Online Appendix

Appendix A. Additional Tables

Table A1. Heterogeneity analysis.

	High-tech	Firm scale	Intellectual property protection
	(1)	(2)	(3)
BCS×High-tech	0.322***		
	(0.042)		
BCS×Big		0.141***	
		(0.035)	
BCS×IPR			0.116***
			(0.036)
Control variables	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Observations	22915	22915	22915
R^2	0.425	0.423	0.423

Notes: *** denotes significance at the 1% level, ** denotes significance at the 5% level, and * denotes significance at the 10% level. All control variables, year fixed effects, and firm fixed effects are included in each regression. Robust standard errors clustered at the city level are shown in parentheses. All lower-order terms are included in the regression.

Appendix B. The institutional background of the BCS

China's internet infrastructure was established in 1994 and began offering internet services to the public in 1995. However, the average speed of the internet in China was only 10 Kbps at the beginning of the construction period and the usage costs were expensive, which limited the development of the internet in China (Yu et al., 2023). According to the 32nd Statistical Report on Internet Development in China and the State of the Internet published by Akamai in 2013, China's internet penetration rate was less than 50% by 2013, with broadband internet penetration at just 14.1%. By 2012, the national average internet speed was a mere 1.8 Mbps. The proportion of broadband with speeds above 4 Mbps in China was 5.4% in 2012, compared to 86% in South Korea, 76% in Japan, and the global average of 42%. Therefore, if we follow the FCC's definition of broadband internet (connection speed of more than 4 Mbps), China's broadband internet penetration rate in 2012 was only 5.4%, significantly lower than the global average. Furthermore, China's internet development exhibited substantial disparities between regions and between urban and rural areas.

To improve the quality of China's internet and promote the balanced development of internet infrastructure across regions, the Chinese government announced the implementation of the BCS in August 2013, which is divided into three phases. The first phase is the comprehensive speed-up phase, which focuses on replacing the existing network with a fiber-optic infrastructure to improve the user experience; the second phase is the diffusion and penetration phase, which aims to expand broadband internet coverage and deepen application penetration; and the third phase is the optimization and upgrading phase, which seeks to continuously improve broadband service quality through technological advancements. Following the implementation of BCS, China's internet connection quality improved rapidly. The average internet connection speed in China increased to 3.4 Mbps in 2014 after the implementation of BCS, and the proportion of broadband connections exceeding 4 Mbps rose to 27%.

Figure B1 illustrates the changes in the number of broadband ports and the number of broadband users in China before and after the implementation of BCS, showing significant growth in both metrics. This demonstrates the crucial role of BCS in expanding broadband coverage.

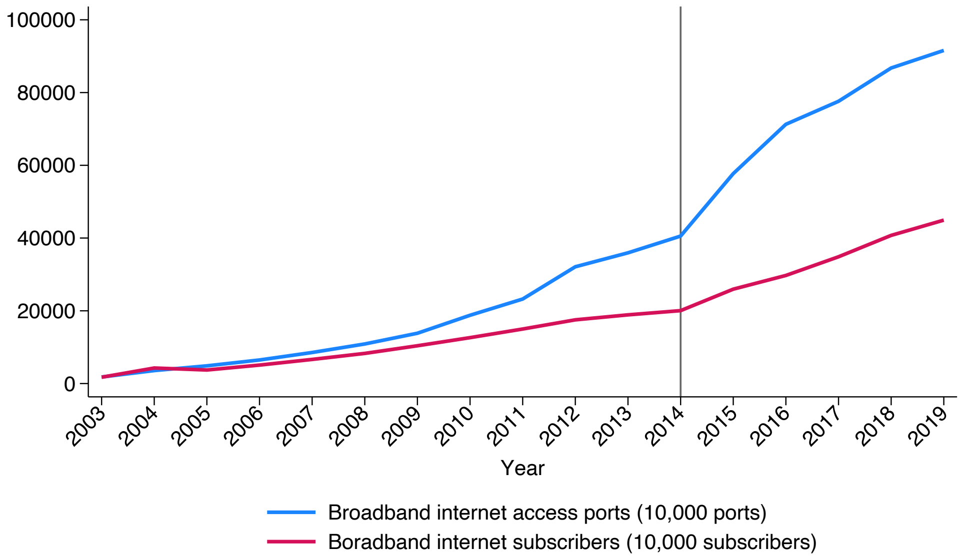

Figure B1. Development of broadband in China.

