The Impacts of Foreign Ownership Restrictions on Agricultural Land Prices and Productivity

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Executive Summary

Since 2023, several US states have passed laws restricting foreigners' ability to own property. While these laws are meant to address national security concerns, their long-term economic effects remain uncertain. To understand the potential economic impacts, this study examines the effects of laws passed by several Midwestern states restricting foreign ownership of agricultural land in the late 1970s and early 1980s. The effects on state-level land prices and agricultural productivity are examined for two groups of states: (1) states that passed No Foreign Ownership restrictions on agricultural land and (2) states that passed Acreage Restrictions on the foreign ownership of agricultural land. The analysis uses state-level data on agricultural productivity and land prices over 50 years and applies a staggered difference-in-differences panel-data framework. Panel data regressions are also employed as a robustness check. Results show statistically significant reductions in land prices across most model specifications, though most productivity results are not statistically significant. The panel-data robustness check largely confirms these findings.

Introduction

In response to public outcry over a Chinese firm's purchase of land near a US airbase, North Dakota enacted Senate Bill (SB) 2371 in 2023. This law bans foreign businesses and governments in six designated countries from owning property in the state. Over the next two years, several other states followed with their own laws. The primary justification for these laws is national security.[1] Recent academic research also suggests that these concerns were the driver of these decisions by states, particularly regarding China.[2]

[1] For example, Florida's governor, Ron DeSantis, justified his state's law by stating that it was in the interest of "national security." See "Governor Ron DeSantis Cracks Down on Foreign Countries of Concern, Launches SecureFlorida for Property Registration," press release, Executive Office of the Governor (Tallahassee, Fla.), November 13, 2023, https://www.flgov.com/eog/news/press/2023/governor-ron-desantis-cracks-down-foreign-countries-concern-launches-secureflorida. Similarly, Alabama's governor, Kay Ivey, justified her state's law by claiming that the ownership of land by "foreign entities" could pose a national security risk. See "Governor Ivey Signs House Bill 379, Secures Alabama's Lands," press release, Office of the Governor of Alabama (Montgomery, Ala.), May 31, 2023, https://governor.alabama.gov/newsroom/2023/05/governor-ivey-signs-house-bill-379-secures-alabamas-lands/.

[2] Lin Lin and David L. Ortega, "Drivers of State Legislative Actions Restricting Foreign Holdings of U.S. Agricultural Land," Food Policy 134 (2025): 102859, https://doi.org/10.1016/j.foodpol.2025.102859.

While the main justifications are national security concerns, there is the potential for unintended economic consequences. These restrictions cover several property types, including agricultural land, natural resources, and general real estate. By reducing the pool of potential buyers, these laws may prevent the buyer who values the property most from obtaining it. Since the buyer who values a property most highly is likely also the one who can generate the greatest returns from it, restricting the buyer pool could mean that property is not allocated to its most productive use. This could result in less investment in new, more productive technologies, and potentially lower prices and productivity over time.

Furthermore, basic supply and demand theory suggests that in the short run, the supply of property is relatively fixed and cannot quickly adjust. When the pool of eligible buyers is reduced, demand shifts inward while supply remains constant, putting downward pressure on prices across affected property markets. This price decline could signal to the market that the property is expected to generate lower future returns. Thus, existing domestic property owners whose property values decline may forgo several economic activities. These include reduced hiring, fewer economic transactions, and lower overall economic activity.

Given how recent these laws are, such as the North Dakota Senate Bill, it is difficult to measure their economic impact. Nonetheless, several other state-level laws restricting foreign ownership of agricultural land were enacted decades earlier. In the late 1970s and early 1980s, several Midwestern states passed legislation prohibiting foreign ownership of agricultural land. These laws share a common theme with more recent laws: the perception that foreign ownership poses a potential threat.[3] This study examines the effect of these laws on land prices and productivity. Compared to the present laws, these older laws differ in two main ways. The first difference is that they are aimed exclusively at agricultural land.[4] The second difference is that these laws apply to all foreigners.[5] Because these laws have been in place for decades, data exist that can be used to estimate their economic impacts.

[3] For example, according to Bryce Shelman, due to the oil crisis of the early 1970s and the influx of foreign capital, many people in Iowa felt that the domestic food supply would be threatened if foreign ownership was allowed. See Bryce T. Shelman, "Realization of the American Dream by Foreign Investors: Alien Agricultural Land Ownership in Iowa," 42 J. Corp. L. 731 (2016). Similarly, a 1978 Government Accountability Office (GAO) report notes that Joseph Teasdale, then-governor of Missouri, argued that foreign ownership of land would have negative cultural consequences, such as the destruction of the "family farm." See US Government Accountability Office, Foreign Ownership of U.S. Farmland: Much Concern, Little Data, CED-78-132 (Washington, DC: GAO, 1978), https://www.gao.gov/assets/ced-78-132.pdf.

[4] For example, Iowa's 1980 law applies to land that is for "the production of agricultural crops." See Iowa Code ch. 9I, "Nonresident Aliens — Land Ownership," §§ 9I.1–9I.12 (2024), https://www.legis.iowa.gov/docs/code/9I.pdf.

[5] For example, South Dakota's law applies to "Foreign governments," "Foreign person[s]," and "Foreign entit[ies]." See South Dakota Legislature, "South Dakota Codified Laws § 43-2A," accessed October 14, 2025, https://sdlegislature.gov/Statutes/43-2A. In contrast, recent laws typically apply only to nations legally designated "foreign adversaries." See 15 C.F.R. § 791.4 (2025), "Determination of Foreign Adversaries," US Department of Commerce, Bureau of Industry and Security, accessed October 14, 2025, https://www.ecfr.gov/current/title-15/part-791/section-791.4. This rule currently designates China, Cuba, Iran, North Korea, Russia, and Venezuela as "foreign adversaries."

Impacts of Foreign Ownership Restrictions

Several studies examine the impact of foreign investment on whole economies, using various measures of economic performance. The studies that examine the impact of Foreign Direct Investment (FDI) on economic growth fall into one of two categories. The first category looks at several countries over time. For example, Borensztein et al. (1998) examine the impact of FDI for 69 developing countries from 1970 to 1989.[6] They find that FDI is correlated positively with economic growth, but the effect depends on the recipient country's existing level of human capital.[7] Alfaro et al. (2004) look at how FDI impacts economic growth, specifically examining the role of a country's preexisting financial system in its ability to absorb FDI.[8] Using data on several countries from 1975 to 1995, they find that while the effects of FDI on growth are ambiguous, countries with well-developed financial markets gain from FDI.[9] Finally, Hansen and Rand (2006) find that from 1970 to 2000, FDI was correlated with positive GDP growth in 31 developing countries.[10]

The other category of studies examining the impact of FDI on economic growth uses individual country-specific studies. Iqbal et al. (2010) examine the impact of FDI on economic growth in Pakistan from 1998 to 2009.[11] They find that FDI has a positive impact on economic growth.[12] Chandio et al. (2019) examine the effects of FDI on Pakistan's agricultural sector from 1991 to 2013.[13] They find that FDI led to positive growth in the agricultural sector.[14] Fazaalloh (2024) examines the effects of FDI on economic growth at the provincial and sectoral level in Indonesia from 2010 to 2019.[15] He finds that across all provinces, FDI is associated with positive growth over the time period.[16]

[6] Eduardo Borensztein, José De Gregorio, and Jong-Wha Lee, "How Does Foreign Direct Investment Affect Economic Growth?" Journal of International Economics 45, no. 1 (1998): 115–135, https://doi.org/10.1016/S0022-1996(97)00033-0

[7] Ibid

[8] Laura Alfaro, Areendam Chanda, Sebnem Kalemli-Ozcan, and Selin Sayek, "FDI and Economic Growth: The Role of Local Financial Markets," Journal of International Economics 64, no. 1 (2004): 89–112.

[9] Ibid

[10] Henrik Hansen and John Rand, "On the Causal Links Between FDI and Growth in Developing Countries," The World Economy 29, no. 1 (2006): 21-41.

[11] Muhammad Saeed Iqbal, Faiz Muhammad Shaikh, and Abdul Hameed Shar, "Causality Relationship between Foreign Direct Investment, Trade and Economic Growth in Pakistan," Asian Social Science 6, no. 9 (2010): 82.

[12] Ibid

[13] Abbas Ali Chandio, Amir Ali Mirani, and Rashid Usman Shar, "Does Agricultural Sector Foreign Direct Investment Promote Economic Growth of Pakistan? Evidence from Cointegration and Causality Analysis," World Journal of Science, Technology and Sustainable Development 16, no. 4 (2019): 196–207, https://doi.org/10.1108/WJSTSD-05-2019-0025.

