# Lockdowns and Innovation: Evidence from the 1918 Flu Pandemic*

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**Abstract**

Does social distancing harm innovation? We estimate the effect of non-pharmaceutical interventions (NPIs)—policies that restrict interactions in an attempt to slow the spread of disease—on local invention. We construct a panel of issued patents and NPIs adopted by 50 large US cities during the 1918 flu pandemic. Difference-in-differences estimates show that cities adopting longer NPIs did not experience a decline in patenting during the pandemic relative to short-NPI cities, and recorded higher patenting afterward. Rather than reduce local invention by restricting localized knowledge spillovers, NPIs adopted during the pandemic may have preserved other inventive factors.

Keywords: Non-pharmaceutical interventions, public health, invention, patents, influenza, localized knowledge spillovers

JEL classifications: I18, N92, O31, R11

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# 1 Introduction

Knowledge spillovers in cities are a key input in the production of new ideas. Urban densities facilitate interactions that promote the recombination of existing ideas into new ones (Marshall, 1890; Jacobs, 1969; Lucas, 1988; Romer, 1990; Glaeser, 1999). Nearby inventors are more likely to cite each other and create similar inventions, suggesting that proximity encourages knowledge flows (Jaffe et al., 1993; Murata et al., 2014; Ganguli et al., 2020). Moreover, inventors in dense cities create more novel patents, suggesting that cities are the engines of innovation (Carlino et al., 2007; Packalen and Bhattacharya, 2015; Berkes and Gaetani, 2021).

The widespread adoption of non-pharmaceutical interventions (NPIs) by local and national governments to slow the spread of COVID-19 has limited social interactions in cities, potentially reducing the benefits of density. Will these limitations have long-lasting effects on cities' fortunes, aggregate invention rates, and economic growth? To shed light on these questions, we estimate the effect of NPIs on local patenting rates during the 1918 flu pandemic, when US cities widely adopted a range of interventions analogous to the ones implemented in response to the COVID-19 pandemic. These interventions drastically restricted social interactions in an attempt to limit disease spread, and included school and business closures, public gathering bans, and isolation and quarantine of infected people.

Our analysis combines high-frequency, city-level data on NPI duration and patenting rates (number of patents filed, and subsequently issued, divided by city population). For each city, we construct monthly patenting rates by filing date from the Comprehensive Universe of US Patents (CUSP, Berkes, 2018), which provides the city of each inventor, filing and award dates, technology class, and ownership status for the near-universe of US patents. We combine these data with the lengths of NPIs adopted by 50 large US cities during the 1918 pandemic. We extend the NPI database of Markel et al. (2007) by collecting data for seven additional cities from an updated version of the same archival source, the *Influenza Archive 2.0* (2016). The resulting dataset is a balanced panel at the city-by-month level covering 1910–1926. The cities in our sample accounted for 21% of the population and 39% of patents filed in the US in 1910.

We estimate the effect of NPIs on patenting rates during and after the pandemic using a difference-in-differences (DD) design. We classify cities into two groups: *long-NPI* cities (the treatment group) with cumulative NPI durations of more than 90 days, and *short-NPI* cities (the control group) with NPI durations of less than 90 days.[1] Overall, we find that patenting rates increase by 6–9% in long-NPI cities relative to short-NPI cities. We also distinguish between the period during the pandemic (September 1918–March 1919) and after the pandemic subsided (April 1919 and later). During the pandemic, cities that adopted longer NPIs experienced a statistically insignificant increase in relative patenting rates. After the pandemic, however, cities that adopted longer NPIs saw a pronounced and statistically significant increase in patenting rates by 7–12% relative to short-NPI cities. Our preferred specification includes city-by-calendar-month, Census-region-by-year, and month-by-year fixed effects. These fixed effects control for time-invariant confounders that correlate with NPI adoption and patenting activity (e.g., the presence of nearby universities) and for national trends, local seasonality, and differential regional trends (e.g., rapid growth in Western cities).

Nonetheless, the late 1910s and early 1920s was a period of demographic, cultural, and economic change in the United States. A primary concern is that faster-growing cities, which would have experienced larger increases in patenting anyway, adopted longer NPIs. Our results are robust to several population normalizations and to controls for differential trajectories by observable city characteristics (including age composition, education, and ethnic origin). Furthermore, we find no evidence of differential trends in patenting by NPI length before the pandemic. In some specifications, we analyze the effects of NPIs on patenting rates within patent classes. This allows us to control for the differential evolution of technology classes across cities. Taken together, the pre-trend analysis and alternative estimates suggest that our results are unlikely to be driven by omitted factors or unobserved trends.

Our analysis contributes to a literature on the importance of localized knowledge spillovers for invention (Jaffe et al., 1993; Carlino and Kerr, 2015; Catalini, 2018; Atkin et al., 2020).

[1] We also report estimates where the treatment is continuous and defined as the number of days of NPIs in each city.

Andrews (2019) provides evidence that Prohibition reduced invention by reducing interpersonal communication, especially informal interactions. Our results instead emphasize that during a pandemic, NPIs may mitigate losses to other innovation factors.

We also contribute to the literature on the effects of the 1918 pandemic and the NPIs adopted in response. NPIs appear to have reduced disruptions in business activity, according to measures of contemporaneous economic activity such as industrial production or business failures (Velde, 2020; Bodenhorn, 2020; Correia et al., 2022). Our analysis instead examines the effects of NPIs on patenting, which is informative about the long-run evolution of future productivity and output (Kogan et al., 2017). Moreover, our addition of seven cities to those in Markel et al. (2007) provides better coverage of fast-growing cities in the US South and West.

Longer NPIs adopted in response to the 1918 pandemic did not, on net, reduce patenting rates by limiting social interactions. Instead, those interventions may have had positive effects on inventive activities through other channels. By favoring a coordinated response to the pandemic, longer NPIs may have reduced uncertainty, anchored expectations, and preserved business capital, increasing post-pandemic invention. While our data and design do not allow us to formally test for this channel, we find evidence consistent with it. In particular we find larger effects of NPIs on multi-inventor patents, patents owned by external assignees, and patents in expanding technological fields.[2] In other words, NPIs have larger positive effects on inventions requiring more complex organizational structure and entailing a higher degree of technological or market risk.

## 2 Historical background and conceptual framework

The 1918 influenza pandemic was brief and severe. In the US, the first outbreaks occurred in the spring and summer of 1918, confined mostly to soldiers. The second wave, beginning in September 1918, was more serious. This wave was responsible for most of the pandemic's deaths in the US. Markel et al. (2007), studying 43 US cities, report that the first flu cases

[2] Patents owned by external assignees are often sponsored by a corporate or industrial R&D lab (Nicholas, 2010; Buzard et al., 2017).

usually occurred in September, with one city (Philadelphia) reporting a case in late August, and two cities reporting their first cases in early October. Mortality accelerated in late September to early October, and excess deaths peaked in late October to early November. A third wave started in January 1919 and ended in April 1919, the month that we define as the end of the pandemic.[3]

US cities adopted a variety of NPIs to restrict social interactions and limit disease spread. Markel et al. (2007) classify NPIs into three categories: public gathering bans, school closures, and isolation and quarantine of confirmed and suspected cases. The earliest measures were enacted in mid-September 1918, although some cities did not enact NPIs until mid-October. There was significant variation across cities in the type and duration of those interventions. In our sample of 50 cities, the total number of days of NPIs of all types ranged from 28 to 170 days (see Appendix Table A.1).

How did NPIs adopted by US cities affect local economic activity in general and invention specifically? In the rest of this section, we review relevant existing evidence, with an emphasis on the effects of the pandemic and NPIs on drivers of innovation.

First, NPIs may have been directly contractionary, as in the case of mandated social distancing and business closures. Further, limits on social interactions may have hindered local invention by reducing localized knowledge spillovers (Jaffe et al., 1993; Carlino and Kerr, 2015; Catalini, 2018; Atkin et al., 2020). Andrews (2019) provides evidence that Prohibition reduced invention by reducing interpersonal communication, especially informal interactions. However, the types and lengths of NPIs adopted during the 1918 pandemic suggest limited effects on invention through a social interaction channel. Markel et al. (2007) report that the NPIs were generally short-lived, with the median duration across categories of interventions between one and six weeks. Also, the most common types of NPIs—school closures—seem less likely to affect the kind of informal knowledge flows inventors rely on.

