardless of the correlation structure of unobserved shocks, relying only on the correlation structure of the recentered instrument that is implied by knowledge of the shock assignment process.

An even more challenging problem that is solved by RI is when few shocks are observed in the data, such as the single earthquake in Carvalho et al. (2020). A small number of shocks makes the asymptotic approach inapplicable: even absent spillovers, it is generally impossible to consistently estimate the effect of earthquakes if only one region is treated. The lack of consistency, however, does not preclude informative inference provided shock counterfactuals are specified. For example, if the true effect is zero, it is unlikely that unobserved shocks hit exactly the same region where the earthquake randomly happened. RI-based confidence intervals capture this idea formally.

4.4 Linear and Nonlinear Shift-Share Instruments

We next consider instruments of the form $z_{\ell} = f(g, w_{\ell})$, where w_{ℓ} is a vector capturing the exposure of observation ℓ to the set of shocks g (usually of the same dimensionality N). Conventional shift-share

⁴⁴ Among firms with a given number of direct suppliers (a covariate Carvalho et al. (2020) control for), the probability of having at least one supplier’s supplier in the randomly assigned earthquake zone (i.e. at distance of two) still depends on how connected its suppliers are. Nonlinearity of the network distance as a function of shocks makes this probability also depend on the correlation structure of the shocks. Consider a firm for which the set of second-degree suppliers is not very large but is geographically dispersed. This firm is much more likely to be affected by a shock hitting random geographic areas (like an earthquake) than a shock hitting firms regardless of their geography.

instrument variables (SSIVs) set $f(\cdot)$ to take an exposure-weighted average of the shocks: typically a regional instrument z_ℓ is constructed from a set of industry shocks g_n as $z_\ell = \sum_n w_{\ell n} g_n$, where $w_{\ell n}$ measures the industry’s share (of, say, employment) in the region (e.g., Bartik (1991) and Autor et al. (2013)). These instruments are often employed when the treatment variable can similarly be represented as a share-weighted average $x_\ell = \sum_n w_{\ell n} \tilde{x}_{\ell n}$, where component $\tilde{x}_{\ell n} = g_n + u_{\ell n}$ includes a potentially endogenous $u_{\ell n}$. More recently, the SSIV approach has inspired a number of nonlinear instrument constructions for treatments that combine exogenous shocks with local shares in more complex ways (e.g. Boustan et al. 2013; Berman et al. 2015; Chodorow-Reich and Wieland 2020).

We first connect the Section 3 framework to the earlier work of Borusyak et al. (2019) in showing that simple regression controls are typically enough to purge OVB when using linear shift-share instruments, without specifying counterfactual shocks. We then present two sets of novel insights. First, we formalize the class of nonlinear SSIVs and propose an approach to identification with them. Explicit specification of counterfactual shocks is typically necessary for nonlinear SSIVs to be valid, but linear approximations to such instruments may obviate this need at an efficiency cost. Second, asymptotic inference is generally challenging in the nonlinear SSIV setting but valid randomization inference based on shock counterfactuals is straightforward. Moreover, we find in Monte Carlo simulations that in the linear SSIV case RI may serve as a useful complement to the existing asymptotic approach of Adão et al. (2019). Throughout this analysis we follow Borusyak et al. (2019) in assuming the shocks g_n are as-good-as-randomly assigned while the exposure shares $w_{\ell n}$ may be endogenous (for example, if unobserved industry shocks affect regions via the same shares).⁴⁵

OVB with Linear SSIVs Borusyak et al. (2019) establish the validity of linear SSIVs under the assumption of quasi-random shock assignment, formalized as equal conditional expectations of g_n across n , and that the exposure shares sum to one across n for each observation. When the second assumption fails (what they label the “incomplete shares case”), they show how OVB is purged by controlling for the sum of shares. They further show how simple quasi-random shock assignment may be relaxed to allow the conditional expectation of g_n to depend on shock-level observables, by controlling for share-weighted averages of these potential confounders.