[14] Ibid

[15] Al Muizzuddin Fazaalloh, "FDI and Economic Growth in Indonesia: A Provincial and Sectoral Analysis," Journal of Economic Structures 13, no. 1 (2024): article 3, https://doi.org/10.1186/s40008-023-00323-w.

[16] Ibid

He also finds that in all sectors but agriculture, FDI was associated with positive economic growth.[17] Mallick and Tanwar (2025) examine how FDI impacted India's economic growth from 2000 to 2024, and find that FDI inflows positively impacted GDP for that period.[18] However, they also find that simultaneous outward FDI by domestic Indian firms had a negative impact on India's GDP, due to reduced capital available for domestic investment.[19]

Other macroeconomic studies examine the impact of FDI on productivity measures. For example, Baltabaev (2014) examines data on 49 countries from 1974 to 2008, and finds that FDI did increase the productivity of those countries.[20] However, he notes that the impact of FDI is predicated on the ability of a recipient nation to absorb it.[21] Herzer and Donaubauer (2018) examine the impact of FDI on productivity.[22] Looking at 59 countries from 1981 to 2011, they find that FDI has a negative long-run impact on productivity in developing countries.[23] However, several preexisting characteristics of the recipient nations determine the impact of FDI on productivity.[24] That is, in countries with low human capital and low financial openness, they may be unable to absorb FDI fully.[25] Lastly, Abdullah and Chowdhury (2020) examine how FDI affects Total Factor Productivity (TFP) using data from 77 countries between 1980 and 2008.[26] While they find that FDI does not have a statistically significant impact on TFP, they note that a country's ability to absorb new technologies, for example the ability of the domestic workforce to build on the technological knowledge from foreign firms, could be an important reason for whether FDI has a positive or negative effect on TFP.[27]

Beyond examining macroeconomic effects, other studies look at firm-level effects of FDI.

[17] Ibid

[18] Satyajit Mallick and Rashmi Tanwar, "An Empirical Analysis of the Relationship between FDI Inflow and Outflow with the Economic Growth of India," Margin: The Journal of Applied Economic Research 19, no. 1 (2025): 90–110, https://doi.org/10.1177/00252921251362673.

[19] Ibid

[20] Botirjan Baltabaev, "Foreign Direct Investment and Total Factor Productivity Growth: New Macro-Evidence," The World Economy 37, no. 2 (2014): 311–334, https://doi.org/10.1111/twec.12115.

[21] Ibid

[22] Dierk Herzer and Julian Donaubauer, "The Long-Run Effect of Foreign Direct Investment on Total Factor Productivity in Developing Countries: A Panel Cointegration Analysis," Empirical Economics 54, no. 2 (2018): 309–342, https://doi.org/10.1007/s00181-016-1206-1.

[23] Ibid

[24] Ibid

[25] Ibid

[26] Mohammed Abdullah and Murshed Chowdhury, "Foreign Direct Investment and Total Factor Productivity: Any Nexus?," Margin: The Journal of Applied Economic Research 14, no. 2 (2020): 164–190, https://doi.org/10.1177/0973801020904473

[27] Ibid

Arnold and Javorcik (2009) look at the relationship between foreign ownership and firm performance.[28] They find that, relative to the control group, foreign-owned firms experienced a 13.5% increase in productivity three years after being acquired by foreign owners.[29] Another example is by Javorcik and Poelhekke (2017), who examine 157 cases of foreign firms divesting from Indonesian firms between 1990 and 2009.[30] They find that in the first year, log total factor productivity declined by 0.038 log points for the divested Indonesian firms, and the decline persists in the two subsequent years.[31] Lastly, Liu and Wang (2023) examine the effects of China's 2002 FDI deregulation.[32] Looking at data from 1998 to 2007, they find that a 1% increase in a firm's foreign equity ownership is associated with a 0.5% increase in that firm's patent applications in the same year.[33]

Although there are a limited number of academic studies that examine the impacts of restrictions on foreign investment, those that do tend to find a negative impact on asset prices and productivity. For example, Beaulieu and Saunders (2014) examine Canada's 2013 ban on foreign state-owned enterprises from operating in oil extraction.[34] They find that after the ban took effect, stock prices declined among smaller oil extraction firms.[35] Another example is from Genthner and Kis-Katos (2019), who examine Indonesian firms that were prohibited from receiving foreign investment.[36] They find that one year after an Indonesian firm was prohibited from receiving investment, the firm experienced a 3% reduction in its productivity.[37] Pavlov, Somerville, and Wetzel (2023) analyze the impact of British Columbia's foreign buyers' tax.[38] They find that when the tax was implemented, prices of single-family homes in areas with a high concentration of foreign buyers declined by 6% relative to similar homes in areas with lower foreign buyer presence.[39]

[28] Jens Matthias Arnold and Beata S. Javorcik, "Gifted Kids or Pushy Parents? Foreign Direct Investment and Plant Productivity in Indonesia," Journal of International Economics 79, no. 1 (2009): 42–53.

[29] Ibid

[30] Beata Javorcik and Steven Poelhekke, "Former Foreign Affiliates: Cast Out and Outperformed?," Journal of the European Economic Association 15, no. 3 (2017): 501–39.

[31] Ibid

[32] Yan Liu and Xuan Wang, "The Impact of Foreign Direct Investment on Innovation at Domestic Firms: Evidence from the Deregulation of Foreign Investment in China," Canadian Journal of Economics / Revue canadienne d'économique 56, no. 2 (2023): 676–718

[33] Ibid

[34] Eugene Beaulieu and Matthew Saunders, "The Impact of Foreign Investment Restrictions on the Stock Returns of Oil Sands Companies," SPP Research Paper 7, no. 16 (2014)

[35] Ibid

[36] Robert Genthner and Krisztina Kis-Katos, Foreign Investment Regulation and Firm Productivity: Granular Evidence from Indonesia, cege Discussion Paper No. 345 (Göttingen: University of Göttingen, Center for European, Governance and Economic Development Research, 2019).

[37] Ibid

[38] Andrey Pavlov, Tsur Somerville, and Jake Wetzel, "Foreign Buyer Taxes and Housing Affordability," Real Estate Economics 52, no. 3 (2023): 928–950.

[39] Ibid

Thus, the literature suggests that foreign ownership restrictions can have unintended economic consequences.

Agricultural Land Prices and Productivity

A number of theoretical and empirical papers seek to explain the value of farmland. One of the earliest studies was by Tweeten and Martin (1966), who asked why land prices had increased more than 100% in the US since 1950, while net farm income remained stable.[40] Looking at data from 1923 to 1963, they find that current farm income is not a main driver of farmland prices.[41] Rather, they argue that factors including farm consolidation, farm program benefits, and speculation account for the majority of the price increase.[42] Melichar (1979) examines how, during the 1970s, the value of farm assets such as land rose faster than farm income.[43] After adjusting for inflation and isolating the return to assets from labor and management income, he finds that the current profit from land is not the primary determinant of its price.[44] Additionally, Burt (1986) builds on Melichar's work by testing an econometric model using Illinois farm data from 1960 to 1983.[45] He finds that current rental income, which he identifies as an indirect measure of expected future returns, explains a majority of the prices in those years.[46]

Other empirical work supports the idea that expected future earnings are key in determining land prices. Alston (1986) examines the causes of farmland price growth in the United States from 1963 to 1982.[47] He examines whether the growth in land prices is due to growth in rental income or inflation.[48] Looking at eight Midwestern states over that period, he finds that land prices grew at a rate similar to income growth, supporting the view that expected future earnings primarily drive farmland values.[49] In contrast, inflation is found to have little effect on land prices.[50]

[40] Luther G. Tweeten and James E. Martin, "A Methodology for Predicting U.S. Farm Real Estate Price Variation," Journal of Farm Economics 48, no. 2 (1966): 378–393.

[41] Ibid

[42] Ibid

[43] Emanuel Melichar, "Capital Gains versus Current Income in the Farming Sector," American Journal of Agricultural Economics 61, no. 5 (1979): 1085–1092.

[44] Ibid

[45] Oscar R. Burt, "Econometric Modeling of the Capitalization Formula for Farmland Prices," American Journal of Agricultural Economics 68, no. 1 (1986): 10–26, https://doi.org/10.2307/1241645.

[46] Ibid

[47] Julian M. Alston, "An Analysis of Growth of U.S. Farmland Prices, 1963-82," American Journal of Agricultural Economics 68, no. 1 (February 1986): 1-9.