Second, NPIs may have anchored business expectations and mitigated the loss of organizational or intangible capital. The pandemic itself disrupted economic activity, even absent NPIs.

[3]See Beach et al. (2020) for more details on the historical context.

For example, labor supply contracted due to either sickness or fear of infection and businesses may have reduced investment in response to increased uncertainty. Increased uncertainty, business disruptions, and business failures can reduce R&D investment or even accumulated intangible or organizational capital (Dixit and Pindyck, 1994; Goel and Ram, 2001). By favoring a coordinated response to the pandemic, NPIs may have reduced uncertainty, anchored expectations, and preserved business capital, thus increasing post-pandemic invention. A complementary explanation is that NPIs may have had large indirect effects in mitigating output declines during a pandemic by shielding core sectors from surges in infection that could disproportionately affect output (Bodenstein et al., 2020).

There is mixed evidence on the effects of NPIs on economic activity. Examining outcomes at annual and biennial frequencies, Correia et al. (2022) find that NPIs had modest positive effects on manufacturing activity in the decade following the pandemic. However, Lilley et al. (2020), accounting for pre-trends in population growth 1910–1917, conclude that the estimated effect of NPIs on economic activity is "a noisy zero." Correia et al. (2022) also document a positive effect of NPIs on National Bank assets in the years immediately following the pandemic.

There is more agreement that the pandemic disrupted high-frequency measures of business activity, the production of intermediate inputs, and capital flows. Industrial production and coal production dropped (Velde, 2020), as did textile and lumber (Bodenhorn, 2020). Correia et al. (2022), using contemporary news sources, document widespread business disruption and uncertainty during and in the aftermath of the pandemic. Fluegge (2022) provides theory and evidence that temporary shocks to the local business environment during the pandemic led to persistent negative effects on city growth, driven by destruction of local entrepreneurial capital.

NPIs appear to have reduced peak mortality, with smaller to null effects on overall mortality. Correia et al. (2022) find, consistent with Markel et al. (2007) and Hatchett et al. (2007), that NPIs reduced peak mortality, and, to a lesser extent, cumulative excess mortality.[4] Consistent with this, Bodenhorn (2020), using weekly data on business disruptions and mortality, finds that

[4]Chapelle (2020) finds that NPIs significantly reduced mortality in 1918, but these reductions were partially offset with higher mortality in subsequent years. Finally, Clay et al. (2018) and Barro et al. (2020) find smaller effects of NPIs in reducing mortality that are statistically insignificant.

the economic losses due to the pandemic were due primarily not to NPIs, but to periods of high excess mortality reducing labor supply and therefore output. Bodenhorn (2020) also provides suggestive evidence that peak mortality, not NPIs, modestly increased business failures.

Thus, the net effect of NPIs on invention depends on the relative strength of these channels. If the preservation of organizational or intangible capital from lower peak mortality, reductions in business disruptions, or stabilized business expectations were stronger than the decrease in knowledge spillovers from limited social interactions, then NPIs might have had a net positive effect on patenting.

# 3 Data

We construct a new panel on NPIs and patenting rates at the city–month level. We add seven cities to the 43-city NPI database by Markel et al. (2007), drawing from an update of the same source archive (*Influenza Archive 2.0* (2016)). To be consistent with Markel et al. (2007), we define NPI length as the cumulative number of days of NPIs across three categories of interventions (public gathering bans, school closures, and isolation and quarantine). Like Markel et al. (2007), we are unable to provide a breakdown of NPI length by type, but a virtue of our extended sample is better coverage of fast-growing western and southern cities (see Appendix Table A.2).[5] Systematic information on the duration of NPIs is unavailable beyond the 50 cities in our extended sample.

We combine this with data on *ever-granted* patents from CUSP, which includes the near-universe of patents issued by the US Patent and Trademark Office (Berkes, 2018). We select patents for which at least one inventor resides in our sample of 50 cities. We construct a city-specific patent count variable by dividing each grant by the number of co-inventors and assign the corresponding fraction to each city-by-month observation. Importantly, CUSP includes the application filing date for each ever-granted patent, allowing us to assign patents to the month

[5] This extended coverage allows us to better identify Census-region-by-year fixed effects, which control for the substantial regional variation in city growth in the early decades of the 20th century.

of application instead of month of issuance (thus better reflecting the date of invention).[6] City populations are from the US decennial Censuses between 1900 and 1930,[7] and literacy rates, age, and ethnic-origin compositions are from the 1910 Census. Appendix A1 provides details on the various data sources used in the analysis.

Across our 50-city sample, the monthly patenting rate by city (patent count per 100,000 population) ranges from 0 to 28, with an average of 5.19 (see Appendix Table A.1). This amounts to an overall average of 27 patents per city per month, most of which are from single-inventor patents. The average duration of NPIs is 85.2 days, with a minimum of 28 and a maximum of 170. The share of cities classified as long-NPI cities (treatment group) is 0.36. Appendix Table A.3 shows that long- and short-NPI cities are balanced across many observable characteristics in 1910 and excess death rate during the 1918 pandemic. While some of the co-variates display small but statistically significant differences (Column 3)—with long-NPI cities recording higher patenting and literacy rates, average age, and share of population in prime age (20-60 years old), and a lower excess death rate—the two groups are statistically indistinguishable once we control for Census-region fixed effects, as in our preferred specification below (Column 4). This is consistent with the view that variation in NPI length is largely controlled by geographical factors and is otherwise uncorrelated with local demographic characteristics (Barro, 2020; Hatchett et al., 2007).

Figure 1 shows the mean log monthly patenting rates for long- and short-NPI cities between January 1916 and December 1920. The dashed lines show residualized log monthly patenting after demeaning by city and removing city-specific seasonality (with city-by-calendar-month fixed effects). To better visualize the underlying patterns, the solid lines show smoothed values of the residualized series estimated by local polynomial regression (with a bandwidth of 1.5 months on either side).

A few key patterns emerge from Figure 1. Before September 1918, both long- and short-

[6]Our measure of patenting rate based on the filing date of the patent (as opposed to patent granted date) is not impacted by delays at the patenting office caused by the pandemic. Further, there is no evidence of pandemic-related effects on the number of filed applications (see *Annual Reports of the Commissioner of Patents* (1918, 1919)).

[7]Intercensal values are linearly interpolated between the Aprils of each Census year.

NPI cities had similar trends in monthly patenting rates. Monthly patenting rates peaked in early 1917 and declined thereafter until late 1918. These declines likely reflect US entry into World War I and the mobilization of resources and labor for the war effort.[8] Both long- and short-NPI cities show sharp rebounds in patenting rates starting around October–November 1918. Short-NPI cities briefly exhibit higher patenting rates in mid-1919. However, through the end of 1920, long-NPI cities show a persistent increase in patenting rates compared with short-NPI cities.

# 4 Empirical Framework

We use a difference-in-differences (DD) design to identify the causal effect of NPIs on patenting rates. Figure 1 shows the timeline of the pandemic and how it relates to the time periods in our analysis. We define the months before September 1918 as the *Pre-treatment* period. In some specifications, we partition *Pre-treatment* into a *Before* period, from the beginning of the sample until one year before the pandemic started (August 1917), and a *Baseline* period covering the year before the pandemic (September 1917–August 1918). The *Post-treatment* period begins in September 1918, consistent with the onset of the most devastating wave of the pandemic and the implementation of earliest NPIs.

We use a Poisson Pseudo-Maximum Likelihood (PPML) estimator, which handles two features of the patenting data appropriately: zeros and heteroskedasticity. The baseline DD specification that compares outcomes pre- and post-pandemic makes the following conditional mean assumption for patenting rates:

$$E\left[\frac{Y_{cmt}}{P_{cmt}}\right] = \exp\left(\delta_{cm} + \delta_{my} + \delta_{ry} + \beta \cdot g(\mathrm{NPI}_c) \times \mathbb{1}[\mathrm{Post}\ 9/18]\right), \tag{1}$$

where $Y_{cmt}$ and $P_{cmt}$ are the number of (ever-granted) patents filed and population in city $c$ during month $m$ and year $t$, respectively. The indicator $\mathbb{1}[\mathrm{Post}\ 9/18]$ equals one in the *Post-*

[8] There may also have been some suppression of defense-related patent applications, though such efforts were more systematic in World War II (Gross, 2019).

*treatment* period. Since the 1910s and 1920s saw rapid social and economic changes, we test the robustness of the estimates to alternative choices for the starting and ending years.