In our general framework, these insights can be seen to follow from the linearity of the expected shift-share instrument, since

$$\mu_\ell = \mathbb{E}[z_\ell | w] = \sum_n w_{\ell n} \mathbb{E}[g_n | w] \tag{12}$$

is an exposure-weighted average of the expected shocks $\mathbb{E}[g_n | w]$. When the expected shock is constant ($\mathbb{E}[g_n | w] = \alpha$ for some α) and the exposure shares sum to one ($\sum_n w_{\ell n} = 1$ for all ℓ) the expected

⁴⁵Our framework also nests the alternative linear SSIV framework of Goldsmith-Pinkham et al. (2020) in which the shares are exogenous but shocks need not be. With shocks considered non-stochastic and with *iid* data, as in Goldsmith-Pinkham et al. (2020), OVB does not arise as all observations are similarly exposed to the exogenous shares. In Appendix A.9 we generalize this setup and show that identification requires additional assumptions when the shocks are stochastic and endogenous.

instrument is also constant ($\mu_\ell = \alpha$) and no correction to the linear SSIV is needed to avoid OVB. In the incomplete shares case where $W_\ell = \sum_n w_{\ell n}$ varies, it is enough to linearly control for W_ℓ since $\mu_\ell = \alpha W_\ell$. Under the weaker assumption of conditional random shock assignment, i.e. $\mathbb{E}[g_n | w] = q'_n \alpha$ for some vector of observables q_n , it is furthermore enough to control for the share-weighted sums of confounders, $Q_\ell = \sum_n s_{\ell n} q_n$, as they absorb $\mu_\ell = Q'_\ell \alpha$. The Section 3 framework also implies an alternative solution to this general case: instead of controlling for Q_ℓ one can recenter the *shocks* by their conditional expectation. This follows because the recentered instrument is also a linear SSIV:

$$\tilde{z}_\ell = z_\ell - \mu_\ell = \sum_n w_{\ell n} \tilde{g}_n, \quad (13)$$

where $\tilde{g}_n = g_n - \mathbb{E}[g_n | w]$. SSIV approaches which first residualize shocks on observables (e.g., Greenstone et al. (2020)) may be interpreted as following this recentering logic.

It is worth highlighting that the linearity of SSIV relaxes Assumption 3: the conditional expectation $\mathbb{E}[g_n | w]$ is the only moment of the shock assignment process that needs to be specified. Unmodeled higher moments, such as shock heteroskedasticity and clustering, do not pose problems for instrument recentering or controlling, as they might in the general case of nonlinear instrument constructions.

Nonlinear SSIVs The SSIV logic has recently been extended to cases where the treatment variable can be represented as a nonlinear function of some predetermined exposure shares w_ℓ and potentially endogenous “shifts,” i.e. $x_\ell = f(\tilde{x}_\ell, w_\ell)$ for $\tilde{x}_\ell = (\tilde{x}_{\ell n})_{n=1}^N$. Chodorow-Reich and Wieland (2020), for example, study the effects of a regional labor reallocation index x_ℓ measuring the dispersion of local industry growth rates $\tilde{x}_{\ell n}$ with initial industry employment shares $w_{\ell n}$ as weights. If the researcher observes an exogenous shifter g_n for $\tilde{x}_{\ell n}$, that is not observation-specific (i.e., measured nationally or in other countries), an intuitive instrument can be constructed as $z_\ell = f(g, w_\ell)$, predicting x_ℓ via exogenous shocks and predetermined exposure. We believe this general formulation of such nonlinear SSIVs is novel, while nesting several applied examples, including the predicted change in a regional Gini coefficient in Boustan et al. (2013), the predicted share of migrants in Basso and Peri (2015), and the predicted foreign demand instrument of Berman et al. (2015).

Nonlinearity of $f(\cdot)$ generically leads to a challenging OVB problem. Even with fully exchangeable shocks and “complete shares,” where linear SSIV is valid without correction, the expected instrument μ_ℓ is a complex function of w_ℓ . Moreover, unlike in the linear case, second and higher moments of the shock assignment process may be relevant to the expected instrument and must generally be specified for the appropriate recentering. Shock heteroskedasticity and clustering, in particular, are potentially problematic for nonlinear SSIV.

