Pandemics Depress the Economy, Public Health Interventions Do Not: Evidence from the 1918 Flu

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Abstract

Do non-pharmaceutical interventions (NPIs) aimed at reducing mortality during a pandemic necessarily have adverse economic effects? We use variation in the timing and intensity of NPIs across U.S. cities during the 1918 Flu Pandemic to examine their economic impact. While the pandemic itself was associated with economic disruptions in the short run, we find these disruptions were similar across cities with strict and lenient NPIs. In the medium run, we find suggestive evidence that, if anything, NPIs are associated with better economic outcomes. Our findings indicate that NPIs can reduce disease transmission without necessarily further depressing economic activity.

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1 Introduction

Do non-pharmaceutical interventions (NPIs) such as social distancing have economic costs, or can public health measures intended to contain the spread of a pandemic also reduce its economic severity? The outbreak of the COVID-19 pandemic has sparked urgent questions about the impact of pandemics and the associated public health responses on the real economy. In this paper, we examine the economic effects of non-pharmaceutical interventions during the largest influenza pandemic in U.S. history, the 1918 Flu Pandemic.

In our empirical analysis, we exploit variation in the speed and intensity of the implementation of NPIs across U.S. cities during the fall of 1918. NPIs implemented in 1918—although less extensive—resemble policies used to reduce the spread of COVID-19, including school, theater, and church closures, public gathering bans, quarantine of suspected cases, and restricted business hours.

We start by studying the impact of NPIs on mortality. Consistent with existing evidence from the epidemiology literature (Markel et al., 2007; Hatchett et al., 2007), we find that NPIs achieved substantial reductions in peak mortality, of about 45%, thereby *flattening the infection curve*. We also find evidence that cities that intervened both early and aggressively experienced a modest reduction in cumulative excess mortality of about 20%. Thus, NPIs were successful in slowing the rate of disease transmission and, to a lesser extent, cumulative infection rates, potentially by mitigating epidemic overshoot (Bootsma and Ferguson, 2007).

Our main analysis examines the impact of NPIs on economic activity in U.S. cities in the short and medium-run. In theory, the economic effects of NPIs could be either positive or negative. All else equal, NPIs constrain social interactions and thus economic activity that relies on such interactions. However, economic activity in a pandemic is also reduced in absence of such measures, as households reduce consumption and labor supply to lower the risk of becoming infected, and firms cut investment in response to increased uncertainty. Moreover, while the direct effect of NPIs is to lower economic activity, they also mitigate the impact of the original shock: the pandemic itself. By containing the pandemic, NPIs can thus also mitigate the pandemic-related economic disruptions.

To study the short-term impact of NPIs on local economic activity, we construct a city-level index of business disruptions at a monthly frequency based on a contemporary trade journal. Our index implies that the pandemic itself is associated with an increase in business disruptions in the fall of 1918. The increase in business disruptions is supported by narrative evidence from contemporary newspapers, which report significant declines in output and sales across a wide range of industries due to labor shortages and falling demand. However, when we compare cities with strict and lenient NPIs, we find that the increase in business disruptions in the fall and winter of 1918 was quantitatively similar across the two sets of cities. Our findings thus indicate that NPIs did not clearly exacerbate the economic downturn during the pandemic.

Further, we examine the economic impact of NPIs in the medium run. We find no evidence that cities that intervened earlier and more aggressively perform worse in the years after the pandemic, measured by local manufacturing employment and output and the size of the local banking sector. At a minimum, our estimates reject that cities with stricter NPIs experienced a large decline in employment and output in the years following the pandemic, relative to cities with lenient NPIs. If anything, high NPI cities experience a relative increase in economic activity from 1919 onwards. Altogether, our findings suggest that, while pandemics are associated with economic disruptions, NPIs may reduce disease transmission without exacerbating the pandemic-induced downturn.

Our findings are subject to the concern that policy responses are endogenous and may be driven by factors related to future economic outcomes. This concern is somewhat mitigated by the insight that cities that experienced outbreaks at later dates tended to implement NPIs sooner within their outbreak, as they learned from the experiences of cities affected earlier (Hatchett et al., 2007). Thus, as the flu moved from east to west, cities located further west were faster in implementing NPIs. Importantly, we also show that our results are robust to controlling for time-varying shocks correlated with characteristics that differ between western and eastern cities, such as the exposure to agriculture, past population growth, density, and proxies for the quality of local institutions.

We emphasize caution when generalizing these results to the current COVID-19 outbreak. The 1918 Flu Pandemic was significantly deadlier than what current estimates suggest for COVID-19, especially for working-age individuals. Thus, the economic merits of NPIs may have been greater in 1918. NPIs implemented in 1918 were also less extensive than those used during the COVID-19 outbreak. Moreover, the structure of the U.S. economy and society has evolved substantially over a century. Nevertheless, our results suggest that it is not a foregone conclusion that there is a trade-off between reducing disease transmission and stabilizing economic activity in a pandemic.

The rest of the paper is structured as follows. Section 2 discusses the historical background on the 1918 Flu Pandemic and non-pharmaceutical interventions. Section 3 describes our dataset. Sections 4 present our results, and Section 5 offers a discussion and concluding remarks.

2 Historical Background and Related Literature

2.1 The 1918 Flu Pandemic

The 1918 Flu Pandemic lasted from January 1918 to December 1920, and it spread worldwide. The number of deaths is estimated to be at least 50 million globally, with about 550,000 to 675,000 occurring in the U.S. (Johnson and Mueller, 2002). The pandemic thus killed about 0.66 percent of the U.S. population. A distinct feature of the 1918-19 influenza pandemic was that it resulted in high death rates for 18-44 year old adults and healthy adults. Figure A1 shows the sharp spike in mortality from influenza and pneumonia in 1918 in the U.S.

The pandemic came in three different waves, starting with the first wave in spring 1918, a second wave in fall 1918, and a third wave in the winter of 1918 and spring of 1919. The pandemic peaked in the U.S. during the second wave in the fall of 1918. This highly fatal second wave was responsible for most of the deaths attributed to the pandemic in the U.S. Deaths typically occurred about 10 days after contracting the virus (Markel et al., 2007). In the U.S., the virus was first identified in military personnel in spring 1918. Mass troop movements during the closing stages of WWI contributed to the spread of the flu in the U.S. and around the world (Crosby, 2003).

Velde (2020) presents a comprehensive account of the economic impact of the 1918 Flu Pandemic in the U.S. and documents that it was associated with a short and moderate recession in the aggregate. Garrett (2009) finds that geographic areas with higher influenza mortality saw a relative increase in wages from 1914 to 1919 census years, consistent with labor shortages. Barro et al. (2020) uses country-level data and finds that the 1918 Flu Pandemic lowered real GDP by 6-8% in the typical country. Dahl et al. (2020) find that the 1918 pandemic resulted in a V-shaped recession using municipality-level data from Denmark. Using more disaggregated variation, Guimbeau et al. (2019) and Almond (2006) find negative effects of the 1918 flu on long-term health and productivity. Using regional data from Sweden, Karlsson et al. (2014) find that the 1918 pandemic led to a persistent increase in poverty rates and a reduction in the return on capital.

2.2 Non-Pharmaceutical public health interventions

Most U.S. cities applied a range of NPIs during the second wave in fall 1918. The measures applied include social distancing measures such as the closure of schools, theaters, and churches, the banning of mass gatherings, but also other measures such as mandated mask wearing, case isolation, making influenza a notifiable disease, and public disinfection/hygiene measures. Measures in 1918 were not as extensive as measures used to combat COVID-19 in terms of closing non-essential businesses. For instance, rather than closing businesses altogether, staggered business hours were introduced mostly to avoid crowding in public transportation.

The epidemiology literature has studied NPIs and their effect on mortality during the 1918 pandemic in depth. Altogether, the evidence suggests that the implementation of

NPIs was associated with reduced disease transmission (see, e.g., Bootsma and Ferguson, 2007; Hatchett et al., 2007; Markel et al., 2007). The literature finds that early and aggressive NPIs—measures undertaken right after the flu arrived in a location—led to significant (around 50%) reductions in peak mortality, i.e. flattening the curve. However, these studies find more moderate (10-30%) reductions in cumulative mortality.[1]

3 Data

We build a city-level dataset for the years around the 1918 pandemic with information on non-pharmaceutical public health interventions, influenza mortality, economic activity, and bank balance sheets. For city-level NPIs, we rely on data from Markel et al. (2007), who gather detailed information on NPIs for 43 major U.S. cities from municipal health department bulletins, local newspapers, and reports on the pandemic (see Appendix Table A1). We also draw on Markel et al. (2007) for estimates of peak and cumulative excess mortality from influenza and pneumonia during the 24-week period from September 8, 1919 to February 22, 1919. In addition, we collect annual data on influenza mortality at the city level from the Center for Disease Control's (CDC) Mortality Statistics tables.

To study the short-run economic impact of the 1918 Flu Pandemic and associated NPIs, we construct a monthly city-level measure of business disruptions. We digitize information on business conditions from *Bradstreet's* weekly "Trade at a Glance" tables.[2] These tables provide city-level one-word summaries of the conditions of wholesale trade, retail trade, and manufacturing. We categorize these words into an indicator variable of whether trade was "Not disrupted" or "Disrupted."[3] We then aggregate this measure into monthly frequency, as information for some cities is not reported every week. This results in a monthly series of business disruptions for 25 cities with NPI measures from January

[1] Adda (2016), using high-frequency data from France, finds that NPIs reduce the spread of viruses, but argues that NPIs are not necessarily cost-effective.

[2] In concurrent work, Velde (2020) also uses the Trade at a Glance tables to study the impact of mortality acceleration and business closures on local trade conditions.

[3] For robustness, we also construct a three-valued measure that ranks business conditions into "Bad," "Fair," and "Good".

1918 to September 1919. Further details are provided in the data appendix.

To study the medium-run impact of NPIs, we digitize information on city-level manufacturing activity from the Census of Manufactures. We use manufacturing data on employment and output for the years 1904, 1909, 1914, 1919, 1921, 1923, 1925, and 1927. The data appendix describes in detail how we adjust for changes in the Census' methodology and city boundaries over time. We also use city-level annual bank assets as a proxy for local economic activity, digitized from the *Annual Reports of the Comptroller of the Currency*. Finally, we collect variables used to control for baseline economic differences across cities. We collect state agricultural employment share, city population, and city density from various decennial censuses. We also use city-level public health spending per capita from Swanson and Curran (1976) and city-level third Liberty Loan subscriptions from a 1918 hearing before the House of Representatives Committee on Ways and Means.

4 Non-pharmaceutical Interventions and Economic Activity

4.1 Measures of non-pharmaceutical interventions

Our empirical approach uses variation in the speed and intensity of the implementation of non-pharmaceutical interventions (NPIs) across major U.S. cities in fall 1918. Drawing on the variables constructed by Markel et al. (2007), we measure NPIs in three ways. First, we measure the intensity of NPIs by the cumulative sum of the number of days where three types of NPIs were activated (school closure, public gathering bans, and quarantine/isolation of suspected cases) in fall 1918, denoted by $NPI\,Intensity$.

Second, we measure how quickly an NPI was implemented by the number of days elapsed between when the city death rate exceeded twice its baseline death rate and the first day city officials enforced a local NPI. We multiply the day count by minus one so that higher values indicate a faster response and denote this measure by $NPI\,Speed$.

Third, given that the most effective interventions are likely early and aggressive, we also construct an indicator variable equal to one for cities with both $NPI\,Speed$ and

NPI Intensity above their medians, which we refer to as *High NPI*. *High NPI* equals one for 18 cities and zero for 25 cities in the sample of 43 NPI cities. This *High NPI* measure is our preferred measure of NPI implementation.

All of the 43 cities analyzed in Markel et al. (2007) eventually adopted at least one of the three types of NPIs. School closures and cancellation of public gatherings were the most common. However, there was variation across cities in the speed and aggressiveness of these measures. The median duration was four weeks, with the longest lasting ten weeks (Markel et al. (2007)). *High NPI* cities on average implemented the first NPI about 1.5 days after the mortality rate reached twice its baseline level, whereas *Low NPI* cities reacted on average only after twelve days (see Table A2 in the Appendix). Similarly, *High NPI* cities had an average NPI intensity of 133, compared to 56 for the *Low NPI* cities.

4.2 Identification

An important concern is that NPIs may be endogenous to local health and economic outcomes. For instance, officials may be more inclined to intervene if the historical exposure to the flu is higher, which in turn may be correlated with other factors such as socio-demographic or geographic characteristics (Bootsma and Ferguson, 2007). Another concern is that interventions reflect the quality of institutions, including the health care system. Places with better institutions may have a lower cost of intervening, as well as better economic prospects.

These concerns can in part be addressed by studying the variation in NPIs across cities. Local responses were not driven by a federal response, as no coordinated pandemic plans existed. Instead, as the fall wave of the pandemic swept the country from east to west, cities in the west that were affected later implemented NPIs faster, as they were able to learn from cities in the east that were affected earlier (see, e.g., Crosby, 2003; Hatchett et al., 2007). As a result, distance to the East Coast explains a large part of the variation in NPIs across cities (see Figure A2). In line with being further west, *High NPI* cities are located in states whose industry tends to be oriented more toward agriculture (see Table A2). Further, they have lower influenza mortality in previous years. On the other hand, $High\ NPI$ cities are not significantly different in terms of health spending per capita, density, population in the 1910 census, or manufacturing employment. The main identification concern is therefore that differences between cities with aggressive and less aggressive NPIs are driven by differential responses of cities in the west to the end of WWI, for instance, because they are more exposed to the agricultural boom and bust (Rajan and Ramcharan, 2015).

Beyond distance from the eastern U.S., there is variation within regions in the speed and intensity of NPIs driven by the different decisions of local policymakers with limited information reacting to rapidly changing events. For example, local officials in Minneapolis moved swiftly to ban public gatherings and close schools in early October. Right across the Mississippi River, St. Paul remained largely open into November, as its leaders were confident they had the epidemic under control and believed NPIs would not be effective.[4]

To address endogeneity concerns, in our regressions we control for several relevant city-level observables. We control for log city population in 1900 and 1910, city density in 1910, public health expenditure in 1917 relative to 1910 population, manufacturing employment in 1914 to 1910 population, and the state agriculture employment share in 1910 ("Baseline controls"). Further, to capture baseline differences in influenza exposure we also control for lagged influenza and pneumonia mortality in 1917.

4.3 Non-pharmaceutical interventions and mortality

We first examine the relation between NPIs and mortality by estimating city-level regressions of the form

$$Mort_c = \alpha + \beta\, NPI_c + X_c\, \delta + u_c, \tag{1}$$

[4]See "A look back at the 1918 flu pandemic and its impact on Minnesota," *MinnPost*, March 4, 2020.

where $Mort_c$ is a city-level measure of mortality from influenza and pneumonia and NPI_c is one of the three NPI measures. Panel A of Table 1 studies the impact of NPIs on weekly peak mortality. Columns (1)-(3) report the regressions without controls. The estimates are statistically significant for $NPI\ Intensity$ and $High\ NPI$. For $NPI\ Speed$, the point estimate is negative but not statistically significant, and the R^2 is substantially lower. Columns (4)-(6) show that the estimates are similar with the inclusion of our baseline controls for city characteristics. Next, columns (7)-(9) reveal that when we include lagged influenza mortality, the estimates fall slightly, but the estimates on $NPI\ Intensity$ and $High\ NPI$ remain statistically significant. In terms of magnitudes, the estimate in column (9) implies that high NPI cities experienced a 45% reduction in peak mortality relative to the mean. NPIs in the fall of 1918 were thus successful in flattening the curve.

In Panel B of Table 1, we examine the relation between NPIs and cumulative excess mortality over the 24-week period from September 8, 1918 to February 22, 1919. Columns (1)-(3) show that $NPI\ Intensity$ and $High\ NPI$ are associated with statistically significantly lower cumulative excess mortality in a regression without controls (see also Figure A3). As in the regressions for peak mortality in Panel A, the estimate on $NPI\ Speed$ is negative but not statistically significant. Columns (4)-(6) show that the estimates are similar with the inclusion of our baseline controls for city characteristics. However, columns (7)-(9) show that the estimates decline substantially when controlling for lagged mortality. Only the estimate on $High\ NPI$ in column (9) remains significant at the 5% level. This estimate implies a reduction in cumulative mortality of 22% relative to the mean, a magnitude similar to Hatchett et al. (2007) (20% reduction) and Bootsma and Ferguson (2007) (10-30% reduction).[5]

[5]These results are broadly consistent with Barro (2020), who finds that NPIs measured by Markel et al. (2007) led to a reduction in peak mortality, but finds that NPIs did not reduce cumulative mortality. Barro (2020) suggests this may be because they were not in place long enough. An important difference relative to Barro (2020) is that we also examine the impact of both timely *and* aggressive NPIs using the $High\ NPI$ measure, which suggests a modest impact also on cumulative mortality.