In our preferred specification, we include city-by-calendar-month fixed effects (e.g., separate effects for Philadelphia Januaries vs. Februaries) to control for time-invariant determinants of inventive activities specific to a city, such as proximity to transportation networks (Perlman, 2016; Agrawal et al., 2017) or universities (Kantor and Whalley, 2014; Andrews, 2020), while also controlling for differential seasonality across cities. We also include month-by-year fixed effects (i.e., a separate effect for January 1916 and January 1917) to control for national trends in patenting, and Census-region-by-year fixed effects to control for the differential evolution of patenting trends in the North, South, Midwest, and West driven by factors such as the Great Migration of African-Americans from the South to northern cities (Collins and Wanamaker, 2014) or growing industrialization of the West (Kim and Margo, 2004).[9] With the inclusion of city-by-calendar-month, month-by-year, and Census-region-by-year fixed effects, any remaining threat to identification would have to operate through unobserved time-varying confounders at a spatial scale finer than region, e.g., state-by-year shocks correlated with both NPI adoption and inventive activities.

The primary treatment is a function $g(\cdot)$ of the number of days of NPIs imposed by each city. We focus on a binary treatment indicator that compares cities with shorter and longer NPIs (NPI duration of 90 days or more), but we also report estimates where the treatment is continuous and defined as the number of days of NPIs in each city. The goal of the analysis is to identify the causal effect of NPI length on the local patenting rate, as represented by the parameter $\beta$.

Identification of $\beta$ requires correct specification of the mean conditional on controls and fixed effects. This can be interpreted as a parallel trends assumption: patenting outcomes in short-NPI cities provide a valid counterfactual for patenting outcomes in long-NPI cities, in the absence of longer restrictions. We test the identifying assumption by probing the robustness of

[9] Andrews and Whalley (2020) describe the economic geography of innovation in the US over the last 150 years.

the estimates to different sets of fixed effects and controls that may predict invention rates (e.g., educational attainment trends). We also report pre-adoption (i.e., "pre-trends") estimates of the NPI effect as a test for possible bias due to uncontrolled confounders.[10]

We provide additional specifications to examine effect timing and to control for changes in industrial composition across cities. Our extended timing specification divides *Pre-treatment* into *Before* and *Baseline* periods and divides *Post-treatment* into *During* and *After* periods (see Figure 1 for a timeline). This directly tests for differences in pre-trends and highlights dynamic effects of longer NPIs on innovation *During* versus *After* the pandemic. In patent class specifications, we define the outcome as the number of patents in a city in a particular patent class; we then interact fixed effects with patent classes. This fixed effects regime controls for both differences in the ex ante distribution of patenting by technology class across cities and in the evolution of classes. A possible driver of these differences is industrial composition, which was evolving rapidly in response to WWI and the subsequent armistice.

# 5 Results

Table 1 reports the baseline estimates of $\beta$ from Equation (1) as well as from the extended specification. The PPML estimates can be interpreted as the effect of NPI length on log patenting rates (i.e., in percentage terms). Panels A-D present estimates for two different functional form assumptions on $g(\cdot)$ and two different specifications of the DD model. First we consider the patenting rate in a city-month. Columns (1) to (3) introduce different sets of fixed effects in the regressions while Column (4) focuses on an alternative time window for the sample. Columns (5) and (6) consider patenting rates in a class-city-month, using the same sample and specification as Column (3), but adds class-city-month fixed effects. Standard errors clustered by city are reported in parentheses.

[10] Our application follows the standard DD model with one treatment group, one control group, and a single time period where treatment status changes in the treatment group. In such models, the DD estimand identifies the average treatment effect on the treated even in presence of treatment-effect heterogeneity (Card and Krueger, 1994; Abadie, 2006), unlike in the staggered adoption setting (e.g., de Chaisemartin and D'Haultfœuille, 2020; Goodman-Bacon, 2018).

The results in Panel A show that long-NPI cities had higher patenting rates than short-NPI cities after the pandemic. The simple DD estimates indicate a 5.6% to 8.6% increase in monthly patenting.[11] All these estimates are statistically significant at the 5% level; the choice of fixed effects and starting and ending years is mostly inconsequential. Our preferred specification in Column (3) includes city-by-calendar-month, Census-region-by-year, and month-by-year fixed effects. In that specification, the effect of long NPIs on patenting rate is 7.4%, with a 95% confidence interval ranging between 1.7% and 13.1%. The magnitude of these estimates is similar to the effect of prohibition estimated by Andrews (2019), who finds that the adoption of statewide prohibition reduced patenting in previously wet counties by 8% to 18%.

Columns (5) and (6) support the same conclusion by comparing patenting within technology classes. These specifications—which use three-way fixed effects that interact two-way fixed effects for city-month, month-year, and region-year with patent-class fixed effects—control for different specialization in innovation and production to specific technology classes across cities (e.g., textiles, transportation, etc). This is potentially important since the armistice at the end of WWI affected production (and innovation) differentially across technology classes, potentially confounding the impact of NPI status on patenting activity. However, the similarity of Columns (5) and (6) to the baseline estimates suggests that this confounding factor is not a first-order threat.

The extended estimates in Panel B show that the positive effect of NPI length on invention is largely driven by the period *After* the pandemic ended, versus *During* the pandemic (although the *After* - *During* differences are not statistically significant). Importantly, the extended DD estimates also show no statistically significant evidence of pre-pandemic differences in patenting rates across treatment and control cities. This result supports the main identifying assumption of difference-in-differences models.[12]

Panel C presents the same analysis as in Panel A, but for a specification where NPI length

[11]We follow the convention of interpreting log point differences as approximating percentage differences in the rest of the paper.

[12]The fact that our results are driven by the years after the pandemic suggests that they are unlikely to be explained by NPIs affecting the time lag between conception of an idea and patentable invention.

enters linearly in number of days (divided by 30 for ease of interpretation). While less precise, the results in Panels C and D confirm the findings in Panels A and B. Cities with more months of NPIs had higher monthly patenting rates in the period during and after the pandemic. The extended DD results in Panel D further confirm the absence of significant pre-trend differences across NPI groups length and that the NPI effect on patenting rate primarily operates through an impact on patenting rates in the *After*-pandemic period.

Finally, in order to more flexibly investigate pre- and post-pandemic differences in patenting rates between long- and short-NPI cities, we also estimate event-study variants of Equation (1). Figure 2 reports these results. We aggregate the data to an annual frequency, shifting the start of each year by four months earlier so that no years cover both pre- and post-pandemic months (e.g., 1918 sums patents over September 1917–August 1918). The top panel shows the coefficient estimates on the binary long-NPI treatment interacted with year indicators, while the bottom panel interacts the continuous measure of cumulative NPI days with year indicators. The shifted year of September 1917 to August 1918 is the reference category and the regression models for Figure 2 include city and year fixed effects. Both specifications of NPI length show little evidence of pre-trend differences, as the point estimates (black circles) are small and the 95% confidence intervals (in gray) always include zero. Beginning in 1919 (i.e., 9/18–8/19), however, these estimates become larger, positive, and significantly different from zero in about half the years.

## 5.1 Robustness

We briefly discuss the results of the robustness analyses reported in Appendix Table A.4. Taken together, the evidence in Appendix Table A.4 confirms our main finding in Table 1 that longer NPI periods led to a positive and statistically significant increase in patenting rates. In particular, the results are robust to using only the original 43-city sample of Markel et al. (2007) (Columns 3 and 4), to controlling for city-levels indicators of pandemic severity (Columns 5 and 6), and to the inclusion of linear time trends interacted with city-specific indicators of human capital (Columns 7 and 8) and age composition (Columns 9 and 10). In Columns (11) and (12) we control for linear time trends interacted with the 1910 share of local population of German descent, to account for the potentially confounding effect of the decline (and later resumption) of knowledge flows between US and German scientists during World War I (Iaria et al., 2018). Adding a linear time-trend interacted with city-specific NPI length or city-specific linear time trends to the baseline specification leads to a 35% to 55% increase in the standard errors, but does not meaningfully change the magnitude of the estimates of the NPI effect (Columns 13 and 14). Appendix Table A.7 further documents the robustness of the central estimates to alternative approaches for calculating the population estimates used to construct the patenting rates.[13]

In order to probe the potential influence of any particular city on our baseline results, we perform a jackknife-like exercise and report the estimated coefficients for 50 DD regressions where each specification drops one city from the estimation sample. Appendix Figure A.2 shows that the estimates of the impact of NPIs on patenting activity are generally stable and statistically significant across all jackknife samples. Finally, in Appendix Figure A.3 we investigate the robustness of our main estimates to alternative thresholds to define long- and short-NPI cities. We consider a series of regressions where the threshold in days of NPIs to switch from "short" to "long" varies from a low of 41 days to a high of 154 days (the 10th and 90th percentiles in the NPI duration distribution). The results indicate that for a range of thresholds from 78 days or greater to 143 days or greater, the corresponding estimates are similar in magnitude to those in Table 1 and statistically significant.