Notes: Data sourced from the China Internet Network Information Center and CEIC database, as compiled by the authors.

As the implementing departments of the BCS, the Ministry of Industry and Information Technology (MIIT) and the National Development and Reform Commission (NDRC) of China announced the establishment of pilot cities for the BCS in 2014 to accelerate the upgrading of broadband internet services in cities. Three batches of cities were recognized as BCS pilot cities in 2014, 2015, and 2016, respectively. Figure B2 illustrates the spatial distribution of BCS pilot cities over the period from 2014 to 2016. The spatiotemporal distribution of BCS pilot cities indicates that there was no significant clustering among the various batches of cities designated in different years.

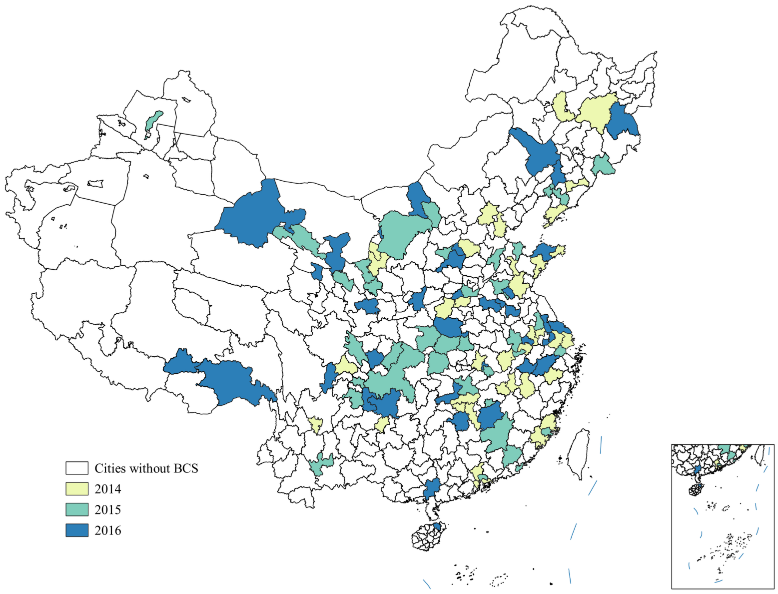

Figure B2. City-by-city rollout of BCS over time.

Notes: This map illustrates the timeline and geographic distribution of BCS pilot cities. The data on pilot cities is sourced from the MIIT of China.

Appendix C. Robustness test

C.1. Event study

To test the parallel trend assumption, we estimate the following regression:

$$Y_{ict} = \alpha + \sum_{\tau=-5,\tau\neq-1}^{5} \beta_\tau BCS_{c\tau} + X'_{ict}\gamma + Z'_{ct}\theta + \mu_t + \delta_i + \varepsilon_{ict}, \tag{1}$$

where τ is the relative time to BCS. $BCS_{c\tau}$ takes the value of 1 if city c is a pilot city in period τ and 0 otherwise.

Figure C1 illustrates the dynamic effect of BCS. Coefficients for periods -5 to -2 are insignificant, validating the parallel trend assumption. After the implementation of BCS, the number of firm patent transactions significantly increases.