[48] Ibid

[49] Ibid

[50] Ibid

Just and Miranowski (1993) examine the US farmland price appreciation of the 1970s and the depreciation that began in 1982.[51] Using state-level data from 1963-1986, they find that inflation, changes in the real return on capital, and farming returns were major factors.[52] For example, in 1979, they attribute 39%, 38%, and 25% of the farmland price appreciation to increased farming returns, inflation effects, and returns to saving, respectively.[53] For the depreciation that began in 1982, they find that rising real interest rates and declining inflation were the main drivers.[54]

Some studies examine how government programs affect the price of land. Barnard et al. (1997) examine how government payments affect per-acre cropland values in the US.[55] They find wide spatial variation in the effects of government payments on cropland values.[56] They estimate that in Texas, the southern Corn Belt, and the Southeast, cropland values are 50% higher than they would have been without government payments.[57] They also find that for North Dakota, Kansas, and the northern Corn Belt, the effect is lower, ranging from 10% to 20%.[58] Additionally, Burnett et al. (2024) look at how factors such as spillover from neighboring counties and past land value impact current US cropland values.[59] They find that both of these factors influence current cropland values.[60]

In addition to the large number of studies examining factors influencing agricultural land prices, several others examine factors influencing US agricultural TFP. The concept of TFP was first articulated by Solow (1957), who examined the sources of economic growth in the United States from 1909 to 1949.[61] He identifies two sources of output growth: increases in inputs such as capital and labor, or improvements in the efficiency with which those inputs are used.[62] By subtracting the contribution of input growth from total

[51] Richard E. Just and John A. Miranowski, "Understanding Farmland Price Changes," American Journal of Agricultural Economics 75, no. 1 (1993): 156–168.

[52] Ibid

[53] Ibid

[54] Ibid

[55] Charles H. Barnard, Gordon Whittaker, David Westenbarger, and Mary Ahearn, "Evidence of Capitalization of Direct Government Payments into U.S. Cropland Values," American Journal of Agricultural Economics 79, no. 5 (1997): 1642–1650.

[56] Ibid

[57] Ibid

[58] Ibid

[59] Burnett, John W., Donald J. Lacombe, and Steven Wallander. "Spatial and Temporal Spillovers in US Cropland Values." Journal of Agricultural and Resource Economics 49, no. 1 (2024): 63–80.

[60] Ibid

[61] Robert M. Solow, "Technical Change and the Aggregate Production Function," Review of Economics and Statistics 39, no. 3 (1957): 312–320, https://doi.org/10.2307/1926047.

[62] Ibid

output growth, Solow derives TFP.[63] He finds that 87.5% of US labor productivity growth during this period was attributable to the efficient use of inputs rather than to capital accumulation.[64] Building on Solow's framework, Ball et al. (1997) develop TFP for US agriculture.[65] By incorporating capital, labor, and intermediate inputs such as feed, seed, fertilizer, and chemicals, they find that productivity growth averaged 1.94% annually from 1948 to 1994.[66]

Griliches (1964) provides one of the first pieces of empirical evidence that exogenous factors beyond conventional inputs drive agricultural TFP.[67] He finds that both farmer education and public Research and Development (R&D) are positively correlated with agricultural TFP in the US.[68] Huffman (1974) builds on this finding by showing that more educated workers adopt new technologies more quickly.[69]

A number of studies examine the specific effects of public R&D investment on agricultural productivity. Huffman and Evenson (2006) examine whether federal formula grants or competitive grants have a greater impact on agricultural TFP.[70] Using data for the 48 contiguous US states from 1970 to 1999, they find that federal funding has a larger positive effect on agricultural TFP than competitive grant funding.[71] Plastina and Fulginiti (2012) examine how the internal rate of return (IRR) of state-level public agricultural R&D affects TFP.[72] They find that the weighted average own-state IRR has a 17% impact on TFP.[73]

Research has also examined the role of private sector R&D in driving state-level agricultural productivity. Clancy and Wang (2023) look at how state-level private sector

[63] Ibid

[64] Ibid

[65] Ball, V. Eldon, Jean-Christophe Bureau, Richard Nehring, and Agapi Somwaru. "Agricultural Productivity Revisited." American Journal of Agricultural Economics 79, no. 4 (1997): 1045–1063. https://doi.org/10.2307/1244263.

[66] Ibid

[67] Zvi Griliches, "Research Expenditures, Education, and the Aggregate Agricultural Production Function," American Economic Review 54, no. 6 (1964): 961–974.

[68] Ibid

[69] Wallace E. Huffman, "Decision Making: The Role of Education," American Journal of Agricultural Economics 56, no. 1 (1974): 85–97, https://doi.org/10.2307/1239349.

[70] Wallace E. Huffman and Robert E. Evenson, "Do Formula or Competitive Grant Funds Have Greater Impacts on State Agricultural Productivity?," American Journal of Agricultural Economics 88, no. 4 (2006): 783–798.

[71] Ibid

[72] Alejandro Plastina and Lilyan Fulginiti, "Rates of Return to Public Agricultural Research in 48 US States," Journal of Productivity Analysis 37 (2012): 95–113, https://doi.org/10.1007/s11123-011-0252-0.

[73] Ibid

R&D impacts TFP.[74] They examine patents on 6 agricultural subsectors (machinery, plants, research inputs, animal health, fertilizer, and biocide) from 1976 to 2015.[75] After removing non-stationary variables to ensure valid statistical inference, they find that patents on plants and research inputs have significant positive effects on state TFP, with coefficients of 0.019 and 0.014, respectively.[76] However, all other coefficients in that model are not statistically significant.[77]

There are also studies suggesting that R&D spillovers are another important factor influencing TFP. Evenson (1989) provides empirical evidence that R&D spillovers are critical to productivity but are geographically bounded by geoclimatic similarity.[78] Using data for 42 states from 1950 to 1982, he finds evidence for the impact of spillover for crop and livestock research.[79] However, livestock production shows larger spillover benefits from climatically similar regions than crop production, reflecting the nature of local soil and climate conditions.[80] Alston et al. (2011) also examine the effect of state spillovers on research.[81] Looking at 48 states from around 1949 to 2002, they find benefit-cost ratios of 21:1 for own-state R&D returns rising to 32:1 when interstate spillovers are included.[82] This means that each dollar invested in public agricultural R&D generates approximately twenty-one dollars in economic returns, rising to thirty-two dollars when spillover benefits are counted.[83]

Several studies have examined how climate variables affect TFP. Liang et al. (2017) use data for the US from 1981 to 2010 and find that growing-season temperature and precipitation explain approximately 70% of the variation in year-to-year TFP growth.[84] Ortiz-Bobea, et al. (2018) examine how US agricultural TFP responds to climatic

[74] Matt Clancy and Sun Ling Wang, "The Impacts of Private Sector R&D on U.S. Agricultural Productivity Growth" (paper presented at the Agricultural & Applied Economics Association Annual Meeting, Washington, DC, July 23-25, 2023)

[75] Ibid

[76] Ibid

[77] Ibid

[78] Evenson, R.E. "Spillover Benefits of Agricultural Research: Evidence from U.S. Experience." American Journal of Agricultural Economics 71, no. 2 (1989): 447–452.

[79] Ibid

[80] Ibid

[81] Alston, Julian M., Matthew A. Andersen, Jennifer S. James, and Philip G. Pardey. "The Economic Returns to U.S. Public Agricultural Research." American Journal of Agricultural Economics 93, no. 5 (2011): 1257–1277. https://doi.org/10.1093/ajae/aar044.

[82] Ibid

[83] Ibid

[84] Xin-Zhong Liang, You Wu, Robert G. Chambers, Daniel L. Schmoldt, Wei Gao, Chaoshun Liu, Yan-An Liu, Chao Sun, and Jennifer A. Kennedy, "Determining Climate Effects on US Total Agricultural Productivity," Proceedings of the National Academy of Sciences 114, no. 12 (2017): E2285–E2292, https://doi.org/10.1073/pnas.1615922114

variations and how this sensitivity has evolved regionally over 1960–2004.[85] Their results show that Midwestern agricultural productivity is most sensitive to weather changes.[86] A summer that is 2°C warmer than the regional historical average is associated with an 11.3% drop in TFP in 1960-1982 and a 29.3% drop in 1983-2004.[87] Next, Sabasi and Shumway (2018) examine how various climate variables affect components of agricultural TFP.[88] For climate-related independent variables, they use annual total precipitation; growing degree days (accumulated exposure between 8°C and 32°C during the growing season); damaging degree days (accumulated exposure above 32°C during the growing season); and 30-year moving averages of temperature and precipitation.[89] For the TFP component dependent variables, they use technological change, changes in technical efficiency, and changes in scale and mix efficiency.[90] For all climatic variables overall, they find statistically significant effects, but they are heterogeneous across regions and TFP components.[91]

Lastly, while substantially fewer than climate-focused and R&D studies, some research examines the relationship between credit access and agricultural productivity. Sabasi et al. (2021) examine how credit access impacts state-level TFP in US agriculture.[92] Using state-level data for the 48 contiguous states from 1966 to 2003, they measure credit access through both direct measures (real total debt per farm, real agricultural production loans per farm) and indirect measures (real farm assets per farm, real bank net income per bank office).[93] They find positive and significant associations between credit access and TFP for all six measures.[94] However, they also note that this positive relationship assumes the benefits of additional credit outweigh its potential negative effects, such as allowing less-efficient farms to persist rather than exit the market.[95]

[85] Ariel Ortiz-Bobea, Erwin Knippenberg, and Robert G. Chambers, "Growing Climatic Sensitivity of U.S. Agriculture Linked to Technological Change and Regional Specialization," Science Advances 4, no. 12 (2018): eaat4343, https://doi.org/10.1126/sciadv.aat4343.

