Social Networks and Technology Adoption: Evidence from Church Mergers in the U.S. Midwest

Fiona Burlig and Andrew W. Stevens

Abstract: How do social networks impact technology adoption? Exploiting a natural experiment in the mid-twentieth century U.S. Upper Midwest, we find that social network expansions, in the form of mergers between congregations of the American Lutheran Church, led to increased rates of agricultural technology adoption among farmers. In counties that experienced a merger, the number of farms using chemical fertilizer increased by over 5 percent and the total fertilized acreage increased by over 10 percent relative to counties without a merger. These effects are consistent with increased information sharing between farmers due to congregational mergers.

Keywords: Agriculture, religion, social networks, technology adoption

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Running head: Church mergers and technology adoption

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Technology adoption is an essential component of economic growth (Hall and Khan 2003; Foster and Rosenzweig 2010; Perla and Tonetti 2014). In 2015 alone, the World Bank committed over eight billion dollars to projects encouraging people to adopt new technologies. Over the past decade, economists and policymakers have recognized that social networks can facilitate technology adoption. Information barriers hinder the take-up of new technologies and social networks can spread information and reduce these frictions. Understanding the ways in which these networks impact the take-up of new technologies is relevant for policymakers across the developed and developing world.

Economists face a fundamental challenge when trying to study social networks, since these networks are endogenously formed: people choose their own friends. Though there is a broad theoretical literature on social networks,[1] endogenous network formation poses a significant challenge for empirical research (Manski 1993; Goldsmith-Pinkham and Imbens 2013; Jackson 2014; Choi, Gallo, and Kariv 2016). In response to these difficulties, recent work in economics has relied on randomized experiments that act on or through existing social networks in field settings.[2] Other work uses detailed data on network structures to study how information moves within existing networks.[3] These papers represent a major development in our understanding of how information is transmitted through social networks, but they are unable to analyze how naturally arising changes in these networks affect economic activity.

We directly estimate the causal effects of increases in social network size and composition on technology adoption in the context of U.S. agriculture. We take advantage of a natural experiment to isolate exogenous shocks to social networks. These shocks take the form of mergers between rural congregations of the American Lutheran Church between 1959 and 1964 in the Upper Midwest of the United States. These mergers were prompted by factors outside of the control of individual congregations: national-level church mergers, church building fires, and pastoral employment constraints. Using county-level data from the American Census of Agriculture, we employ a difference-in-differences approach to study how these mergers affected farmers' adoption of chemical fertilizer—at the time, a relatively new technology.

We demonstrate that congregational mergers had an economically meaningful effect on technology adoption among farmers. The number of farms using chemical fertilizer increased by over 5 percent, and the total fertilized acreage increased by over 10 percent, in counties with merging congregations relative to those without. These increases were most pronounced on the region's major commercial crop: counties with mergers used 15 percent more fertilizer on corn. We utilize randomization inference methods and conduct multiple robustness checks to provide evidence that our results are caused by congregational mergers rather than other factors.

Our results are consistent with the hypothesis that information sharing is the primary mechanism through which social networks facilitate technology adoption. While we are unable to completely rule out all possible alternative mechanisms, our results point to the role of information as a potentially important driver. In particular, mergers only affected the use of fertilizer, a new technology, and its complements. In contrast, congregational mergers did not lead to increases in the use of existing, well-understood technologies. We also find no effects of mergers on durable goods with high fixed costs, meaning mergers did not seem to ease capital constraints. Taken together, our findings suggest information may have been an important mechanism driving the effects of congregational mergers on technology adoption.

Several papers have attempted to estimate the effects of plausibly exogenous shocks to existing social networks on economic outcomes. Most of these papers focus on how social networks affect labor market outcomes (Munshi 2003; Edin, Fredriksson, and Åslund 2003; and

Beaman 2012).[4] Though none of these papers studies technology adoption, there is a separate rich literature in economics studying the diffusion and take-up of new technologies, particularly in agricultural settings (Suri 2011; Maertens and Barrett 2012; Krishnan and Patnam 2013; Liu 2013; Karlan et al. 2014; Magnan et al. 2015; Mekonnen, Gerber, and Matz 2018; and Michler et al. 2019).[5] Our work is directly related to several recent papers which study the role of social networks in agricultural technology adoption.[6] Foster and Rosenzweig (1995) and Munshi (2004) study the network determinants of technology adoption during India's Green Revolution; Conley and Udry (2001; 2010) study farmer learning about fertilizer use and pineapple in Ghana; Vasilaky (2013) and Vasilaky and Leonard (2018) randomly connect women with agricultural extension agents, finding that this dramatically improves productivity; and Beaman et al. (2021) and BenYishay and Mobarak (2018) randomize which person within a social network receives information to study optimal targeting for information about agriculture.

Our work is also related to existing literature on the impacts of religion on economic activity. Bandiera and Rasul (2006) find that family and religious communities matter for technology adoption in Mozambique; Deller, Conroy, and Markeson (2018) show that religious organizations can increase small business activity in the U.S.; and Bryan, Choi, and Karlan (2021) show that exposure to Protestant evangelism can increase household income in the Philippines.[7]

The paper most closely related to ours is Murphy, Nourani, and Lee (2022). These authors show that in Kenyan villages, joint attendance at a religious institution increases the probability that individuals share information with one another. Our work differs from theirs in several important ways. First, we document a similar pattern in a very different context (the U.S. Upper Midwest in the 1960s). Second, although we are unable to directly observe information sharing between peers as Murphy, Nourani, and Lee (2022) do, we can observe what individuals do with the information they share with each other. In this sense, we analyze a longer causal chain of economic behavior than Murphy, Nourani, and Lee (2022). [8]

The remainder of this paper is organized as follows: we first describe our context in more detail. We then detail our data and describe our empirical strategy. Next, we present our results and robustness checks. Finally, we conclude.

Context

We study the effects of social networks on the adoption of a new technology in the Upper Midwest of the United States during the 1950s and 1960s: chemical fertilizer.[9] Between 1940 and 1970, the use of chemical fertilizers increased dramatically. Figure 1 displays the sharp increase in usage of chemical fertilizer for corn production in the United States.

[Figure 1 about here]

Between 1940 and 1949, average annual consumption of chemical fertilizer in the United States was 13.6 million tons; between 1950 and 1959, this number rose to 22.3 million tons; and between 1960 and 1969, use had increased further to 32.4 million tons (Campbell, Campbell, and Hughes 2004).[10] This increase in usage had tangible results: between 1950 and 1975, agricultural productivity in the United States increased faster than ever before or since (Trautmann, Porter, and Wagenet 1998). In 1950, the average American farmer supplied the materials to feed and clothe 14 people; by 1960, the average farmer was sustaining 26 people (Rogers 1995).

Although today, over ninety-five percent of corn acres are fertilized (U.S. Department of Agriculture 2016) and fertilizer is well-known to increase yields, during the 1950s and 1960s farmers were far from being fully informed about optimal fertilizer usage and its benefits. Communication between farmers in different social circles was infrequent (Salamon 1992;

Amato and Amato 2000; Cotter and Jackson 2001), but information sharing within farmers' social networks was a major means of spreading professional knowledge. Religion was an important driver of farmers' social connections (Lazerwitz 1961; Azzi and Ehrenberg 1975; Swierenga 1997; Cotter and Jackson 2001) and the Upper Midwest had a high rate of religious adherence: according to the Association of Religion Data Archives, in 1952, 64 percent, 62 percent, 58 percent, and 61 percent of the population of Minnesota, North Dakota, South Dakota, and Wisconsin, respectively, were religious. We focus our analysis on these four states because they contained large Lutheran populations: 51 percent, 48 percent, 33 percent, and 37 percent of religious Minnesotans, North Dakotans, South Dakotans, and Wisconsinites belonged to a Lutheran church. Furthermore, these states had relatively similar agriculture in the 1950s and 1960s. The states with the next-highest proportion of Lutherans, Nebraska and Iowa, were less agronomically comparable to their Northern neighbors.

Figure 2 demonstrates the prevalence of religion in the United States in the 1950s, as well as the concentration of Lutheranism in Minnesota, North Dakota, South Dakota, and Wisconsin.[11]

[Figure 2 about here]

Church and Congregational Mergers

In the 1950s and 1960s, national Lutheran church bodies underwent significant institutional consolidation. At an April 1960 meeting in Minneapolis, Minnesota, three of the largest national Lutheran church bodies – the American Lutheran Church (ALC), the United Evangelical Lutheran Church (UELC), and the Evangelical Lutheran Church (ELC) – voted to merge and form The American Lutheran Church (TALC). This merger officially took effect on January 1, 1961.

A similar merger between the United Lutheran Church in America, the Finnish Evangelical Lutheran Church of America, the American Evangelical Lutheran Church, and the Augustana Evangelical Lutheran Church created the Lutheran Church in America (LCA) in 1962. In 1963, the Lutheran Free Church (LFC), composed largely of congregations that originally opted out of the 1960 TALC merger on theological grounds, decided to join TALC as well, extending the scope of this major Lutheran branch (Wolf 1966).[12]

Figure 3 depicts the major mergers between Lutheran church bodies in the United States since the 1950s. For historical context, we focus primarily on TALC for two reasons. First, congregations of TALC were geographically clustered in the Upper Midwest whereas congregations of the LCA were more dispersed throughout the country. Second, we have access to yearbooks from TALC detailing a handful of congregational-level statistics throughout the 1960s.