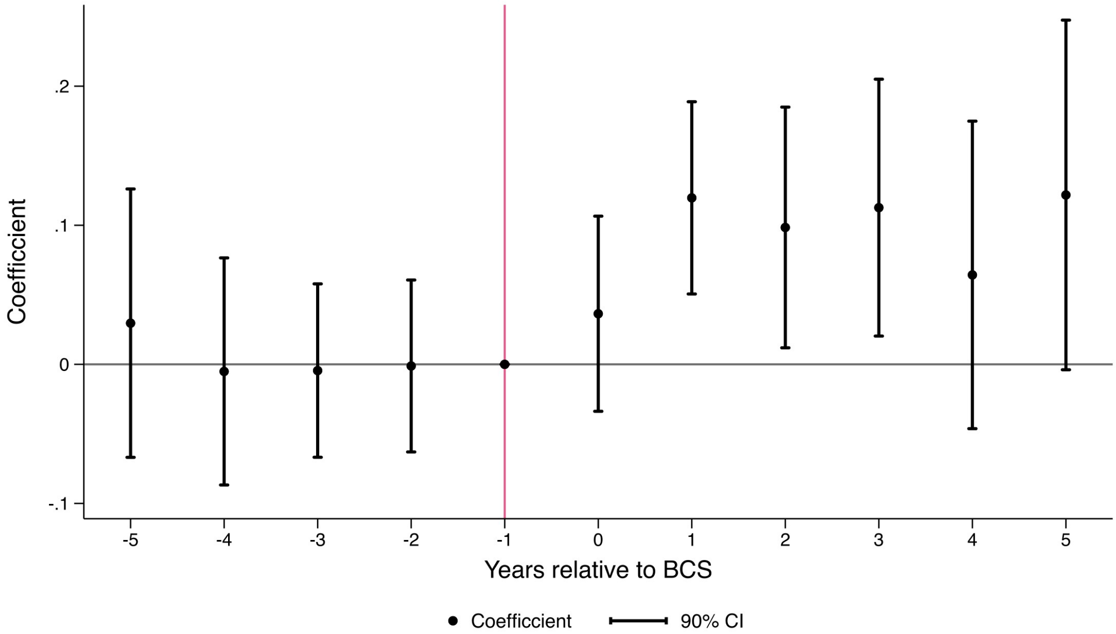

Figure C1. Event study.

C.2. Placebo test

We conduct a placebo test by randomly assigning pilot cities and implementation years, employing the identification method from column 3 of Table 2. Figure C2 shows placebo coefficients clustered around zero, while our baseline estimates (solid black line) significantly deviate, confirming the robustness of the findings.

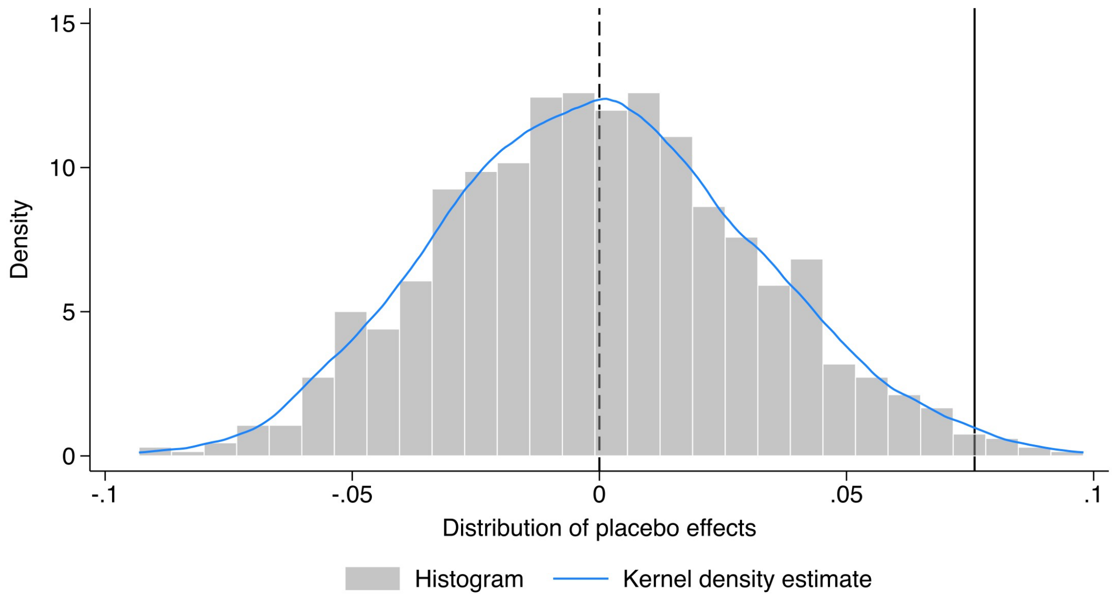

Figure C2. Distribution of the estimated coefficients of the placebo test.

C.3. Excluding interference of other policies

To account for concurrent policies (e.g., Smart City pilot, Big Data Comprehensive Zones, and Made in China 2025), we include corresponding dummy variables in our regression.[1] Table C1 shows that the coefficients of BCS remain significantly positive, confirming our results are robust to these concurrent policies.

Table C1. Results of excluding other policy interferences in the same period.

	lnPatentTransaction			
	(1)	(2)	(3)	(4)
BCS	0.073^{**}	0.076^{**}	0.075^{**}	0.073^{**}
	(0.037)	(0.038)	(0.038)	(0.036)
Smart city	Yes	No	No	Yes
Big data zone	No	Yes	No	Yes
Made in China 2025	No	No	Yes	Yes

[1] Since the "Made in China 2025" initiative was launched nationwide in 2015, we follow Li and Branstetter (2024)Li, G., & Branstetter, L. G. (2024). Does "Made in China 2025" work for China? Evidence from Chinese listed firms. *Research Policy*, *53*(6), 105009. and define firms that mention "Made in China 2025" (in Chinese) in their annual reports as the time when the firm is affected by the policy.