[86] Ibid

[87] Ibid

[88] Darlington Sabasi and C. Richard Shumway, "Climate Change, Health Care Access and Regional Influence on Components of U.S. Agricultural Productivity," Applied Economics 50, no. 57 (2018): 6149–64.

[89] Ibid

[90] Ibid

[91] Ibid

[92] Darlington Sabasi, C. Richard Shumway, and Lyudmyla Kompaniyets, "Analysis of Credit Access, U.S. Agricultural Productivity, and Residual Returns to Resources," Journal of Agricultural and Applied Economics 53, no. 3 (2021): 389–415, https://doi.org/10.1017/aae.2021.17

[93] Ibid

[94] Ibid

[95] Ibid

Thus, while increased credit access generally supports productivity, excessive debt could have diminishing or even negative returns.[96]

Despite the large number of studies that have examined factors influencing agricultural land prices and productivity, none have examined the impacts of restrictions on foreign ownership.

Methodology

This study uses a difference-in-differences research design to evaluate the impact of the restrictions. In this context, treated states are those that enacted foreign ownership restrictions, while control states are those that did not enact any restrictions. A basic panel regression, which analyzes data across multiple states and time periods, would not explicitly construct a counterfactual by using control states to compare with the treated states. Without this counterfactual, it becomes difficult to attribute any change in the outcome to the restrictions themselves. Difference-in-differences addresses this problem by calculating the change in the outcome for treated states from before to after treatment, and separately calculating the same change for control states over the same period. Subtracting the control group's change from the treated group's change isolates the effect attributable to the restriction itself.

While several Midwestern states applied their laws during the same period (late 1970s and early 1980s), the specific years of enactment differ for each. Goodman-Bacon (2021) notes that in settings where the treated units have different treatment times, a standard two-way fixed effects (TWFE) difference-in-differences estimator may combine invalid treatment comparisons across cohorts.[97] The problem arises because TWFE implicitly compares late-adopting states (for example, Iowa in 1980) to both never-treated states and earlier-adopting states (for example, Missouri in 1978).[98] As a result, using already-treated states as controls can produce biased and inconsistent estimates of the average treatment effect. To address this issue, a staggered difference-in-differences approach is employed using the method developed by Callaway and Sant'Anna (2021).[99] This method estimates group-time average treatment effects (ATT) for each cohort g at each time

[96] Ibid

[97] Andrew Goodman-Bacon, "Difference-in-Differences with Variation in Treatment Timing," Journal of Econometrics 225, no. 2 (2021): 254–277, https://doi.org/10.1016/j.jeconom.2021.03.014

[98] Ibid

[99] Brantly Callaway and Pedro H.C. Sant'Anna, "Difference-in-Differences with Multiple Time Periods," Journal of Econometrics 225, no. 2 (2021): 200–230.

t, using never-treated states as the comparison group. These group-time effects are then aggregated into an overall average treatment effect across all groups and time periods.[100]

The validity of any difference-in-differences design rests on the parallel trends assumption. This assumption holds that, had the foreign ownership restrictions not been enacted, both the treated and control states would have followed the same trajectory for the dependent variable of interest. Should treated and control states not follow the same path, any estimated effect could instead reflect other factors that cause states to evolve differently over time, rather than the effect of treatment itself. As supporting evidence for this assumption, pre-treatment event study plots are presented in the Appendix for all four models. If these plots show divergence between treated and control states before treatment, the validity of the difference-in-differences approach would be called into question. Across all four models, the plots show no such divergence in the pre-treatment period, supporting the parallel trends assumption.[101]

Seven states enacted foreign ownership restrictions in the late 1970s and early 1980s. Three states enacted laws restricting foreigners from owning any agricultural land: Minnesota in 1977, Missouri in 1978,[102] and Iowa in 1980. Four states enacted laws that allowed foreign ownership of some agricultural land, but with strict limits on the amount that could be owned: North Dakota in 1979, South Dakota in 1979, Pennsylvania in 1980, and Wisconsin in 1982. Because the study examines two outcomes (agricultural land prices and productivity) and two types of restrictions (No Foreign Ownership and Acreage Restrictions), this study estimates four separate staggered difference-in-differences models. The analysis includes 40 states for the No Foreign Ownership models and 41 states for the Acreage Restrictions models, with the remaining states in each sample serving as never-treated controls. Treated states from one model are excluded from the control group of the other. The No Foreign Ownership states are not used as controls in the Acreage Restrictions model, and vice versa. Several states are excluded for distinct reasons. Alaska and Hawaii lacked sufficient data; Illinois was excluded due to uncertainty about its foreign ownership policy status; and South Carolina, Mississippi, and Nebraska were excluded because they have foreign ownership laws dating to the 1890s.

100 A state level analysis is chosen because each of the laws are implemented on the state level. Therefore, it would be most appropriate to capture their impact.

101 See the Appendix for details.

102 Missouri's original 1978 law constituted a full ban on foreign ownership of agricultural land. A 2013 amendment introduced a 1% threshold. This allows for foreign ownership of up to 1% of the total aggregate agricultural acreage in the state. Since the original law was a full ban, and the amendment fall towards the end of the period of study, Missouri is still treated as a No Foreign Ownership State. See "Soil for Sale? State Legislative Efforts to Restrict Foreign Investments — Part Three," National Agricultural Law Center, February 13, 2025, https://nationalaglawcenter.org/soil-for-sale-state-legislative-efforts-to-restrict-foreign-investments-part-three/

Including them would contaminate the control group.

To measure the impact of foreign ownership restrictions on land prices, yearly state-level data on land price per acre from the US Department of Agriculture's (USDA) National Agricultural Statistics Service (NASS) Quick Stats database are used.[103] These values are self-reported survey estimates, in which agricultural producers are asked to report the price at which their land would sell at market value. The variable also includes the value of any dwellings that exist on the land, capturing the full market value. The prices are then converted to 2015 dollars using the consumer price index. However, self-reported land prices may differ from actual transaction prices. Bigelow and Jodlowski (2026) find that measurement error between actual and self-reported prices can arise from a number of factors.[104]

For productivity, the USDA’s Total Factor Productivity (TFP) is used, which measures agricultural output growth that exceeds input growth on the US state level.[105] Using a Törnqvist index approach, yearly TFP is calculated by subtracting the cost-share-weighted growth of inputs (labor, capital including land and machinery, and intermediate materials including feed, seed, fertilizer, and chemicals) from the revenue-share-weighted growth of outputs (crops such as corn and soybeans, livestock including cattle and hogs, and other agricultural products).[106] Input quantities are quality-adjusted to account for changes such as improvements in chemical efficacy and demographic shifts.[107] Values greater than 1 indicate productivity growth; values less than 1 indicate productivity decline.[108] All variables are measured on the state-year level from 1960 to 2015.

In addition to the staggered difference-in-differences models, fixed-effects panel regressions are estimated as a robustness check. Unlike the Callaway and Sant'Anna approach, these specifications allow both types of foreign ownership restrictions to be included in the same regression. Separate panel models are estimated for land prices and productivity. The land price panel data model includes no control variables for several reasons. First, given that current land prices are expected to be based on future

[103] US Department of Agriculture, National Agricultural Statistics Service (USDA NASS), Quick Stats Database, accessed June 11, 2025, https://www.nass.usda.gov/Quick_Stats/.

[104] Daniel P. Bigelow and Margaret Jodlowski, "Nonclassical Measurement Error in Farmland Markets with Implications for Ricardian Analysis," American Journal of Agricultural Economics 108, no. 2 (2026): 599–629

[105] US Department of Agriculture, Economic Research Service, Agricultural Productivity in the United States: Methods, accessed June 24, 2025, https://www.ers.usda.gov/data-products/agricultural-productivity-in-the-united-states/methods.