4.4 Non-pharmaceutical interventions and economic disruptions in the short run

Were NPIs that flattened the curve associated with a worse downturn in fall 1918? We next examine the impact of the pandemic and NPIs on city-level business disruptions during the pandemic. For this, we rely on a monthly index of business disruptions constructed from *Bradstreet's* trade conditions reports. Figure 1 plots the average of our "No disruptions" variable across cities with above-median *NPI Intensity* and *NPI Speed* in the sample of 25 cities for which the index is available. "No disruptions" are assigned a value of 100; "Disruptions" are assigned a value of 0. Panel (a) plots the combined index for Wholesale Trade, Retail Trade, and Manufacturing, and the remaining panels plot the index for each sector separately.[6]

The first take-away from Figure 1 is that the pandemic itself was associated with disruptions in economic activity. Panel (a) shows that from September 1918 to February 1919 there is a decline in the combined index. Panels (b)-(d) reveal that the disruptions were most widespread in manufacturing, followed by wholesale trade. The decline in retail trade was more modest, and retail trade saw a rebound already in December 1918. The business disruptions index then displays a gradual recovery through spring 1919. Given the qualitative nature of the business conditions reports, we cannot ascertain whether the recovery was to the previous trend or to a lower trend.

The economic disruption in fall of 1918 is also reflected in contemporary newspaper accounts.[7] Reports from the time indicate that the pandemic depressed the economy through both supply and demand-side channels in the form of productivity reduction, labor shortages, and falling demand for retail goods. For example, on October 24, 1918, the *Wall Street Journal* reported:

[6]Figure A6 shows results are similar when splitting cities into those with above and below median *NPI Intensity*. Figure A7 shows the results are similar when using a three-valued index for whether trade conditions are "Bad" (=1), "Fair" (=2), or "Good"(=3).

[7]See Appendix C for more extensive evidence of economic disruptions from contemporary newspaper accounts. Garrett (2008) also provides narrative evidence from local newspaper reports that the pandemic caused severe disruption to businesses in many sectors of the economy.

> *In some parts of the country [the influenza epidemic] has caused a decrease in production of approximately 50% and almost everywhere it has occasioned more or less falling off. The loss of trade which the retail merchants throughout the country have met with has been very large. The impairment of efficiency has also been noticeable. There never has been in this country, so the experts say, so complete domination by an epidemic as has been the case with this one.* (WSJ, Oct. 24, 1918.)

Newspaper accounts in Appendix C also reveal that output declines were seen across many sectors of the economy, including coal and copper mining, shipbuilding, textile production, retail and wholesale trade, and entertainment.[8]

The second take-away from Figure 1 is that the decline in activity was similar in high and low NPI cities. In particular, Panel (a) shows that high and low NPI cities see approximately equal declines in the combined business disruptions index. For example, from September 1918 to February 1919, high NPI cities saw a 41 point decline in the index, while low NPI cities saw a 52 point decline. The patterns in Panel (b) for wholesale trade are similar. Retail trade in Panel (c) also shows similar swings in high and low NPI cities. Panel (d) for manufacturing shows that high and low NPI cities saw similar declines in business conditions. The figure indicates that high NPI cities saw a slightly accelerated decline based on the December 1918 value, as well as a slightly delayed recovery in spring 1919.

To more systematically examine the patterns in Figure 1, Table 2 presents results from estimating difference-in-differences models of the form

$$TradeDisruptions_{ct} = \alpha_c + \tau_t + \beta(NPI_c \times Post_t) + (X_c \times Post_t)\Gamma + \epsilon_{ct}, \tag{2}$$

where $TradeDisruptions_{ct}$ is one of the four trade disruptions indexes from Bradstreet's, NPI_c is one of the three NPI measures, and X_c contains a set of city-level controls, reported

[8] In Appendix A, we provide additional suggestive evidence on the economic effects of the pandemic in the medium run by exploiting cross-sectional variation in exposure to the pandemic across U.S. states and cities and studying manufacturing outcomes. In line with Garrett (2007); Barro et al. (2020), we find that the pandemic may have had adverse economic consequences also in the medium run.

in the table.[9] The estimation period is January 1918 to February 1919, and $Post_t$ is a dummy that equals one from August 1918 onward. Across all three NPI measures, higher NPIs are generally not associated with significant reductions in the combined index (column 1), the wholesale trade index (column 2), retail trade (column 3), or manufacturing (column 4). While the estimates are generally negative, they are small, and none is statistically significant.

Taken together, monthly information on business disruptions indicates that cities that flattened the curve through NPIs did not experience clearly larger disruptions in local business activity. The pandemic itself was disruptive for the economy, but public health interventions did not exacerbate the disruptions.

4.5 Non-pharmaceutical interventions and economic activity in the medium run

Our findings above indicate that cities with stricter NPIs did not experience different degrees of short-term business disruptions. We now ask whether the same is true in the medium run. To do so, we use city-level data from the Census of Manufacturers on employment and output. The advantage of Census data over the high-frequency Bradstreet disruption index is that Census data are measures of actual economic outcomes instead of qualitative measures. Moreover, Census data cover all 43 cities with NPI data. However, a drawback of the Census of Manufactures is that it was only collected every five years until 1919 and every two years from 1919 onwards. To address this drawback, we also proxy for local economic activity using data on total national bank assets per city, which are available at an annual frequency.

We begin by studying the correlation between NPIs and growth in local manufacturing activity between 1914 and 1919. Figure 2a shows a city-level scatterplot of the growth in manufacturing employment between the 1914 and 1919 census years against $NPI\ Intensity$.

[9] Table A4 presents the same table excluding $X_c \times Post_t$ controls. The results without controls generally suggest even smaller effects of NPIs on economic disruption.

If NPIs had a substantial negative effect on the economy, one would expect a negative relation between the two. However, Figure 2a reveals a positive correlation. An important concern is that cities with stricter NPIs could have been on different growth trajectories prior to the 1918 Flu. A natural placebo test to address this concern is to correlate NPIs with the growth in manufacturing employment between the pair of census years preceding the pandemic, 1909-1914. Figure 2b shows that there is no correlation between NPIs and previous growth in manufacturing. These patterns thus provide initial suggestive evidence that NPIs are not associated with weaker economic activity in the aftermath of the 1918 flu.

To more formally study the medium-term impact of NPIs around the 1918 Flu Pandemic and to control for other observable characteristics and longer pre-trends, we estimate a dynamic difference-in-differences equation of the form

$$Y_{ct} = \alpha_c + \tau_t + \sum_{j \neq 1914} \beta_j \, NPI_{c,1918} \, \mathbf{1}_{j=t} + \sum_{j \neq 1914} X_s \, \gamma_j \, \mathbf{1}_{j=t} + \varepsilon_{ct}, \tag{3}$$

where Y_{ct} is a measure of economic activity in city c, such as the log of manufacturing employment or national bank assets, and NPI_{c1918} is one of the NPI measures. The set of coefficients β_j capture the relative dynamics of cities with stricter NPIs. Moreover, X_s is a set of control variables that are interacted with time indicator variables to allow for changes in the relation between outcome variables and controls.

Panels (c)-(d) of Figure 2 present the results from estimating Equation (3) for manufacturing employment using the $NPI\ Intensity$ and $High\ NPI$ measures as regressors.[10] The estimates without controls show that, relative to 1914, cities with stricter NPIs had a higher level of employment from 1919 onward than those with more lenient NPIs. For instance, the estimate for 1919 implies that $High\ NPI$ cities experienced 18% higher employment growth from 1914 to 1919. Further, the confidence bands indicate that growth lower than 2% can be rejected at the 95% level.

[10] Figure A8 shows the results on manufacturing output and value added, as well as with the $NPI\ Speed$ measure.

However, the estimates without controls also show that cities with stricter NPIs grew faster between 1904-1909, indicating a pre-trend from 14 to 9 years before the pandemic. This raises the concern that the results may be driven by more general city-growth patterns. This is not entirely surprising given that most cities with strict NPIs were located further west. As the structure of the U.S. economy and U.S. cities changed quickly around the turn of the 20th century, this implied that cities like Los Angeles and Seattle with stricter NPIs also changed considerably between 1904 and 1909.[11]

One approach to addressing this concern is to control for observable differences across cities with strict and lenient NPIs. The estimates with controls in panels (c) and (d) of Figure 2 indicate that there are no apparent differences between cities with stricter and more lenient NPIs in the years prior to the 1918 Flu. Once we include controls, the estimates after 1918 remain positive but are not always significant. For instance, the point estimate suggests that manufacturing employment in *High NPI* cities is around 5 percent higher in 1919 compared to 1914, but the coefficient is not significant. The 95% confidence intervals suggests that, if anything, cities with stricter NPIs saw a relative increase in manufacturing employment after 1914, and at the lower bound, we can reject growth lower than -6% between 1914 and 1919.

To confirm this visual pattern, Table 3 compares the pre- and post-period average in manufacturing employment and output, while controlling for city observables. The estimates with controls suggest that *High NPI* cities see around 11% higher manufacturing employment and 18% higher output after the pandemic (see column 6) . The estimates using the other two measures of NPIs are not always significant, but the point estimates suggest moderate positive effects. The confidence intervals reject a large negative effect of NPIs on both measures of economic activity.

As mentioned above, a key drawback of using the Census of Manufacturers data is that data are not available from 1915 through 1918. This raises the concern that cities with strict and lenient NPIs may have been on different trajectories in this time period. To address

[11]For a detailed discussion of pre-trends in this context, see Lilley et al. (2020a); Correia et al. (2020); Sant'Anna (2020).

this concern, Figure 2e and Figure 2f depict estimates of Equation (3) using annual data on log total national bank assets as the dependent variable.[12] With controls, there is no indication of a pre-trend in national bank assets for cities with stricter NPIs between 1910 an 1917. This is reassuring because if cities with stricter NPIs were growing at a faster pace before 1918, this should arguably be reflected in the size of the local banking system.

Further, Figure 2e and Figure 2f suggest a slight uptick in bank assets using both the *High NPI* and *NPI Intensity* measure after August 1918. At a minimum, for both NPI measures, we can reject large negative growth in banking assets for the years 1919 and 1920. Moreover, the overall pattern suggests that there was a sight increase in the size of the local banking system in the medium term for cities with strict NPIs, although the confidence bands become wider after 1920. These patterns can be confirmed by comparing the pre- and post-period average in bank assets in Panel C of Table 3, which suggest that national bank assets growth tended to be higher in cities with stricter NPIs after 1918.

5 Discussion and Conclusion

This paper examines the impact of non-pharmaceutical interventions during the 1918 Flu Pandemic on mortality and economic activity. We find that while NPIs flattened the curve of disease transmission, they were not associated with worse economic performance during or after the pandemic. Instead, our findings suggest that the main source of economic disruption was the pandemic itself.

There are several important caveats to keep in mind with our analysis. First, our sample is limited to only 43 cities. Second, we cannot carefully examine pre-trends for manufacturing outcomes in the years 1915, 1916, or 1917, as the data is not available at an annual frequency. Third, the economic environment toward the end of 1918 was unusual

[12] The reporting date for the OCC data is either August or September of a given year. We normalize coefficients to August 31, 1918. In order to account for Liberty Bond issuance impacting the local banking system (see, e.g., Hilt and Rahn, 2020), we control for the ratio of the amount subscribed to the Third Liberty Bond to total bank assets in 1918. Moreover, to account for the 1913 founding of the Federal Reserve System as well as for heterogeneity across Federal Reserve Districts, we also include reserve district-year fixed effects.

due to the end of WWI. Fourth, our cross-regional analysis does not allow us to capture aggregate equilibrium effects of NPIs.

With these caveats in mind, our findings nonetheless raise the question: Why might NPIs not be economically harmful during a pandemic, and possibly even beneficial in the medium-term? It is challenging to shed light on the exact mechanisms through which NPIs affected the economy with the limited data available for 1918, but we offer some potential channels. The direct effect of NPIs such as theater closures and public gathering bans is contractionary, as these policies necessarily restrict economic activity. However, the pandemic itself can be highly disruptive for the economy. Many activities that NPIs restrict would likely not have occurred even in the absence of NPIs. To avoid contracting the virus, households cut back on consumption and labor supply (see, e.g., Eichenbaum et al., 2020), while businesses reduce investment in response to labor shortages, lower demand, and increased uncertainty. As a result, the counterfactual without NPIs would still involve a downturn.

Moreover, NPIs may have indirect economic benefits by addressing the root of the economic disruption—the pandemic itself— in a coordinated fashion. Mitigating the pandemic can prevent an ultimately worse economic downturn. For example, Bodenstein et al. (2020) present a two-sector model where NPIs mitigate the decline in output by flattening the curve, even without significantly reducing cumulative infection. In their model, production in the sector essential to the economy is less disrupted when illness and the risk of contracting a virus at a given point in time is lower, leading to a smaller overall decline in output. Further, by reducing cumulative infection rates, NPIs may have medium-term economic benefits by directly reducing illness and mortality and by reducing the costs associated with increased morbidity.

More specific historical details also shed light on why NPIs in 1918 did not worsen the economic downturn. NPIs implemented in 1918 were milder than the measures adopted in some countries during COVID-19. More severe measures such as closures of business likely increase the cost of NPIs. School closures were less costly in 1918, as female labor force participation was lower. Estimates suggest that 1918 Flu was more deadly than COVID-19, especially for prime-age workers, which also suggests more severe economic impacts of the 1918 Flu and greater medium-run benefits of NPIs. The 1918 H1N1 virus also had a shorter incubation period than COVID-19, which facilitated identifying and isolating suspected cases. As a result, we stress the limits of external validity of lessons from the 1918 Flu Pandemic.

Despite these important differences, ongoing research finds that NPIs implemented in 2020 have reduced disease transmission without leading to substantial further economic disruptions (see, e.g., Andersen et al., 2020; Lin and Meissner, 2020), and countries that implemented NPIs in the earlier stages of the COVID-19 pandemic have better short-term economic outcomes (see, e.g., Demirgüç-Kunt et al., 2020). We look forward to future research that disentangles the net impact, direct costs, and indirect benefits of NPIs implemented during COVID-19 in both the short and medium run.