[Figure 3 about here]

National-level mergers, arranged by the constituent churches' theological and institutional leadership, had far-reaching impacts. The TALC merger was reported in local newspapers across the Upper Midwest (Johnston 1960; Dugan 1960; Press 1960) and national mergers forced local congregations to adopt new constitutions, bringing them into alignment with the newly formed national church (Nelson 1975). Prior to the national mergers, many towns had congregations from multiple church branches. As a result of the national mergers, these congregations suddenly found themselves in the same denomination. This frequently led to mergers between local congregations that were previously impossible (Trinity Lutheran Church 2012; United Lutheran Church Laurel 2013). These mergers brought previously socially disparate groups of people into contact with one another.

Each of the merging national-level church bodies (and their associated congregations) were linked to a different ethnic group: the ALC had German roots, the ELC had a Norwegian background, and the UELC was historically Danish (St. John Evangelical Lutheran Congregation 2014). Especially in the early parts of the twentieth century, this often meant that congregations across the street from one another were holding services in different languages. Some congregations were even conducting multiple services, each in a different language (Bethel Lutheran Northfield Church 2014; Murray County 2014).[13] Cross-branch mergers between local congregations were large shocks to churchgoers' social networks, since the congregants were not likely to have interacted nearly as frequently prior to the merger.

In addition to the local mergers that were precipitated by national church changes, a number of congregational mergers resulted from other plausibly random events. Several congregations initiated mergers after natural disasters destroyed congregation buildings (Bethlehem Lutheran Church 2014; St. Mark's Lutheran Church 2014). Other congregations merged due to difficulties hiring full-time clergy. Pastors, trained in centralized seminary programs, were a scarce and expensive resource, occasionally serving multiple congregations at once. Pastors in these roles frequently pressured their congregations to consolidate resources and merge into a single entity (Thoreson 2013; Grace Lutheran Church 2014).[14]

In our data, we do not have definitive information about the proximate cause of each congregational merger. Therefore, we are unable to distinguish mergers resulting from national-level branch mergers from mergers following a church fire, for example. However, as we have argued above, the historical record suggests all these congregational mergers occurred for reasons that were orthogonal to agricultural fertilizer use. In Table 1 and Figure 5, described in further detail below, we present evidence that counties with merging congregations are statistically similar to counties without merging congregations along a variety of dimensions both in the pre-merger cross-section and in pre-merger trends.

Even conditional on mergers being plausibly exogenous, whether these mergers led to increased information diffusion and technology adoption is an empirical question. If the new congregations encouraged cross-group discussion, social integration, and trust, this likely led to technology transfer. However, it is also possible that mergers did not facilitate social integration—for example if members who previously belonged to separate congregations attended separate services post-merger, or if this increased antagonism between groups. As a result, we use data to empirically test the extent to which mergers drove technology adoption.

Data

We use data on churches and agriculture to estimate the effects of social network expansions on technology adoption. Specifically, we use data from 262 counties in Minnesota, North Dakota, South Dakota, and Wisconsin. We exclude the 16 counties that included a Standard Metropolitan Statistical Area, as defined by the 1960 Census, to restrict our sample to agricultural regions.

Church Data

We acquired a dataset on mergers between Lutheran congregations from the archives of the ELCA. Church archivists compiled a database of mergers between Lutheran congregations, the earliest of which occurred in 1810.[15] These data are derived from two main sources: annual national church yearbooks, which in turn were compiled from reports congregations made to their governing bodies; and The American Lutheran Church (1960), a record of all the details surrounding the TALC merger. This dataset includes all mergers between congregations of the UELC, ALC, and ELC, which merged to form TALC in 1960, including mergers between congregations which no longer exist. For each congregation involved in a merger, the dataset records the state, county, local post office, location[16], synod[17], congregation name, founding date, merger date, details on which other congregations were involved in the merger, and additional historical notes. Ideally, these data would also include information on congregational demographic composition. Unfortunately, such variables were not recorded and we are unable to explore how merger effects might differ by congregants' average age or ethnic background.

In our main analysis, we analyze the mergers that took place between 1959 and 1964.[18] A total of 56 mergers occurred during this time in Minnesota, North Dakota, South Dakota, and Wisconsin. In our main study period, between 1959 and 1964, 47 counties experienced one merger, three counties experienced two mergers, and one county experienced three mergers. We consider counties that experienced at least one merger between 1959 and 1964 to be treated, and counties that did not experience a merger during this time to be untreated. Figure 4 displays the spatial distribution of treated and untreated counties. The counties in blue experienced at least one merger between 1959 and 1964; the counties in white did not; and the counties in gray included a major urban area and were excluded from the sample.

[Figure 4 about here]

We restrict our analysis to Minnesota, North Dakota, South Dakota, and Wisconsin for two main reasons. First, as figure 2 shows, these are the states where Lutheranism is most prevalent. We believe Lutheran congregational mergers are likely to be more economically relevant in areas where Lutheranism is a prevalent or predominant religion. Second, these four states are fairly geographically and agronomically similar, meaning that farmers are likely to have adopted fertilizer for use in similar cropping systems. There is a steep drop-off in the prevalence of Lutheranism between our four chosen states and the next-most-Lutheran states: Nebraska and

Iowa. Additionally, agricultural practices and crop choices in the 1960s in Nebraska and Iowa differed in meaningful ways from Minnesota, Wisconsin, and the Dakotas.

Agriculture Data

We combine our data on merging congregations with data from the United States Department of Agriculture (USDA)'s Census of Agriculture. In the 1950s and 1960s, the Census was designed to have full coverage of every farm in the U.S. (U.S. Census Bureau 1967). Censuses were taken every five years, and data gathering took place in the fall. Enumerators visited every dwelling and administered the Census to any household engaged in agriculture. After collection, the Census underwent a multi-stage quality control process. The final dataset is available at the county level. Wherever possible, we use a digitized version of the dataset maintained by the University of Michigan's Inter-university Consortium for Political and Social Research (Haines, Fishback, and Rhode 2015). Some variables were unavailable in the digitized data; we hand coded these from PDFs made available from the USDA's own archive.[19]

We use the data from the Census of Agriculture from 1945, 1950, 1954, 1959, and 1964. Our main analysis uses the 1959 and 1964 Censuses due to data limitations with earlier and later data. In particular, the 1950 and 1945 censuses did not include county-level information on fertilizer use. We are unable to use later waves of data, because after the 1964 wave, data was only collected for farms selling over $2,500 worth of goods per year, and there is no way to reconcile the two sampling frames. We use the 1954 Census to test for differential trends in fertilizer use among counties with and without congregational mergers. Though the 1950 and 1945 Censuses did not include information on fertilizer, we can and do use data from these datasets to analyze pre-merger trends in other variables of interest. Having performed these tests for parallel trends using the earlier data, we perform our main analyses using the 1959 and 1964

Censuses. We combine the Census of Agriculture data from these years with our congregational data to create a balanced panel of 262 counties.

The Census of Agriculture contains data on our main outcomes of interest: the number of farms using fertilizer, acres fertilized, tons of chemical fertilizer used, corn acres fertilized, and tons of dry and liquid fertilizer used on corn. It also contains information on the use of agricultural lime, a complement to chemical fertilizer. In addition, the Census includes data on other agricultural practices, such as strip cropping and irrigation; other types of land use, such as orchards; and capital-intensive farm durables, including vehicles.

Other Data

In addition to the data described above, we also use data on historical weather including average annual precipitation, average annual temperature, heating degree days, and cooling degree days, aggregated to the climatic division level, all sourced from the National Oceanic and Atmospheric Administration (NOAA 2007). We also use county-year-level data on open 2-digit highways sourced from Baum-Snow (2007). Finally, we obtain basic county-level demographic data in 1950, 1960, and 1970 from the U.S. Census Bureau.

Summary Statistics

Table 1 presents summary statistics from the 1954 Census of Agriculture and several Population Censuses for counties that did and did not experience congregational mergers between 1959 and 1964. Although there are some apparent differences between treated and untreated counties (treated counties tend to be more rural and agriculturally focused), few of these differences are statistically significant. The major exceptions are in harvested acreage and miles of highway: treatment counties harvested approximately 57,500 more acres and had 0.02 more miles of open 2-digit highway than control counties in 1954. These differences are statistically significant at the five percent level.

[Table 1 about here]

Overall, table 1 reveals that treatment and control counties are reasonably similar to one another prior to the congregational mergers that took place between 1959 and 1964. These statistics support the notion that mergers were not driven by the agricultural sector or other potentially endogenous factors. Additionally, our preferred empirical specification includes county fixed effects that will account for any time-invariant discrepancies between treated and control counties.

In addition to assessing balance in the cross-section, we can also examine the extent to which outcomes in treated and untreated counties were trending differentially over time. Figure 5 presents graphical evidence on pre-merger trends for six variables: the number of farms using fertilizer, the number of farms using lime, irrigated acreage, acres in orchards, harvested acreage, and the number of farms with trucks over the full time series for which we observe each variable.

[Figure 5 about here]

Across all these variables, we find counties that experienced a congregational merger between 1959 and 1964 were trending similarly prior to 1959. This suggests that we can compare counties with and without mergers before and after these mergers took place to recover causal estimates of the effects of mergers on agricultural outcomes.

As one such comparison using the raw data, figure 6 reports kernel density plots of the change in farms using fertilizer between 1959 and 1964 for counties that did and did not experience a congregational merger. On average, counties without a merger saw a reduction of 21 farms using fertilizer, and counties with a merger saw an increase of seven farms. While figure 6 seems to tell a compelling story that congregational mergers increased the number of farms using fertilizer, it does not control for other factors that may affect fertilizer use. To capture these possible confounding factors, our empirical strategy employs a difference-in-differences approach and controls for county and state-by-year fixed effects.