Our framework yields two solutions for OVB with nonlinear SSIVs, which give a likely tradeoff between efficiency and robustness. A researcher may recenter a nonlinear z_ℓ given a specification of shock counterfactuals. Alternatively, she may take a first-order approximation of z_ℓ around some fixed

vector of shocks g^0 (e.g., $g^0 = 0$) to obtain a linear SSIV of $\check{z}_\ell = \check{w}'_\ell g$, for $\check{w}_\ell = \frac{\partial f}{\partial g}(g^0, w_\ell)$. As before, the linear instrument is valid when the shocks have a common mean and $\sum_n \check{w}_{\ell n}$ is controlled for (or, more generally, when the means of shocks depend linearly on some q_n and $\sum_n \check{w}'_{\ell n} q_n$ is controlled for). As an approximation to z_ℓ the linear SSIV is likely to predict x_ℓ less well and thus be less efficient. On the other hand, its validity depends on correct specification of fewer moments of the shock assignment process making it more robust. Such linear approximations have indeed motivated SSIV instruments in the context of some economic models (e.g., Kovak (2013), Adão et al. (2019), and Adão et al. (2020)) but the same logic applies generally, e.g. to predicted labor reallocation indices and Gini coefficients.

To make concrete the potential for OVB with nonlinear SSIVs, and our two solutions, we take a stylized example of a popular instrument which could be called a ‘‘SSIV in logs’’ (e.g., Berman et al. 2015; Berthou et al. 2019; Costa et al. 2019). Suppose $x_\ell = \log \frac{X_{\ell t}}{X_{\ell 0}}$ denotes the growth rate of some regional variable which can be represented as a sum of industry components, $X_{\ell t} = \sum_n \tilde{X}_{\ell n t}$. For example, $X_{\ell t}$ may denote the total demand for a regional output that aggregates demand across industries n . Then x_ℓ can be rewritten as a nonlinear function of initial shares $w_{\ell n} = \frac{\tilde{X}_{\ell n 0}}{X_{\ell 0}}$ and regional growth rates $\tilde{x}_{\ell n} = \frac{\tilde{X}_{\ell n 1}}{\tilde{X}_{\ell n 0}}$, as $x_\ell = \log \sum_n w_{\ell n} \tilde{x}_{\ell n}$. Suppose that a researcher suspects endogeneity in regional growth rates but observes an industry characteristic G_{nt} with plausibly exogenous growth rates $g_n = \frac{G_{n1}}{G_{n0}}$ that predict the $\tilde{x}_{\ell n}$. This motivates a nonlinear SSIV:

$$z_\ell = \log \sum_n w_{\ell n} g_n. \quad (14)$$

Although exogeneity of g_n makes $\sum_n w_{\ell n} g_n$ a valid instrument (after controlling for $\sum_n w_{\ell n}$), the log transformation in equation (14) generally introduces OVB. This is because the log function is concave, so the expected instrument $\mu_\ell = \mathbb{E}[z_\ell | w]$ is systematically higher for regions where $\sum_n w_{\ell n} g_n$ has a lower variance. In particular, regions with more diversified economies (i.e., with $w_{\ell n}$ more dispersed across n) will tend to have systematically higher z_ℓ , while they may also have systematically different unobservables. For example, diversification may make the local economy more resilient to unobserved shocks, which would generate an upward bias in an IV estimator which takes some measure of regional economic growth as an outcome.⁴⁶

Both of our solutions to OVB are quite intuitive for SSIVs in logs. One could recenter equation (14) by the appropriate measure of diversification μ_ℓ , which generally requires specifying and simulating counterfactual shocks. Alternatively one may take a log-linear approximation around $g_n = 1$, which here yields an intuitive linear SSIV: $\check{z}_\ell = \sum_n w_{\ell n} \log g_n$. Removing OVB from this instrument requires

⁴⁶There is a further problem with the use of (14) in practice. In panel specifications with unit and period fixed effects the instrument is commonly specified as $z_{\ell t} = \log \sum_n w_{\ell n} G_{nt}$ (e.g., Berman et al. 2015). In first differences this corresponds to $\Delta z_\ell = \log \sum_n \tilde{w}_{\ell n} g_n$ where shares are reweighted, $\tilde{w}_{\ell n} = \frac{w_{\ell n} G_{n0}}{\sum_{n'} w_{\ell n'} G_{n'0}}$. These shares do not align with the economic intuition behind the instrument construction and are likely to lead to a weaker first-stage. The correct construction is $z_{\ell t} = \log \sum_n w_{\ell n} \frac{G_{nt}}{G_{n0}}$.