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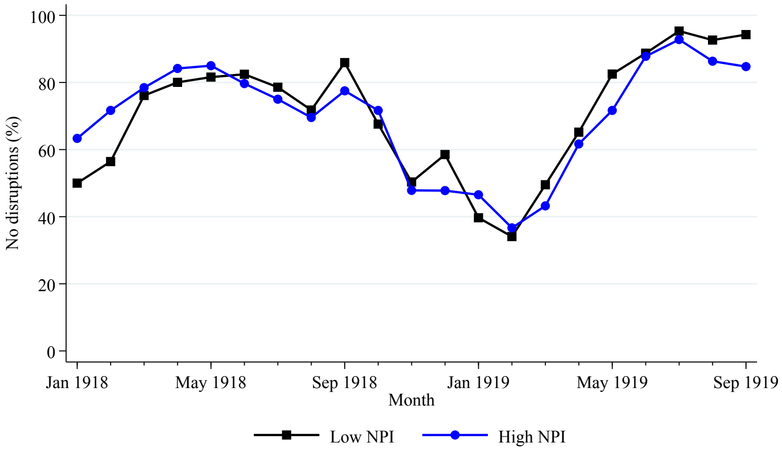
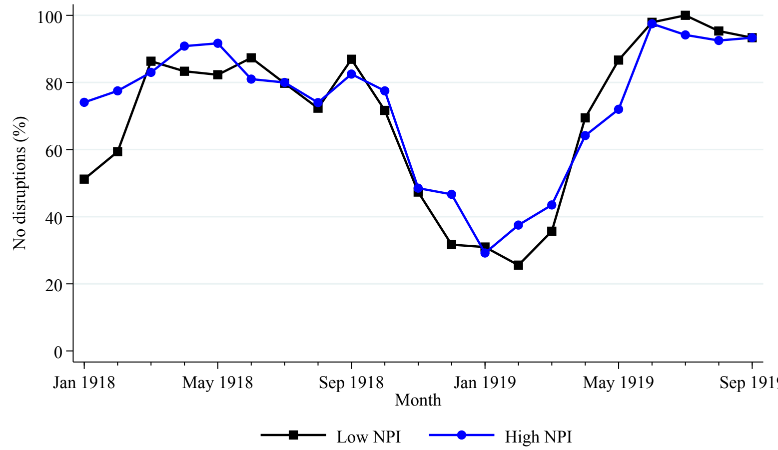
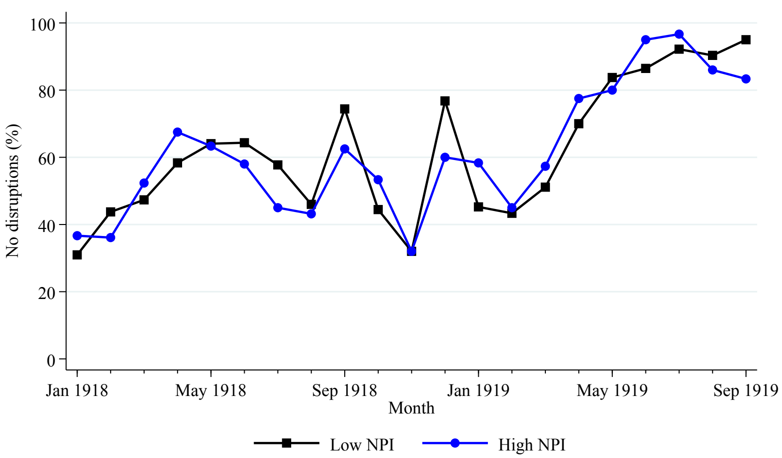


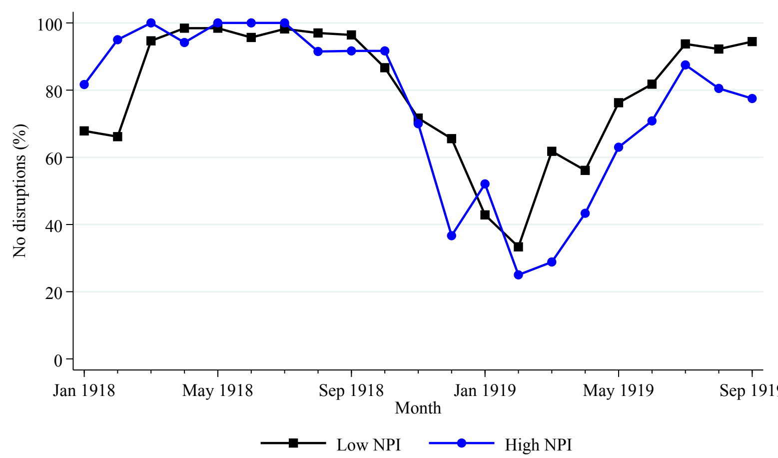
(a) Combined index (wholesale, retail, and manufacturing)



(b) Wholesale

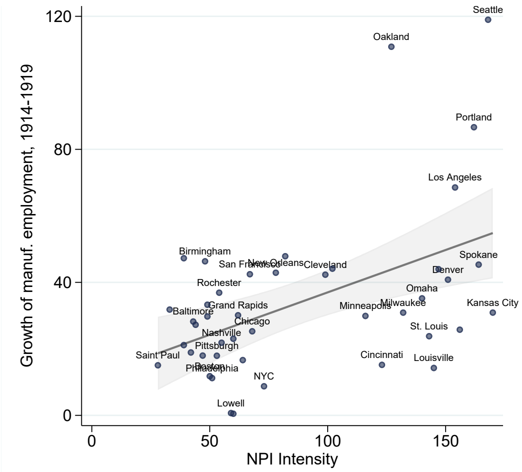


(c) Retail

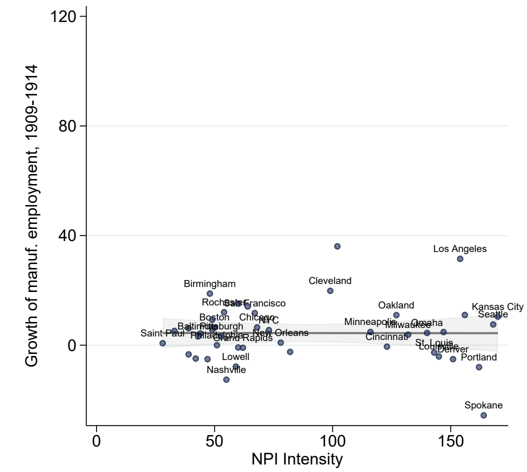


(d) Manufacturing

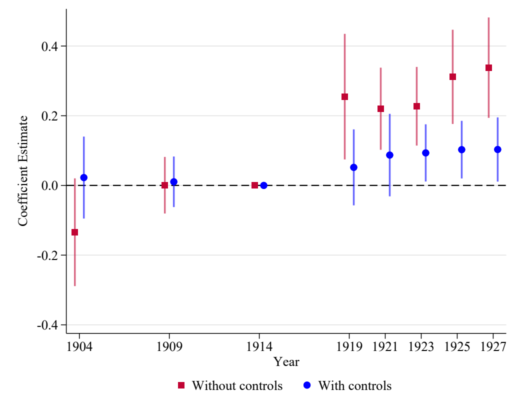
Figure 1: **Non-pharmaceutical interventions and short-run economic disruptions.** This figure plots the average across high and low NPI cities of an indicator variable for whether the Bradstreet Trade conditions suggest "disruptions" in specific sectors. High NPI cities are defined as cities with above median *NPI Intensity* and *NPI Speed*.



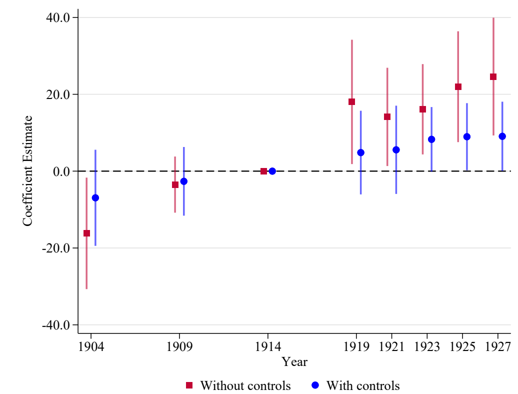
(a) *NPI Intensity* and log manufacturing employment growth 1914 to 1919.



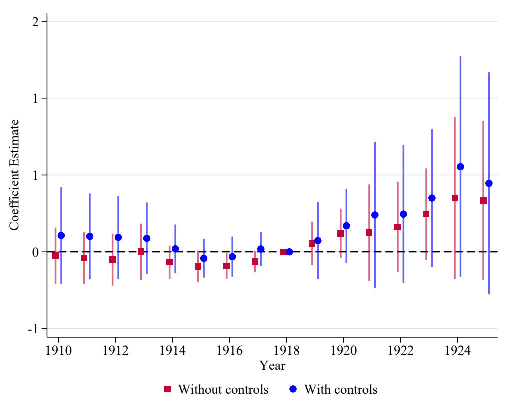
(b) *NPI Intensity* and log manufacturing employment growth 1909 to 1914.



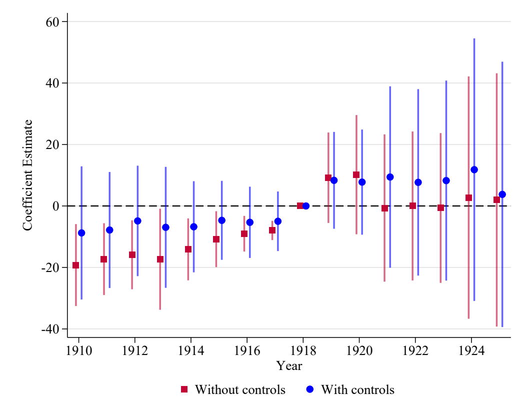
(c) *NPI Intensity* and log manufacturing employment.



(d) *High NPI* and log manufacturing employment.



(e) *NPI Intensity* and log National Bank Assets.



(f) *High NPI* and log National Bank Assets.

Figure 2: **Non-pharmaceutical interventions in fall 1918 and medium-run economic outcomes.** Panel (c) through (f) show results from estimating Equation (3) for various outcomes with and without controls. 95% confidence bands.

Table 1: **Non-pharmaceutical interventions, peak mortality, and cumulative mortality.**

	Panel A: Peak Mortality								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$NPI\ Intensity_c$	-0.62			-0.55			-0.45		
	(0.11)			(0.16)			(0.19)		
$NPI\ Speed_c$		-1.00			-0.55			-0.42	
		(0.96)			(1.13)			(0.87)	
$High\ NPI_c$			-56.8			-52.6			-44.2
			(11.5)			(14.6)			(17.4)
R^2	.33	.025	.32	.4	.22	.43	.45	.34	.47
Effect size (%)	-55.4	-22.8	-57.6	-49.3	-12.5	-53.3	-40.5	-9.7	-44.8
	Panel B: Cumulative Excess Mortality								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$NPI\ Intensity_c$	-1.03			-1.28			-0.62		
	(0.37)			(0.46)			(0.42)		
$NPI\ Speed_c$		-3.20			-2.82			-2.21	
		(2.61)			(3.37)			(1.82)	
$High\ NPI_c$			-140.8			-163.2			-109.3
			(35.0)			(40.5)			(42.3)
R^2	.12	.033	.26	.24	.12	.37	.52	.5	.6
Effect size (%)	-18	-14.2	-27.9	-22.4	-12.5	-32.3	-10.9	-9.8	-21.6
N	43	43	43	43	43	43	43	43	43
Baseline Controls				Yes	Yes	Yes	Yes	Yes	Yes
Lagged Mort. Control							Yes	Yes	Yes

Notes: This table presents city-level regressions of peak mortality (panel A) and cumulative excess mortality (panel B). Mortality refers to influenza and pneumonia mortality. Data on peak and cumulative mortality and NPIs are from Markel et al. (2007). Peak mortality is the weekly excess death rate per 100,000 in the first peak of the fall 1918 pandemic. Cumulative excess mortality is the total excess death rate from September 8, 1918 to February 22, 1919. "Baseline Controls" are city log 1900 and 1910 population, city 1914 manufacturing employment to 1910 population, city public health spending per capita, city density, and state 1910 agriculture employment share. "Lagged Mort. Control" is the city-level influenza and pneumonia mortality in 1917. "Effect size" for $NPI\ Intensity$, $NPI\ Speed$, and $High\ NPI$ variables are calculated as $100\,\hat{\beta}\,\overline{NPI\ Intensity}/\overline{Y}$, $100\,\hat{\beta}\,\overline{DaysToPeak}/\overline{Y}$, and $100\,\hat{\beta}/\overline{Y}$, respectively. Here $\overline{NPI\ Intensity} = 88$, $\overline{DaysToPeak} = 22.4$, and $\overline{Y}$ is the mean of the dependent variable. $DaysToPeak$ is the number of days between the acceleration and the peak of deaths rates. Robust standard errors in parentheses.

Table 2: **Non-pharmaceutical interventions and short-term economic disruptions in Bradstreet's Trade Conditions.**

	Combined: W+R+M	Wholesale Trade	Retail Trade	Manufacturing
Panel A: NPI Intensity Measure				
	(1)	(2)	(3)	(4)
$NPI\ Intensity_c \times Post_t$	-0.088	-0.12	-0.030	-0.12
	(0.065)	(0.13)	(0.15)	(0.12)
R^2 (Within)	.0097	.0074	.00036	.012
N	344	343	342	340
Panel B: NPI Speed Measure				
	(1)	(2)	(3)	(4)
$NPI\ Speed_c \times Post_t$	-0.11	-0.64	0.097	0.12
	(0.45)	(0.61)	(0.94)	(0.33)
R^2 (Within)	.00052	.0069	.00013	.00041
N	344	343	342	340
Panel C: High NPI Measure				
	(1)	(2)	(3)	(4)
$High\ NPI_c \times Post_t$	-3.86	-6.69	7.65	-15.3
	(7.78)	(13.4)	(14.5)	(10.2)
R^2 (Within)	.0028	.0033	.0035	.028
N	344	343	342	340
No of cities	25	25	25	25
City and Time FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Notes: This table presents estimates of equation (2). The dependent variables are monthly city-level indexes of economic disruptions that take a value of 100 for "No disruptions" and 0 for "Disruptions" (see Appendix D for details). Controls interacted with $Post_t$ are log 1900 and 1910 city population, 1910 city density, 1917 health spending per capita, manufacturing employment in 1914 to 1910 population, 1910 state agriculture employment share, and 1917 influenza and pneumonia mortality. Standard errors are clustered at the city level.

Table 3: **Non-pharmaceutical interventions and local manufacturing employment, output, and bank assets.**

Panel A: Manufacturing Employment						
	(1)	(2)	(3)	(4)	(5)	(6)
$NPI\ Intensity_c \times Post_t$	0.317	0.080				
	(0.081)	(0.038)				
$NPI\ Speed_c \times Post_t$			0.753	0.381		
			(0.402)	(0.258)		
$High\ NPI_c \times Post_t$					25.757	10.879
					(8.201)	(4.834)
R^2 (Within)	.19	.45	.03	.45	.14	.46
N	344	344	344	344	344	344
No of Cities	43	43	43	43	43	43
Panel B: Manufacturing Output						
	(1)	(2)	(3)	(4)	(5)	(6)
$NPI\ Intensity_c \times Post_t$	0.213	0.029				
	(0.088)	(0.063)				
$NPI\ Speed_c \times Post_t$			0.714	0.371		
			(0.397)	(0.390)		
$High\ NPI_c \times Post_t$					20.503	9.497
					(9.009)	(7.472)
R^2 (Within)	.066	.25	.021	.25	.071	.26
N	344	344	344	344	344	344
No of Cities	43	43	43	43	43	43
Panel C: National Bank Assets						
	(1)	(2)	(3)	(4)	(5)	(6)
$NPI\ Intensity_c \times Post_t$	0.246	0.265				
	(0.129)	(0.149)				
$NPI\ Speed_c \times Post_t$			1.124	1.125		
			(0.611)	(0.619)		
$High\ NPI_c \times Post_t$					15.657	14.228
					(10.973)	(9.281)
R^2 (Within)	.026	.083	.034	.089	.021	.075
N	683	683	683	683	683	683
No of Cities	43	43	43	43	43	43
City and Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes

Notes: This table reports results from estimating a regression of the form:

$$Y_{ct} = \alpha_c + \tau_t + \beta \times NPI_{c1918} \times \text{Post}_t + \gamma \times X_t \times \text{Post}_t + \epsilon_{ct},$$

where NPI_c is either *High NPI*, *NPI Speed* or *NPI Intensity*; Post_t=1 from 1919 onwards; X_s contains the 1910 state-level agriculture employment share, city-level 1914 manufacturing to 1910 population, city-level 1910 and 1900 log population, 1910 city density, per capita city health spending, and city-level mortality in 1917. The dependent variable is average manufacturing employment for Panel A and output for Panel B, using data from the 1904, 1909, 1914, 1919, 1921, 1923, 1925, and 1927 census. In Panel C, the dependent variable is total assets of national banks per city, using data at annual frequency from 1910 to 1925. Further, columns 2, 4, and 6 of Panel C also control for city-level subscriptions to the third Liberty Loan (May 1918) normalized by total national bank assets as of August 1918, and Federal Reserve District fixed effects. Standard errors clustered at the city level in parentheses.

ONLINE APPENDIX

Appendix A: Mortality and Manufacturing Outcomes

Appendix B: Supplementary Tables and Figures

Appendix C: Narrative Evidence from Historical Newspaper Archives

Appendix D: Data

A The Effects of Mortality On Manufacturing Outcomes

In this appendix, we provide suggestive evidence that the severity of the 1918 Flu is correlated with lower economic activity using variation across U.S. states and cities. In particular, we use two approaches to estimate the impact of the 1918 Flu Pandemic on the real economy. The first approach is a standard difference-in-differences specification:

$$Y_{st} = \alpha_s + \tau_t + \sum_{j \neq 1914} \beta_j \, Mortality_{s,1918} \, \mathbf{1}_{j=t} + \sum_{j \neq 1914} X_s \gamma_j \mathbf{1}_{j=t} + \varepsilon_{st} \tag{4}$$

where Y_{st} is an outcome such as log manufacturing employment in a local area s in year t. The sequence of coefficients β_j captures the dynamics of severely affected areas such as Pennsylvania relative to mildly affected areas such as Minnesota. Our baseline measure of local exposure to the 1918 pandemic is the local mortality rate from influenza and pneumonia per 100,000 inhabitants in 1918, $Mortality_{s,1918}$.