[Figure 6 about here]

Empirical Strategy

We employ a differences-in-differences approach to test our hypothesis that congregational mergers increased fertilizer use. We use data from 1959 and 1964 to estimate the following specification:

$$y_{cst} = \beta(\mathbf{1}[Merger]_{cs} \times \mathbf{1}[Year = 1964]_t) + \mu_{cs} + \gamma_{st} + \varepsilon_{cst} \quad (1)$$

where y_{cst} is an outcome of interest in county c belonging to state s in year t, $\mathbf{1}[Merger]_{cs}$ is equal to one if county c experienced a merger between 1959 and 1964, $\mathbf{1}[Year = 1964]_t$ is equal to one if the year is 1964, μ_{cs} are county fixed effects, γ_{st} are state-by-year fixed effects, and ε_{cst} is an idiosyncratic error term. The coefficient of interest is β, which captures the effect of congregational mergers on outcome y. We cluster our standard errors at the county level to allow for arbitrary error dependence over time between observations in the same county.[20]

We hypothesize that congregational mergers should increase the adoption rate of new technologies through increased information sharing. In our empirical context, this suggests that mergers should lead to increased adoption of chemical fertilizer. We test for adoption along the extensive margin by estimating equation 1 with the number of farms using fertilizer as the outcome of interest. We also test for effects of mergers on the number of acres fertilized and tons of fertilizer applied, which capture both intensive and extensive margin effects.

We also expect the use of agricultural lime to increase with congregational mergers. Nitrogen, the primary component of many chemical fertilizers, adds acidity to soil, which can impede crop growth. Agricultural lime helps to counteract this process, making it a natural complement to fertilizer use. We test for these effects by estimating equation 1 using the number of farms using lime, the number of acres limed, and tons of lime used as outcomes.

We also test for the effects of congregational mergers on fertilizer use on corn, which benefits greatly from the use of fertilizer (Barber and Stivers 1962) and is one of the region's major commercial crops.[21] We expect congregational mergers to increase total fertilizer use on corn. The Census of Agriculture distinguishes between dry and liquid fertilizer used on corn. We expect to find stronger effects on dry fertilizer, since the major technological advances of the time occurred in dry, rather than liquid, fertilizers (Russel and Williams 1977; Young and Hargett 1984).

If congregational mergers affect technology adoption through information sharing, then we hypothesize congregational mergers should not affect adoption of technologies about which all farmers are already informed. In the Upper Midwest in the 1950s, we can test this theory using three established technologies: strip cropping, irrigation, and orchards. Strip cropping, in which farmers alternate crop types in tight rows to prevent soil erosion, has been used for nearly a century in the United States. It was introduced to Minnesota in the early 1930s (Helms et al. 1996) and was in use throughout the region by 1940 (Granger and Kelly 2005). Irrigation was another well-known technology: the most common irrigation system in use in this area was the center pivot system, which had spread to farmers by the late 1950s (Kenney 1995; Granger and Kelly 2005). Finally, using land for orchards, vineyards, groves, and nut trees was an established—if not common—practice in the Midwest by the 1950s (Gordon 1997; Burrows 2010; Smith 2011). We hypothesize that, since farmers were informed about these practices prior to our study period, strip cropping, irrigation, and orchard lands should not be affected by congregational mergers.

Our difference-in-differences approach relies on the parallel trends identifying assumption that $E[\varepsilon_{cst}|(\mathbf{1}[Merger]_{cs} \times \mathbf{1}[Year = 1964]_t),\mu_{cs},\gamma_{st}] = 0$, or that there are no time-varying unobserved factors that are different between counties with and without mergers. We believe this assumption is reasonable: as discussed in the previous context section, exogenous factors including national-level church branch mergers and building fires prompted the congregational mergers we study. Although we fundamentally cannot empirically test our identifying assumption, we provide evidence in support of it in several ways. First, as discussed in our data section, Table 1 shows reasonable balance between counties that did and did not experience a congregational merger in the cross-section prior to treatment. Secondly, Figure 5 demonstrates that in harvested acreage, the number of farms with trucks, irrigated acres, and acres in orchards, counties with mergers were trending similarly to counties without mergers over the fifteen years prior to treatment.

In Table 2, we estimate equation 1 using data from 1954 and 1959 for four of our main outcomes of interest: the number of farms using fertilizer, acres fertilized, the number of farms using lime, and acres limed. Our "post" period, 1959, is before our congregational mergers, so we should expect to find no statistically significant effects of mergers on our outcomes of interest.

[Table 2 about here]

In all cases, we fail to reject the null hypothesis that counties with mergers are trending similarly to counties without mergers prior to treatment. Figure 5 demonstrates this graphically, showing that, from 1959 to 1959, counties that experienced mergers were on a similar path to counties that did not. It was only after 1959, when our mergers occurred, that the groups of counties began to diverge. This evidence supports our empirical approach.

There may also be concerns about the stable unit treatment value assumption, that is, that the treatment status of county *c* will not affect the outcome in any other county. We believe that this assumption is plausible in our empirical context since none of the mergers in our data cross county boundaries. Furthermore, any spillovers from merging to non-merging counties are likely to be positive, which would attenuate our treatment effects: if a merger in county *i* leads to an increase in fertilizer use in (untreated) county *j*, our empirical approach will understate the true effect of congregational mergers.[22] It is also possible that information spillovers between counties take time. Nevertheless, any information spillovers which lead to more technology adoption in untreated counties will continue to lead us to underestimate the true treatment effect. Moreover, we believe that this concern is mitigated by the fact that our main outcome data is measured only once every five years. Our estimates are therefore lower bounds on the true effects of networks on technology adoption. Finally, we measure the Moran's I statistic for five key variables (the number of farms using fertilizer, fertilized acres, tons of fertilizer, fertilized corn acres, and tons of fertilizer used on corn), and find very little change between 1959 and 1964, suggesting that mergers are not increasing spatial autocorrelation in these variables.

Results

We organize our results into two sections. First, we present our main results and describe a randomization inference procedure we apply to all our analyses. Second, we present a series of robustness checks including (1) analyses of alternative dependent variables, (2) placebo tests, and (3) explorations of alternative explanations.

Main Results

We first estimate the effects of congregational mergers on fertilizer use on the extensive margin, using the number of farms using fertilizer as the dependent variable. We estimate five specifications, each with a different set of controls. Table 3 reports the results. Column (1) is the most parsimonious specification, including only the interaction term of interest (Year = 1964 × merger), a 1964 dummy, and a "merger county" dummy. In Column (2), we replace the "merger county" dummy with county fixed effects. Column (3) adds four weather controls: temperature, precipitation, heating degree days, and cooling degree days.[23] In order to control for time-varying unobservables, we include state-by-year fixed effects in place of the weather controls in Column (4). In Column (5), we include both state-by-year fixed effects and weather controls. This is our preferred specification. Finally, in Column (6) we re-estimate the specification in Column (5) without observations from Wisconsin, which is the least Lutheran and most agronomically dissimilar of the four states.

[Table 3 about here]

The results in Table 3 are consistent with our main hypothesis: as expected, counties that experienced congregational mergers see higher rates of fertilizer adoption than those that did not. These effects are economically meaningful and relatively stable across specifications. Given our limited sample size, we are somewhat underpowered. Across specifications, we find positive—and economically meaningful—point estimates, ranging from 28.43 to 37.73 additional farms adopting fertilizer as a result of congregational mergers. In Columns (4) and (5), these estimates are statistically significant at the ten percent level based on traditional standard errors clustered at the county level. With the inclusion of weather controls in Column (5), our preferred specification, we find that congregational mergers caused 36.09 additional farms per county to begin using fertilizer, an increase of 5 percent over the mean in the control group. These results are statistically significant at the 10 percent level, which, given our relatively small sample size, is encouraging. When we drop Wisconsin, the result attenuates slightly, to 33 additional farms (on a smaller mean of 552), and we lose statistical significance, though this is not surprising considering the smaller sample.

Given our relatively small sample size, traditional inference that relies on asymptotic theory may be inefficient relative to nonparametric approaches such as bootstrapping or randomization inference (Horowitz, 2019). Therefore, we supplement traditional inference methods with a randomization inference procedure. We randomly reassign exactly 51 counties to treatment 5,000 times. For each iteration, we estimate every specification in Table 3 and store the estimated coefficient $\hat{\beta}$. We display the results of this procedure in Figure 7. The gray histograms show coefficients from these 5,000 random draws and the blue lines denote the treatment effect using the real assignment vector. In each case, the real effect lies in the far-right portion of the distribution—and in our preferred specification, lies above the 97th percentile—which suggests that our results are not an artifact of random chance. This randomization inference exercise also provides exact p-values that are valid for small samples. We report these “randomization inference p-values” in brackets in all our results tables and prefer this approach to inference in our small sample setting. Using this method, the p-value for our preferred specification in Table 3 is 0.029.

[Figure 7 about here]

Since we observe each merged congregation’s size (membership), it is natural to explore whether mergers have heterogeneous effects by congregation size: Do larger mergers have larger effects on technology adoption than smaller mergers? Unfortunately, our sample size is too small to answer this question: only 51 counties experienced a congregational merger at all between 1959 and 1964. Every specification we estimate that includes a heterogeneous treatment effect fails to yield any statistically significant treatment effects whatsoever. Therefore, we instead focus on mergers' average treatment effects.