only specifying (and appropriately controlling for) the expected $\log g_n$.⁴⁷

Inference and Monte Carlo Simulations We finally discuss how our framework brings new tools to SSIV inference. We are not aware of any general asymptotic theory for the nonlinear case (unless restrictive independence assumptions are placed on the residuals), making RI an attractive approach to inference. For the linear case we argue that RI may serve as a useful complement to the asymptotic theory of Adão et al. (2019).⁴⁸

The choice between RI and asymptotic approaches involves tradeoffs. An advantage of RI is its validity even with relatively few or concentrated shocks, when the asymptotics approximation may not be accurate. At the same time, RI requires specification of the shock assignment process, rather than its first moment only, and thus assumptions of homoskedasticity (or a known parametric form of heteroskedasticity, as in Appendix A.5), distribution symmetry, or similar conditions which are not required for asymptotic exposure-robust inference. On other dimensions the two approaches are hard to compare in general: they both require constant treatment effects (and their sensitivity to treatment effect heterogeneity is not known), and their power may differ.⁴⁹

We therefore examine the power and robustness properties of RI in a Monte Carlo simulation. Our simulation is based on the influential SSIV study by Autor et al. (2013), who estimate the effects of import competition with China on U.S. local labor markets. The simulation process follows Borusyak et al. (2019) in redrawing import competition shocks according to a wild bootstrap (to preserve shock heteroskedasticity), holding fixed the exposure shares and estimated structural residuals (see Appendix B.4 for details). We consider two asymptotic approaches to SSIV inference: the conventional “exposure-robust” standard errors of Adão et al. (2019) (obtained via the equivalent shock-level regression in Borusyak et al. (2019)) and a version designed for better finite-sample coverage by imposing the null hypothesis. We contrast these approaches with an RI procedure based on randomly flipping the signs of the simulated shocks, which leverages a known symmetry of the shock distribution shape.