[13]We performed additional tests and analyzes to confirm our main results are not confounded by other events that occurred around the time of the pandemic. Appendix Table A.5 shows that accounting for the implementation of prohibition during our sample period does not meaningfully alter our baseline estimates of the impact of NPIs. To assuage concerns that our results are driven by confounding city-level political preferences, we verify in supplementary analysis that NPI length is uncorrelated with local political leaning (as measured by county-level outcomes in the 1916 presidential election) and that our results are robust to mayoral party affiliation.

## 5.2 Interpretation and Additional Results

Table 2 explores various dimensions of heterogeneity in the impact of NPIs that can be addressed with our detailed patent data. For brevity, we focus only on our preferred empirical specification (column 3 in Table 1).

Panel A shows the estimated effect of NPIs separately by inventor status (single versus multiple investors) and by patent ownership status (patents owned by the inventors themselves versus other assignees such as firms or universities). The estimates indicate that long NPIs increased patenting rates across most categories of inventor status and patent ownership. Notably, the positive effect of long NPIs is substantially larger for teams of multiple inventors irrespective of patent ownership (20%, in column 2) compared to single inventor patents (6.2%, in column 1). The estimates in Columns (7) and (8) further show a larger effect for multi-inventor patents with an assignee. The increasing magnitudes of the estimates moving rightward in Columns (5)–(8) point to an ordering of these impacts: there is no statistically significant effect for single-inventor, no-assignee patents; there is a small significant effect for single inventors associated with an assignee; there are larger effects for multi-inventor patents without an assignee; and the largest effects are for multi-inventor patents with an assignee.

Panel B reports estimates of the effect of NPI length on patenting rates for different technology classes. We use the 8 main patent categories in the Cooperative Patent Classification, labelled A through H; example of classes include "Human necessities" (Class A), "Mechanical engineering" (Class F), and "Electricity" (Class H). The estimated effect of NPI length on invention varies across technology classes. We find statistically significant positive impacts of longer NPIs for Class D (Textiles and paper), Class F (which includes Mechanical engineering, Lighting, and Heating), and Class H (Electricity). For the other classes, the estimates are not statistically significant. Two of the three technology classes that saw increased patenting in response to longer NPIs (classes F and H) were rapidly expanding and gaining importance in the invention landscape in the 1910s and 1920s, at the expense of shrinking fields, such as class A, which includes Agriculture (Kelly et al., 2021, Berkes et al., 2022).

Although there are several possible interpretations of these findings, these results provide useful evidence that helps inform some of the channels linking NPI length with increased patenting during and after the pandemic. Because multi-inventor patents were not negatively affected in long-NPI cities, reductions in social interactions during the pandemic do not seem to have produced, on net, a decline in invention. Instead, the results on patents owned by an assignee and multi-inventor patents suggest that NPIs may have preserved inventive factors important for coordinating inventing teams or organizations.

Further, the results by technology classes suggest that NPIs may have had a positive impact on local invention by reducing business uncertainty and stabilizing local financing conditions. Emerging fields were likely characterized by a higher degree of technological and market risk, making the access to the necessary resources difficult and the presence of a healthy system of financial intermediation critical for invention.[14] Correia et al. (2022) analyze contemporaneous news data and find that the pandemic generated considerable stress on the financial markets. However, they also find that national banks' assets grew more in cities with longer NPIs after the pandemic (although the difference is not statistically significant). Insofar as national banks' assets partly reflect the strength of the local banking sector, an improvement in the conditions of financial intermediation and an overall decrease in economic uncertainty can explain the larger positive response of invention to NPIs in newly emerging, and possibly more risky, technological domains.

Panel C tests for differential impacts of NPIs on patenting rates by inventor age, mobility inside the United States, and birthplace (Columns 1-6). Based on this simple analysis we do not find definitive evidence that NPIs differentially impacted inventors based on age, mobility, and birthplace. This suggests that variation in NPIs length across cities did not lead to a migratory response by inventors, and that changes in international migration flows after WWI are unlikely to confound our baseline results.

Finally, we also estimate the impact of NPIs for "higher quality" patents. To this end,

[14]Nanda and Nicholas (2014) show that during the Great Depression measures of local bank distress were associated with lower firm-level patenting rates and a shift towards more incremental and less risky inventions.

we focus on patenting rates for two definitions of "breakthrough patents" (Kelly et al., 2021). The estimates, reported in Columns (7) and (8) in Panel C are qualitatively similar to those from the overall patenting rate, albeit with larger standard errors. This rules out, at least to a first order, that our baseline results are driven by an impact of NPIs on low-quality patents, providing evidence that the effect of NPIs on patenting reflects, at least in part, an increase in valuable innovation.

# 6 Discussion and conclusion

This paper analyzes the effect of NPIs on invention using a difference-in-differences approach and panel data on patenting rates and NPI duration for 50 large US cities in the context of the 1918 flu pandemic. Cities that responded to the pandemic with longer NPIs did not experience a relative decline in patenting rates during the pandemic—in fact, they experienced significantly higher relative patenting rates in the years after the pandemic ended. Longer NPIs had even larger positive effects on patenting by multiple inventors, patents owned by external assignees, and patenting in rapidly growing classes.

These findings contribute new evidence on the economic consequences of the 1918 pandemic (Beach et al., 2020). Previous research has focused on contemporaneous measures of economic activity, such as manufacturing employment (Correia et al., 2022, Lilley et al., 2020). In contrast, we study patenting rates, a measure that links current economic activity to future outcomes. The literature has linked invention rates to long-run aggregate and firm-level productivity growth (Jones, 1995; Kortum, 1997; Kogan et al., 2017) and to variation in long-run population growth across cities (Kantor and Whalley, 2021; Berkes et al., 2022). In light of this literature, our findings suggest that the adoption of NPIs might have contributed to higher aggregate productivity growth, and that cities adopting more stringent NPIs might have experienced a positive persistent effect on their long-run economic performance.

Why did restrictions on interactions and activity during the 1918 pandemic not cause a reduction in patenting rates? Modern evidence suggests that personal interactions are an important factor in innovation (Atkin et al., 2020; Boudreau et al., 2017). Historically, reduced social interactions during Prohibition had significant negative effects on patenting rates (Andrews, 2019). Two factors reconcile our results with previous findings. First, the most common NPI in 1918 was school closure (Markel et al., 2007). Compared with business closures or public gathering bans, school closures seem least likely to hinder the interactions that matter for invention. Second, even in the absence of mandated closures or social distancing, people in short-NPI cities may have voluntarily limited their own social interactions to reduce their risk of exposure.[15] Rather than challenging the evidence on the importance of in-person contacts for invention, our results suggest that NPIs might have helped prevent a decline in patenting rates by decreasing uncertainty, and preserving intangible and organizational capital, without disproportionately hindering knowledge flows in cities.

Although both the 1918 pandemic and the COVID-19 pandemic featured contagious respiratory viruses and the adoption of NPIs to slow the spread of disease, a few factors complicate direct comparison. First, modern communication technologies might be a substitute for many of the social interactions that favor idea flows. Second, the two pandemics were markedly different in overall mortality and extent of public health response. NPIs in 1918 were shorter and less extensive than the ones implemented in 2020. These factors make it difficult to extrapolate from the positive effects of NPIs in 1918 on patenting rates to the present day.

What can we learn from history, then? Our evidence suggests that the decrease in local interactions constitutes only part of the effect of NPIs on invention rates. These results highlight the importance of considering the impact of NPIs on invention rates through a wide range of channels, and can be informative for future research on how the design of NPIs can help promote invention rates and productivity growth in the aftermath of pandemics.

[15] Goolsbee and Syverson (2020) find that during the COVID-19 pandemic, mandatory mobility restrictions in the US explain only a small fraction of the observed decline in overall consumer traffic. Individual choices played a more important role in reducing mobility.