The identifying assumption behind estimation of (4) is parallel trends, i.e., $Mortality_{s,1918}$ is not correlated with other time-varying, regional economic shocks. While there is significant geographic variation in the severity of the pandemic, studies using state-level variation in $Mortality_{s,1918}$ argue that the spread of the virus was somewhat arbitrary and that regional variation in mortality was largely orthogonal to *ex ante* economic conditions (Brainerd and Siegler, 2003). Eastern states and cities were more severely affected, as the influenza arrived from Europe and travelled from east to west, but there is variation within U.S. regions.

It is not obvious that $Mortality_{s,1918}$ would be correlated with other economic shocks that differentially affected U.S. regions. Nevertheless, the period 1918-1921 witnessed a variety of macroeconomic shocks, most notably the end of WWI, a large agricultural boom and bust cycle, and a severe recession in 1920-21, (see, e.g., Velde, 2020). To account for potential differential exposure to these shocks, at the state level we control for the agriculture employment share, manufacturing employment share, urban population share, population, income per capita, and a war production dummy from Garrett (2007), represented by X_s in (4). At the city level, we control for 1910 population, manufacturing employment in 1914 to 1910 population, health expenditures in 1917 to 1910 population, city density in 1910, state agriculture employment share in 1910, and the state-level war production dummy. As in the main body of the text, all controls are measured before the

1918 pandemic and are always interacted with time fixed effects to control for time-varying shocks that are correlated with baseline differences across regions. We also present tests using a variety of additional controls.

A concern with exploiting variation in $Mortality_{s,1918}$ is that mortality may be endogenous to economic outcomes. For example, higher mortality may have been a result of dire economic trajectories, so the parallel trends assumption may not be satisfied using variation in $Mortality_{s,1918}$. To address this challenge, in our second approach, we instrument $Mortality_{s,1918}$ with *ex ante* exposure to military camps, similar to the empirical strategy suggested by Hilt and Rahn (2020). WWI military camps were important clusters and vectors of infection. For instance, Fort Devens in Boston was the first cluster after the virus arrived from Europe, and it was transmitted to other camps via troop movements (Crosby, 2003). Troops lived in close quarters, leading to rapid spread within camps, and troop movements disseminated virus across camps and to nearby cities. Camp location choice often also driven by historical military sites. For instance, Camp Funston in Kansas was built close to the historical military base in Fort Riley. Camp Colt in Pennsylvania was installed the historical Civil War site of Gettysburg.

We collect information on the location and size of 40 military camps used on the continental U.S. in use during WWI (see Appendix D.3 for details). For each city, we construct a city-level instrument based on the weighted inverse distance to military camps:

$$Z_c = \sum_j \frac{\ln(camp\, size_j)}{\ln(dist_{c,j})}.$$

$camp\, size_j$ is the average number of troops stationed in camp j from July to September 1918, the onset of the second wave of the 1918 Flu Pandemic. $dist_{c,j}$ is the shortest path distance between city c and camp j. In this formulation, larger camps receive a higher weight, and each camp's relative importance to city c is weighted by the distance to city c. We also construct this instrument at the state-level using the distance between camp j and the state centroid.

Our military camps instrument Z_c is a reasonably strong predictor of 1918 city-level influenza and pneumonia mortality at the city-level. The first-stage F-statistic of a regression of 1918 city level mortality on Z_c is 19.9 without controls and 13.6 with baseline controls. At the state level, the first-stage F-statistics for the instrument are 3.25 without controls and 11.31 with controls.

One potential concern with military camps exposure as an instrument for $Mortality_{s,1918}$ is that military camps may have been close to locations with war production. During the war, these locations may have benefited from stronger growth in output. This concern is mitigated by examining the evolution of manufacturing outcomes from 1914, before the ramp-up in production, to 1919. Moreover, we find that controlling for a dummy variable for whether a state was heavily involved in war production from Garrett (2007) does not alter the results. Based on the measure from Garrett (2007), the location of military camps does not appear to be strongly related to major centers of war production.

A.1 Results

Panels (a) and (b) of Figure A4 present the results from estimation of (4) for state-level manufacturing employment and employment-to-population. The results show that high $Mortality_{s,1918}$ exposure is associated with a significant decline in manufacturing employment and output from 1914 to 1919 census years. Both log employment and the employment-to-population ratio decline, indicating that the fall in employment is not only a direct consequence of deaths caused by the pandemic. Instead, the pandemic appears to be correlated with a broader disruption in manufacturing activity.

Panel (c) of Figure A4 presents the city-level estimates of the impact of 1918 Flu Pandemic exposure on manufacturing outcomes. Because the OLS difference-in-differences estimates display a more severe negative pre-trend at the city-level, at the city level the estimates of $Mortality_{c,1918}$ instrumented by exposure to military camps can arguably be interpreted as a more reliable estimate of the economic impact of the pandemic. The instrumental variables difference-in-differences estimates at the city level indicate that the pandemic was associated with a decline in log manufacturing employment and output in high relative to low exposure cities. Further, the city-level instrumented estimates suggest there was a partial recovery in activity from 1919 to 1921 in more relative to less affected areas, but the relative decline seems nonetheless somewhat persistent.

Our findings on the negative effect of mortality during the 1918 Flu on manufacturing outcomes are in line with existing evidence on the adverse effect of the Flu Barro et al. (2020), Guimbeau et al. (2019), Karlsson et al. (2014), and Almond (2006) However, it is important to note that our findings presented in this section are not necessarily robust to using other outcome variables. For instance, our evidence presented in Section 4 suggests that the disruptions from the Flu had largely passed by mid 1919. Further, Velde (2020) carefully documents that the impact of the Flu was rather short term than long term across a variety of different data sources. Thus, our evidence on manufacturing outcomes need to be evaluated with this caveat in mind.

Finally, the findings on mortality discussed in this section can also be considered jointly with the effect of NPIs. Figure A5 shows the city-level correlation between 1918 Flu mortality and the growth in manufacturing employment from 1914 to 1919 census years. As the figure reveals, higher mortality during the 1918 Flu is associated with a relative decline in economic activity. The figure further splits cities into those that were more and less aggressive in their use of NPIs. Cities that implemented stricter NPIs (green dots) tend to be clustered in the upper-left region (low mortality, high growth), while cities with more lenient NPIs (red dots) are clustered in the lower-right region (high mortality, low growth). This suggests that NPIs can play a role in attenuating mortality, but without reducing economic activity. If anything, cities with stricter NPIs during the pandemic perform better in the year after the pandemic.

B Supplementary Figures and Tables

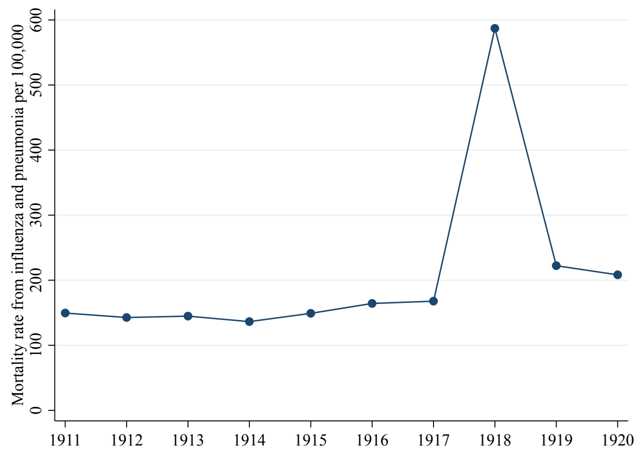


Figure A1: **U.S. mortality rate from influenza and pneumonia, 1911-1920.** Source: CDC *Mortality Statistics*.

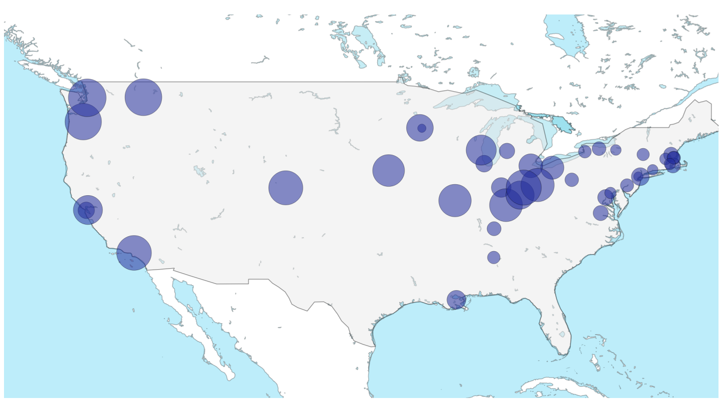
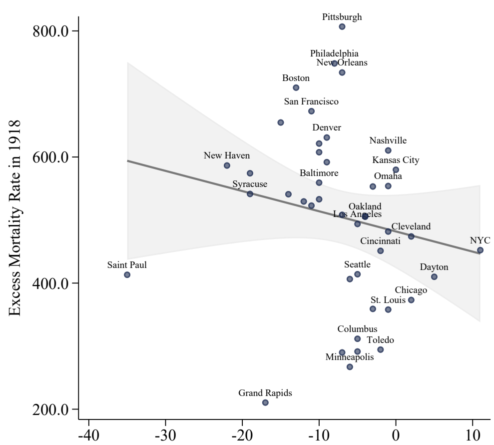
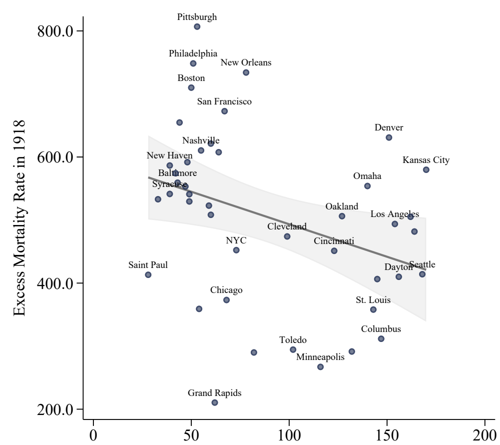


Figure A2: **Sample of 43 cities with NPIs in fall 1918.** Radius is scaled by *NPI Intensity* .

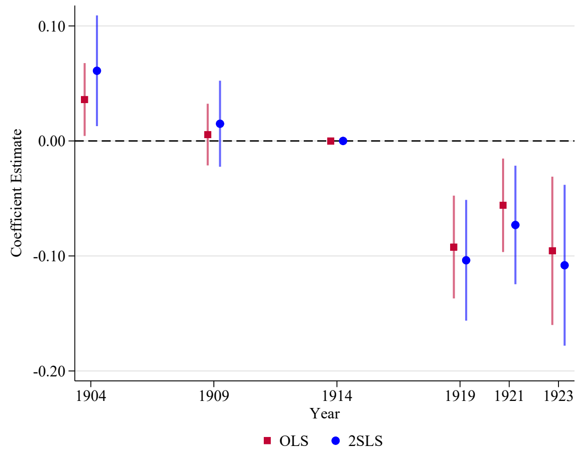


(a) Excess mortality and speed of NPIs.

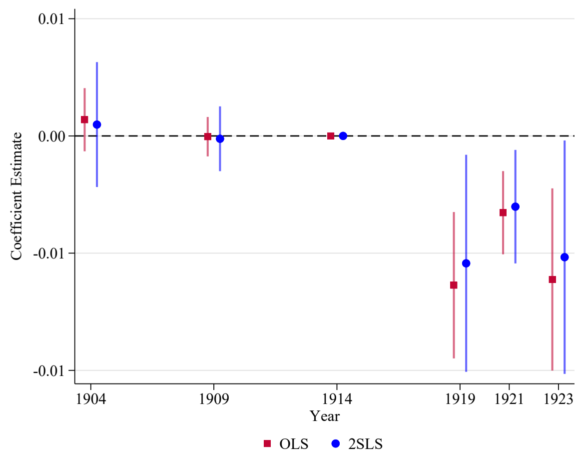


(b) Excess mortality and NPI intensity.

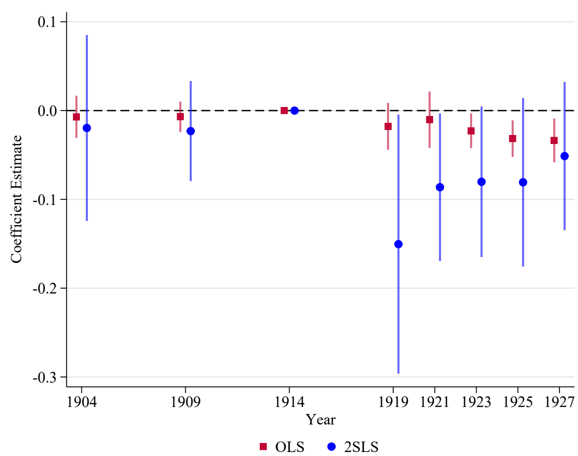
Figure A3: **Non-pharmaceutical interventions and city-level excess mortality.** This figure correlates the excess pneumonia and influenza related mortality (24 week excess mortality) with the speed and intensity of NPI implementation during fall 1918. Data are from Markel et al. (2007).



(a) Log manufacturing employment (state-level)



(b) Manufacturing employment to population ratio (state-level)



(c) Manufacturing employment (city-level)

Figure A4: **Exposure to the 1918 Flu Pandemic and manufacturing employment.** Results from estimating equation (4) at the state and city level when using mortality in 1918 or mortality instrumented by the exposure to military camps as the treatment. 95% confidence bands.

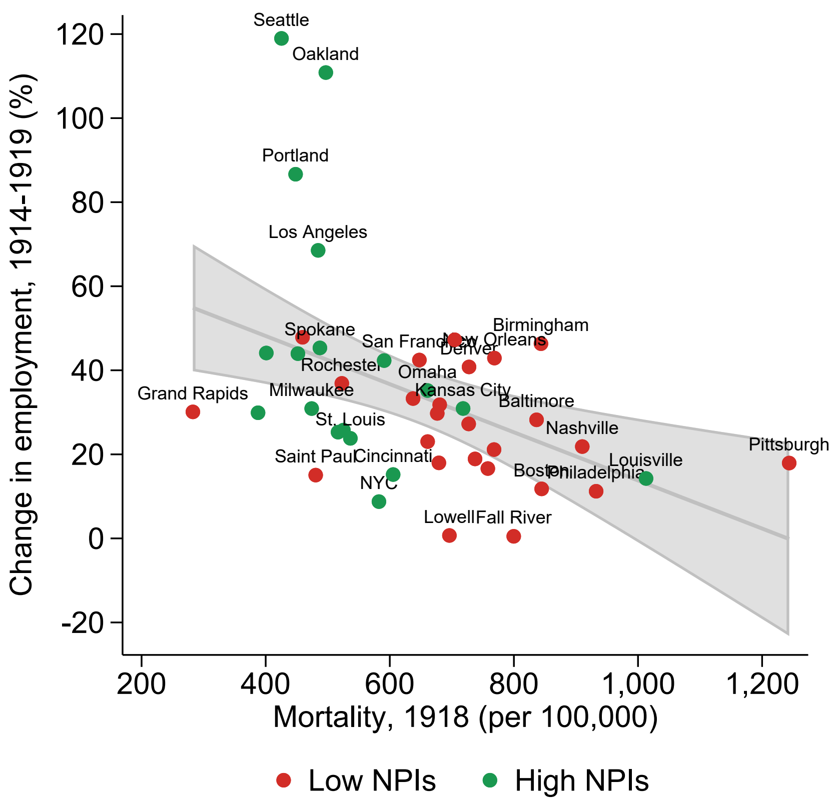
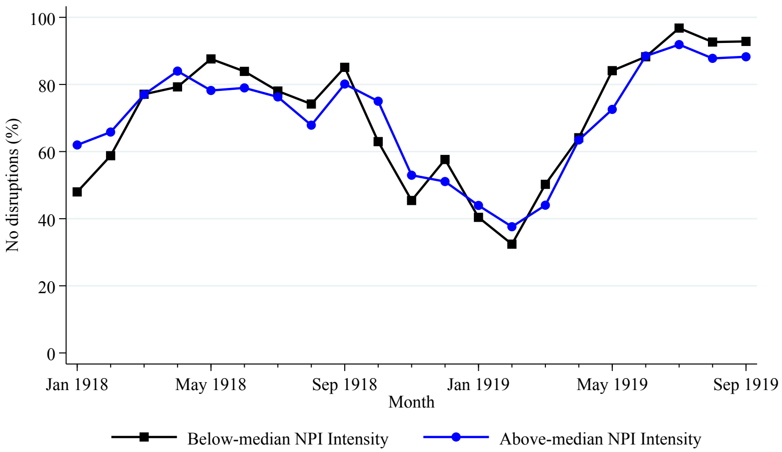
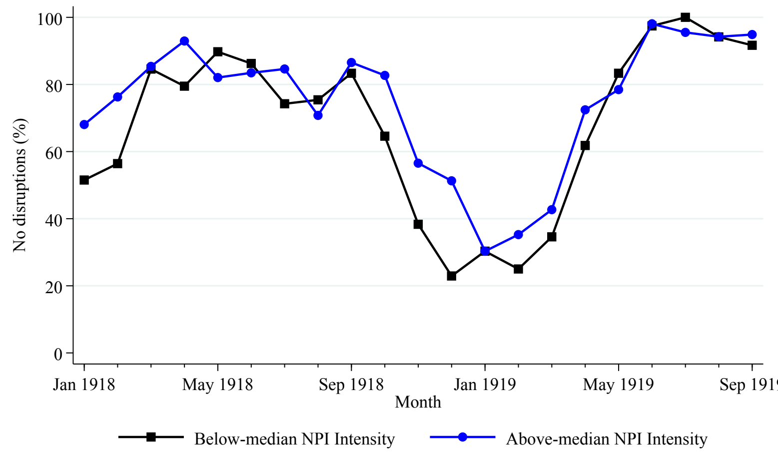