[106] Ibid

[107] Ibid

[108] Ibid

earnings, most information is either expected to already be internalized or captured by state and year fixed effects. Additionally, candidate control variables are excluded due to endogeneity concerns. Specifically, while the literature confirms that government payments are an important determinant of farmland values, there are potential reverse causality concerns from programs targeting the most valuable land. Similarly, crop prices and acres operated are also potentially endogenous, as farm size and planting decisions are themselves influenced by prevailing land prices.

The productivity model includes the following control variables. First, the model controls for climate conditions using growing season average temperature and precipitation, defined as April through October. Both variables are obtained from the National Oceanic and Atmospheric Administration (NOAA).[109] Given that the literature suggests that climate variables explain a substantial amount of variation in TFP, not controlling for climate conditions risks attributing weather-driven productivity changes to the foreign ownership restrictions. Higher temperatures and precipitation during the growing season should increase agricultural productivity. However, at a certain threshold, high temperatures and precipitation should start to negatively impact productivity. To capture the nonlinear effects of temperature and precipitation on agricultural TFP, squared terms for both variables are included. The model also controls for own-state public agricultural R&D as an accumulated capital stock, measured in constant 2015 US dollars (millions).[110] The variable sums past state level public research expenditures over a 35-year window with time-varying weights reflecting the impact profile. Effects start small as technology is developed, peak after 15-20 years as innovations are widely adopted, and decline as technology becomes obsolete.[111] Greater accumulated R&D is expected to improve productivity through technological innovation and adoption. The R&D spillover variable is calculated in the same manner as the own-state R&D variable.[112] However, it captures public research conducted in geographically similar states within the same USDA Farm Resource Region.[113] Like the first R&D variable, this variable is expected to positively influence TFP through technological innovation. Finally, farmer education, measured as mean years of education for farmers and farm operators in a given state and year, is included. This variable is constructed from individual-level microdata from the US

[109] United States Department of Commerce. National Oceanic and Atmospheric Administration. Accessed February 6, 2026. https://www.ncei.noaa.gov/.

[110] State-level public agricultural R&D capital stock data (own-state, constant 2015 US$ millions) provided by Dr. Keith Fuglie, personal communication, September 4, 2025

[111] Ibid

[112] Ibid

[113] Ibid

Decennial Census (1960-2000) and the American Community Survey (2005-2015), and sourced via IPUMS USA.[114] For intercensal years without direct observations (1961-1969, 1971-1979, 1981-1989, 1991-1999, 2001-2004), values are estimated using linear interpolation.[115] Higher levels of farmer education are expected to positively impact TFP through improved adoption of new technologies and enhanced farm management practices. The model formulations are listed below:

Callaway and Sant'Anna DiD Models

Land Price Model:

- $ATT_{LP}(g,t) = E\left[LandPrice_t(g) - LandPrice_t(0) \mid G_g = 1\right], \quad \text{for } t \geq g$

Productivity Model:

- $ATT_{TFP}(g,t) = E\left[TFP_t(g) - TFP_t(0) \mid G_g = 1\right], \quad \text{for } t \geq g$

Panel Data Models

Land Price Model:

- $LandPrice_{it} = \beta_1 NFO_{it} + \beta_2 AR_{it} + \alpha_i + \gamma_t + \varepsilon_{it}$

Productivity Model:

- $TFP_{it} = \beta_1 NFO_{it} + \beta_2 AR_{it} + \beta_3 Temp_{it} + \beta_4 Temp_{it}^2 + \beta_5 Precip_{it} + \beta_6 Precip_{it}^2 + \beta_7 RD_{it}$ $+ \beta_8 RDSpill_{it} + \beta_9 Educ_{it} + \alpha_i + \gamma_t + \varepsilon_{it}$

Where:

- $ATT(g,t)$ represents the average treatment effect on the treated — the average difference between the actual outcome and the counterfactual outcome (had treatment not occurred) for states first treated in period g, measured at time t
- $LandPrice_t\,(g)$ is the potential land price per acre (real 2015 $/acre) at time t for a state first treated in period g
- $LandPrice_t\,(0)$ is the potential land price per acre in the absence of any foreign ownership restriction
- $Gg = 1$ indicates membership in the cohort of states first treated in period g

[114] Steven Ruggles et al., IPUMS USA: Version 15.0 [dataset] (Minneapolis: IPUMS, 2024), https://doi.org/10.18128/D010.V15.0

[115] Ibid

- $TFP_t(g)$ is the potential Total Factor Productivity index at time t for a state first treated in period g
- $TFP_t(0)$ is the potential TFP index in the absence of any foreign ownership restriction
- g denotes the first period of treatment.
- $LandPrice_{it}$ is the real price per acre of agricultural land in state i in year t, measured in real 2015 dollars
- TFP_{it} is the Total Factor Productivity index in state i in year t
- NFO_{it} is a binary indicator equal to 1 for No Foreign Ownership states (Iowa, Minnesota, Missouri) after the enactment of their restriction, and 0 otherwise
- AR_{it} is a binary indicator equal to 1 for Acreage Restriction states (South Dakota, North Dakota, Pennsylvania, Wisconsin) after the enactment of their restriction, and 0 otherwise
- $Temp_{it}$ is the average growing season temperature (April–October) in state i in year t, measured in degrees Fahrenheit
- $Temp^2_{it}$ is the square of the average growing season temperature in state i in year
- $Precip_{it}$ is the average growing season precipitation (April–October) in state i in year t, measured in inches
- $Precip^2_{it}$ is the square of the average growing season precipitation in state i in year t
- RD_{it} is the accumulated public agricultural R&D capital stock in state i in year t, measured in real 2015 dollars (millions)
- $RDSpill_{it}$ is the accumulated public agricultural R&D spillover stock from neighboring states, measured in real 2015 dollars (millions). The variable is constructed using USDA Farm Resource Regions, which group states by shared agricultural and climatic characteristics. For each state, the spillover stock is calculated by summing the accumulated R&D capital stocks of all other states within the same Farm Resource Region using the same 35-year weighted lag structure as RD_{it}, and excluding the state's own contribution.
- $Educ_{it}$ is the mean years of education for farmers and farm operators in state i in year t
- α_i represents state fixed effects
- γ_t represents year fixed effects
- ε_{it} is the error term. Standard errors are clustered at the state level

Summary statistics for all of the variables are shown in Table 1.

Table 1. Descriptive Statistics

	Mean	SD	Min	Max
Agricultural Productivity (TFP Index)	0.88	0.30	0.34	2.20
Land Price (Real 2015 $/acre)	2,649.31	2,503.32	176.30	18,747.15
Temperature (degrees F)	63.41	6.58	50.76	79.13
Precipitation (inches)	3.26	1.30	0.25	7.53
RD Stock (2015 $ millions)	1,206.57	1,060.96	47.27	6,696.14
RD Spillover (2015 $ millions)	6,631.72	3,308.51	652.00	14,973.83
Farmer Education (years)	8.96	2.33	3.07	14.82

Notes: N = 2688 state-year observations across 48 states.

Full sample of 48 continental states shown.

Summer temperature and precipitation measured April-October.

RD Stock and RD Spillover expressed in real 2015 dollars (millions).

Results

Table 2 presents the results for the Callaway and Sant'Anna land price models. The first column shows results for No Foreign Ownership states and the second column shows the results for Acreage Restrictions states. The effect of No Foreign Ownership restrictions is estimated to reduce prices by $1008 in the treated states compared to the control states, at the 0.05 level of significance. The effect of the Acreage Restrictions is estimated at a $537 reduction, at the same level of significance.

Table 2. Effect of Foreign Ownership Restrictions on Land Prices (Real 2015 $/acre)

	No Foreign Ownership	Acreage Restrictions
Average Treatment Effect	-1008.120** (428.423)	-537.486** (229.161)
Observations	2240	2296
States	40	41
Treated States	Iowa (1980), Minnesota (1977), Missouri (1978)	SD (1979), ND (1979), PA (1980), WI (1982)
Control Group	Never-Treated States	Never-Treated States
Covariates	None	None

Notes: Bootstrap standard errors clustered at state level in parentheses.
* $p<0.10$, ** $p<0.05$, *** $p<0.01$.
Overall ATT aggregated across all groups and time periods using Callaway and Sant'Anna (2021).
No Foreign Ownership treated states: Iowa (1980), Minnesota (1977), Missouri (1978).
Acreage Restriction treated states: South Dakota (1979), North Dakota (1979),
Pennsylvania (1980), Wisconsin (1982).
Excluded states: Illinois, South Carolina, Mississippi, Nebraska.
Control group: never-treated states only. (Alaska and Hawaii not included due to lack of data)

Table 3 presents the results of the panel-data robustness check for land prices. The model estimates a negative effect on land prices from laws that prevent foreign ownership and those that limit the acres foreigners may own. However, the effect of the laws limiting the acres owned by foreigners is not statistically significant.