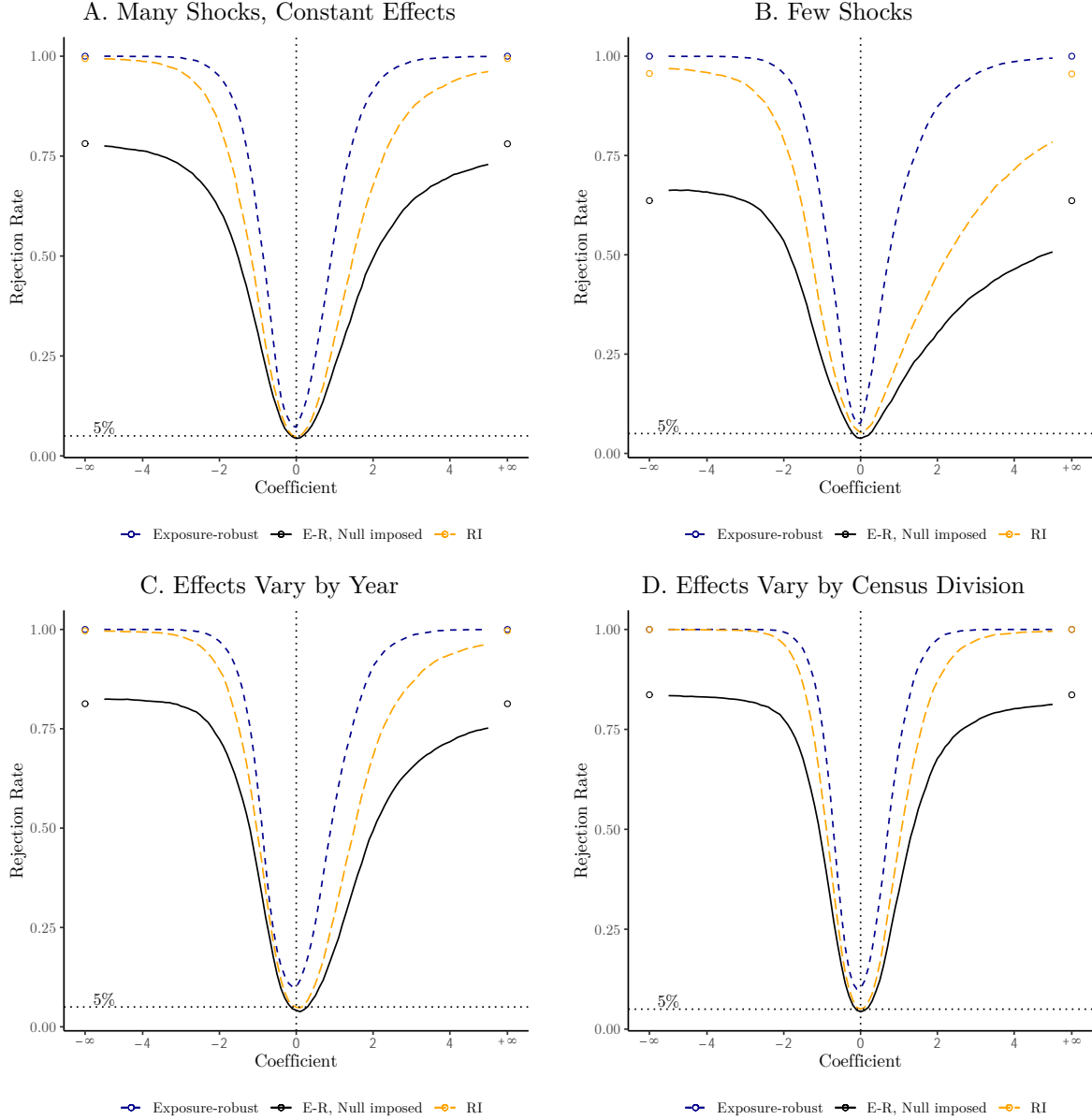
Panel A of Figure 4 presents a simulated power curve for the main Autor et al. (2013) data-generating process with 794 industry-by-period shocks (for 397 manufacturing SIC4 industries in two periods). We plot simulated rejection rates for each of the three modes of inference across both a

⁴⁷More robust linear SSIVs may more generally be obtained by taking the nonlinear transformation before averaging. Derenoncourt (2019), for example, addresses a skewed distribution of shocks by taking the sample percentiles of a shift-share instrument, yielding a nonlinear SSIV. Taking instead an exposure-weighted average of shock percentiles yields a linear SSIV that is valid without recentering, provided these percentiles are as-good-as-randomly assigned and the weights sum to one.

⁴⁸Adão et al. (2019) consider an asymptotic sequence with many uncorrelated (or weakly correlated) shocks and sufficiently dispersed exposure. Linearity of the instrument is indispensable for their results. This is clear from the equivalence result in Borusyak et al. (2019), which shows that the Adão et al. (2019) standard errors can be obtained from conventional calculations after transforming the data to the level of shocks. For nonlinear SSIV no such equivalence generally exists, making it difficult to draw on conventional asymptotic theory.

⁴⁹Adão et al. (2019) show that when treatment effects vary their standard errors are asymptotically conservative, but this result only applies to reduced-form SSIV regressions under certain conditions on the exposure shares. We know of no such guarantees in the IV case.

Figure 4: Simulated Size and Power of Alternative Shift-Share IV Inference Procedures



Notes: This figure plots simulated power curves for different shift-share IV inference procedures. The baseline data-generating process in Panel A comes from Autor et al. (2013), as described in Section 4.4 and Appendix B.4. In Panel B we reduce the number of industry shocks in each period from 397 to 20. In Panels C and D we specify a data-generating process with heterogeneous effects by period or Census division. Exposure-robust tests are obtained from the equivalent shock-level regressions of Borusyak et al. (2019). Hollow circles indicate the power at $\beta = \pm\infty$, approximated by $\beta = \pm 1,000$.

true causal effect of $\beta = 0$ and a range of alternative hypotheses (normalized such that the SSIV estimate in the data has $\beta = 1$). Consistent with Borusyak et al. (2019) we find that conventional exposure-robust standard errors yield a mild overrejection of the nominal 5% level test (see their Table C6, rows (b)), while the other two procedures exhibit no significant size distortions. Despite correct coverage, power of the null-imposed asymptotic inference procedure is low, failing to reject arbitrarily large values of β (i.e. yielding infinite 95% confidence intervals) over 20% of the time. In contrast the RI procedure has power similar to that of conventional exposure-robust asymptotics, if a bit smaller.