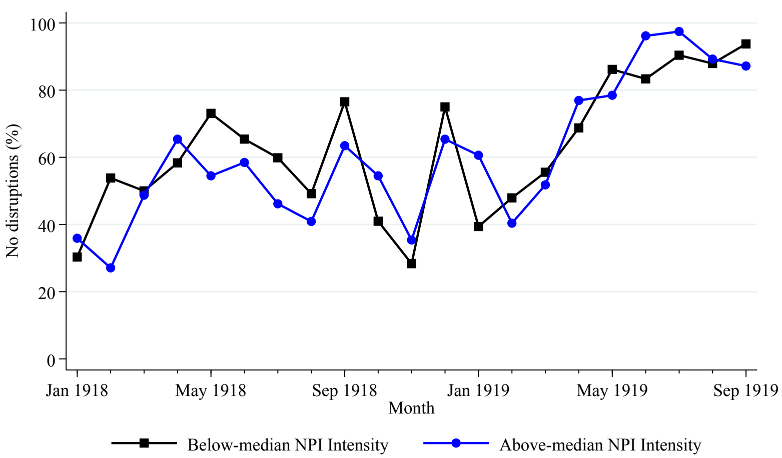
Figure A5: **1918 Flu Pandemic depressed the economy, but public health interventions did not.** Dots represent city-level 1918 influenza mortality and the change in log manufacturing employment around the 1918 Flu Pandemic. Manufacturing employment is available for 1914 and 1919 from the Census of Manufactures. Green (red) dots are cities with non-pharmaceutical intervention intensity above (below) the median in fall 1918 based on Markel et al. (2007).



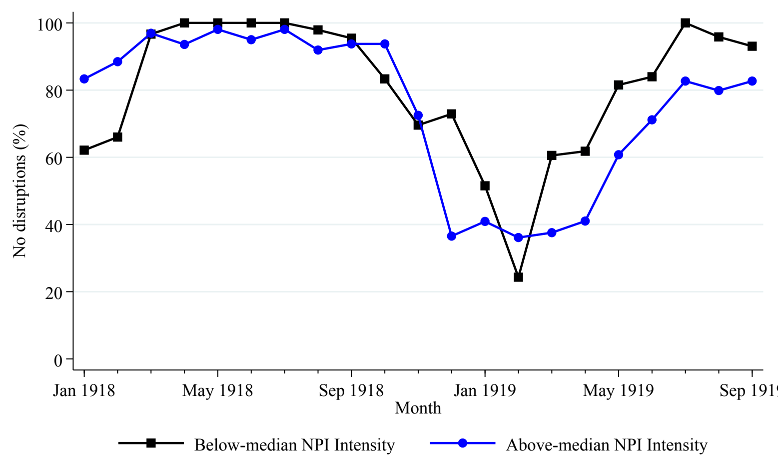
(a) Combined



(b) Wholesale

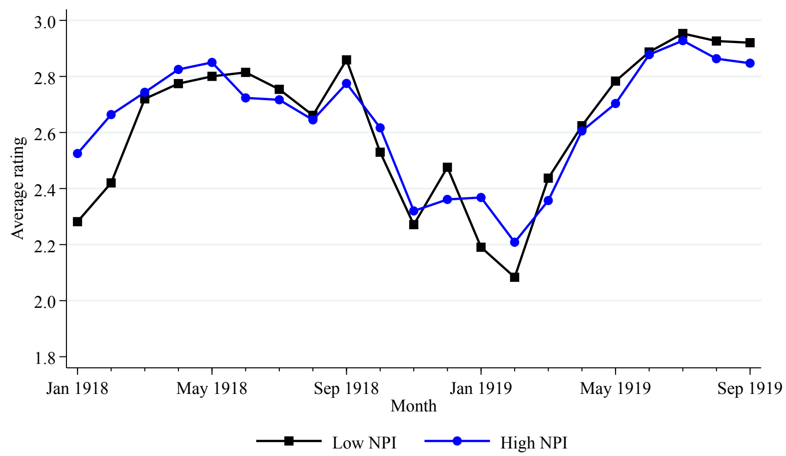


(c) Retail

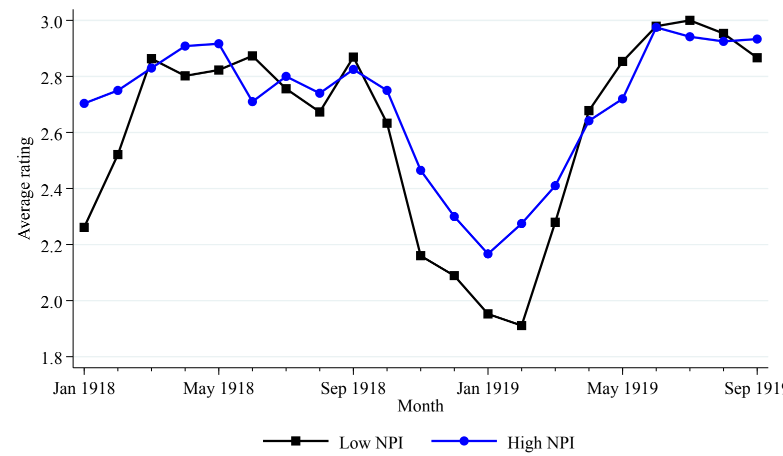


(d) Manufacturing

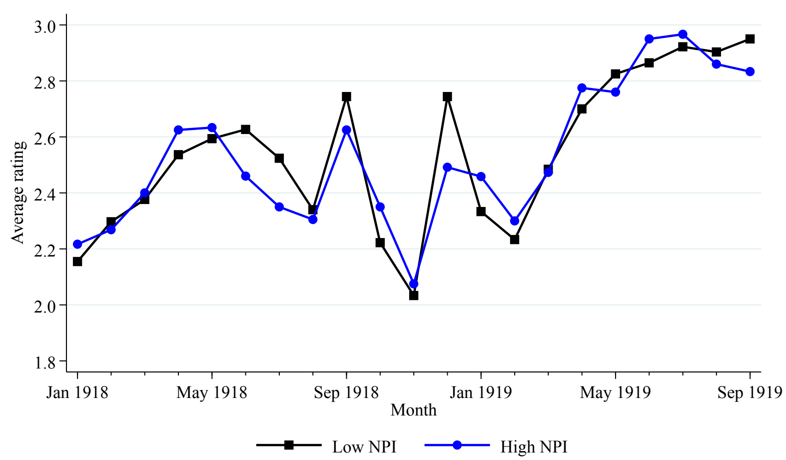
Figure A6: **Non-pharmaceutical interventions and short-term economic disruptions: Robustness to splitting by above- and below-median** ***NPI Intensity*****.**



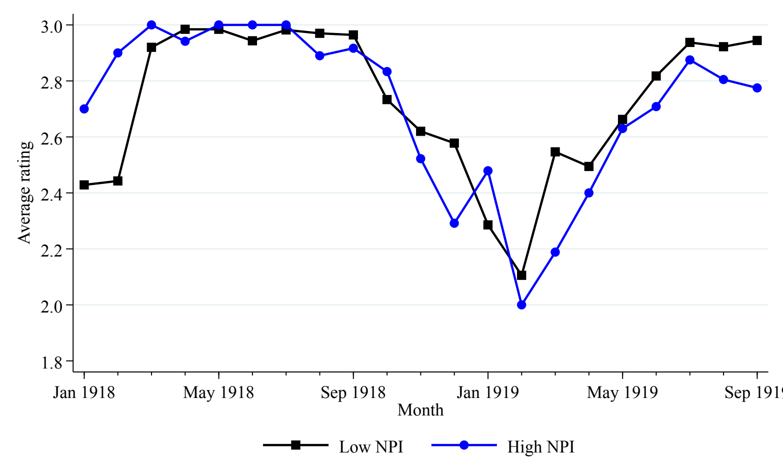
(a) Combined



(b) Wholesale

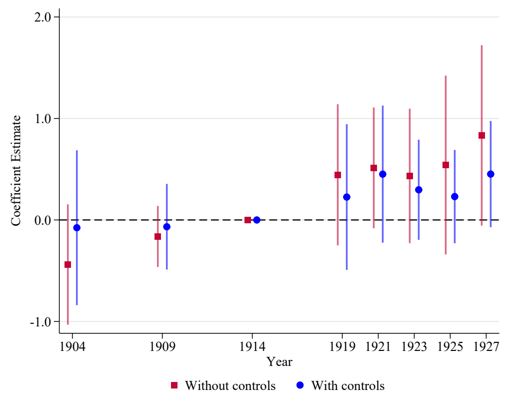


(c) Retail

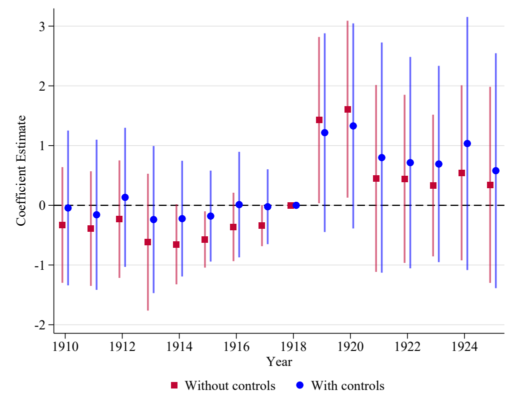


(d) Manufacturing

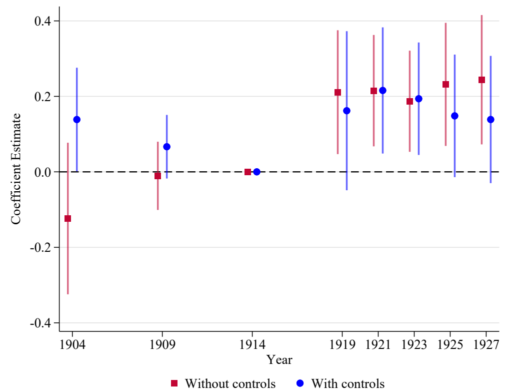
Figure A7: **Non-pharmaceutical interventions and short-term economic disruptions: Robustness to a three-step index of trade conditions.** High NPI cities are defined as cities with above median *NPI Intensity* and *NPI Speed*.



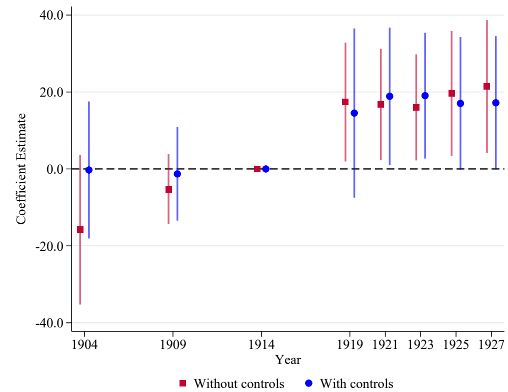
(a) *NPI Speed* and log manufacturing employment.



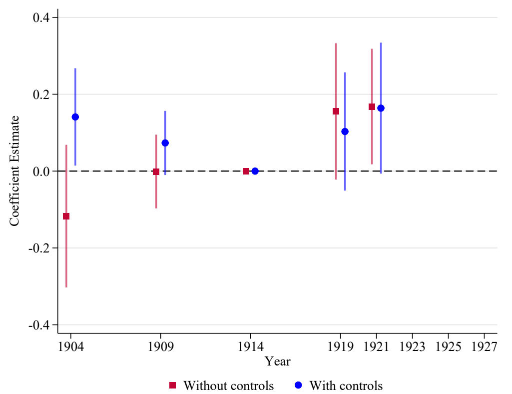
(b) *NPI Speed* and national bank assets.



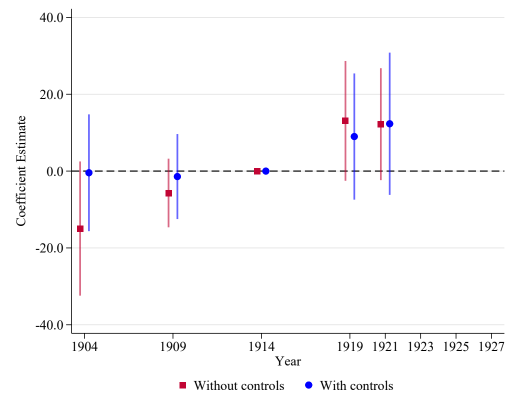
(c) *NPI Intensity* and log manufacturing value.



(d) *High NPI* and log manufacturing value.



(e) *NPI Intensity* and log manufacturing value added.



(f) *High NPI* and log manufacturing value added.

Figure A8: **Non-pharmaceutical interventions in fall 1918 and medium-run economic outcomes.** All panels show results from estimating Equation (3) for various outcomes with and without controls. 95% confidence bands.

Table A1: **Non-pharmaceutical interventions (NPI) in 43 cities during Fall 1918** (Markel et al., 2007).