Table 3. Panel Data Model: Effect of Both Restriction Types on Land Prices

	Combined
No Foreign Ownership	-623.803** (262.206)
Acreage Restrictions	-516.961 (411.557)
Observations	2464
States	44
R-squared (within)	0.009
State Fixed Effects	Yes
Year Fixed Effects	Yes
Covariates	None

Notes: Robust standard errors clustered at state level in parentheses.
* $p<0.10$, ** $p<0.05$, *** $p<0.01$.
Two-way fixed effects (state and year).
No Foreign Ownership treated states: Iowa (1980), Minnesota (1977), Missouri (1978).
Acreage Restriction treated states: South Dakota (1979), North Dakota (1979), Pennsylvania (1980), Wisconsin (1982).
Excluded states: Illinois, South Carolina, Mississippi, Nebraska. (also, Alaska and Hawaii)

The decline in agricultural land prices is consistent with economic theory. While the supply of agricultural land remains effectively fixed, these laws remove foreign entities from the pool of potential buyers, reducing demand. With supply held constant, this reduction in demand causes land prices to fall.

This price drop could yield several unintended consequences. First, as discussed in the literature review, the price of land largely reflects the present value of its expected future earnings. A drop in land prices may therefore signal to the market that future earnings from the land are expected to be lower, which could discourage investment in productive assets. Second, the restriction may reduce investments in productivity-enhancing technology and capital that foreign ownership could otherwise have brought, as the foreign buyers who valued the land most highly may also have been the ones most likely to deploy advanced agricultural technologies.

Turning to the effects on productivity, Table 4 shows the results for the Callaway and Sant' Anna TFP models. The first column shows the No Foreign Ownership model and the second column shows the Acreage Restrictions model. The estimated effect for both models shows a slight positive increase at 0.0961 for No Foreign Ownership and 0.0173 for Acreage Restrictions. However, neither model is statistically significant.

Table 4. Effect of Foreign Ownership Restrictions on Agricultural Productivity

	No Foreign Ownership	Acreage Restrictions
Average Treatment Effect	0.0961 (0.0986)	0.0173 (0.0352)
Observations	2240	2296
States	40	41
Treated States	Iowa (1980), Minnesota (1977), Missouri (1978)	SD (1979), ND (1979), PA (1980), WI (1982)
Control Group	Never-Treated States	Never-Treated States

Table 4. (Continued)

	No Foreign Ownership	Acreage Restrictions
Covariates	None	None

Notes: Bootstrap standard errors clustered at state level in parentheses.
* p<0.10, ** p<0.05, *** p<0.01.
Overall ATT aggregated across all groups and time periods using Callaway and Sant'Anna (2021).
No Foreign Ownership treated states: Iowa (1980), Minnesota (1977), Missouri (1978).
Acreage Restrictions treated states: South Dakota (1979), North Dakota (1979),
Pennsylvania (1980), Wisconsin (1982).
Excluded states: Illinois, South Carolina, Mississippi, Nebraska.
Control group: never-treated states only. (Alaska and Hawaii not induced due to lack of data).

Finally, Table 5 shows the panel data results for productivity. As with the Callaway and Sant' Anna models, the coefficients indicate a positive effect. The Acreage restrictions model shows a statistically significant positive effect at the 0.10 level. R&D spillover shows a statistically significant positive effect, while growing season precipitation shows a statistically significant nonlinear effect.

Table 5. Panel Data Model: Effect of Both Restriction Types on Agricultural Productivity

	Combined
No Foreign Ownership	0.0515 (0.0589)
Acreage Restrictions	0.0547* (0.0321)
Temperature (Apr-Oct)	-0.01721 (0.04015)

Table 5. (Continued)

	Combined
Temperature Squared (Apr-Oct)	0.00016 (0.00033)
Precipitation (Apr-Oct)	0.07443*** (0.01952)
Precipitation Squared (Apr-Oct)	-0.00610** (0.00251)
Own R&D Stock	0.00005 (0.00003)
R&D Spillover Stock	0.00003*** (0.00001)
Mean Years Education	-0.00171 (0.01413)
Observations	2464
States	44
R-squared (within)	0.103
State Fixed Effects	Yes
Year Fixed Effects	Yes

Table 5. (Continued)

	Combined
Covariates	Weather v2 (quadratic), R&D, Education

Notes: Robust standard errors clustered at state level in parentheses.
* p<0.10, ** p<0.05, *** p<0.01.
Two-way fixed effects (state and year).
Covariates: growing season temperature (Apr-Oct) and its square,
growing season precipitation (Apr-Oct) and its square,
own R&D stock, R&D spillover stock, mean years education.
No Foreign Ownership treated states: Iowa (1980), Minnesota (1977), Missouri (1978).
Acreage Restrictions treated states: South Dakota (1979), North Dakota (1979),
Pennsylvania (1980), Wisconsin (1982).
Excluded states: Illinois, South Carolina, Mississippi, Nebraska.

Consistent with the literature, the models had little impact on TFP. Several factors could explain why productivity effects are weaker and less consistent than the effect on land prices. First, there is an important distinction between land ownership and land operation. That is, those who work the land are not always the same as those who own it. Because these laws do not directly affect farm inputs or operational decisions, productivity may be largely unaffected.

Another possible explanation is that restrictions affect land prices more quickly than productivity. Price movements in land markets reflect expectations of future profits. Market participants are likely to see the effects of restrictions on foreign investment on future profits immediately. They understand that these restrictions may impede the adoption of advanced technologies and future capital investment, and incorporate that expectation into the land's perceived value. Additional study may be needed to disentangle this puzzle.

Conclusion

This study examines the effects of state-level restrictions on foreign agricultural land ownership enacted in the late 1970s and early 1980s on land prices and agricultural productivity. Using a staggered difference-in-differences framework, the analysis compares states that enacted No Foreign Ownership restrictions and states that enacted Acreage Restrictions over the period from 1960 to 2015.

The results suggest that foreign ownership restrictions reduced agricultural land prices but did not affect agricultural productivity. No Foreign Ownership restrictions are associated with statistically significant reductions in land prices. Acreage Restrictions show a similar negative effect on land prices, but only in the Callaway and Sant'Anna models. In contrast, neither No Foreign Ownership restrictions nor Acreage Restrictions have any statistically significant effect on agricultural productivity, as measured by TFP, in the Callaway and Sant'Anna models. However, the panel data robustness check finds a slight positive and statistically significant effect on productivity for Acreage Restrictions states, but finds no statistically significant effect for No Foreign Ownership states. One possible explanation for why foreign ownership restrictions did not affect TFP is that land ownership and farm operation are often separate. A restriction on who can own the land does not necessarily change who works it. Since TFP is calculated from production inputs and labor, which could be determined by the farm operators rather than the owner, foreign ownership restrictions may not always affect productivity.

In contrast, prices tend to move more quickly and capture information about market dynamics. The drop in demand from a smaller-than-expected pool of buyers would immediately signal to the market that the asset is likely to generate lower future returns. This could then discourage future investment in that asset.