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Figure 1: Monthly Patenting Rates in Short- and Long-NPI Cities

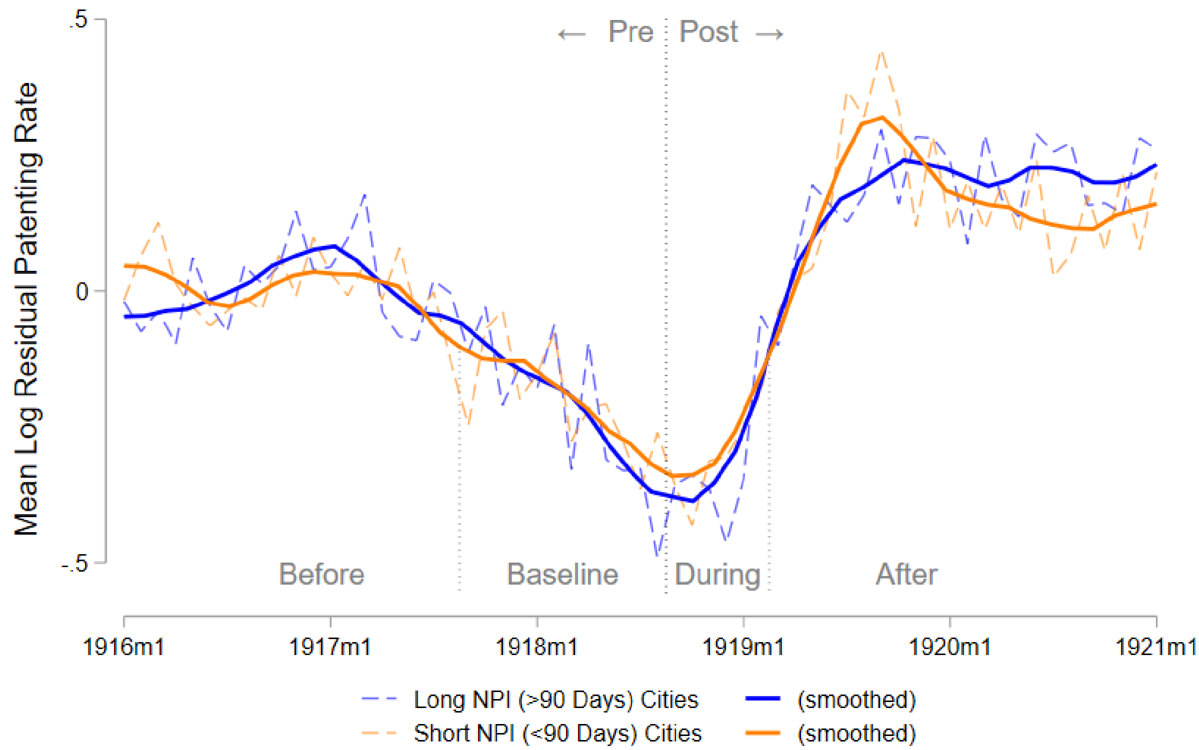


Figure 1 displays residualized log monthly patenting rates after demeaning by city and removing city-specific seasonality with city-by-calendar-month fixed effects (dashed lines). The average residualized monthly patenting rate for long- (short-)NPIs cities is shown in blue (orange). The solid lines are smoothed values of the residualized series estimated by local polynomial regression with a bandwidth of 3 months. Figure 1 also provides the timeline of the pandemic and defines the critical time periods that underlie our analysis. The *Pre-treatment* period corresponds to the period before September 1918, which we also divide into the *Before* period (from the beginning of the sample until one year before the pandemic started (August 1917)), and the *Baseline* period (September 1917 - August 1918). The *Post-treatment* period begins in September 1918 and is split into a *During* period (the period of seven months during which the flu was most active (September 1918 through the end of March 1919)), and an *After* period (from April 1919 to the end of the sample).

Figure 2: Event Study Analysis (Aggregated to Shifted Years)

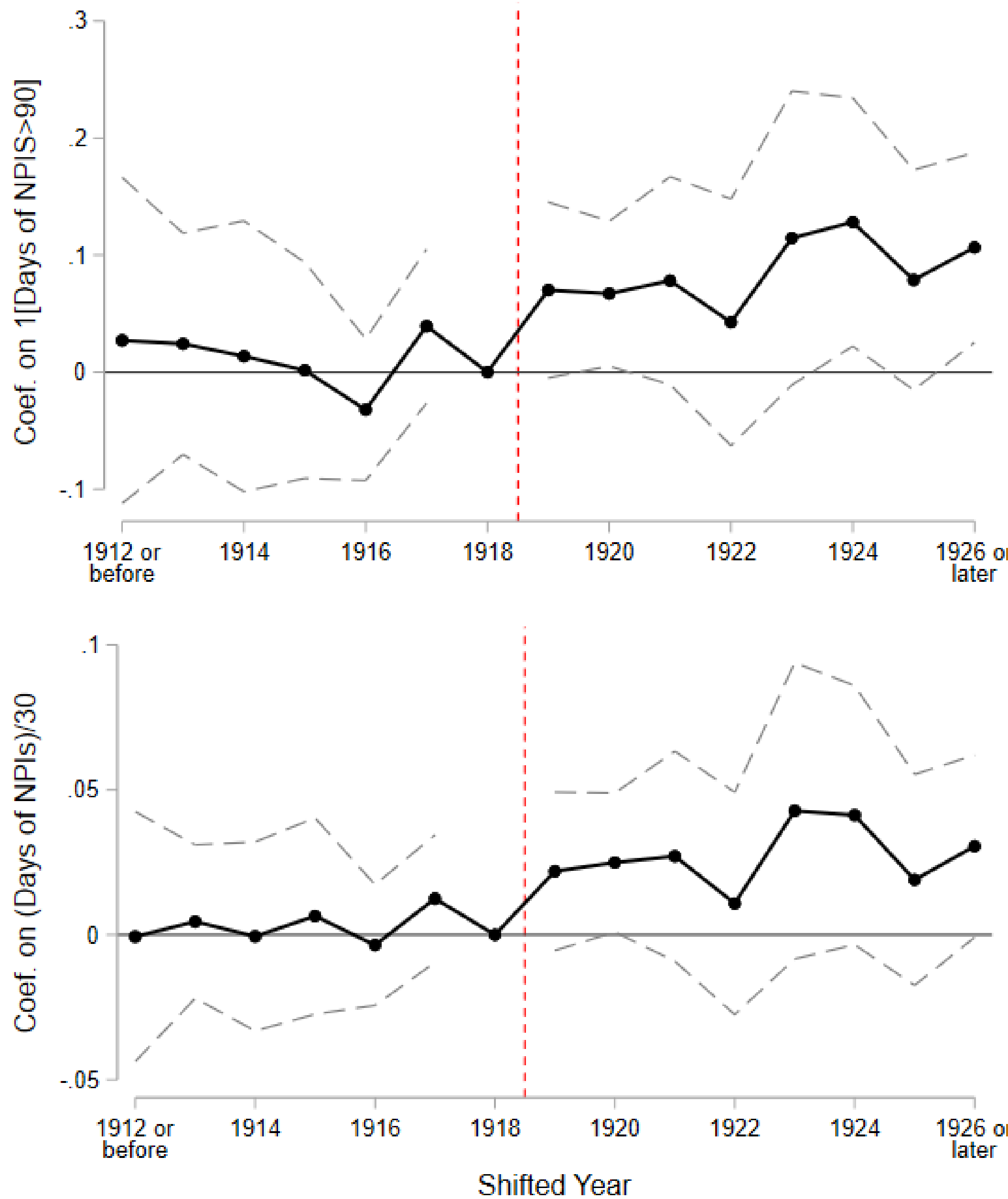


Figure 2 reports estimates from event-study variants of Equation (1). The city-month data on NPIs and patenting rates is aggregated to an annual frequency represented by shifted years (where the start of each year is shifted by four months to the left (e.g., so that 1918 includes September of 1917 to August of 1918)). The top panel shows the coefficient estimates on the binary measure of treatment (cumulative NPIs longer than 90 days) interacted with year indicators, while the bottom panel reports the same for the continuous measure of cumulative days of NPIs (divided by 30). The regression models include city and year fixed effects, and the shifted year corresponding to September of 1917 to August of 1918 is the reference category. Point estimates are shown by the black circles, and the 95% confidence intervals (dashed lines in gray) are cluster-robust at the city level.