Robustness Checks

We conduct a series of robustness checks to complement our primary analysis. These include analyzing alternative dependent variables, conducting placebo tests, and exploring alternative explanations.

Alternative dependent variables

First, we explore the impact of congregational mergers on alternative dependent variables to ensure our findings are not unique to the raw number of farms using fertilizer. In Table 4, we estimate our preferred specification using acres fertilized and tonnage of fertilizer applied as dependent variables. We also report effects on the number of farms using agricultural lime, acres limed, and the tons of lime applied. Since lime is a complement to fertilizer, we expect to find positive effects of congregational mergers on lime use.

[Table 4 about here]

Table 4 shows that, as expected, congregational mergers increased acres fertilized. Counties with mergers fertilized, on average, 6,133 acres more than counties without mergers, a 10 percent increase over the control group mean, and statistically significant at the one percent level using randomization inference. We do not find a corresponding increase in the tonnage of fertilizer applied on all crops, though this is likely driven in part by noise involved in measuring the tonnage of fertilizer used.[24] We do find the expected positive effects of congregational mergers on the number of farms using lime: 21 additional farms use lime in the treatment group relative to the control group, a large increase of 17.6 percent, statistically significant at the one percent level using randomization inference. We find corresponding increases in the number of acres limed and the tons of lime used (though the latter is not statistically significant), with acreage limed increasing by over 16 percent, and tons of lime used increasing by close to 17 percent. Taken together, these results suggest that congregational mergers led to an economically meaningful and statistically significant increase in fertilizer and lime use, as predicted.

We also look specifically at corn. In the Census of Agriculture data, there is information about the tonnage of both wet and dry chemical fertilizer applied for corn, so we can separate mergers' impact on these two different forms. In addition, the Census data contain information on fertilized acreage. Results are reported in Table A1 of the online appendix. We find that fertilizer used on corn increases due to congregational mergers. Acreage fertilized increases by 3,599, a change of 15 percent. This is statistically significant at the one percent level using randomization inference. We also find that there is an increase in the tonnage of fertilizer used on corn, and that this increase is driven by dry fertilizer use. We find an increase of 271.63 tons of dry fertilizer, statistically significant at the one percent level using randomization inference, and no statistically significant increase in the tonnage of wet fertilizer applied. The total tonnage of fertilizer applied to corn increases by 325.30 tons, statistically significant at the one percent level using randomization inference. The impact of congregational mergers on corn fertilizer use is not only statistically significant, but also represents an economically meaningful change.

Next, we re-estimate the specifications in Table 3 using the proportion of farms in a county that report using fertilizer rather than the raw number of farms. Table A2 of the online appendix contains the results, which tell a similar story to our main results. Specifically, we find that in counties with a congregational merger, the proportion of farms that use fertilizer is two percentage points higher than in counties without a merger. Unsurprisingly, these results are less precisely estimated than those in Table 3, but the estimate from our preferred specification is still statistically significant at the ten percent level using randomization inference.

In sum, the information in Tables 3 and 4 and Tables A1 and A2 of the online appendix demonstrates that congregational mergers increase fertilizer (and lime) use, as expected.

Placebo tests

We conduct two different placebo tests to provide further evidence our main findings are not spurious. First, we analyze what we call "placebo outcomes." That is, we estimate the effect of congregational mergers on agricultural practices that were already well established by the 1960s. Specifically, we analyze the effect of mergers on the number of farms using strip cropping and the acres under strip cropping; the number of farms reporting irrigation use and the total number of irrigated acres; and the total number of acres in fruit orchards, groves, vineyards, and nut trees. None of these practices were new during the merger period, and thus we hypothesize mergers should have no impact on their adoption. The results of this analysis are reported in Table A3 of the online appendix: We see no statistically significant impacts of congregational mergers on strip cropping, irrigation, or orchard acreage. Furthermore, the magnitudes of the coefficients we do see are relatively small, and in the case of irrigated farms and orchard acreage, have negative signs. The absence of results in this table further supports the conclusion that congregational mergers are driving the changes in input uses observed earlier. Specifically, we only observe mergers having an effect on farmers' use of new technologies, and not on their use of established technologies.

Second, we analyze a "placebo treatment" where we re-estimate our preferred specifications from Tables 3 and 4 but use mergers between 1964 and 1967 instead of mergers between 1959 and 1964 as our definition of treatment. We do this to see whether mergers that occurred after

1964 impact our outcomes in 1964, before these mergers actually occurred. We use the time period 1964 to 1969 because it includes the same number of years as our actual treatment period. Table A4 of the online appendix reports our results and shows that there is no statistically significant effect of future congregational mergers on 1964 input outcomes. In addition, comparing these effects to those in Tables 3, 4, and 5, the magnitudes of the coefficients are quite small. This helps confirm that the effects we observed earlier are real and driven by congregational mergers, rather than by something unobserved.[25]

Alternative explanations

Despite the evidence presented above, it is still possible that our findings are the result of something other than a congregational-merger driven information effect. Here, we explore several other possible explanations for our results. The first is the presence of agricultural extension. Agricultural extension, formally introduced in the United States by the Smith-Lever Act of 1914, plays a major role in information dissemination in agriculture. There is a large literature on the effect of agricultural extension, both in the United States and elsewhere, on agricultural productivity and technology adoption (Huffman 1974; Huffman 1977; Birkhaeuser, Evenson, and Feder 1991; Dercon et al. 2009). Despite the importance of extension, we argue that it is in fact congregational mergers and not extension services that generate the results we find: because of the fixed effects strategy, in order for agricultural extension to be driving these results, we would need to see agricultural extension services changing differently over time in treatment counties than in control counties, having removed the state time trend, only over the 1959 to 1964 time period. This is potentially plausible, but seems unlikely, especially because extension funding and the number of extension agents allowed is governed by state laws, which do not change often. For example, the Minnesota statutes outlining extension were first passed in

1923, updated in 1953, and were not revised again until 1969 (Minnesota Legislature 2013). The law allows for "the formation of one county corporation in each county in [Minnesota]" to act as an extension agency, with in most cases one extension agent and a specified budget, based on the number of townships in the county (Minnesota Legislature 1953). While county extension offices documented their activities for mandatory state reports, these reports were inconsistent across different counties and years. Also, many of the variables measured were endogenous, such as the number of phone calls received or the number of attendees at extension events. As a result, it is impossible to credibly measure the intensity and efficacy of extension efforts over our sample period.[26]

We argue that congregational mergers most likely impact fertilizer use through information sharing. Another plausible explanation would be that mergers also facilitated increased access to capital. To provide evidence against this possibility, we estimate equation (1) again, this time with the number of farms with each of a variety of capital-intensive technologies as outcome variables. Table 5 shows the impact congregational mergers have on the number of farms with cars, trucks, tractors, bailers, and freezers.[27]

[Table 5 about here]

As expected, we find no statistically or economically significant effect of congregational mergers on capital-intensive inputs: the standard errors are quite wide, and the effect sizes small: The coefficient on cars, for example, is less than a 0.1 percent increase relative to the control group mean, and the standard error is almost fifteen times the size of the coefficient. This suggests that congregational mergers did not substantially increase access to capital, and provides additional evidence that information is the main channel through which congregational mergers impacted technology adoption.

Another possible explanation of our results is that newly built transportation infrastructure could be driving both congregational mergers and fertilizer adoption during our study period. Using county-level data on open 2-digit highways (Baum-Snow 2007), we explore whether there is a statistical relationship between the prevalence of highways and congregational mergers. Our results are reported in Table A5 of the online appendix and show no statistically significant effect. In fact, the point-estimate of this analysis suggests that counties with mergers saw fewer miles of highways built between 1959 and 1964 than counties without mergers.

Finally, one might worry that by only using TALC congregational mergers in our analysis, we are understating the true treatment effect. We argue above that the TALC mergers are exogenous and, due to the heavily Lutheran populations in these regions, are the particular mergers for which we would expect to see an effect. Indeed, the congregations that are merging in these data have, on average, over 300 baptized members. Therefore, seeing an additional 36 farms begin to use fertilizer is an entirely reasonable effect size. There is another major Lutheran church branch, the Lutheran Church – Missouri Synod (LCMS), that was not directly involved in the TALC merger, but whose mergers could be attributed to increased discussion about merger surrounding TALC. We collected data from Concordia Historical Institute, the LCMS seminary, on congregational mergers between LCMS churches during the sample period. There is only one merger that occurs in a non-metropolitan county during this period and the inclusion of said merger does not produce a statistically distinguishable result from using only the TALC mergers.

Although our findings are consistent with the conclusion that congregational mergers affect fertilizer adoption through an information pathway, we are unable to provide direct evidence to support this conclusion. Ideally, we would have data on the structure and function of individual church congregations' social networks, as well as farm-level data on fertilizer use. Such data would allow us to map the spread of information about fertilizer technology through a social network and observe whether the network seems to explain adoption behavior. Unfortunately, these data do not exist, and we are left to draw conclusions from a coarser analysis. We nonetheless find our results compelling and highly suggestive that information sharing is the most likely explanatory mechanism, in line with recent findings from Murphy, Nourani, and Lee (2022).