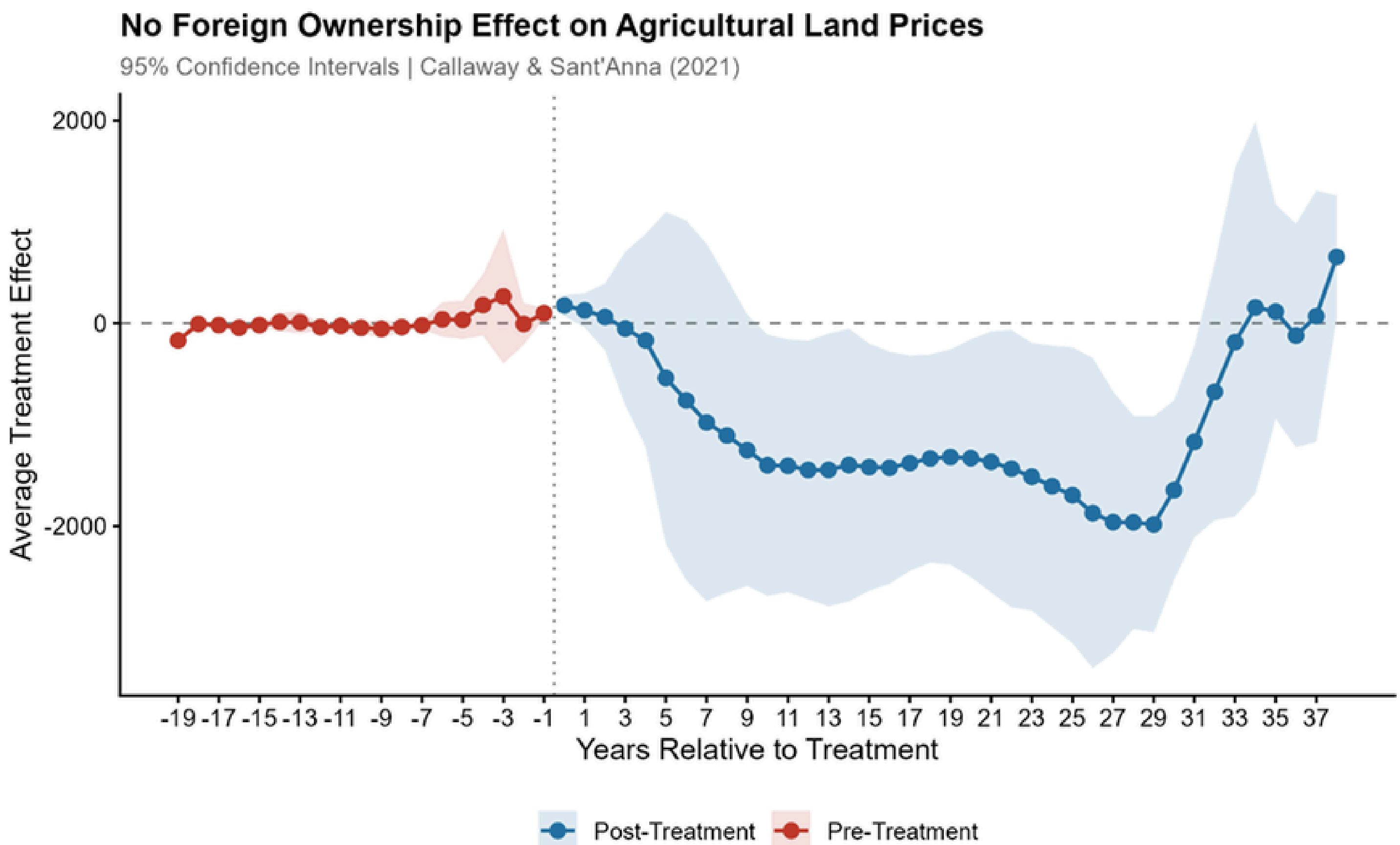
The results of this study have important implications for contemporary debates over foreign land ownership. While not targeting agricultural land specifically, several states have expanded foreign ownership restrictions in recent years, covering property types including agricultural land, natural resources, and general real estate. States that have expanded these restrictions or are considering doing so should consider the potential adverse economic impacts. By limiting who can purchase property, these laws may prevent the buyer who values the asset most from obtaining it, thereby preventing the property from being allocated to its most productive use. This could result in less investment in productivity-enhancing technologies and lower prices over time.

Furthermore, by reducing the pool of eligible buyers, these restrictions could put downward pressure on prices across several property markets. This price decline could signal to domestic owners that affected assets are expected to generate lower future returns. This could discourage further investment and lead to broader spillover effects such as fewer hires and lower overall economic activity. Policymakers considering similar restrictions will need to weigh these potential economic consequences against the national security concerns motivating these laws.

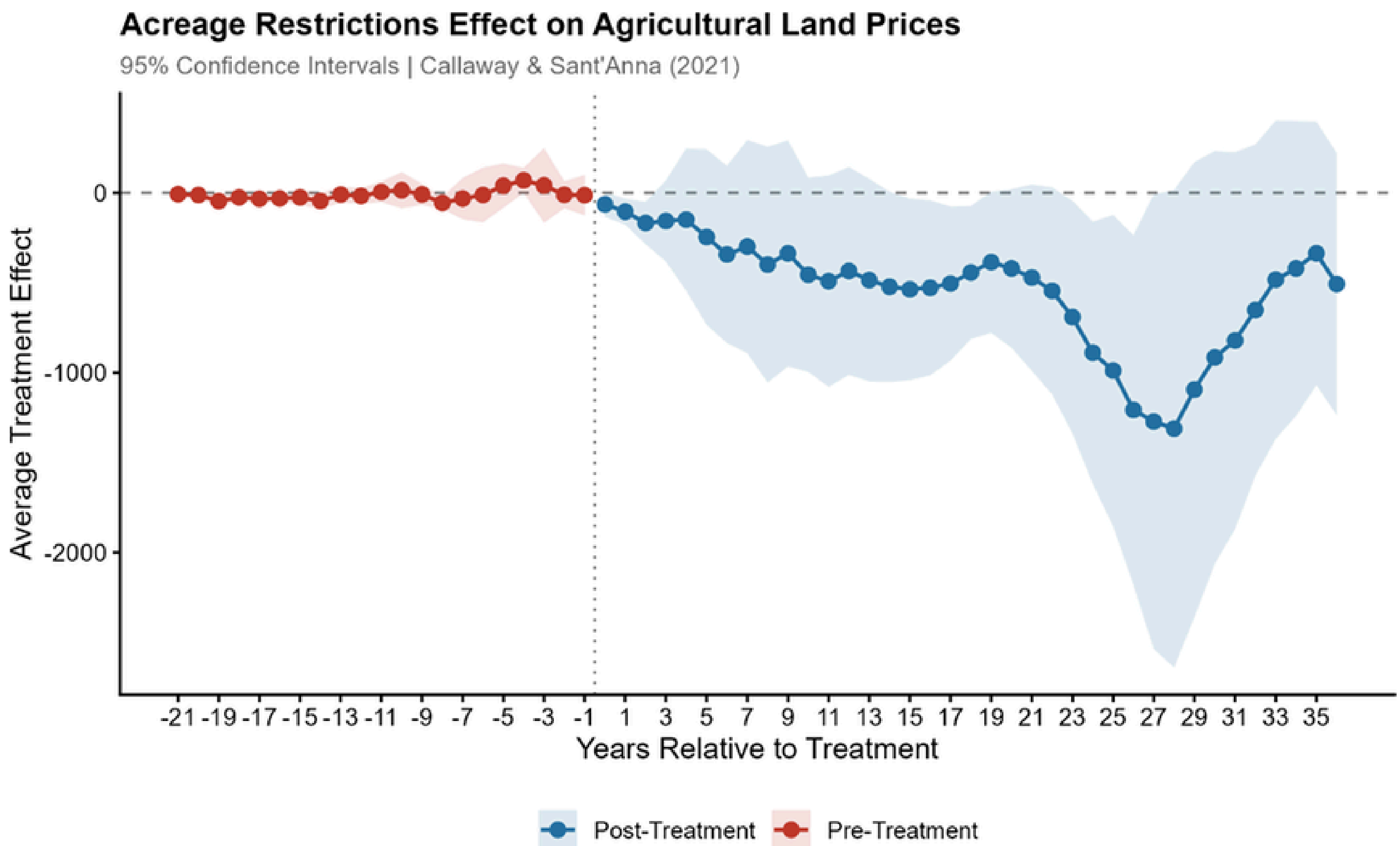
Several limitations should be noted. First, the analysis relies on state-level aggregated data, which may mask heterogeneous effects at more localized levels. Second, the study period (1960-2015) includes numerous concurrent policy and market changes that, while controlled for via fixed effects, may still influence the estimated effects. Second, the control group includes all states without foreign ownership restrictions, which encompasses considerable geographic and agricultural heterogeneity. Treated states are concentrated in the Midwest and Northeast US, while control states span diverse regions with different agricultural systems, climate conditions, and economic structures. Finally, as noted by Bigelow and Jodlowski, there are circumstances where the price reported by farmers differs from the actual market price. Future research should use observed market transaction prices rather than self-reported values where data availability permits.

Several avenues for future research emerge from this study. First, future work could employ more granular data at the county or farm level to examine spatial heterogeneity in policy effects. Second, researchers could investigate other mechanisms through which ownership restrictions affect land prices, such as changes in transaction volumes, buyer composition, or credit market access. Third, future studies could examine whether these price effects translated into broader economic impacts. For example, changes in farm income, agricultural employment, or rural economic development. Lastly, future studies could adjust the control group to make the treated and control states as comparable as possible.