Table 1: Effect of NPI Length on Patenting Rate

| | Patents in City-Month | | | | Patents in Class-City-Month | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| **NPI Length = 1[NPIs > 90 Days]** | | | | | | |
| *Panel A. Simple DD* | | | | | | |
| Post Pandemic × NPI Length | 0.056* | 0.065* | 0.074* | 0.086* | 0.066* | 0.057* |
| | (0.027) | (0.027) | (0.029) | (0.044) | (0.029) | (0.027) |
| *Panel B. Extended DD* | | | | | | |
| Before Pandemic × NPI Length | 0.013 | 0.007 | 0.012 | 0.032 | 0.011 | 0.013 |
| | (0.027) | (0.027) | (0.033) | (0.038) | (0.034) | (0.034) |
| During Pandemic × NPI Length | 0.049 | 0.040 | 0.058 | 0.069+ | 0.065 | 0.046 |
| | (0.041) | (0.041) | (0.040) | (0.039) | (0.041) | (0.039) |
| After Pandemic × NPI Length | 0.068* | 0.076* | 0.088* | 0.117** | 0.075* | 0.071+ |
| | (0.033) | (0.032) | (0.035) | (0.045) | (0.034) | (0.037) |
| **NPI Length = Days of NPIs ÷ 30** | | | | | | |
| *Panel C. Simple DD* | | | | | | |
| Post Pandemic × NPI Length | 0.017+ | 0.020* | 0.024* | 0.030+ | 0.019 | 0.016 |
| | (0.010) | (0.010) | (0.011) | (0.017) | (0.012) | (0.012) |
| *Panel D. Extended DD* | | | | | | |
| Before Pandemic × NPI Length | 0.008 | 0.006 | 0.008 | 0.009 | 0.003 | 0.004 |
| | (0.010) | (0.010) | (0.011) | (0.015) | (0.011) | (0.011) |
| During Pandemic × NPI Length | 0.016 | 0.011 | 0.018 | 0.023+ | 0.022 | 0.016 |
| | (0.014) | (0.014) | (0.013) | (0.013) | (0.015) | (0.015) |
| After Pandemic × NPI Length | 0.023+ | 0.026* | 0.032* | 0.040* | 0.020 | 0.019 |
| | (0.013) | (0.013) | (0.016) | (0.020) | (0.015) | (0.016) |
| Fixed Effects | | | | | | |
| City | X | - | - | - | - | - |
| Month-Year(-Patent Class) | X | X | X | X | X | X |
| City-Month(-Patent Class) | - | X | X | X | X | X |
| Region-Year(-Patent Class) | - | - | X | X | X | X |
| Sample coverage | | | | | | |
| begins January of | 1916 | 1916 | 1916 | 1910 | 1916 | 1916 |
| ends December of | 1920 | 1920 | 1920 | 1926 | 1920 | 1920 |
| Patent Class | - | - | - | - | 1 digit | 3 digit |
| *N* | 3000 | 3000 | 3000 | 10200 | 19830 | 101256 |

Notes: Table 1 reports DD estimates of the effect of NPI length on patenting rates. The sample includes the 50 cities for which we have information on NPI length (see Appendix Table A.2). The dependent variable is the patenting rate (patents filed/population) in a city-month in Columns (1)–(4), and in a 1- or 3-digit patent class in a city-month in Columns (5) and (6), respectively. The treatment variable is an indicator of NPI length: a binary indicator for NPI period longer than 90 days (Panels A and B) or the number of days of NPIs divided by 30 (Panels C and D). All specifications are estimated using a Poisson Pseudo-Maximum Likelihood (PPML) with the exposure variable set to a linear interpolation of city population. The estimated coefficients can be interpreted as a percentage change in the patenting rate. Standard errors are cluster-robust at the city level. + p<0.10, * p<0.05, ** p<0.01, *** p<0.001.

Table 2: Effect of NPI Length on Patenting Rate by Co-Inventor and Assignee Status, and by Patent Technology Class

| **Panel A: By Co-Inventor & Assignee Status** | Single Inventor (1) | Multiple Inventors (2) | No Assignee (3) | With Assignee (4) | Single Inventor, No Asgn. (5) | Single Inventor, W/ Asgn. (6) | Multiple Inventors, No Asgn. (7) | Multiple Inventors, W/ Asgn. (8) |
|---|---|---|---|---|---|---|---|---|
| Post Pandemic × NPI Length | 0.062+ | 0.200*** | 0.055 | 0.101* | 0.046 | 0.085* | 0.163* | 0.259* |
| | (0.033) | (0.055) | (0.042) | (0.041) | (0.045) | (0.041) | (0.075) | (0.117) |
| Mean of Dep. Variable | 24.80 | 2.41 | 15.84 | 11.39 | 14.72 | 10.37 | 1.34 | 1.40 |

| **Panel B: By Patent Class** | Class A Human Necessit. (1) | Class B Operat.; Transport. (2) | Class C Chemist.; Metal. (3) | Class D Textiles; Paper (4) | Class E Fixed Construct. (5) | Class F Mech. Engr. (6) | Class G Physics (7) | Class H Electric. (8) |
|---|---|---|---|---|---|---|---|---|
| Post Pandemic × NPI Length | 0.012 | 0.056 | -0.078 | 0.240* | -0.074 | 0.148** | 0.169 | 0.213*** |
| | (0.070) | (0.041) | (0.153) | (0.116) | (0.082) | (0.053) | (0.114) | (0.061) |
| Mean of Dep. Variable | 4.76 | 9.43 | 1.57 | 1.11 | 1.86 | 5.00 | 2.52 | 2.56 |

| | Inventor Age | | Inventor Mobility | | Inventor Birthplace | | Breakthrough Patents | |
|---|---|---|---|---|---|---|---|---|
| **Panel C: Inventor & Patent Heterogeneity** | ≥Median (1) | <Median (2) | Stayers (3) | Non-Stayers (4) | US (5) | Inter-national (6) | 5-year Window (7) | 10-year Window (8) |
| Post Pandemic × NPI Length | 0.102* | 0.166*** | 0.096* | 0.069* | 0.119*** | 0.053 | 0.061 | 0.087 |
| | (0.046) | (0.040) | (0.048) | (0.033) | (0.033) | (0.039) | (0.070) | (0.061) |
| Mean of Dep. Variable | 11.00 | 11.15 | 8.58 | 18.90 | 15.50 | 11.44 | 14.31 | 14.46 |

Notes: Table 2 reports DD estimates of the effect of NPI length on patenting rates by co-inventors and by assignee status (Panel A), patenting class (Panel B), and inventor and patent characteristics (Panel C). The sample includes the 50 cities for which we have information on NPI length and covers the period January 1916 to December 1920. The dependent variable is the patenting rate (patents filed/population) in a city-month. The treatment variable is a binary indicator for NPI period longer than 90 days. Panel C, Columns (1)–(6) probabilistically match patents to inventor characteristics in the Census. Columns (1)–(2) split by median inventor age, 40. Column (3) counts patents by inventors who have filed at least two patents in the same location, once before Sep. 1918 and once after; Column (4) counts the remainder of patents, including those by first-time inventors. Columns (5)–(6) split by inventor birthplace. Columns (7)–(8) consider breakthrough patents defined as patents above the median in terms of the breakthrough measures proposed by Kelly et al. (2021). All specifications are estimated using a Poisson Pseudo-Maximum Likelihood (PPML) with the exposure variable set to a linear interpolation of city population and include month-by-year, city-by-month of year, and region-by-year fixed effects. The estimated coefficients can be interpreted as a percentage change in the patenting rate. Sample size is 3000 city-months before removing collinear fixed effects. Standard errors are cluster-robust at the city level. + p<0.10, * p<0.05, ** p<0.01, *** p<0.001.

# Appendix

## A1 Additional Details on Data Sources

We provide additional details related to the data collection and processing below. The summary statistics for the key variables in our analysis (primary sample window of January 1916 to December 1920) are shown in Table A.1.

### A1.1 Comprehensive Universe of US Patents (CUSP)

Data on the number of patents filed by date at the city level are taken from the Comprehensive Universe of US Patents (CUSP). For the analysis in this paper, these data represent the near universe of the filing date (by city) of all *ever-granted* patents. Details on the procedure behind the data collection and georeferencing can be found in Berkes (2018). CUSP contains information on technology classes (as they appear on the USPTO website in June 2016), name and location (at the city level) of inventors and assignee, filing date, and issue date. The estimated coverage of this data set is above 90% in each year between 1836 and 2010.