Conclusion

We study the impacts of social networks on technology adoption. We combine data on American agriculture in the 1960s Midwest with information on mergers between Lutheran congregations. Using these mergers as exogenous shocks to social networks, we demonstrate that counties that experienced congregational mergers had over 5 percent more farms using fertilizer, and fertilized over 10 percent more acres than did other counties without mergers. Counties with mergers also treated 16 percent more acres with agricultural lime, a complement to fertilizer, and used over 15 percent more fertilizer on corn. These effects are economically and statistically significant and in line with our hypotheses. We also provide evidence for a mechanism: our results are consistent with increased information spreading through larger social networks. Our results are robust to a variety of specifications, as well as to two different placebo tests.

Even though this is an isolated experiment about social networks under specific conditions, these results speak to the broader economic literature on how social networks can facilitate information sharing and technology adoption. Specifically, we provide naturally occurring evidence that contact with innovators can be a meaningful way to increase technology adoption in agriculture. Furthermore, our results shed light on the importance of networks that are not explicitly arranged around economic activity. In particular, we find that religious social networks are an important factor in economic decision-making as churches represent a key focal point for information diffusion among midwestern farmers. Future research should seek out other naturally occurring shocks to social networks to further understand the within-network interactions that drive information diffusion.

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[1] See Jackson (2008) for an overview of the theoretical work on networks.

[2] See, for example, Beaman and Magruder (2012), Banerjee et al. (2014), and Emerick and Dar (2021).

[3] Prominent examples include Kremer and Miguel (2007), who examine peer effects in deworming in Kenya; Banerjee et al. (2013), who develop a model of network information diffusion using data from India; and Alatas et al. (2016), who model network-based information aggregation in Indonesian villages.

[4] In a notable exception, Satyanath, Voigtlaender, and Voth (2017) show that social clubs were a determinant of Nazi party participation in prewar Germany.

[5] See Feder, Just, and Zilberman (1985), Evenson and Westphal (1994), and Chavas and Nauges (2020) for reviews. Griliches (1957) is the seminal paper in this area.

[6] See Foster and Rosenzweig (2010) for a review.

[7] Broader reviews of the economic impacts of religion can be found in Welch and Mueller (2001) and Jackson and Fleischer (2007).

[8] Despite similarities, our work and that of Murphy, Nourani, and Lee (2022) developed concurrently and independently.

[9] Although farmers have been using organic fertilizer for thousands of years, chemical fertilizers as we know them today have their origins in the early twentieth century (Smil 2004).

[10] Throughout this period, agricultural research centers were continually testing new types of fertilizer and best practices around their use (University of Minnesota Extension 1960).

[11] Figure A1 in the online appendix provides additional detail about these statistics for counties with and without a congregational merger between 1959 and 1964.

[12] TALC and the LCA later merged to found what is today the largest Lutheran church in the United States, the Evangelical Lutheran Church in America (ELCA). Despite its name, the ELCA is a mainline Protestant denomination and not part of the modern evangelical Christian movement.

[13] Over time, churches of all backgrounds conducted their services more and more frequently in English.

[14] Congregations' steady, if begrudging, adoption of English as the primary or only language in church was also an impetus for congregational mergers (Lagerquist 1999).

[15] The most recent mergers in this dataset occurred in 2012, the year we received access to the data.

[16] Entries in the "location" field range from addresses to P.O. boxes to information such as "12SE," meaning 12 miles southeast of town. We match these data to the Census of Agriculture at the county level. Where possible, we cross-check the location information with the county. We find no major discrepancies.

[17] A synod is a Lutheran administrative region headed by a bishop. Lutheran synods are somewhat analogous to Catholic dioceses.

[18] We also use the 79 mergers that occurred between 1964 and 1969, after our main analysis period, to perform a placebo test. This exercise is useful since fertilizer use was still rising during this period (see Figure 1).

[19] These PDFs can be found at http://agcensus.mannlib.cornell.edu/AgCensus/.

[20] Ideally, since our unit of treatment is a congregation, we would be able to cluster our standard errors at the congregational level. However, since our data are measured at the county level, such an approach is

infeasible. Clustering at the county level may thus be overly conservative (Abadie et al. 2023), but we do so to account for the possibility of serially correlated outcomes (Bertrand, Duflo, and Mullainathan 2004).

[21] Nitrogen is the chemical fertilizer most heavily used in corn production (Pimentel 1992).

[22] It is conceivable to explicitly account for potential cross-county spillovers, but such an approach is ill-suited to our setting where counties are very large relative to the distance most families would be willing to travel to attend church, and where our sample size is extremely limited.

[23] We report estimates including weather controls for completeness, but weather controls and state-by-year fixed effects are removing similar variation in this context. Specifications including weather data use average annual precipitation, average annual temperature, heating degree days, and cooling degree days, aggregated to the climatic division level, from NOAA (NOAA 2007).

[24] Another possible explanation is that as farmers adopt chemical fertilizer, they may initially over-apply and subsequently reduce their use to a more appropriate level. If this is the case, it is possible that the total number of farms using chemical fertilizer and the total acreage being fertilized can increase while total tonnage of fertilizer used can remain stable.

[25] One limitation of the placebo treatment test is that counties with mergers between 1964 and 1967 could be fundamentally different in unobserved ways from counties without.

[26] After contacting state archives in North Dakota and South Dakota and doing significant research at the University of Minnesota Archives, we are convinced that no adequate measure of extension outreach or capacity exists that would allow us to test extension as an alternative explanation to our findings.

[27] For comparison, we estimate the average annual fertilizer cost for a representative farm in our sample was anywhere from $242 to $1,485, whereas the average cost of a tractor in 1960 was $3,129.

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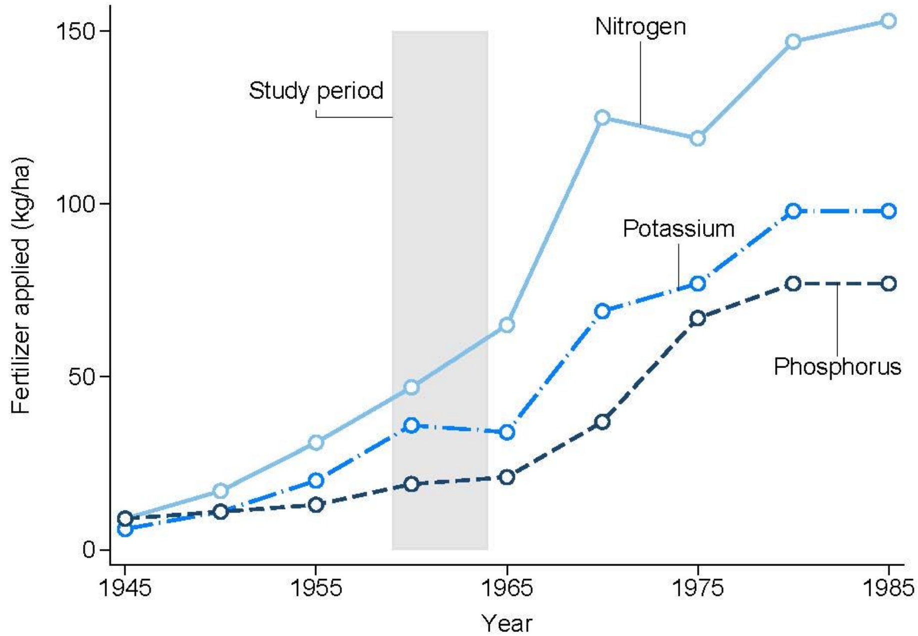
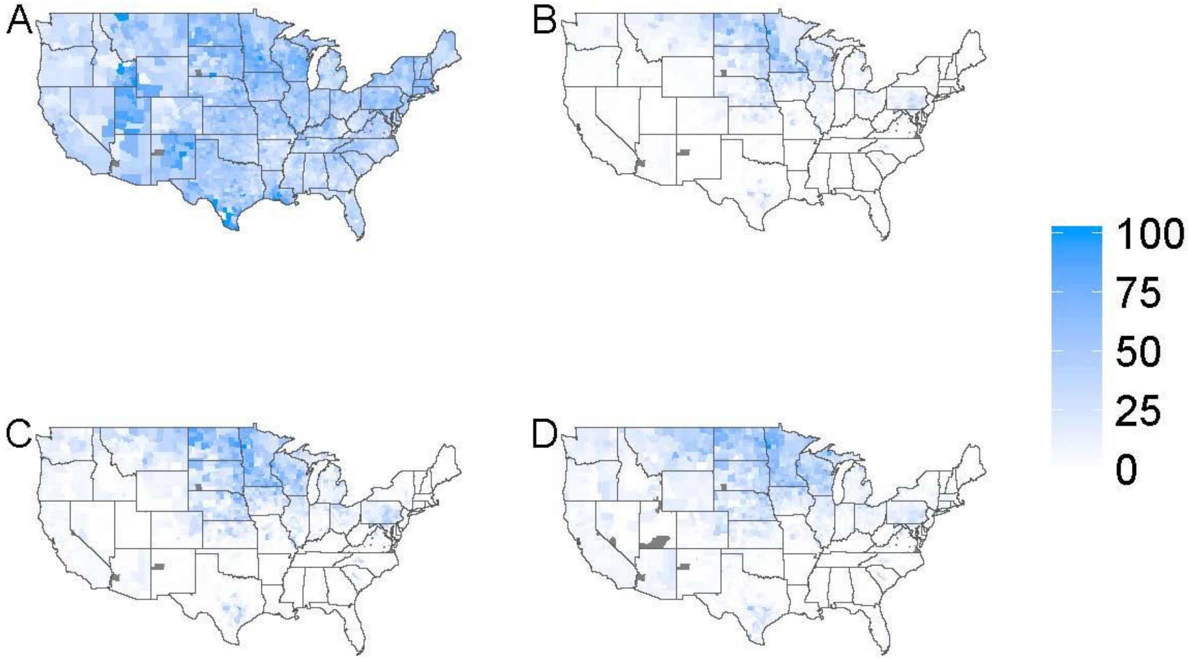
Figure 1: Fertilizer applied in US corn production