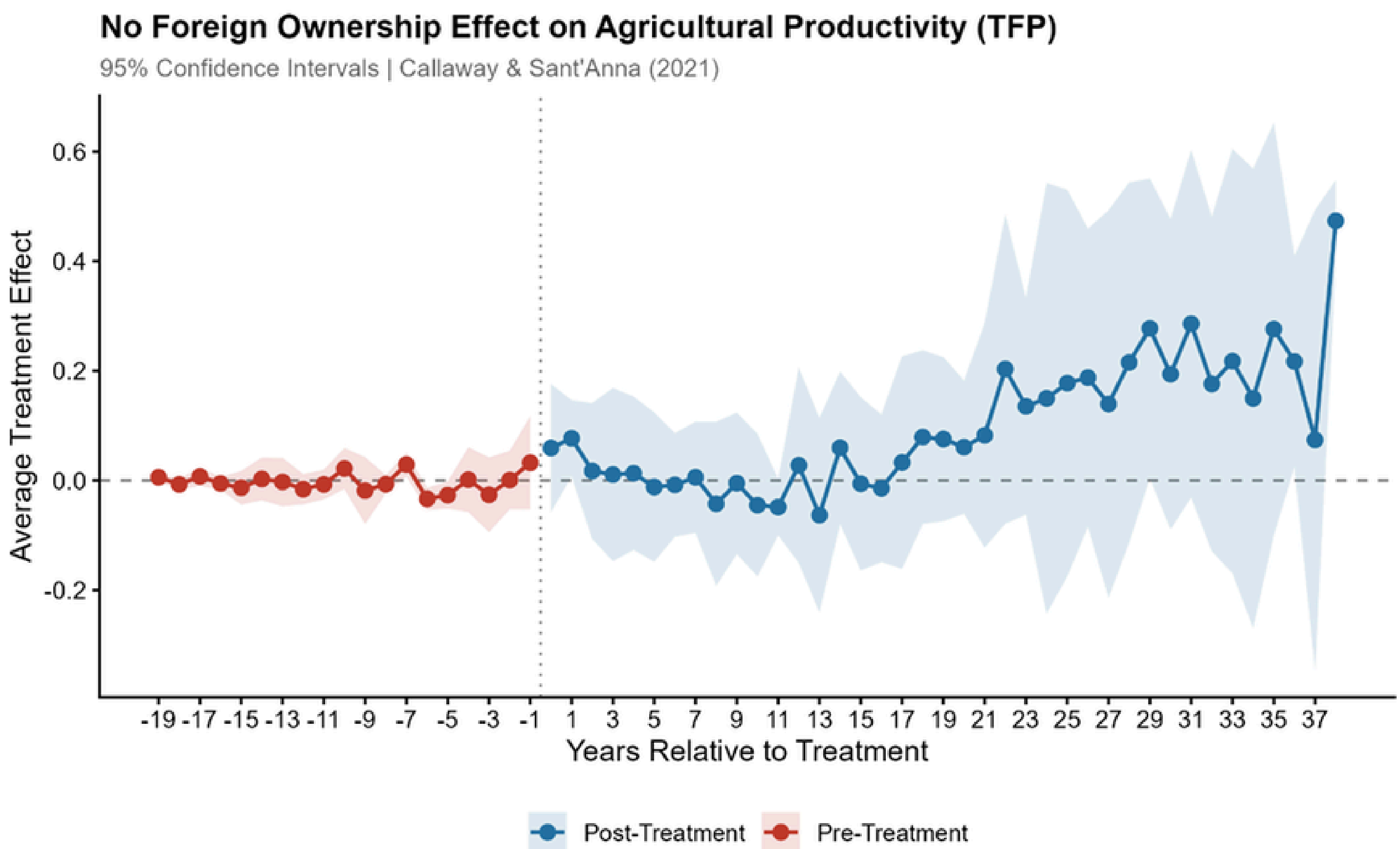
Appendix


No Foreign Ownership Effect on Agricultural Land Prices
95% Confidence Intervals | Callaway & Sant'Anna (2021)
Average Treatment Effect
2000
0
-2000
-19 -17 -15 -13 -11 -9 -7 -5 -3 -1 1 3 5 7 9 11 13 15 17 19 21 23 25 27 29 31 33 35 37
Years Relative to Treatment
Post-Treatment
Pre-Treatment

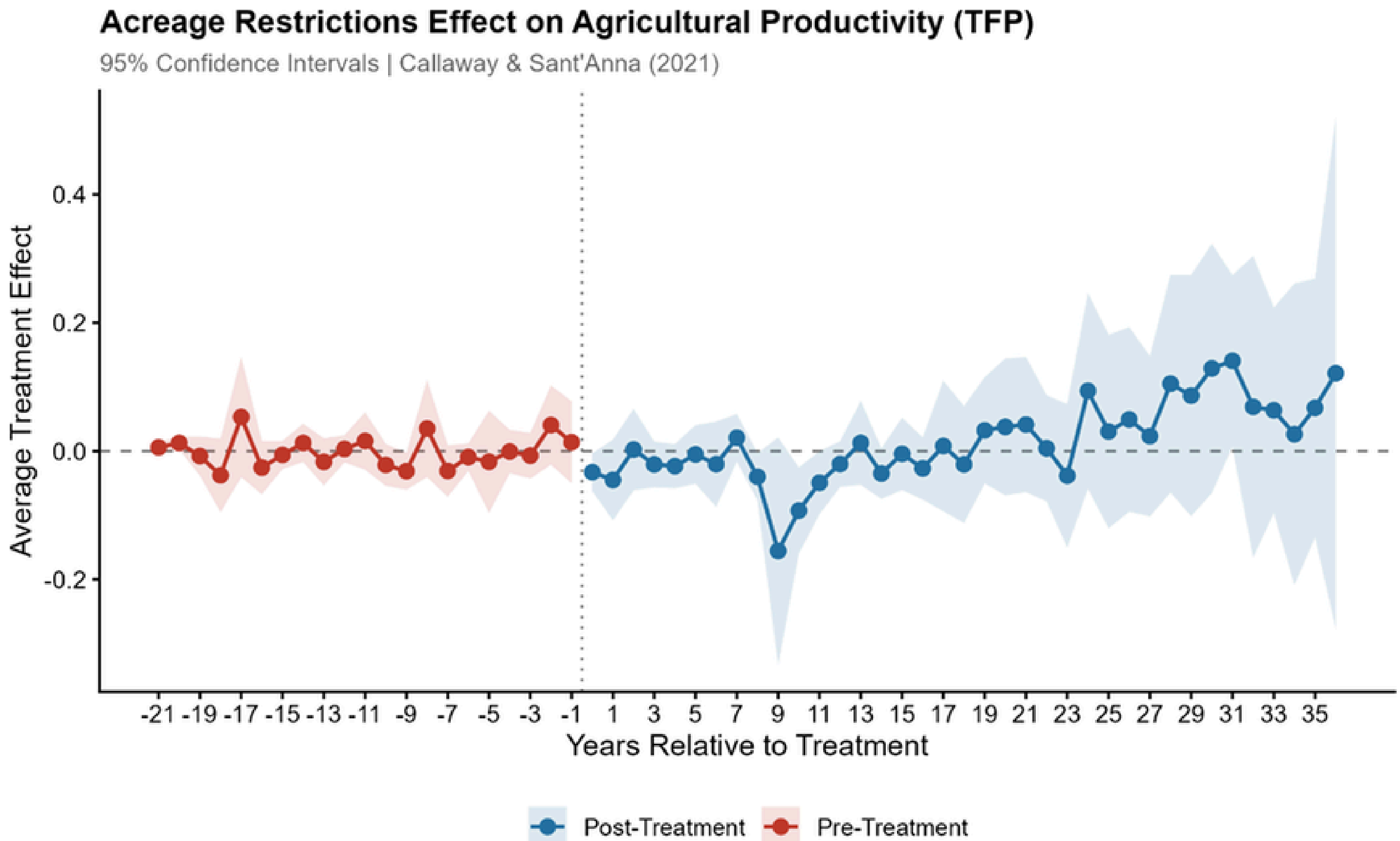
Appendix (continued)



Appendix (continued)



Appendix (continued)



All event study plots display pre-treatment estimates with 95% confidence intervals that cross zero throughout the pre-treatment period. In each of these plots, the estimated treatment effect at each point in time is represented by a line. Prior to treatment, this line remains close to zero, indicating no systematic difference between treated and control states before the restrictions were enacted. This provides supporting evidence for the parallel trends assumption. Post-treatment results for the land price models show negative effects following the enactment of restrictions. However, some time periods have confidence intervals that overlap with zero, reflecting greater uncertainty about the effect. Post-treatment estimates for both productivity models fluctuate around zero with wide confidence intervals. The No Foreign Ownership productivity model shows some evidence of a positive trend in later post-treatment periods, though confidence intervals cross zero throughout, reflecting considerable uncertainty.

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