City	State	First Case	Mortality Acc. Date	Response Date	$NPI\ Speed_{c1918}$	$NPI\ Intensity_{c1918}$	$Mortality_{c1917}$	$Mortality_{c1918}$
Albany	New York	Oct/06/1918	Oct/15/1918	Oct/18/1918	-3	47	187.4	679.1
Baltimore	Maryland	Sep/27/1918	Oct/08/1918	Oct/18/1918	-10	43	251.9	836.5
Birmingham	Alabama	Oct/03/1918	Oct/09/1918	Oct/18/1918	-9	48	334.7	843.6
Boston	Massachusetts	Sep/13/1918	Sep/21/1918	Oct/04/1918	-13	50	228	844.7
Buffalo	New York	Oct/03/1918	Oct/07/1918	Oct/19/1918	-12	49	184	637.5
Cambridge	Massachusetts	Sep/13/1918	Sep/20/1918	Oct/04/1918	-14	49	164.2	676.5
Chicago	Illinois	Sep/26/1918	Oct/07/1918	Oct/05/1918	2	68	201.7	516.6
Cincinnati	Ohio	Oct/03/1918	Oct/13/1918	Oct/15/1918	-2	123	171.3	605.4
Cleveland	Ohio	Sep/29/1918	Oct/16/1918	Oct/14/1918	2	99	198.5	590.9
Columbus	Ohio	Sep/29/1918	Oct/15/1918	Oct/20/1918	-5	147	168.1	451.9
Dayton	Ohio	Sep/29/1918	Oct/14/1918	Oct/09/1918	5	156	157.8	525.2
Denver	Colorado	Sep/26/1918	Oct/06/1918	Oct/15/1918	-9	151	134.4	727.7
Fall River	Massachusetts	Sep/18/1918	Sep/25/1918	Oct/05/1918	-10	60	229.7	799.7
Grand Rapids	Michigan	Oct/02/1918	Oct/11/1918	Oct/28/1918	-17	62	89.6	282.7
Indianapolis	Indiana	Oct/01/1918	Oct/09/1918	Oct/16/1918	-7	82	156.6	459.4
Kansas City	Missouri	Sep/29/1918	Oct/05/1918	Oct/05/1918	0	170	205	718.1
Los Angeles	California	Oct/06/1918	Oct/15/1918	Oct/20/1918	-5	154	93.3	484.5
Louisville	Kentucky	Sep/22/1918	Oct/10/1918	Oct/16/1918	-6	145	209.5	1012.9
Lowell	Massachusetts	Sep/18/1918	Sep/25/1918	Oct/06/1918	-11	59	183.6	696.1
Milwaukee	Wisconsin	Sep/23/1918	Oct/15/1918	Oct/20/1918	-5	132	186.3	474.1
Minneapolis	Minnesota	Sep/30/1918	Oct/15/1918	Oct/21/1918	-6	116	126.3	387.7
Nashville	Tennessee	Sep/30/1918	Oct/15/1918	Oct/16/1918	-1	55	188.6	910.2
New Haven	Connecticut	Sep/23/1918	Oct/02/1918	Oct/24/1918	-22	39	236	768
New Orleans	Louisiana	Sep/19/1918	Oct/10/1918	Oct/17/1918	-7	78	178.5	768.6
New York	New York	Sep/14/1918	Oct/08/1918	Sep/27/1918	11	73	204.5	582.5
Newark	New Jersey	Sep/15/1918	Oct/09/1918	Oct/19/1918	-10	33	184	680.4
Oakland	California	Oct/10/1918	Oct/17/1918	Oct/21/1918	-4	127	96.3	496.9
Omaha	Nebraska	Sep/27/1918	Oct/13/1918	Oct/14/1918	-1	140	207.1	660.8
Philadelphia	Pennsylvania	Sep/05/1918	Oct/04/1918	Oct/12/1918	-8	51	228	932.5
Pittsburgh	Pennsylvania	Sep/13/1918	Oct/06/1918	Oct/13/1918	-7	53	380.4	1243.6
Portland	Oregon	Oct/11/1918	Oct/16/1918	Oct/20/1918	-4	162	72.4	448.2
Providence	Rhode Island	Sep/17/1918	Sep/26/1918	Oct/15/1918	-19	42	221.7	737.4
Richmond	Virginia	Sep/30/1918	Oct/08/1918	Oct/15/1918	-7	60	199.5	661
Rochester	New York	Oct/01/1918	Oct/15/1918	Oct/18/1918	-3	54	151.7	522.7
Saint Paul	Minnesota	Sep/30/1918	Oct/11/1918	Nov/15/1918	-35	28	112	480.6
San Francisco	California	Oct/03/1918	Oct/16/1918	Oct/27/1918	-11	67	126.4	647.7
Seattle	Washington	Oct/03/1918	Oct/10/1918	Oct/15/1918	-5	168	58.9	425.5
Spokane	Washington	Oct/07/1918	Oct/18/1918	Oct/19/1918	-1	164	102.5	487.4
St. Louis	Missouri	Oct/02/1918	Oct/16/1918	Oct/17/1918	-1	143	227	536.5
Syracuse	New York	Sep/21/1918	Sep/27/1918	Oct/16/1918	-19	39	155.2	704.6
Toledo	Ohio	Sep/30/1918	Oct/22/1918	Oct/24/1918	-2	102	152.4	401
Washington	District of Columbia	Sep/20/1918	Oct/02/1918	Oct/12/1918	-10	64	166.8	758
Worcester	Massachusetts	Sep/18/1918	Sep/21/1918	Oct/06/1918	-15	44	192.3	727.1

Notes: This table list all 43 cities used in Markel et al. (2007) for which NPI data are available. NPIs are measures such as the closure of schools and churches, the banning of mass gatherings, but also other measures such as mandated mask wearing, case isolation, and public disinfection/hygiene measures. The table reports our two main measures for *NPI Speed* and *NPI Intensity*. The former is measured as the difference between the response date and the mortality acceleration date which is the day the mortality rate exceeds twice its base. The later counts the cumulative total number of days NPIs measures are activated from Markel et al. (2007).

Table A2: **Comparison of cities with high and low NPIs.**

	Low NPI		*High NPI*		Difference	
	Mean	Std	Mean	Std	Diff	t-stat
Longitude	-80.94	12.05	-96.07	16.48	-15.13	-3.32
NPI Speed	-11.56	7.06	-1.50	4.40	10.06	5.74
NPI Intensity	56.28	23.42	132.72	30.89	76.44	8.85
Influenza mortality, 1917	194.61	63.48	157.72	53.18	-36.89	-2.07
Influenza mortality, 1918	721.04	181.38	544.78	146.10	-176.25	-3.53
Log city population, 1910	12.33	0.72	12.66	1.00	0.33	1.20
Log city population, 1900	12.07	0.79	12.20	1.14	0.12	0.40
City Density, population 1910 per sqm	9033.44	4694.15	7933.83	4371.95	-1099.61	-0.79
Health expenses in 1917/Population in 1910	0.55	0.26	0.52	0.22	-0.04	-0.50
Manuf. Emp./Population in 1910	0.14	0.07	0.11	0.05	-0.03	-1.51
Log manuf. emp, 1914	1023.90	82.21	1035.75	128.49	11.85	0.34
Agr. empl. share in 1910, state-level	19.67	18.44	27.44	10.47	7.77	1.75

Notes: This table reports differences in city-level and state-level characteristics for the 43 cities with NPIs. *High NPI* cities have above-median *NPI Speed* and above-median *NPI Intensity*.

Table A3: **1918 Flu Pandemic exposure and economic activity.**

Panel A: No Controls			
	State-Level		City-Level
	(1) ln(Emp)	(2) Emp/Pop	(3) ln(Emp)
$Mortality_{1918} \times$ Post	-0.067	-0.0044	-0.036
	(0.020)	(0.0010)	(0.017)
R^2 (Within)	.47	.16	.42
N	180	180	394
No of units	30	30	66
Panel B: Baseline Controls × Post			
	(1)	(2)	(3)
$Mortality_{1918} \times$ Post	-0.095	-0.0055	-0.021
	(0.023)	(0.0011)	(0.017)
R^2 (Within)	.53	.18	.45
N	180	180	394
No of units	30	30	66
Panel C: Instrumenting with Distance to Military Camps			
	(1)	(2)	(3)
$Mortality_{1918} \times$ Post	-0.12	-0.0047	-0.15
	(0.026)	(0.0016)	(0.046)
First Stage F-Stat (KP)	13	13	19
R^2 (Within)	.53	.18	.27
N	180	180	394
No of units	30	30	66
State and Post FE	Yes	Yes	Yes

Notes: The table reports results from estimating a regression of the following form:

$$Y_{st} = \alpha_s + \beta \times Mortality_{s,1918} \times \text{Post}_t + \delta \times \text{Post}_t + \gamma \times X_s \times \text{Post}_t + \varepsilon_{st},$$

where $Mortality_{s,1918}$ is state/city mortality from influenza and pneumonia in 1918, Post_t is a dummy variable that takes the value of one after 1918. Controls in X_s for state-level regressions are the 1910 agriculture employment share, 1910 manufacturing employment share, 1910 urban population share, 1910 income per capita, and log 1910 population. Controls for the city-level regressions are 1910 population, manufacturing employment in 1914 to 1910 population, health expenditures in 1917 to 1910 population, city density in 1910, state agriculture employment share in 1910, and the state war production dummy. Census of Manufactures dependent variable outcomes are measured in 1904, 1909, 1914, 1919, 1921,and 1923.

Standard errors clustered at the state or city level in parentheses.

Table A4: **Non-pharmaceutical interventions and economic disruptions in Bradstreets Trade Conditions: Estimates without controls.**

	Combined W+R+M	Wholesale Trade	Retail Trade	Manufacturing
Panel A: NPI Intensity Measure				
	(1)	(2)	(3)	(4)
$NPI\ Intensity_c \times Post_t$	-0.064	-0.0011	-0.0043	-0.14
	(0.075)	(0.10)	(0.13)	(0.082)
R^2 (Within)	.0061	8.4e-07	9.7e-06	.02
N	344	343	342	340
Panel B: NPI Speed Measure				
	(1)	(2)	(3)	(4)
$NPI\ Speed_c \times Post_t$	-0.20	-0.43	0.18	-0.32
	(0.39)	(0.52)	(0.74)	(0.34)
R^2 (Within)	.0019	.0038	.0005	.0029
N	344	343	342	340
Panel C: High NPI Measure				
	(1)	(2)	(3)	(4)
$High\ NPI_c \times Post_t$	-3.86	-4.97	5.41	-11.5
	(7.92)	(10.9)	(12.9)	(10.4)
R^2 (Within)	.0021	.0017	.0015	.012
N	344	343	342	340
No of cities	25	25	25	25
City and Time FE	Yes	Yes	Yes	Yes
Controls	No	No	No	No

Notes: The table is analogous to Table 2 but reports estimates from specifications excluding controls $X_c \times Post_t$.

C Narrative Evidence from Historical Newspaper Articles

This section contains excerpts of newspaper articles contemporaneous to the 1918 Influenza pandemic, documenting the real effects of the pandemic in trade and production, as well as the timeline of policy interventions.

C.1 Real effects

"Holland's Letter: Effect Of Influenza on Loan and Output—Reasons For and Against Imposing a Stamp Tax." *Wall Street Journal*, Oct 24, 1918, p. 2. At a private and informal meeting last week of some of these who are of important in the world of finance and banking, the suggestion was made that a communication be sent to Secretary of the Treasury McAdoo that he wold be justified in extending to another week the campaign for the sale of the Fourth Liberty Loan bonds. ...

One reason alone influenced those who suggested a recommendation of this kind to Secretary McAdoo. That was the prevalence of the grippe or influenza, which had seriously interfered with the sale of the bonds. ...

The effect of the influenza epidemic was not exclusively felt, by the loan, however. In some parts of the country it has caused a decrease in production of approximately 50% and almost everywhere it has occasioned more or less falling off.

The loss of trade which the retail merchants throughout the country have met with has been very large. The impairment of efficiency has also been noticeable. There never has been in this country, so the experts say, so complete domination by an epidemic as has been the case with this one. ...

"Influenza Checks Trade: Less Doing In Retail Shops As Illness and Caution Cut Down the Crowds." *Wall Street Journal*, Oct 25, 1918, p. 10. Widespread epidemic of influenza has caused serious inroads on the retail merchandise trade during the current month. Heads of large organizations report that not only has sickness cut down the shopping crowds, but in many cities the health authorities have shut down the stores.

The chain store companies have felt the effect of the sickness not a little, for in addition to the smaller business done a number of their employees are sick. ...

"5 Theatres Close Tonight: Theatrical Depression Attributed In Large." *New York Times*, Oct 12, 1918, p. 13. Theatrical Depression Attributed in Large Part to Influenza Scare.

An unprecedented theatrical depression, which managers attribute in large part to the influenza scare, resulted in sudden decisions yesterday to close five playhouses tonight. ... In all, more than a dozen local theatres will be dark next week.

"Textile Trade Hit By Spanish Influenza: Many Mills Closed And Others Working Partially—Retail Business Hurt." *Wall Street Journal*, Oct 21, 1918, p. 6. Both the wholesale and retail trades have been hit badly by the Spanish influenza epidemic. Mill production is being curtailed, and even Government business is held up. A great many mills throughout the country have either completely ceased operations or kept only a small fraction of their machinery working. Consequently, deliveries have been held up in many lines. Retailers report that the disease has hurt their fall business, but it is hoped particularly among New York merchants that when the epidemic wanes they will quickly catch upon lagging sales. . . .

"Anthracite Output Affected By Influenza: Collieries Shut Down As ." *Wall Street Journal*, Oct 12, 1918, p. 9. Effect of the influenza epidemic in current anthracite production is substantial . . . Around Minersville, Pa., where the ravages of the disease are said to have been probably as severe as in any part of the region, one entire colliery was shut down, but the washery of this particular company resumed working before the close of the week.

"Copper Shortage Is Acute: Influenza At Refineries And Smelters Further Reduces Output Already Curtailed by Labor Scarcity." *Wall Street Journal*, Oct 25, 1918, p. 6. Scarcity of copper is acute. Even the United States Government is not at present obtaining its full quota of metal, according to interests conversant with the situation. With Government orders unfilled, there is, of course, no surplus available for the outside trade.

Increased curtailment of production is due largely to influenza at the refineries and smelters. With the country's output already seriously impaired by labor shortages, a condition which is believed not likely to improve during the war, incapacitation of a large percentage of employees at nearly all the producing plants is resulting in a contraction in the copper supply which is expected to be more severe than was experienced during the worst months of the labor strikes in 1917.

"Corporation Bonds Comparatively Low: Present Average Price Over Eleven Points Under High Price Reached Since Stock Exchange Reopened." *Wall Street Journal*, Jan 22, 1919, p. 5. High Point Recorded January 18, 1917, and Low Since September 28, 1918—Influenza Epidemic an Influence in Decline of Railroad Bonds Which Are Usually Bought Heavily by Life Insurance Companies

. . . Several other factors which have tended to unsettle the bond market will be removed in the near future. The influenza epidemic, which caused heavy claims on life insurance companies, thus temporarily putting them out of the market for high-grade railroad bonds, is an example.

"Drug Markets Affected By Spanish Influenza: Big Demand For Camphor Causes Advance in Wholesale and Retail Prices—Aspirin, Rhinitis and Quinine Taken in Big Quantities." *Wall Street Journal*, Oct 21, 1918, p. 6. The countrywide epidemic of Spanish influenza has had considerable influence on the drug markets and the demand for camphor, aspirin, quinine and many disinfectants has been unprecedented. . . .

"Influenza Impedes Ship Production: About 6,500 Workers Are Ill At Fall River and Hog Island—Other Yards Affected." *New York Times*, Oct 3, 1918, Special. The epidemic of Spanish influenza has put 10 per cent of the shipyard workers in the Fall River district and at least 8 per cent of those at Hog Island, Philadelphia, temporarily on the ineffective list and is seriously interfering with rapid ship construction. Practically all of the yards as far south as Baltimore are affected to some degree, and extraordinary steps are being take in to fight the disease. At Hog Island and other large plants some of the administration buildings have been converted into hospitals.

C.2 Public health intervention

"Drastic Steps Taken To Fight Influenza Here: Health Board Issues 4 P.M. Closing Orders for All Stores Except Food and Drug Shops. Hours for Factories Fixed. Plan, in Effect Today, to Reduce Crowding in Transportation Lines in Rush Periods. Time Table for Theatres. Radical Regulations Necessary to Prevent Shutting City Up Tight, Says Dr. Copeland." *New York Times*, Oct 5, 1918, p. 1. In order to prevent the complete shutdown of industry and amusement in this city to check the spread of Spanish influenza, Health Commissioner Copeland, by proclamation, yesterday ordered a change in the hours for opening stores, theatres and other places of business.

The Department is of the opinion that the greatest sources of spread of the disease are crowded subway and elevated trains and cars on the surface lines and the purpose of the order is to diminish the "peak" load in the evenings and mornings on these lines by distributing the travelers over a greater space of time. This will reduce crowding to a minimum.

Dr. Copeland's action was taken after a statement made by Surgeon General Blue, Chief of the Public Health Service in Washington, was called to his attention, in which Dr. Blue advocated the closing of churches, schools, theatres and public institutions in every community where the epidemic has been developed. Dr Blue said:

"There is no way to put a nationwide closing order into effect, as this is a matter which is up to the individual communities. In some States the State Board of Health has this power, but in many others it is a matter of municipal regulation. I hope that those having the proper authority will close all public gathering places if their community is threatened with the epidemic. This will do much toward checking the spread of the disease"

. . . One of the decisions reached is to close all stores other than those dealing exclusively in food or drugs at 4 o'clock in the afternoon. . . .

All moving picture houses and theatres outside of a certain district are considered community houses and are held to draw their patronage from within walking distance. There was debate on the proposition to close the schools and churches and other places of assemblage, but it was decided against it at this time. . . .

"The Spanish Influenza." *New York Times*, Oct 7, 1918, p. 12. Under adverse conditions the health authorities of American communities are now grappling with an epidemic that they do not understand very well. But they understand it well enough to know that it spreads rapidly where people are crowded together in railway trains, in theatres and places of amusement, in stores and factories and schools. In some cities and towns where the influenza seems to be malignant the schools and many places of amusement have been closed. Pennsylvania, taking a serious view of the hazards of the disease, because it is raging in the shipyards and increasing ominously elsewhere, has taken drastic measures to protect the public health. The sale of liquor has been generally prohibited in Philadelphia, the courts stand adjourned, Liberty Loan meetings have been abandoned, public assemblies of all kinds have been forbidden, the theatres are not allowed to give performances, and it is recommended that the churches hold no services. In some other parts of Pennsylvania the authorities have gone further, closing churches and Sunday schools. Football games have been canceled. In localities in New Jersey the public schools have been closed. This is the case in Omaha and other Western cities. In Oswego, where about 15 per cent of the population is down with influenza, the Health Board has acted vigorously. . . .