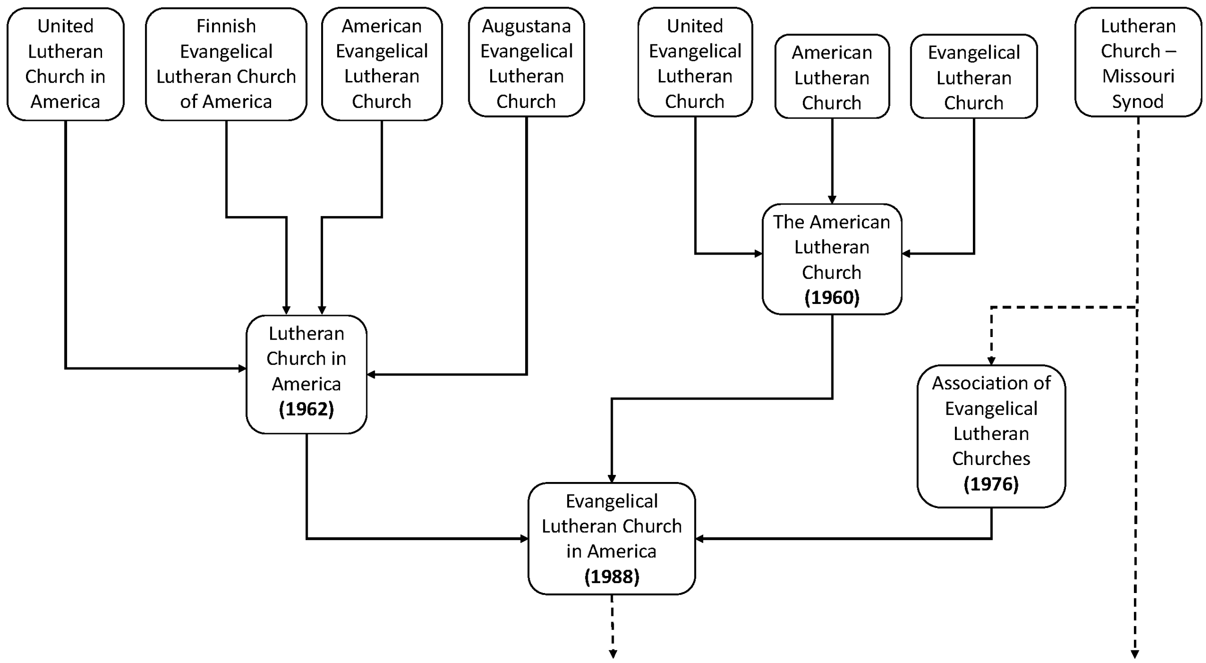
Notes: This figure shows the growth in chemical fertilizer use in corn production in the United States between 1945 and 1985. None of the major fertilizer chemicals, nitrogen, potassium, or phosphorus, had reached a usage steady state by the time of our study, shaded in gray. The sharpest increase in fertilizer use occurred between 1965 and 1970. This figure was modified from Pimentel (1992).

Figure 2: Spatial distribution of Lutherans



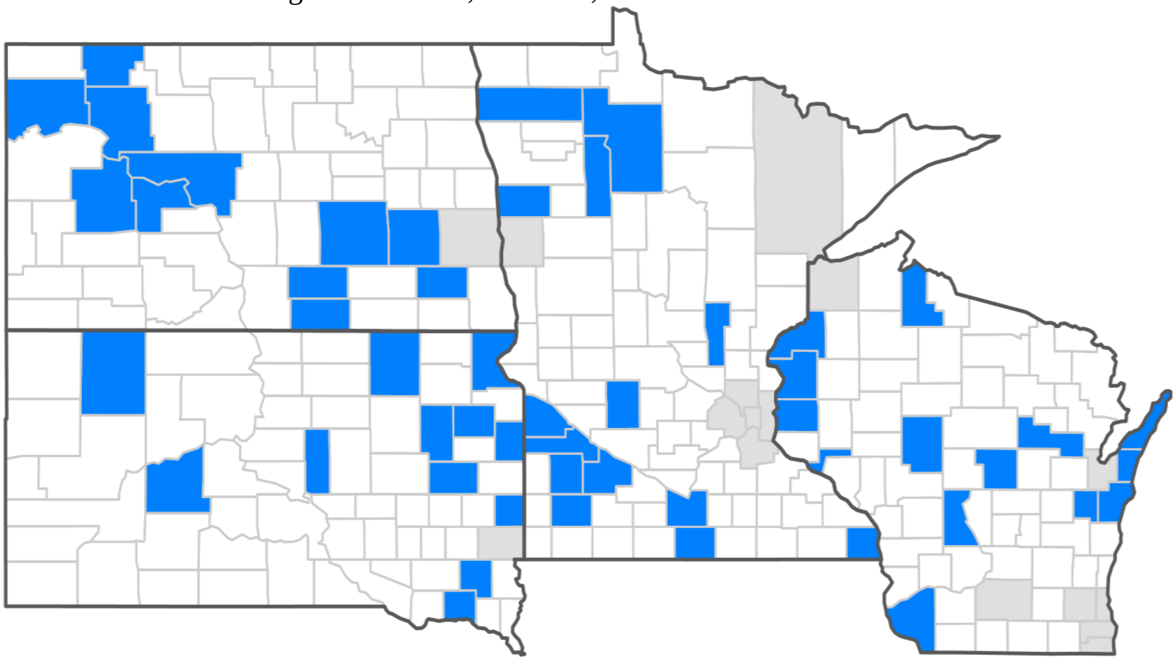
Notes: This figure presents data on religious adherence in the United States in 1952. Panel A displays the fraction of the population in each county that belonged to a religious organization. Panel B shows the fraction of each county's population that belonged to a Lutheran church branch. Panel C shows the fraction of religious members in each county that belonged to a Lutheran church branch. Panel D shows the fraction of churches or religious groups in each county that belonged to a Lutheran church branch. Minnesota, North Dakota, South Dakota, and Wisconsin stand out as the most Lutheran states. Gray counties have missing data. County population data comes from the 1950 Census; religion data are from the Association of Religion Data Archives' 1952 county-level Churches and Church Membership in the United States data file (The Association of Religion Data Archives 2006).

Figure 3: Lutheran Church mergers



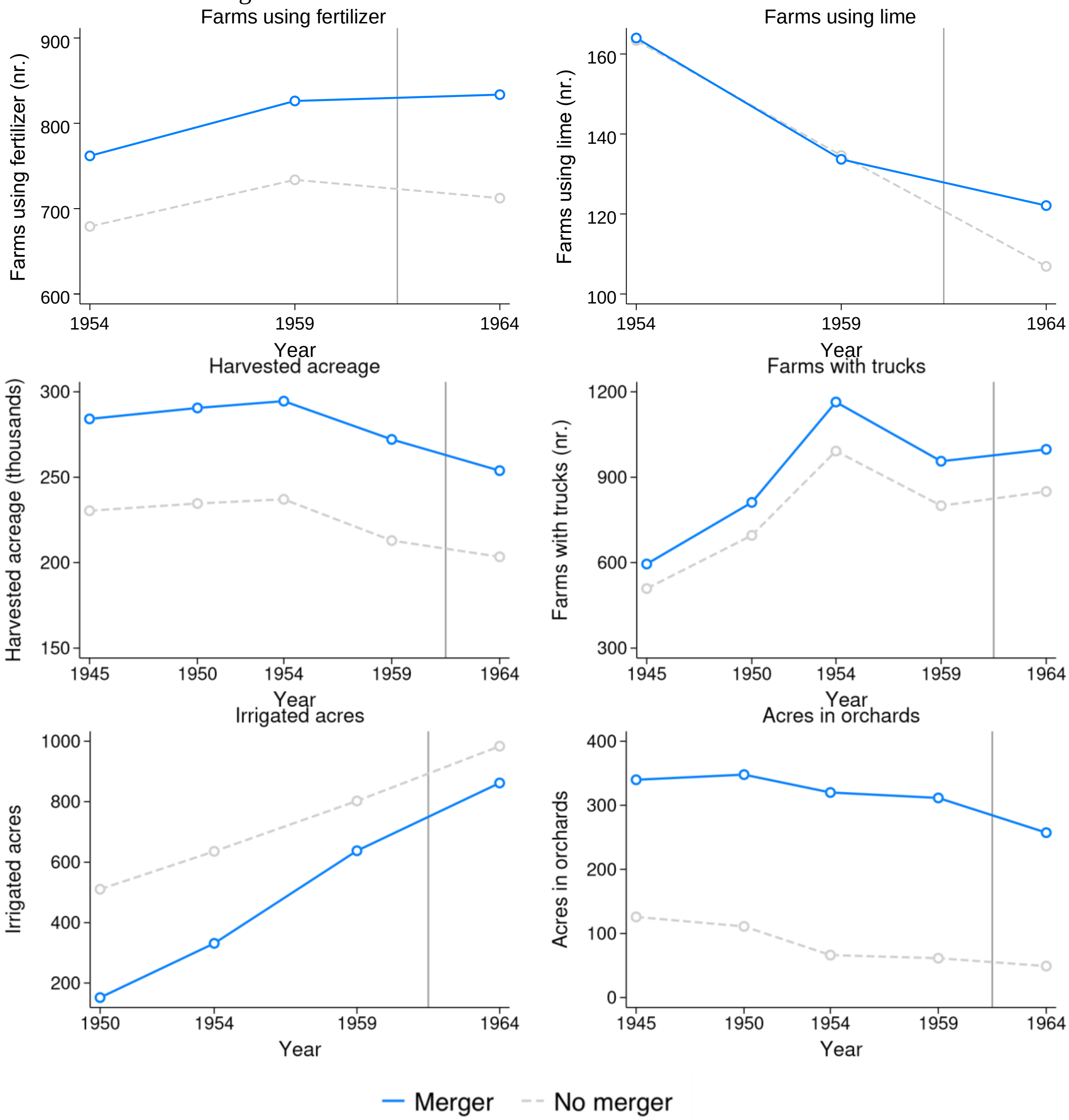
Notes: Each box represents a national-level Lutheran church branch. The mergers creating in The American Lutheran Church and the Lutheran Church in America in 1960 and 1962, respectively, prompted congregational-level mergers in the early 1960s.