In Panel B of Figure 4 we consider a modified data-generating process with fewer shocks by aggregating manufacturing industries to their SIC2 groups (yielding 20 shocks in each period), again following Borusyak et al. (2019). The size of all three methods is similar to that in Panel A, with the overrejection of conventional exposure-robust standard errors increasing slightly. Here the power of RI is asymmetric: similar to that of conventional exposure-robust inference for negative values of β_0 , but weaker for positive values. Still, it is again uniformly much stronger than that of the null-imposed procedure which (approximately) shares the RI property of correct size.

Finally, Panels C and D of Figure 4 explore robustness of the three methods to treatment effect heterogeneity, either across the two periods (Panel C) or nine geographic regions (Census divisions, Panel D), with the original 794 shocks. The data-generating process here is based on the heterogeneous effects estimated in the Autor et al. (2013) data and demeaned in such a way that sets the median SSIV estimate to zero to make coverage well-defined (see Appendix B.4 for details). Although there is no theoretical guarantee for either RI nor the asymptotic approximations in this case, the results are surprisingly similar to those in Panel A: RI coverage is correct and its power is close to that of exposure-robust inference, while substantially exceeding that of the null-imposed version.

4.5 Other Settings

We conclude this section by discussing implications for model-implied optimal instruments (Adão et al. 2020), instruments generated by centralized assignment mechanisms (Abdulkadiroglu et al. 2017; 2019), and instruments based on weather shocks (Gomez et al. 2007; Madestam et al. 2013). We explain how our framework can relax certain restrictions for identification and suggest an alternative basis for inference.

Adão et al. (2020) develop a spatial general equilibrium model and propose a novel way to identify its parameters. They assume that a researcher observes shifters g to some model’s primitives, such as trade costs. They then propose log-linearizing the model around the initial equilibrium to derive an estimating equation for the changes in some regional outcome y_ℓ , of

$$y_\ell = \sum_n m_{\ell n}(w, \beta) g_n + \varepsilon_\ell, \quad (15)$$

where $m_{\ell n}(w, \beta)$ represents the general-equilibrium elasticity of y_ℓ with respect to g_n as a function of predetermined equilibrium variables in w , and ε_ℓ captures unobserved shocks. Adão et al. (2020) treat w as non-stochastic and impose exogeneity of the observed shifters by assuming⁵⁰

$$\mathbb{E}[\varepsilon | g] = 0. \tag{16}$$

They then directly apply the classic efficiency result of Chamberlain (1987) to derive an optimal “model-implied” generalized method of moments estimator of β .

Our framework clarifies and permits a relaxation of an assumption that predetermined equilibrium is exogenous, which Adão et al. (2020) implicitly make in this setting. The identification assumption (16) is stronger than it might appear because the predetermined equilibrium variables in w are implicitly conditioned on, and therefore treated as exogenous. To see this assumption clearly, note that (16) implies $\mathbb{E}[\varepsilon_\ell] = 0$ by the law of iterated expectations. While innocuous under *iid*-sampling, this condition is strong in the interdependent economy, as it requires the unobserved shocks to be on average the same for each region ℓ , regardless of the local characteristics in w like industry composition or migration shares.⁵¹ Our framework allows for a weaker assumption of $\mathbb{E}[\varepsilon | g] = \mathbb{E}[\varepsilon]$ (Assumption 2(i)) without restricting $\mathbb{E}[\varepsilon_\ell] = 0$, and therefore allows the predetermined equilibrium to be endogenous; identification then follows from appropriate recentering. Since the treatment in (15) is linear in shocks, recentering simply requires recentering g_n by their conditional expectation (as in Section 4.4; see also Appendix A.8 for a discussion of how the Section 3 framework is extended to nonlinear equations). Our generalization of Chamberlain (1987) (similarly extended in Appendix A.8) further shows how optimal recentered instruments can be constructed in this setting to allow for both endogeneity of the predetermined equilibrium as well as potential non-*iid*ness of the unobserved error ε_ℓ . As always, randomization inference can be used to account for unspecified error dependence.