New York City has thus far escaped lightly compared with Boston, which has had 100,000 cases, and with Philadelphia, where the total two days ago was 20,000. Up to yesterday only 8,000 cases had been reported in this city of about 6,000,000 people, according to the Health Department, although there are perhaps many cases still unreported. It seems providential that there have been so few cases in our congested districts, and generally in a population that packs the transportation lines. But unless our health authorities are vigilant and practical, there may soon be another story to tell. The precautionary and restrictive regulations adopted by the Department of Health are at best tentative. It is a question whether the schools should not be temporarily closed, as in other places. As business must go on, if not as usual, it was advisable to vary the opening and closing hours of business establishments to regulate the "rush hours" on transportation lines. The opening time of theatres has been changed with a similar purpose. It is evident that the Health Department hesitates to be strenuous, because, as Dr. Copeland says, "this community is not striken with the epidemic".

But it may be if only half measures at taken. A stitch in time saves nine. The closing of the schools is a debatable question. Dr. Copeland's reasons for keeping them open are not altogether convincing. . . .

"Delays In Reports Swell Grip Figures: 1,450 New Cases Recorded, Largest Number for a Single Day Since Epidemic Began. Newark Officials Clash. Mayor Raises Closing

Bank OVer Head of the State Board of Health." *New York Times*, Oct 24, 1918, p. 12. For the twenty-four hours ended at 10 o'clock yesterday morning, 1,450 new cases of Spanish influenza were reported to the Board of Health. This is the largest number of new cases reported in a single day since the disease became epidemic in New York.

. . . Major Gillen of Newark, and the New Jersey State Board of Health yesterday began a controversy over the authority of the city officials in ordering the raising of the closing order on schools, theatres, saloons, soda fountains and churches after the State Board had ruled that all should be closed until it lifted the ban. A meeting of the State Board will be held in Trenton today to consider measures compelling the Newark City Government to enforce the rule. The Newark City Commission also will hold a meeting to discuss whether it has jurisdiction upon health superior to that of the State Board.

. . . After being held twenty-four hours in Quarantine for examination and fumigation the Holland-America liner Nieuw Amsterdam was permitted to leave for the pier to land her 900 passengers yesterday. The health officers at Quarantine said there had been fifty cases of Spanish influenza on the voyage from Holland, but only twelve passengers in the second cabin were still confined to their berths when the steamship reached port on Tuesday. . . .

"Major Closes Theatres, Schools and Churches. Sudden Spread of Spanish Influenza Forces City Officials to Take Drastic Steps. 25 Flu Cases in Seattle Reported." *The Seattle star*, October 05, 1918, p. 1. All churches, schools, theatres and places of assemblage were ordered closed by proclamation of Mayor Hanson at noon Saturday, to check the spread of the Spanish influenza.

Police officers were immediately send to the motion picture houses to enforce the order.

At 2 p.m. policemen had served notice on all the downtown theatres, including movie houses, and the had close their doors.

While latitude was given to officers in orders to close all other assemblages in buildings.

No church services will be permitted Sunday.

"We will enforce the order to the letter," Mayor Hanson declared. "The chief of police has been given orders. Dance halls were ordered closed last night. No private dances must be held. Persons spitting on sidewalks or in street cars are to be immediately places under arrest."

His order followed consultation with Health Commissioner McBride, who reported that there were 25 civilian cases on record at noon.

New cases are being reported every few minutes.

There has been one civilian death. . . .

"Halls and Churches to be Flu Hospitals." *The Seattle star*, October 07, 1918, p. 1. *Don't be grumbler*

Don't grumble because you can't see a movie or play a game of billiards—or because the schools and churches closed.

The health of the city is more important than all else. An ounce of prevention now is worth a thousand cures. In Boston, influenza has taken a toll of thousands. We do not want to court that situation here.

Preparations were under way Monday by Mayor Hanson and municipal health authorities to transform Seattle's big public dance halls, and churches if necessary, into emergency hospitals to care for Spanish influenza cases if the epidemic is not checked.

This action was decided upon as a preparatory measure, supplementing the order of Saturday that closed schools, theatres, motion picture houses, pool halls, and all indoor assemblages. . . .

"We don't know how long it will be necessary to enforce the general closing order," said Mayor Hanson Monday. "I have not made any predictions, and cannot make any. We have received citywide co-operation with practically everyone affected except school authorities, who objected."

"Not Ready to Lift the Influenza Ban." *The Seattle star*, October 23, 1918, p. 3. Twelve influenza and pneumonia cases have been reported in Seattle to the health department within the last 24 hours, while 194 new cases were reported Wednesday morning. Five deaths occurred late Tuesday night and Wednesday morning. . . .

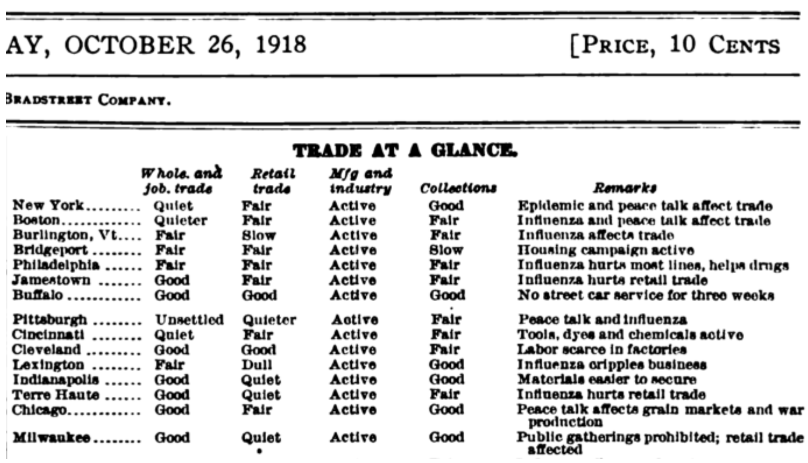
Wednesday, Dr. J. S. McBride, city health commissioner, announces that the crest of the epidemic has been passed, but that great caution must be observed by every individual for some time yet. He has not announced when the ban will be lifted.

D Data Appendix

D.1 Bradstreet's Trade Conditions

To construct the city-level index of economic disruptions, we use the Trade at a Glance report, published most Saturdays as part of the weekly *Bradstreet's - A Journal of Trade, Finance, and Public Economy*. In particular, we use Volumes 46 (1918) and 47 (1919) of Bradstreet's annual publication, which comprise most weekly issues for 1918 and 1919.[13]

As seen on Figure A9, Trade at a Glance summarized for the main cities in the United States the status of wholesale trade, retail trade, manufacturing and industry, collections, and crops (except on Winter months). Each of these categories was described with a single word ("Quiet," "Fair," "Active," etc.), with additional information available for the city as a whole on the "Remarks" section. From these five categories, we focus on wholesale trade, retail trade, and manufacturing, and exclude collections and crops, as the latter are reported more sparsely and are less related to the day-to-day economic activity of the city itself.


AY, OCTOBER 26, 1918 [PRICE, 10 CENTS

BRADSTREET COMPANY.

TRADE AT A GLANCE.

	Whole. and job. trade	Retail trade	Mfg and industry	Collections	Remarks
New York.........	Quiet	Fair	Active	Good	Epidemic and peace talk affect trade
Boston.............	Quieter	Fair	Active	Fair	Influenza and peace talk affect trade
Burlington, Vt....	Fair	Slow	Active	Fair	Influenza affects trade
Bridgeport	Fair	Fair	Active	Slow	Housing campaign active
Philadelphia	Fair	Fair	Active	Fair	Influenza hurts most lines, helps drugs
Jamestown	Good	Fair	Active	Fair	Influenza hurts retail trade
Buffalo	Good	Good	Active	Good	No street car service for three weeks
Pittsburgh	Unsettled	Quieter	Active	Fair	Peace talk and influenza
Cincinnati	Quiet	Fair	Active	Fair	Tools, dyes and chemicals active
Cleveland	Good	Good	Active	Fair	Labor scarce in factories
Lexington	Fair	Dull	Active	Good	Influenza cripples business
Indianapolis	Good	Quiet	Active	Good	Materials easier to secure
Terre Haute	Good	Quiet	Active	Fair	Influenza hurts retail trade
Chicago............	Good	Fair	Active	Good	Peace talk affects grain markets and war production
Milwaukee........	Good	Quiet	Active	Good	Public gatherings prohibited; retail trade affected

Figure A9: Bradstreet's Street At a Glance, October 26, 1918

Because the number of different one-word descriptions we observe in each category over 1918-1919 is quite large[14], we use a series of rules to condense it into a three-valued index that describe conditions as "Bad," "Fair," or "Good." Further, we also use this index to construct a binary variable for whether there were trade disruptions ("Bad" and "Fair") or no trade disruptions ("Good"). "No disruptions" are given a value of 100, while disruptions are given a value of 0.

The classification rule is as follows:

1. Good: good, brisk, excellent, active, liberal, very active, better, record, very good, steady, more active, prompt.

[13]Information from October to November 1919 is not available on Volume 47, and the Trade at a Glance report was not published on the first week of each year.

[14]We observe 27 different values for wholesale trade, 26 for retail trade, and 53 for manufacturing.

2. Fair: fair, moderate, fair to good, satisfactory, close, 3/4 capacity, 60 percent, 75 percent, 75% basis, normal, fair activity, fairly active, hesitating, hesitation, only fair, slowdown, readjusting, half speed, half time, hampered, waiting, slack, uncertain, suspended, many strikes, contracted, disturbed, inactive, short time, retarded, paralyzed, irregular, unsettled, conservative.

3. Bad: quiet, dull, slow, very slow, cautious, interrupted, light, restricted, below normal, curtailed, under normal, poor, lagging, tardy, delayed, backward, drag.

Further, we occasionally found words that describe conditions *relative* to previous reports. For instance, "quieter," "improving," etc. In these cases we apply the following rule:

1. If the current week has any of the following keywords, we reduce the rating by one notch (from Good to Fair, or from Fair to Bad): reduced, quieter, slower, slowing down, smaller, less active, receding.

2. If the current week has any of the following keywords, we increase the rating by one notch (from Fair to Good, or from Bad to Fair): improved, improving, slightly better, enlarging, shifting, enlarging, improvement, increasing.

Lastly, we exclude observations where remarks mention labor strikes[15] and remove cities with only a few observations (Oakland and Denver with only one and eight observations, respectively). We then collapse the data at the city-month level, as information was often sparse and some cities did not report data every week. To create a combined index of wholesale trade, retail trade, and manufacturing, we take a simple average of the monthly disruptions index for the three sectors. This leaves us with a monthly business disruptions index for a total of 25 cities with NPI measures from Markel et al. (2007).

D.2 Census of Manufactures

Data on city-level manufacturing employment, output (value of products), and value added is from the Census of Manufactures. The sources used to obtain the manufacturing data are listed in the following table:

Source	PDF Page	Page	Table	Years Covered
1919 Census of Manufactures	10	293	193	1904-1919
1924 Statistical Abstract	32	754	692	1914-1923
1926 Statistical Abstract	30	774	748	1914-1925 excl. 1921
1931 Statistical Abstract	860	842	815	1923-1929

[15]Results are robust to including observations with labor strikes, but are potentially more difficult to interpret, as strikes could signify a booming local economy where labor is in short supply.

These sources often have overlapping years, which we use to check for data quality (typos in the original data) as well as to alleviate potential measurement errors. In particular, we identified and studied four changes in the canvassing methodology in the years covered in our analysis (1904-1927):

1. The 1914 and 1919 census canvassed data for "automobile repairing." This category was excluded in all other censuses, so 1909-1914 growth rates will be biased upwards and 1919-1921 growth rates will be biased downwards. Both biases are likely to be small for two reasons. First, the automobile repairing industry accounted for only 0.18% of wage earners in 1914 (0.61% in 1919). Second, due to the nature of the industry, its output was distributed relatively uniformly across the country (see Table 49 on page 175 of Volume 8 of the Fourteenth Census of the United States). Moreover, note that this change in classification methodology does not involve any bias in the 1914-1919 growth rates.

2. The 1904-1919 censuses collected data for all factories with a total annual output above $500. This threshold was increased in 1921 to $5,000, thus creating a downward bias in 1919-1921 growth rates. However, output for factories in the $500-$5,000 range were estimated by the Census Bureau (see general note of Table 685, page 723, 1924 Statistical Abstract) to account for only 0.6% of employment and 0.3% of output, so potential biases for 1919-1921 growth rates are likely to be small.

3. In contrast to other years, the 1925 census did not canvas data for the "Coffee and spice, roasting and grinding" industry. Thus, naively collecting the data would underestimate 1923-1925 growth rates and overestimate 1925-1927 growth rates in cities with a coffee roasting industry. To alleviate this potential issue, we exploit the fact that the 1926 Statistical Abstract also reported figures for 1923 that excluded the coffee industry. Thus, we input the 1925 manufacturing figures by first computing the 1923-1925 growth rate from the 1926 Statistical Abstract (which excludes coffee in both years) and then multiplying it with the 1923 figures from the 1931 Statistical Abstract (where the coffee industry was included). Note that all our results are robust to not doing any adjustment, and that the differences are below 1% of employment in all cities in our sample.

4. Occasionally, to preserve the anonymity of figures for specific establishments, the Bureau of the Census must report them in a given city even though they were "located elsewhere in the State." For instance, for the 1921 census the cities of Bridgeport, Cincinnati, and Cleveland included one such establishment. More information on this practice is available e.g. on section 26 of chapter 1 (page 10) of the 1925 Census of Manufacturers.

To address these issues, as well as some changes in city boundaries (discussed below), we build our dataset using the following steps:

1. We use the 1919 Census of Manufacturers (CoM) as the primary source for the years 1904 and 1909, as well as to validate data for 1914 and 1919.

2. We use the 1924 Statistical Abstract (SA) as the primary source for the years 1914, 1919, 1921, and to validate data for 1923.

3. We use the 1926 SA as the primary source for 1925 (by computing 1923-1925 growth rates and scaling by the 1923 values).

4. We use the 1931 SA as the primary source for 1923 and 1927, and to obtain 1925 data corrected for canvassing changes in the coffee industry.

D.2.1 Boundary changes for specific cities

The Census Bureau computed city-level manufacturing statistics by adding up establishments within the corporate limits of each city. Annexations or consolidations have the potential to mechanically bias growth rates upwards on the census years around an event.

In this subsection, we explore these boundary changes and study the most salient ones in order to understand how they might affect our results. We also describe how we adjust the raw values to account for boundary changes in cases where this is feasible. We pay particular attention to boundary changes that occurred between the 1914 and 1919 census years.[16]

For each annexation event and its accompanying boundary change, there are three possible issues in terms of possible biases:

1. Statistics were re-tabulated by the Census Bureau, by combining information from cities that merged ex-post. No adjustments are needed if the re-tabulated reports are used. This is the case of the consolidation of Boston and Hyde Park in Massachusetts.

2. Statistics were not re-tabulated by the Census, but sufficient information is available to combine the data for both cities. This is the case for Omaha's annexation of South Omaha, Nebraska.

3. Statistics were not re-tabulated and the census did not include manufacturing output for the annexed cities. Here, we provide upper bounds on the possible impact of the annexation on growth rates. We find the bounds to be fairly small across most cases.

Boston, MA As stated in Volume I, Page 595, Table 7 of the 1914 Census of Manufactures, information for 1904 and 1909 "includes Hyde Park, consolidated with Boston Jan. 1, 1912."

Pittsburgh, PA As stated in Volume I, Page 1280, Table 7 of the 1914 Census of Manufactures, information for 1904 "includes statistics for Allegheny, annexed in 1907."

[16] In a discussion of our paper, Lilley et al. (2020b) state that "In addition to Omaha, the other cities with NPI data and large incorporations between 1914 and 1919 - Los Angeles, Portland, Richmond, and Toledo - have identical 1909 manufacturing data in the 1910 and 1920 Census". Thus, we place particular emphasis on these cities.

Omaha, NE As stated in Table 193, Page 312 of the 1919 Census of Manufactures, South Omaha was "annexed to Omaha in 1915". Because this document reports totals for South Omaha for 1904, 1909, and 1914, it is straightforward to combine the totals of both cities before the annexation.