Figure 4: Treated, untreated, and excluded counties



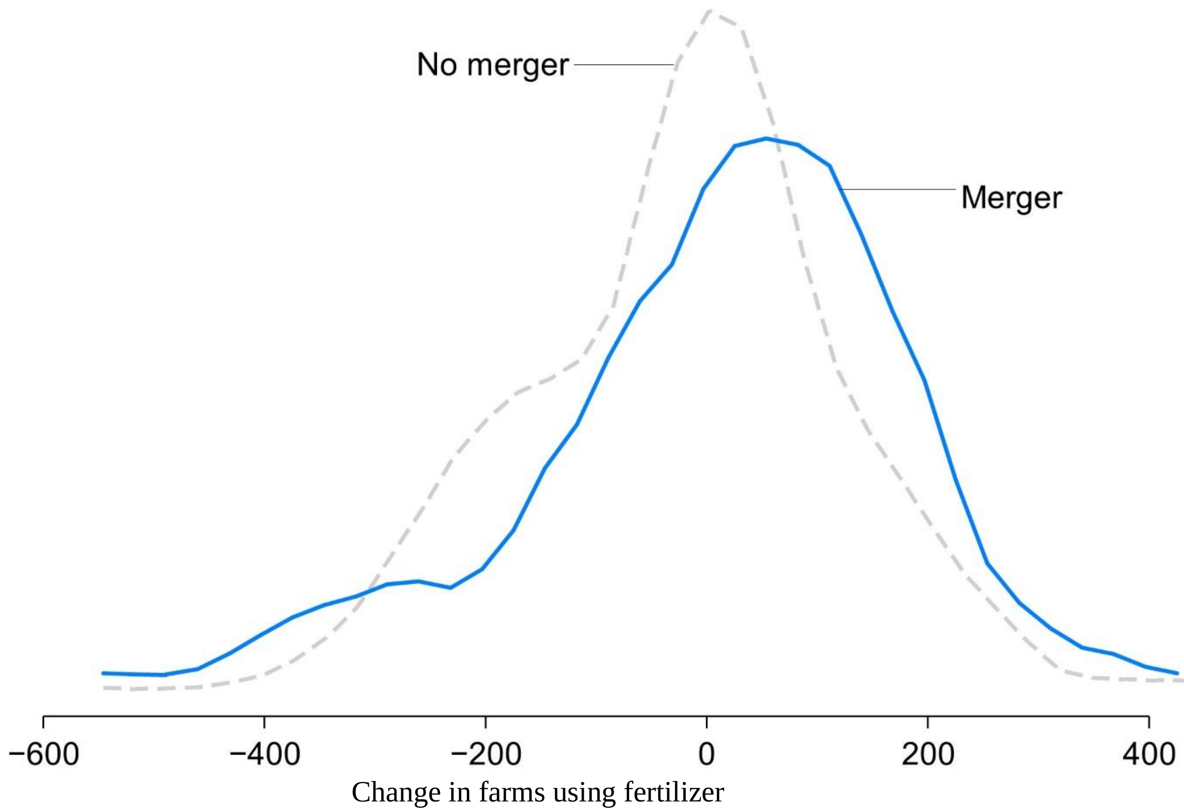
Notes: This figure displays our sample of counties within the states of Minnesota, North Dakota, South Dakota, and Wisconsin. The 51 counties in blue experienced at least one congregational merger between 1959 and 1964. The 211 counties in white did not. The 16 counties in gray contained a major urban area, defined according to the 1960 Census, and were excluded from our analysis.

Figure 5: Trends in outcomes: treated vs. untreated counties



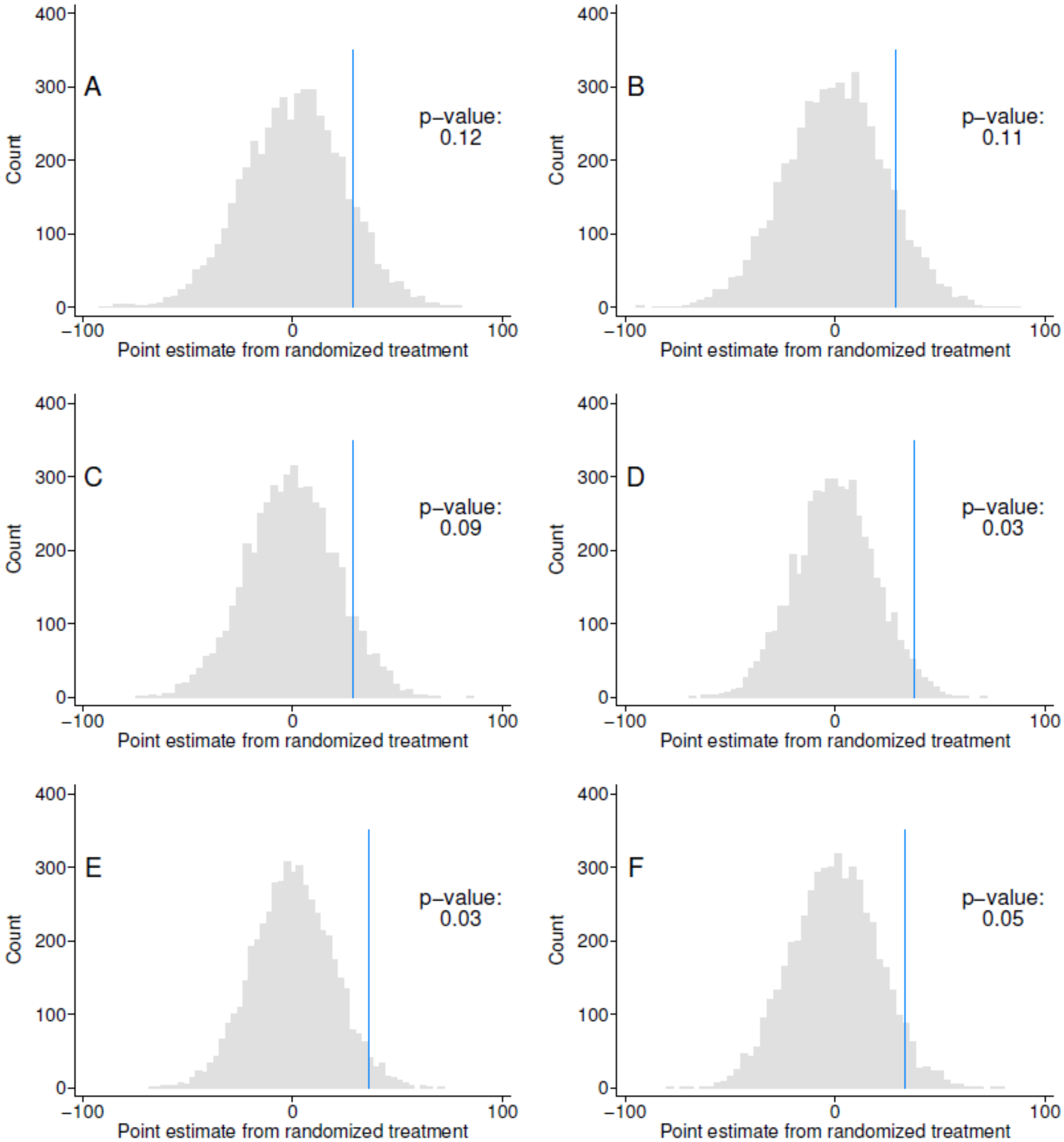
Notes: This figure displays mean values of a variety of outcomes of interest over time, separately for counties that did (solid blue) and did not (dashed gray) experience congregational mergers between 1959 and 1964. For farms using fertilizer and farms using lime, data are only available for the 1954, 1959, and 1964 Censuses of Agriculture. For irrigated acreage and acres in orchards, data are available for 1950, 1954, 1959, and 1964. Finally, for harvested acreage, and farms with trucks, we observe data from the 1945, 1950, 1954, 1959, and 1964 Censuses of Agriculture. We find that all variables display parallel trends prior to 1954. We see evidence that treated counties have a greater number of farms using both fertilizer and lime in the post-treatment period. In all cases, a regression-based test of pre-trends fails to reject the null of parallel trends.