Control variables	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Observations	22915	22915	22915	22915
R^2	0.422	0.422	0.422	0.423

Notes: *** denotes significance at the 1% level, ** denotes significance at the 5% level, and * denotes significance at the 10% level. All control variables, year fixed effects, and firm fixed effects are included in each regression. Robust standard errors clustered at the city level are shown in parentheses.

C.4. Heterogeneous treatment effects

Recent econometric literature suggests that staggered DID methods based on TWFE can be biased when there are heterogeneous treatment effects (Baker et al., 2022; Goodman-Bacon, 2021; Sun & Abraham, 2021). Therefore, we re-estimate the effect of the BCS on firm patent transactions using the estimators proposed by Sun and Abraham (2021) and Gardner (2022) to test the robustness of the estimation result in allowing for heterogeneous treatment effects. The estimation results in Table C2 and Figure C3 show that the estimates using the alternative estimator are consistent with the baseline results, implying that our results remain robust after allowing for heterogeneous treatment effects.

Table C2. Results of using alternative estimators

	lnPatentTransaction	
	(1)	(2)
BCS	0.110^{**}	0.088^{***}
	(0.051)	(0.030)
Control variables	Yes	Yes
Year FE	Yes	Yes
Firm FE	Yes	Yes
Estimator	Sun and Abraham (2021)	Gardner (2022)
Observations	18329	20617

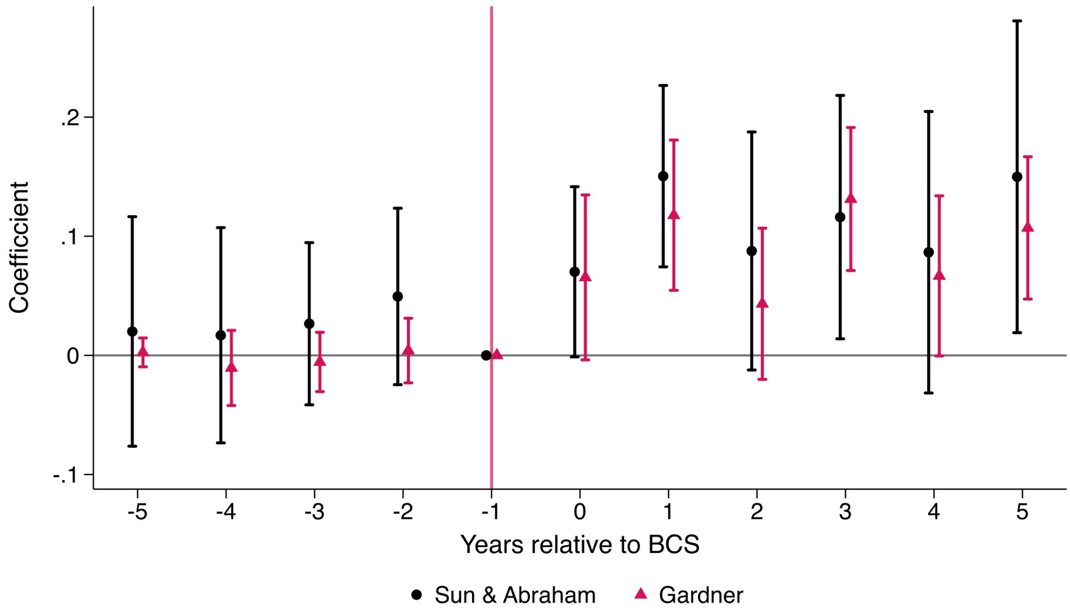

Figure C3. Event study (robust estimator)

C.5. Negative binomial regression

Chen and Roth (2024) demonstrates that applying the log-plus-one transformation in datasets with a high prevalence of zeros can bias parameter estimates. To address this concern, we re-estimate the effect of BCS on firm patent transactions using the raw count of transactions as the dependent variable with negative binomial regression. As shown in Table C3, the estimated coefficients are slightly larger than baseline results, implying that the log-plus-one transformation may underestimate the impact of BCS. Accordingly, our baseline estimates should be interpreted as a conservative lower bound.