Richmond, VA On November 5, 1914, Richmond's corporate limits were extended to include the towns of Barton Heights, Fairmount, and Highland Park. As shown in Figure A10, this annexation included industries in the Chamberlayne industrial district, so this case is worth studying it in more depth.[17]

Figure A10: Map of the manufacturing plants on the outskirts of Richmond, VA as of the 1909 Census, overlaid against a 1923 map of the territorial expansion of the City of Richmond from Richmond's Department of Public Works. While the southern cluster of plants was not annexed into the city, the northern cluster was annexed in 1914.

For this, the 1914 Census of Manufacturers is particularly useful, as it discusses the issue in depth (see Figure A11, from page 14 of the Virginia Volume). In particular, Table 14 on page 13 reports results not only for the city pre-annexation but also for the annexed territory and for the enlarged city. These results show that the annexations were not as substantial, as manufacturing employment in the annexed territory corresponds to only 5.8% of the employment in the boundaries pre-annexation. Results are similar for manufacturing output (5.6%) and value added (4.5%).

Nonetheless, to avoid any bias in the 1914-1919 growth figures, for 1914 we use the numbers of the expanded city, and chain values for 1904 and 1909 accordingly (assuming that the expanded city maintains the same growth rate as the pre-annexation city did). Our results are almost indistinguishable from those obtained without this adjustment.

[17] Figure A10 is compiled from overlaying the 1910 Census Volume 8 Part 2 Page 117 with a map from a 1923 report from Richmond's Department of Public Works.

MAP SHOWING CORPORATE LIMITS OF RICHMOND, VA.: 1910 AND 1914.

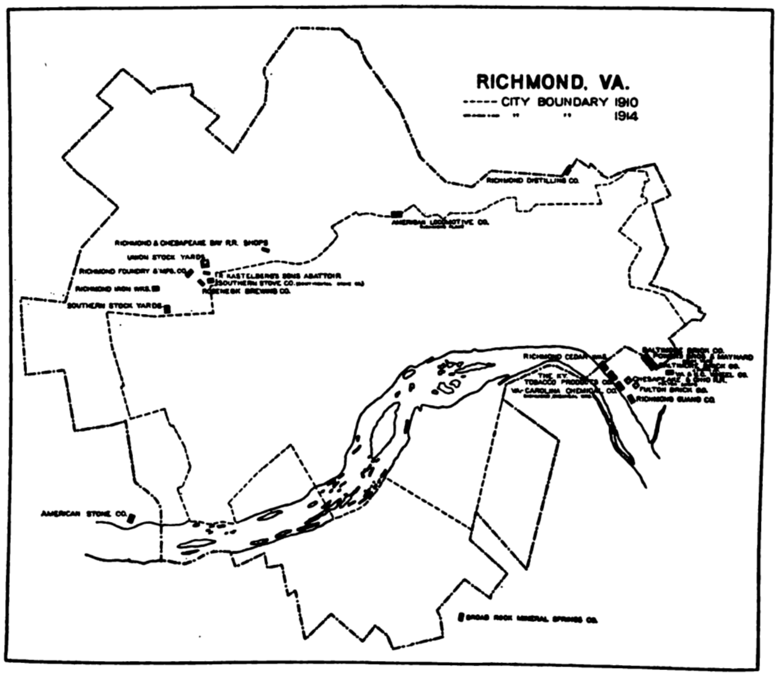


Figure A11: Corporate Limits of Richmond, VA in 1910 and 1914

Los Angeles, CA The city of Los Angeles has expanded dramatically throughout its history. Through the 1914-1919 period, most of the drive for annexation was due to the 1913 opening of the Los Angeles Aqueduct, as the City of Los Angeles had a surplus of water but was contractually not allowed to resell it. Particularly important is the annexation of the San Fernando Valley, which doubled the area of the city. It is still not clear whether these annexations substantially increased the manufacturing activity of the city, as no annexations of incorporated cities happened in these years.[18] Instead, the annexations comprised San Fernando Valley (1915), Palms (1915), Bairdstown (1915), Westgate (1916), West Coast (1917), Griffith Ranch (1918), and Hansen Heights (1918).

First, note that the San Fernando Valley was only sparsely populated, most activity was agricultural, and the annexation excluded the areas with highest population within the valley, such as the City of San Fernando and Rancho el Escorpion. Thus:

> "In the Valley, the tabulations showed 681 voting for annexation to Los Angeles and 25 against." See Jorgensen (1982).

In contrast, as of 1910 the city of Los Angeles had a population of 102,479.

Moreover, as an exercise, we can take advantage of a feature of the 1914 Census to compute upper bounds for how much all the annexed regions accounted for, in terms of

[18]The incorporated cities annexed to Los Angeles include Wilmington (1909), San Pedro (1909), Hollywood (1910), Sawtelle (1922), Hyde Park (1923), Eagle Rock (1923), Venice (1925), Watts (1926), Barnes City (1927), and Tujunga (1932).

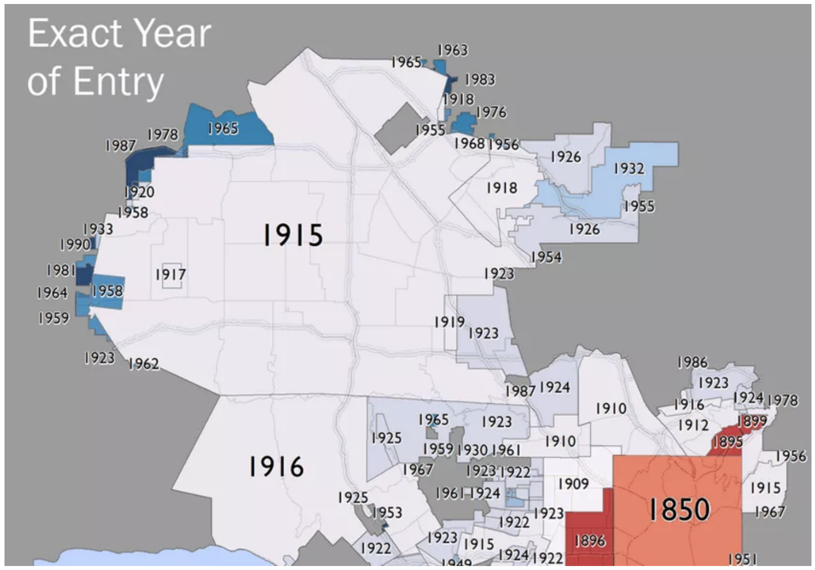


Figure A12: Growth of the City of Los Angeles

manufacturing. In particular, in 1914 (but not 1919) the Census Bureau provided statistics for the "Los Angeles Metropolitan District", which which encompassed the Los Angeles City plus several other cities such as Long Beach City, Pasadena, Santa Monica, Sawtelle, etc. This metropolitan district comprised an area almost four times larger than the city of Los Angeles itself.

Using the information for the Metropolitan District, we can do the following thought exercise: using Figure A14, compute the total manufacturing numbers for the Metropolitan District, excluding the two other cities listed separately (Long Beach and Pasadena, which are still not part of the City of Los Angeles). Assume that in 1914 the City of Los Angeles annexed *the entirety* of the Metropolitan District bar these two cities (which we know is false, as e.g. Santa Monica is still independent). Then, under this extreme scenario the annexations would increase manufacturing employment by only 8.8% (2096/23744). In contrast, growth of manufacturing employment in Los Angeles was 198% between 1914 and 1919. Clearly, even at an upper bound, the annexations are not what drove manufacturing growth in Los Angeles.

Toledo, OH The 1920 census states that "parts of Adams and Washington townships [were] annexed to Toledo city since 1910" (Table 53, Page 565, Volume 1, 1920 Census of Population). We are not aware of any manufacturing data for these townships, but we can do a similar exercise to compute upper bounds using population numbers.

In particular, we can assume that not just *parts* but *all* of these townships were annexed into Toledo, and that both annexations occurred between 1914 and 1919. Then, as seen in Figure A15, the annexations would have accounted for at most 7,433 inhabitants. Considering that as of 1910 the population of Toledo was 168,497, then the annexations accounted for at most 4% of Toledo's 1910 population. This is an order of magnitude lower than Toledo's 1910-1920 reported population growth, of 44%.

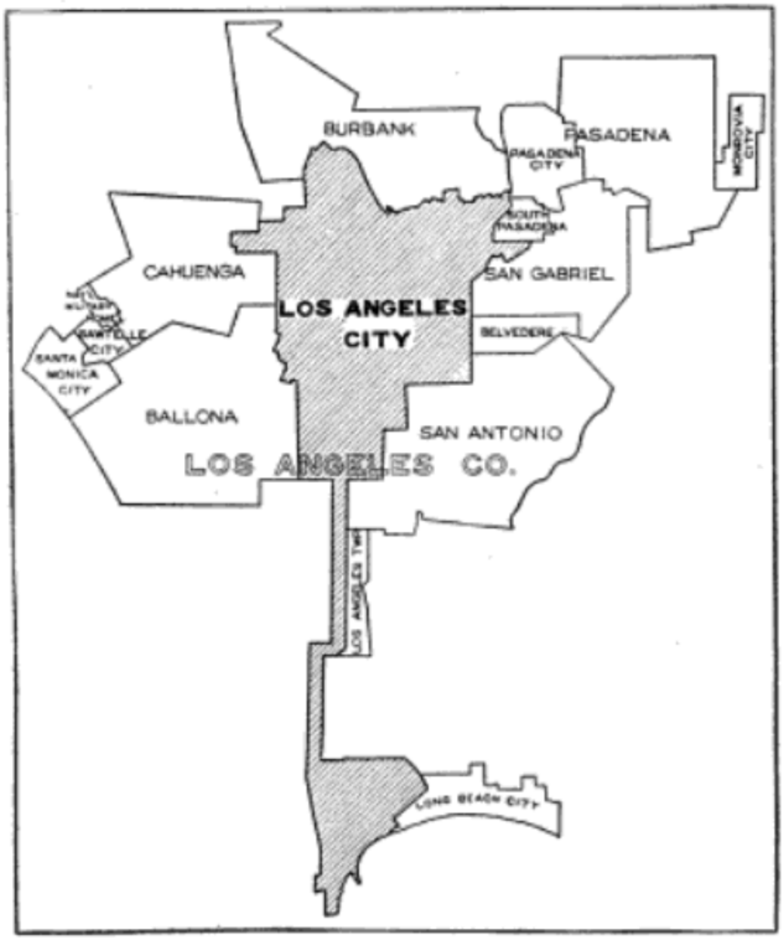


Figure A13: Los Angeles Metropolitan District in 1914

Table 19	LOS ANGELES METROPOLITAN DISTRICT: 1914.						Per cent Los Angeles is of total for district.
	The district.	Los Angeles.	District exclusive of Los Angeles.				
			Total.	Long Beach.	Pasadena.	Remainder.	
Population[1]	568,651	438,914	129,737	24,437	40,880	64,420	77.2
Number of establishments	2,357	1,911	446	94	118	234	81.1
Persons engaged	36,218	31,540	4,678	1,128	804	2,746	8⁷.1
Prporietors and firm members	2,181	1,767	414	82	120	212	81.0
Salaried employees	6,776	6,029	747	161	148	438	89.0
Wage earners (average number)	27,261	23,744	3,517	885	536	2,096	87.1
Primary horsepower	79,039	64,665	14,374	2,999	1,782	9,593	81.8
Capital	$117,598,134	$101,681,050	$15,917,084	$3,228,751	$1,600,714	$11,087,619	86.5
Salaries and wages	29,624,956	26,024,790	3,600,166	805,051	585,485	2,209,630	87.8
Salaries	8,735,702	7,747,713	987,989	198,980	156,227	632,782	88.7
Wages	20,889,254	18,277,077	2,612,177	606,071	429,258	1,576,848	87.5
Paid for contract work	767,747	703,251	64,496	19,577	6,494	38,425	91.6
Rent and taxes (including internal revenue)	2,633,074	2,286,041	347,033	83,026	62,354	201,653	86.8
Cost of materials	68,206,862	58,940,796	9,266,066	1,546,106	880,402	6,850,558	86.4
Value of products	118,799,355	103,457,993	15,341,362	2,944,888	1,971,891	10,424,583	87.1
Value added by manufacture (value of products less cost of materials)	50,592,493	44,517,197	6,075,296	1,398,782	1,102,480	3,574,025	88.0

Figure A14: Excerpt from the 1914 Manufacturing Census - Los Angeles Metropolitan District

Lucas County	**275,721**	**192,728**	**153,559**
Adams township [12]	4,735	2,635	2,090
Jerusalem township	1,297	1,431	1,581
Monclova township	991	1,043	1,031
Oregon township	3,500	2,568	2,702
Providence township	1,147	1,217	1,270
Richfield township, including Berkey village	1,036	1,029	1,136
Spencer township, including Sharples village	817	802	763
Springfield township	1,415	1,176	953
Swanton township	875	851	837
Sylvania township, including Sylvania village	3,141	2,220	1,887
Toledo city [12]	243,164	168,497	131,822
Washington township [12]	8,440	4,798	3,449
Waterville township, including Waterville and Whitehouse villages	1,968	2,154	2,176
Waynesfield township, coextensive with Maumee village	3,195	2,307	1,856

Figure A15: Excerpt from the 1920 Decennial Census - Toledo, OH

Portland, OR For Portland, we can do a similar exercise as with Toledo. As Figure A16 states, between 1910 and 1920, Portland annexed the town of Linnton as well as St. Johns city. Linnton was only incorporated in 1910, so no population figures are available from any census. However, as of 2000 the neighborhood of Linnton had a population of 541 inhabitants. In the case of St. Johns, its 1910 population was 4872, two orders of magnitude lower than Portland's 207,214.

Multnomah County [16]	**275,898**
Precincts Nos. 1 to 298, 317½, and 318, coextensive with Portland city [16]	258,288
Precinct 299, Swift	301

[16] MULTNOMAH.—Part annexed to Hood River County, and parts of Clackamas and Hood River Counties annexed, since 1910. Precinct 109, Linnton, and St. Johns city annexed to Portland city since 1910.

Figure A16: Excerpt from the 1920 Decennial Census - Portland, OR

D.3 Military camps

In appendix A, we use exposure to military camps as an instrument for influenza mortality. We use four sources to collect and validate data on WWI military camps:

1. *Order of Battle of the United States Land Forces in the World War*, Volume 3, Part 2, Center of Military History United States Army (1988): This source contains the lists of all Army and National Guard training camps, embarkment camps, and Army Forts. Further, it lists the strength of each camp, defined as the average number of troops located at each camp in a given month (or that transited through a given camp, in the case of embarkment camps).

2. *Joining the Great War, April 1917-April 1918*, Eric B. Setzekorn (2017): This source lists major training camps and cantonments on pages 28-29, which we use to validate our first source.

3. `http://www.fortwiki.com/World_War_I`: We use this online resource to geolocate all camps, as well as to help establish the founding date of each camp, in cases where the founding date is missing in the sources above.

4. Hilt and Rahn (2020): We use this source to validate our list of camps. This is particularly useful as camps and forts were often located in multiple locations (or in the case of embarkment camps, even in multiple states).

To compute camp strength, we use the "Aggregate" column from the Average Strength tables available for each camp (see Figure A17 for an example). In some cases the information is reported at the fort and not camp level, in which case we utilize a fort as the unit of analysis. For instance, Camp Funston was part of Fort Riley. Moreover, we exclude some specialized training camps with no information on troop counts, such as Fort Harrison (officer training post), Camp Robison (artillery training camp), Camp Colt (tank corps training camp), and Camp Crane (ambulance corps training camp). This leaves us with 40 military camps in total, composed of 19 Army training camps, 16 National Guard training camps, and 5 embarkation camps.

Average Strength 1917–18

Month	Officers	Enlisted men			Aggregate
		White	Colored	Total	
1917					
September	1,276	21,773		21,773	23,049
October	1,396	21,030	1,432	22,462	23,858
November	1,370	24,691	2,932	27,623	28,993
December	1,637	27,451	2,959	30,410	32,047
1918					
January	1,437	25,465	1,955	27,420	28,857
February	1,484	24,633	3,229	27,862	29,346
March	1,479	23,405	5,479	28,884	30,363
April	1,391	21,950	5,205	27,155	28,546
May	1,488	23,710	11,288	34,998	36,486
June	1,001	18,105	10,014	28,119	29,120
July	1,280	33,581	9,381	42,962	44,242
August	1,173	35,798	9,504	45,302	46,475
September	2,040	42,939	9,484	52,423	54,463
October	1,990	40,789	11,267	52,056	54,046
November	1,420	26,387	10,399	36,786	38,206
December	1,012	19,610	5,363	24,973	25,985

Figure A17: Camp Pike - Average Strength