Next, our approach applies to settings in which instruments arise centralized assignment mechanisms, such as those used to allocate public school seats to students. Abdulkadiroglu et al. (2017) first show how centralized school assignments can be used as valid instruments when generated by mechanisms satisfying the “equal treatment of equals” property, in which students with the same school preferences and administrative priorities face the same assignment propensity. They use market design theory to derive large-sample approximations to this assignment risk in deferred acceptance mechanisms and further show how such assignment propensity scores can be simulated by redrawing the randomized lottery numbers which break ties between students with the same preferences and priorities. Abdulkadiroglu et al. (2019) extend this framework to mechanisms with discontinuities in assignment rules (e.g. over scores from a school entrance exam) by showing how assignment risk can

⁵⁰More precisely, they use the property of their model that g_n enter (15) via particular shift-share averages $\eta = (\eta_\ell)_{\ell=1}^L$, and assume $\mathbb{E}[\varepsilon | \eta] = 0$. This detail does not affect our discussion of this setting.

⁵¹This point is more clearly seen in our framework, which views w as potentially stochastic and writes (16) as $\mathbb{E}[\varepsilon | g, w] = 0$, which in turn implies $\mathbb{E}[\varepsilon | w] = 0$.

be derived from a local randomization approach.

Our framework nests this setting by writing indicators for assignment of student ℓ to a given school as $z_\ell = f_\ell(g; w)$, where $(g; w)$ partitions the inputs of a given assignment mechanism. The shock vector g might for example contain the set of tie-breaking lottery numbers in a stochastic deferred acceptance mechanism, with w containing the set of students' rankings over schools and administrative school priorities. Our expected instrument μ_ℓ would then coincide with the assignment propensity scores defined in this literature.

Our analysis offers two new insights to this setting. First, expected assignments may be generated by simulating any mechanism with a random or locally random component g , whether or not it satisfies equal treatment of equals. The validity of assignment instruments that recenter by or control for these propensity scores arises simply from the exogenous variation in g . Second, valid finite-sample tests and confidence intervals can also be obtained by simulating the mechanism. This RI approach accounts for the inherent dependencies of school offers across students. It remains valid when potential outcomes are not independent, as when applicants with similar school preferences and priorities are similar in other unobserved ways, or are affected by common unobserved shocks.

Finally, our approach applies to empirical designs leveraging spatial variation in weather, such as rainfall on the days of elections and other political events. It is standard in this literature to measure “normal weather” in each location as the average of historical data and then subtract it from the actual weather (e.g. Gomez et al. 2007) or control for it (e.g. Madestam et al. 2013). These two approaches to the OVB problem directly parallel our general solutions, given an assumption of stationary weather. Our framework implies that this assumption is not important for identification: any meteorological model that yields a weather distribution for the event days could similarly be used for recentering. Further, randomization inference has not been applied in this context, while Lind (2019) has shown that conventional modes of inference have severely distorted coverage because of the spatial correlation in both weather and residuals. The only exception we are aware of is Madestam et al. (2013), whose placebo test uses historical weather maps to preserve the correlation structure of the data. Applying such simulations to construct confidence intervals for the actual estimates would be in line with our general RI framework.

5 Conclusion

Many studies in economics construct treatments or instruments that combine exogenous shocks from randomized or natural experiments with predetermined measures of shock exposure. We develop a general econometric framework for such settings that allows for the possibility that shock exposure is endogenous. Except in special cases, such endogeneity generates bias in conventional regression estimators, while standard modes of statistical inference may also be invalid when observed and unob-

served shocks affect observations jointly. We show how these identification and inference problems can be solved by specifying an assignment process for the exogenous shocks: namely, a set of counterfactual shocks that might as well have been realized.