Table C3. Results of negative binomial regression

	PatentTransaction	
	(1)	(2)
BCS	0.095^{**}	0.101^{**}
	(0.042)	(0.042)
Control variables	No	Yes
Year FE	Yes	Yes
Firm FE	Yes	Yes

Observations	18329	18329
Log likelihood	-21870.13	-21495.27

C.6. PSM-DID

To address the concern that firms in early-adopting cities may differ systematically from those in late- or non-adopting cities, we implement a propensity score matching difference-in-differences (PSM-DID) approach as a robustness check. We estimate propensity scores using covariates from the baseline regression, and then perform 1:1 nearest-neighbor matching without replacement, ensuring comparability between treated and control group firms.

As shown in Figure C4, the distribution of propensity scores between the treatment and control groups becomes closely aligned after matching, suggesting that the matching procedure is effective in balancing observed characteristics. Figure C5 further confirms the improvement in covariate balance, as the standardized bias across all covariates is substantially reduced after matching. We then re-estimate the difference-in-differences specification using the matched sample. The results, reported in Table C4, remain robust and consistent with our baseline estimates.

Table C4. PSM-DID

	lnPatentTransaction	
	(1)	(2)
BCS	0.143^{**}	0.141^{**}
	(0.066)	(0.061)
Control variables	No	Yes
Year FE	Yes	Yes
Firm FE	Yes	Yes