Some patents have multiple inventors whose locations are not in the same city. For patents with $N \geq 2$ inventors, we assign $1/N$ of the patent to each city associated with an inventor. Because more than 90% of the patents have a single inventor, and inventors for many multi-inventor patents are often in the same city, the precise way that we assign multi-inventor patents makes little difference.

### A1.2 Extending the Markel et al. (2007) Sample

Markel et al. (2007) provide the standard data set for NPI length during the 1918 pandemic. Their data includes the sum of days of enforcement for each type of NPIs in 43 cities for which they obtained a complete history of NPIs *and* weekly influenza data. The limiting factor is availability of weekly influenza data (Beach et al., 2020).

We use the 43 cities included in Markel et al. (2007) and add seven cities for which there is systematic historical documentation of responses to the 1918 pandemic in the *Influenza Archive 2.0* (2016), an extension of the historical data collected in support of Markel et al. (2007). These seven cities are Atlanta, GA; Charleston, SC; Dallas, TX; Des Moines, IA; Detroit, MI; Salt Lake City, UT; and San Antonio, TX. Whereas the 43 original cities in Markel et al. (2007) were primarily located in the Northeast and Upper Midwest, five of these seven additional cities are in Southeast and West.

We collect the number of days of school closures and public gathering bans for these seven cities. If there were multiple closure events, we sum the days across events. We report the information for the seven additional cities in the table below, and show the distribution of NPI length in 5-day bins for the full sample of 50 cities in Figure A.1. Importantly, there appears to be a substantial gap in NPI length around 90 days; no city has NPI length between 82 and 99 days. We view this as a natural gap in the distribution and define our binary definition of treatment around 90 days of NPI length. We assess sensitivity to this threshold in Section A2.1.

NPI Lengths for Seven Additional Cities not Included in Markel et al. (2007)

| City | Days of School Closure | Days of Public Gathering Bans | Mandatory Quar. & Isol. | Total NPI Days | Long NPI |
|---|---|---|---|---|---|
| Atlanta | 27 | 19 | No | 46 | No |
| Charleston | 37 | 32 | Unknown | 69 | No |
| Dallas | 18 | 23 | Unlikely | 41 | No |
| Des Moines | 46 | 10 | No | 56 | No |
| Detroit | 11 | 18 | Unknown | 29 | No |
| Salt Lake City | 60 | 81 | Some | 141 | Yes |
| San Antonio | 45 | 36 | Unknown | 81 | No |

Markel et al. (2007) also include a third category to create their measure of total NPI days: mandatory isolation and quarantine (I&Q) requirements. We were not able to conclusively document legal requirements for I&Q for the seven additional cities, as the historical record covering I&Q appears sparser than those for other categories of NPIs. We were able to establish that Iowa had a statewide regulation banning mandatory I&Q, and Atlanta chose not to implement such measures. Salt Lake City likely had mandatory I&Q measures, but the period of coverage is unknown. On at least one date (10/14/1918), Dallas' health officer decided not to request the power to dictate I&Q. For Dallas and the other three cities, we are unable to determine whether mandatory I&Q measures were ever implemented.

We note several reasons why the NPI data limitation in the additional seven cities is unlikely to meaningfully influence our estimates of the impact of NPI length on patenting rates. First and foremost, our preferred measure of NPI duration discretizes NPI length for periods longer or shorter than 90 days. The contribution of mandatory I&Q to total NPI length is likely less than the school closures or public gathering bans (as discussed below), and so it is unlikely that adding a few days to total NPI length would switch any short-NPI cities to long-NPI cities. Furthermore, short-NPI cities generally had shorter NPIs in all categories. Nevertheless, we test the robustness of our main estimates to varying the threshold of the number of NPI duration in days to separate between short- and long-NPI cities in Figure A.3 below.

Second, mandatory I&Q measures appear to have been unpopular and used less commonly than other measures. Table 2 of Markel et al. (2007) is a bit difficult to parse, but appears to indicate that mandatory I&Q measures are used in fewer instances than other measures and combinations of measures, and when used alone, are used for relatively short periods of time. Moreover, at least one state (Iowa) banned mandatory I&Q measures as a violation of civil liberties, leading us to believe that they were relatively unpopular.

We also show that our primary results hold when considering only the 43 cities in Markel et al. (2007). Columns (3) and (4) in Appendix Table A.4 replicate the Table 1 analysis and show that the estimated coefficient magnitudes in the 43 city and the 50 city samples are similar. Appendix Figure A.2 further documents the robustness of our main results to dropping individual cities from the estimation sample.

### A1.3 Other data sources

We augment our estimation sample with city-level data on total population from the historical US Decennial Censuses, 1900-1930. Intercensal values are linearly interpolated between the Aprils of each Census year. We also construct city-level controls for literacy rates (share of the population that could both read and write) and schooling (share of the population enrolled in school) in the 1910 Decennial Census. For each city we additionally calculate the the share of people whose reported birthplace was Germany or one of its regions (e.g., Bavaria) and whose mother tongue is either German or one of its dialects (e.g., Austrian). These measures allow us to estimate the prevalence of immigrants of German descent in the cities we consider.

## A2 Alternative Results and Robustness

### A2.1 Sample Composition and Treatment Definition

Our sample of 50 cities with NPI length information includes an heterogeneous mix of cities at the time of the 1918 pandemic, from larger ones like New York City and Chicago to relatively smaller cities such as Des Moines and San Antonio (see Appendix Table A.2). We use a jackknife approach to test if our main results are driven by the inclusion or exclusion of any single city in the sample. Figure A.2 shows 50 estimated coefficients obtained by alternately leaving one city out of the estimation sample (jackknifing). The figure reports the estimated coefficients for both the binary and continuous NPI length models as in Table 1, Column (3).[16] The top panel shows jackknife replicates for binary treatment sorted by the duration of NPI length (in days)

[16]This specification includes city-by-month of year and Census-region–year fixed effects and uses the data from 1916–1920.

for the omitted city. All estimates of the effect of longer NPIs on patenting rates range between 0.06 and 0.09 and are statistically significant at the 5% level as in Table 1 (with the exception of 1 sub-sample out of 50, shown by the blue cross). Similarly, the bottom panel shows jackknife replicates for the continuous measure of treatment (number of days of NPI divided by 30). The resulting estimates range between 0.02 and 0.03 and in 44 out of the 50 samples are generally statistically significant at the 5% level. Importantly, across both panels there is no systematic evidence of a correlation between the jackknife replicates and NPI length.

From the jackknife replicates reported in Figure A.2, it is straightforward to calculate jackknife estimates of treatment effects by averaging the 50 leave-one-out estimates. For the binary treatment measure we obtain an estimate of 0.074 with a standard error of 0.035 while for the continuous measure of treatment we obtain an estimate of 0.024 with a standard error of 0.014. Overall the evidence in Figure A.2 is similar to the main results in Column (3) of Table 1, and indicates that these results are stable across the jackknife sub-samples and that no single city has a great leverage on our estimates, which is reassuring given the small number of cities in our sample.

Next we investigate the robustness of our estimates based on the binary treatment to changing the threshold in the number of days that separates long- and short-NPI cities. That is, we want to ensure that our results are not driven by the choice of 90 days of cumulative NPI length to separate long- and short-NPI cities.

Appendix Figure A.3 reports estimates of $\beta$ from Equation (1) (along with the 95% confidence intervals) from a series of regressions where the threshold in days of NPIs to switch from "shorter" to "longer" varies from 41 days (the 10th percentile in the NPI duration distribution) to 154 days (the 90th percentile in the NPI duration distribution). Throughout the regression, models are based on the preferred specification of Column (3) in Table 1. The results indicate that for a range of thresholds from 78 days or greater to 143 days or greater, the corresponding estimates of $\beta$ are similar in magnitude to those in Table 1 and statistically significant. Estimates based on treatment group thresholds at the lower end (most cities in treatment group) and upper end (few cities in treatment group) are less precise.