This general framework has concrete implications for a large number of settings. First, we illustrate the usefulness of specifying counterfactuals for new railroad construction when leveraging this variation to estimate market access effects. Estimates of the effects of high-speed rail on local employment in China are reduced to a statistical zero when adjusting for a region’s expected market access growth. Second, we show how our framework can be used to construct instruments which may be a more powerful alternative to simulated eligibility IVs. Estimates of Medicaid take-up and crowd-out effects are more than twice as precise when obtained by an instrument incorporating both as-good-as-random policy variation and non-random individual exposure. Third, we provide Monte Carlo evidence that randomization inference with linear shift-share IVs can be a valid and powerful alternative to more traditional asymptotics. Finally, we show how our approach allows a researcher to make formal identification arguments and conduct valid randomization inference with network spillover regressions, nonlinear shift-share instruments, and in other settings.

We conclude with a discussion of two important assumptions in our framework and avenues for future research. First, while we show in Appendix A.4 that our solution to OVB applies under general treatment effect heterogeneity, validity of the randomization inference procedure is only guaranteed under constant effects. Valid inference with heterogeneous effects and interdependent data is a difficult challenge, even in a more standard asymptotic approach (Adão et al. 2019), and the RI approach has historically struggled to accommodate such heterogeneity. In simulations of shift-share IVs we do not find that RI yields poor coverage and power under simple departures from constant effects. We recommend that researchers run similar simulations in the context of their own applications.

Second, specifying shock counterfactuals can be challenging in settings with no true randomization. In the paper we illustrate how these challenges can be overcome in a variety of settings by finding appropriate exchangeability of the shocks. For example, permuting the timing of railroad upgrades within observably similar groups may yield a plausible set of counterfactuals for gauging the potential for OVB. We also show how such specifications can be tested. We further consider some partly-specified shock assignment processes in Appendix A.5; future research may yield more flexible approaches.

In our view, specifying shock counterfactuals has innate value in observational studies that claim to leverage a natural experiment, understood as a serendipitous randomized trial (DiNardo 2008). A virtue of randomized trials is that valid causal inference can be conducted without restricting the unobserved residuals (e.g., potential outcomes in simple settings). In the settings we consider, including with observational data, this property is only maintained when an expected instrument adjustment is performed, which generally requires an explicit shock assignment process. Methods

that instead rely on properties of the structural residuals, such as a parallel trends assumption, are instead referred to as quasi-experimental by DiNardo (2008). An instructive example can be found in the setting of Redding and Sturm (2008) who study the effects of German reunification—an event for which parallel trends may plausibly hold while no obvious counterfactuals exist, and thus a natural experiment may not be a fitting term. Generalizing our framework to augment specifications of shock counterfactuals with plausible restrictions on the residual appears a fruitful area for future work and may yield new “double-robust” identification results, in a sense similar to that of Arkhangelsky and Imbens (2019).

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Appendix to “Non-Random Exposure to Exogenous Shocks: Theory and Applications”

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Contents

A Theoretical Appendix	57
A.1 Randomization Inference	57
A.2 Consistency of Recentered IVs	58
A.3 Asymptotic Efficiency	61
A.4 Potential Outcomes and Heterogeneous Treatment Effects	62
A.5 Assignment Processes with Unknown Parameters	65
A.6 Efficiency Controls	68
A.7 Multiple Treatments and Instruments	68
A.8 Nonlinear Outcome Models	71
A.9 Identification with Exogenous Exposure	72
A.10 Recentering in General Simulated Instrument Settings	74
A.11 Recentering Helps with Consistency: An Example	75
B Empirical Appendix	76
B.1 Data for Section 4.1	76
B.2 Data for Section 4.2	78
B.3 Robustness Checks for Section 4.2	79
B.4 Data for Section 4.4	83
C Proofs of Propositions	84
C.1 Proof of Proposition 1	84
C.2 Proof of Proposition 2	85
C.3 Proof of Proposition A4 and Lemma 3	86
C.4 Proof of Proposition A1	88
C.5 Proof of Proposition A2	88
C.6 Proof of Proposition A3	91
C.7 Proof of Proposition A5	93
C.8 Proof of Propositions A6 and A7	94
C.9 Proof of Proposition A8	96
C.10 Proof of Proposition A9	96
C.11 Proofs of Lemmas A2 and A3	97
Appendix Figures and Tables	98