Observations	9172	9172
R^2	0.480	0.492

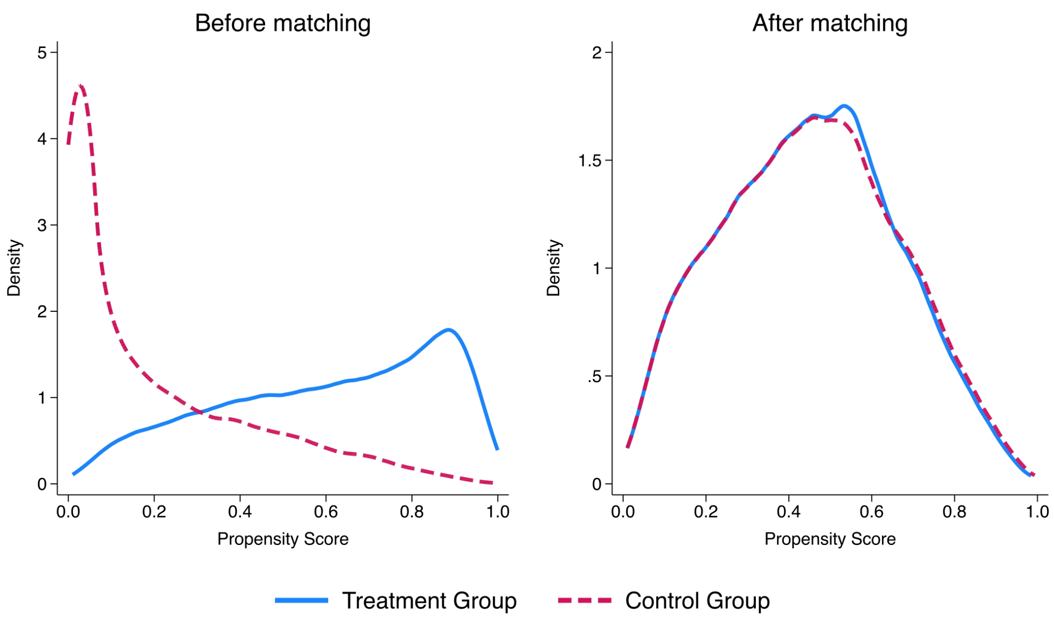

Figure C4. Propensity score distributions before and after matching

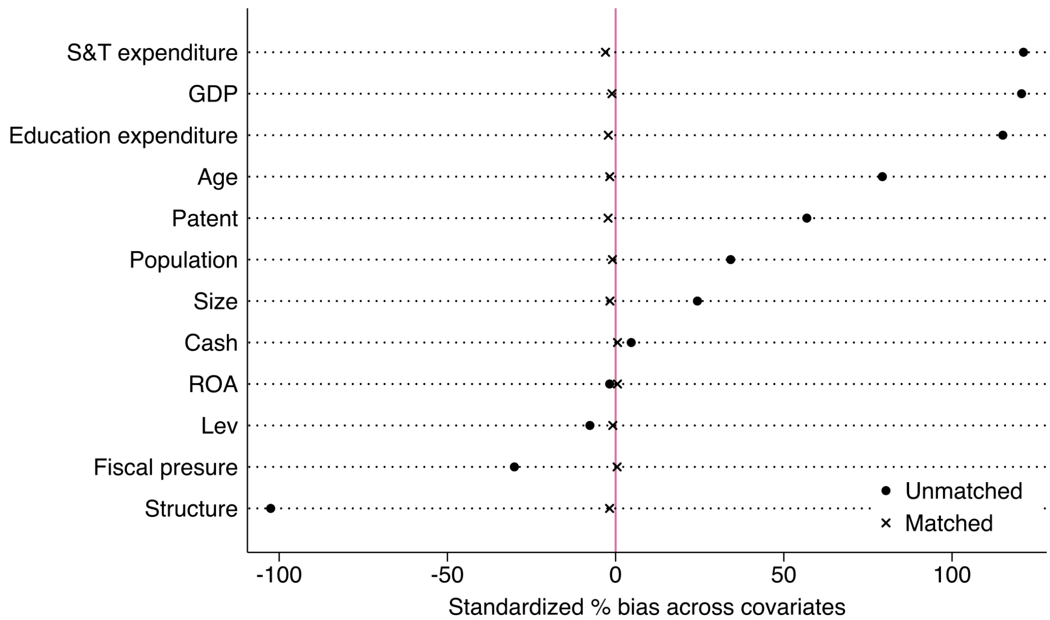

Figure C5. Standardized Bias of Covariates Before and After Matching

C.7. Accounting for the Spatial Spillovers effect

To account for the potential spatial spillover effects of broadband infrastructure, we extend the baseline specification by incorporating indirect exposure through geographic proximity. Following Butts (2021), we identify untreated cities that share borders with BCS pilot cities and define a dummy variable, *Spillover*, which equals one for firms in those neighboring cities starting from the year when any adjacent city implemented the BCS, and zero otherwise. In this setting, the *BCS* indicator captures the direct effect of broadband expansion on firms in treated cities, while *Spillover* captures indirect effects on geographically proximate but untreated firms.

This extension addresses possible violations of the stable unit treatment value assumption (SUTVA). By explicitly modeling spillover exposure, we mitigate bias arising from treatment spillovers across city boundaries, ensuring more credible identification of the policy's direct impact (Butts, 2021).

Results in Table C5 show that both *BCS* and *Spillover* coefficients are positive and statistically significant, with the former being notably larger. These findings indicate that while broadband infrastructure has the strongest impact on firms in directly treated cities, it also generates substantial positive spillovers for firms in neighboring areas. After accounting for the spatial spillovers of the BCS, the estimated coefficient increases relative to the baseline regression, suggesting that failing to consider these spillovers likely results in an underestimation of the effect of BCS on firm patent transactions.

Table C5. Results accounting for spatial spillovers

	$lnPatentTransaction$	
	(1)	(2)

BCS	0.159***	0.135***
	(0.046)	(0.039)
Spillover	0.103**	0.095**
	(0.049)	(0.043)
Control variables	No	Yes
Year FE	Yes	Yes
Firm FE	Yes	Yes
Observations	22915	22915
R^2	0.397	0.422

References

Baker, A. C., Larcker, D. F., & Wang, C. C. Y. (2022). How much should we trust staggered difference-in-differences estimates? *Journal of Financial Economics*, *144*(2), 370-395.

Butts, K. (2021). Difference-in-differences estimation with spatial spillovers. *arXiv preprint arXiv:2105.03737*.

Chen, J., & Roth, J. (2024). Logs with Zeros? Some Problems and Solutions*. *The Quarterly Journal of Economics*, *139*(2), 891-936.

Gardner, J. (2022). Two-stage differences in differences. *arXiv preprint arXiv:2207.05943*.

Goodman-Bacon, A. (2021). Difference-in-differences with variation in treatment timing. *Journal of Econometrics*, *225*(2), 254-277.

Li, G., & Branstetter, L. G. (2024). Does “Made in China 2025” work for China? Evidence from Chinese listed firms. *Research Policy*, *53*(6), 105009.

Sun, L., & Abraham, S. (2021). Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. *Journal of Econometrics*, *225*(2), 175-199.

Yu, L., Ma, T., Wu, S., & Lyu, Z. (2023). How does broadband internet affect firm-level labor misallocation: The role of information frictions. *China Economic Review*, 102067.