Figure 6: Change in farms using fertilizer, 1959 to 1964



Notes: This figure plots kernel densities of the difference in the number of farms using fertilized between 1959 and 1964 for counties that experienced a congregational merger and those that did not. These densities were generated using an Epanechnikov kernel. The mean (SD) of the no-merger distribution is -21.45 (148.14), and the mean (SD) of the merger distribution is 7.41 (176.72).

Figure 7: Randomization inference – Number of farms using fertilizer



Notes: This figure displays the results of a randomization inference procedure for the six regression specifications in table 3 used to estimate the effects of congregational mergers on the number of farms using fertilizer. Panel A includes the difference-in-difference treatment variable, a post-treatment dummy, and a merging county dummy on the right-hand side. Panel B adds county fixed effects. Panel C adds weather controls. Panel D removes weather controls, but adds state-by-year fixed effects. Panel E includes the treatment variable, county fixed effects, state-by-year fixed effects, and weather controls. Panel F contains the same variables as panel E, but excludes Wisconsin. These panels correspond to Columns (1), (2), (3), (4), (5), and (6) of table 3, respectively. For each specification, we randomly reassigned mergers to exactly 51 counties 5,000 times, re-estimated the appropriate specification, and saved the coefficient of interest. These coefficients are plotted in gray. The blue lines denote the $\hat{\beta}$ values estimated using the real merger assignment vector. The p-value is the fraction of coefficients that are smaller than the real coefficient.

Table 1: Pre-treatment Summary Statistics

Variable	Untreated	Treated	Difference
Farms (nr.)	1,512	1,779	266*
	(951)	(820)	[145]
Acres in county	638,271	683,093	44,822
	(358,481)	(384,087)	[56,726]
Acres in farms	507,221	572,876	65,655
	(318,184)	(351,513)	[50,690]
Acres harvested	237,091	294,557	57,466**
	(147,062)	(168,563)	[23,629]
Farms using fertilizer (nr.)	679	762	83
	(737)	(777)	[116]
Acres fertilized	35,577	36,045	468
	(45,892)	(40,968)	[7,020]
Fertilizer used (tons)	2,810	3,047	237
	(3,504)	(3,357)	[542]
Farms using lime (nr.)	163	164	1
	(295)	(333)	[47]
Acres limed	2,473	2,389	−84
	(4,708)	(4,927)	[741]
Lime used (tons)	4,818	4,490	−328
	(10,386)	(9,903)	[1,606]
Acres harvested (corn)	45,965	54,767	8,802
	(39,901)	(49,108)	[6,527]
Corn yield (bushels/acre)	23.16	22.90	−0.26
	(17.55)	(17.84)	[2.75]
Miles of all 2-digit highway open	0.00	0.02	0.02**
	(0.00)	(0.14)	[0.01]
Percent change in population (1940-1950)	−2.48	–3.59	−1.11
	(10.41)	(2.75)	[1.54]
Percent change in population (1950-1960)	0.61	–2.36	−2.98
	(16.02)	(10.84)	[2.37]
Percent change in population (1960-1970)	−1.30	–4.40	−3.10
	(14.81)	(12.56)	[2.25]
Percent non-white (1950)	3.15	1.52	−1.64
	(10.56)	(2.73)	[1.49]
Percent non-white (1960)	3.23	1.74	−1.49
	(10.32)	(3.03)	[1.46]
Rural farm population (1950)	6,721	7,716	995
	(4,515)	(3,880)	[687]
Rural farm population (1960)	5,376	6,190	815
	(3,788)	(3,432)	[581]
Rural population per square mile (1950)	15.48	16.81	1.32
	(12.27)	(11.85)	[1.90]
Rural population per square mile (1960)	15.55	16.06	0.51
	(13.51)	(11.65)	[2.06]
Employed in agriculture (1950)	2,427	2,823	396
	(1,621)	(1,414)	[247]
Employed in agriculture (1960)	1,666	2,002	336*
	(1,135)	(1,029)	[174]
Median family income (1950)	2,507	2,567	59
	(626)	(394)	[92]
Median family income (1960)	4,150	3,987	–163
	(912)	(727)	[137]
Number of counties	211	51	

Notes: This table shows summary statistics for counties that did not experience a congregational merger between 1959 and 1964 (untreated) and counties that did experience a merger during this time period (treated). Unless otherwise indicated, variables reflect data from 1954. Standard deviations in parentheses; standard errors on the *t*-tests between untreated and treatment in brackets. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 2: Test of Differential Pre-trends

VARIABLES	(1) Fertilizer: farms	(2) Fertilizer: acres	(3) Lime: farms	(4) Lime: acres
Year = 1959 × *Merger*	13.76	5,616.31	–5.94	107.48
	(22.78)	(4,207.25)	(15.16)	(208.60)
	[0.260]	[0.085]	[0.316]	[0.323]
Mean of dependent variable	706.45	46,461.29	148.99	2,465.92
Observations	524	524	524	524
Number of counties	262	262	262	262
County FE	YES	YES	YES	YES
State-by-year FE	YES	YES	YES	YES

Notes: This table shows results from estimating Equation (1), using data from 1954 and 1959. *Year* = 1959 × *Merger* is equal to one if the year is 1959, and the county experienced a congregational merger between 1959 and 1964. The dependent variables are, in column (1), the number of farms reporting fertilizer use; in column (2) the number of acres fertilized; in column (3) the number of farms reporting lime use; and in column (4) the number of acres limed. Standard errors are in parentheses and are clustered at the county level. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Brackets contain p-values derived from a randomization inference procedure as described in the *Main Results* section.

Table 3: Impact of Congregational Mergers on Fertilizer Use – Farms

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
Year = 1964 × *Merger*	28.86	28.86	28.43	37.73*	36.09*	33.00
	(26.66)	(26.63)	(24.39)	(21.55)	(21.73)	(21.20)
	[0.116]	[0.112]	[0.087]	[0.025]	[0.029]	[0.050]
Mean of dependent variable	723.05	723.05	723.05	723.05	723.05	551.98
Observations	524	524	524	524	524	396
Number of counties	262	262	262	262	262	198
County FE	NO	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES
State-by-year FE	NO	NO	NO	YES	YES	YES
Weather controls	NO	NO	YES	NO	YES	YES
Wisconsin	YES	YES	YES	YES	YES	NO

Notes: This table shows results from estimating Equation (1). The dependent variable is the number of farms in a county reporting fertilizer use. *Year* = 1964 × *Merger* is equal to one if the year is 1964, and the county experienced a congregational merger between 1959 and 1964. Note that state-by-year fixed effects nest year fixed effects. Weather controls include temperature (°F), precipitation (in), heating degree days, and cooling degree days. Column (6) includes only counties in Minnesota, North Dakota, and South Dakota. Standard errors are in parentheses and are clustered at the county level. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Brackets contain p-values derived from a randomization inference procedure as described in the *Main Results* section.

Table 4: Impact of Congregational Mergers on Fertilizer Use – Fertilizer and Lime

VARIABLES	(1) Fertilizer: acres	(2) Fertilizer: tons	(3) Lime: farms	(4) Lime: acres	(5) Lime: tons
Year = 1964 × *Merger*	6,133.07**	−215.57	21.26*	374.24*	1,030.85
	(2,686.20)	(503.17)	(11.35)	(222.85)	(726.19)
	[0.008]	[0.319]	[0.010]	[0.034]	[0.044]
Mean of dependent variable	60,115.43	2,246.48	120.76	2,282.15	5,951.66
Observations	524	524	524	524	524
Number of counties	262	262	262	262	262
County FE	YES	YES	YES	YES	YES
State-by-year FE	YES	YES	YES	YES	YES
Weather controls	YES	YES	YES	YES	YES

Notes: This table shows results from estimating Equation (1). *Year* = 1964 × *Merger* is equal to one if the year is 1964, and the county experienced a congregational merger between 1959 and 1964. The dependent variables are, in column (1), acres fertilized; in column (2), tons of chemical fertilizer used; in column (3), the number of farms reporting the use of lime; in column (4), acres limed; and in column (5), tons of lime used. Weather controls include temperature (°F), precipitation (in), heating degree days, and cooling degree days. Standard errors are in parentheses and are clustered at the county level. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Brackets contain p-values derived from a randomization inference procedure as described in the *Main Results* section.

Table 5: Impact of Congregational Mergers on Capital

VARIABLES	(1) Cars	(2) Trucks	(3) Tractors	(4) Bailers	(5) Freezers
Year = 1964 × *Merger*	1.08	0.78	25.76	12.65	8.79
	(15.01)	(13.45)	(39.25)	(12.44)	(17.40)
	[0.476]	[0.460]	[0.159]	[0.201]	[0.328]
Mean of dependent variable	1,104.26	824.99	1,069.95	467.92	839.05
Observations	524	524	524	524	524
Number of counties	262	262	262	262	262
County FE	YES	YES	YES	YES	YES
State-by-year FE	YES	YES	YES	YES	YES
Weather controls	YES	YES	YES	YES	YES

Notes: This table shows results from estimating Equation (1). *Year* = 1964 × *Merger* is equal to one if the year is 1964, and the county experienced a congregational merger between 1959 and 1964. The dependent variables are, in column (1), farms reporting car ownership; in column (2), farms reporting truck ownership; in column (3), farms reporting tractor ownership; in column (4), farms reporting bailer ownership; and in column (5), farms reporting freezer ownership. Weather controls include temperature ($^{\circ}$F), precipitation (in), heating degree days, and cooling degree days. Standard errors are in parentheses and are clustered at the county level. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Brackets contain p-values derived from a randomization inference procedure as described in the *Main Results* section.