## A2.2 Addressing Potentially Confounding Covariates and Trends

Appendix Table A.3 compares demographic characteristics computed from the 1910 decennial Census and a measure of severity of the 1918 pandemic across long and short-NPI cities. To measure pandemic severity at the city level, we use the excess pneumonia and influenza mortality per 100,000 population during the 24 weeks from September 8, 1918, through February 22, 1919, as reported in Markel et al. (2007).[17] While some of the variables in Table A.3 display small but significant differences between the two groups (Column 3), with long-NPI cities recording higher patenting and literacy rates, higher average age, a higher share of population in prime age (20-60 years old), and lower excess mortality during the pandemic, the covariates in the two groups become statistically indistinguishable once we look at variation within Census regions, as in our preferred specification (Column 4).

Columns (5) and (6) in Appendix Table A.4 estimate the same set of models as Table 1 using the binary specification of treatment, but include an interaction between the log of excess mortality at the city level and indicators for the Post period (Simple DD model), and indicators for the *Before*, *During*, and *After* periods (Extended DD model). The results in Appendix Table A.4 show that adding the interactions with city-level pandemic severity does not alter our baseline estimates of the effect of NPIs on patenting rates. Moreover, the interactions with pandemic severity themselves are imprecisely estimates and statistically insignificant (estimates not reported).

Columns (7) and (8) in Appendix Table A.4 are based on the preferred specification in Table 1, but include controls for literacy and schooling. Since education may be linked to invention, this specification investigates whether controlling for differences in educational attainment across cities alters our baseline results. Data on educational attainment at the city level during the early 20th century are limited. We draw on two variables available from the 1910 Census: the share of the adult population that is literate, and the share of the total population enrolled in school. Since we are concerned that these measures of education after the pandemic may reflect a response to the pandemic (and thus be 'bad controls'), we include them in the models with a linear time trend interacted with the 1910 shares of these variables. The age composition of the local population might also confound the results, since it is likely correlated with innovation capacity and with service in World War I. To account for this, in Columns (9) and (10) we display estimates of a specification in which we control for the 1910 share of local population in prime age (20 to 60 years old) interacted with a linear time trend. Finally, the disruption (and later resumption) in the knowledge flows between German and US inventors during World War I might also be a confounding factors. In Columns (11) and (12), we show results when including a control for the 1910 share of population of German descent, again interacted with a linear time trend.

The results in Columns (7)-(12) of Appendix Table A.4 indicate that adding these measures of education, age composition, and ethnic origin does not meaningfully alter our baseline results. There are two main findings: First, the coefficients on NPI length in Columns (7)-(12) are very similar to the baseline estimates (Columns 1 and 2) and remain statistically significant

[17] This measure is only available for the 43 cities in Markel et al. (2007). Unfortunately, the weekly mortality data in Markel et al. (2007) does not extend past February 1919.

at the 5% level. Second, the coefficient estimates (not reported) on the trends in the three covariates are either marginally significant (with places with higher 1910 literacy and older 1910 population being on slightly upward patenting trends) or not statistically significant (for the share of population of German descent). This suggests that differential patenting trends in cities with different literacy rates, age composition, and share of German descent do not significantly confound the baseline results.

We also provide two tests that control for heterogeneous trends in patenting rates across cities. Specifically, in Columns (13) and (14) of Appendix Table A.4 we add a linear time-trend interacted with city-specific NPI length (Column 13) and city-specific linear time trends (Column 14). Both tests allow pre-trends to vary across cities, but absorb a substantial amount of variation in the data. This is evident when examining the estimated standard errors which are inflated by 35% and 55% when compared to those in Column (1). The doubling of the standard errors in Column (14) is due to the fact that allowing for city specific time trends adds 49 additional coefficients to be estimated, relative to the model in Column (1). Importantly, however, the magnitudes of the point estimates in Columns (13) and (14) are very similar to those in Column (1), mitigating concerns about differential trends across cities contaminating the baseline estimates.

Finally, we explored whether the presence of a local university could constitute a confounding factor in our estimates. While our specification includes city fixed effects, the presence of a local university may be correlated with NPI adoption and, at the same time, may have improved local invention capacity after the pandemic. However, we verified that there is no meaningful variation in our sample in whether cities hosted a university. In particular, using a variety of sources including the historical appendix of Andrews (2020), we confirmed that all but one of the cities in our sample were home to at least one university in 1916, the beginning of the sample period in our preferred specification.

### A2.3 Patent Assignment and Population Robustness

The baseline measure of patenting as used to estimate Equation (1) assigns multi-inventor patents to cities in a particular manner and imputes city-level population. Our results are robust to alternative choices of assignment and imputation.

Specifically, in our baseline specification, we assign $1/N_p$ of a patent to each inventor's city, where $N_p$ is the total number of inventors on patent $p$. Table A.6 repeats our baseline results in Column (1), and then reports the results from Equation (1) using two alternative assignment rules. In Column (2), only the first inventor's city is used, and it receives the full patent count. In Column (3), each inventor's city receive a full patent count (rather than a fraction). Results are similar to the baseline specification.

In Equation (1), we impute city-level population in a month-year cell linearly between census observations. High-frequency population measures by city are unavailable for our full sample, so instead we show that our results are robust over a range of population imputations, and—because the consequences of incorrect population imputation are most troubling if the drift in a confounding manner—over a range of sample windows. Table A.7 first repeats our baseline specification from Table 1, but now also shows intermediate sample windows. Panel A drops population from the specification. This increases standard errors, as expected, but results are consistent with the baseline specification, especially over shorter windows. Note that in Panel A, city fixed effects capture average population. Panel B includes imputed population as a control with a free coefficient (in the baseline specification, it is included with a coefficient fixed to 1, which is equivalent in a PPML specification to setting the dependent variable to the log of patenting rate). Results are again consistent, if noisier over longer windows. Panel C simply uses the 1910 census population to define the patenting rate, and as such is most likely to confound population change and patenting. However, because of the fixed effects present, this is identical to Panel A. Finally, Panel D uses 1910 census population to define the patenting rate and includes imputed population as a control. But, because of the fixed effects present, its results are also identical to Panel B. In sum, our results are robust to reasonable alternative manners of controlling for population change.

Figure A.1: Distribution of NPI Length Across 50 Cities

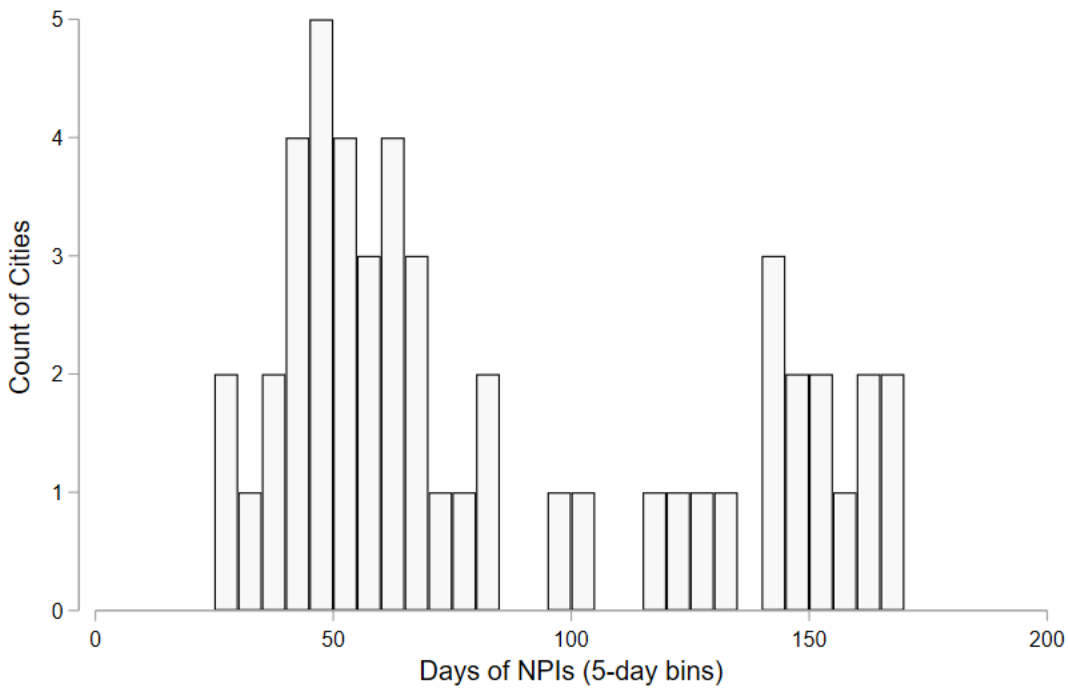

Figure A.2: Leave-One-Out Estimates of the Effect of NPI Length on Patenting Rates

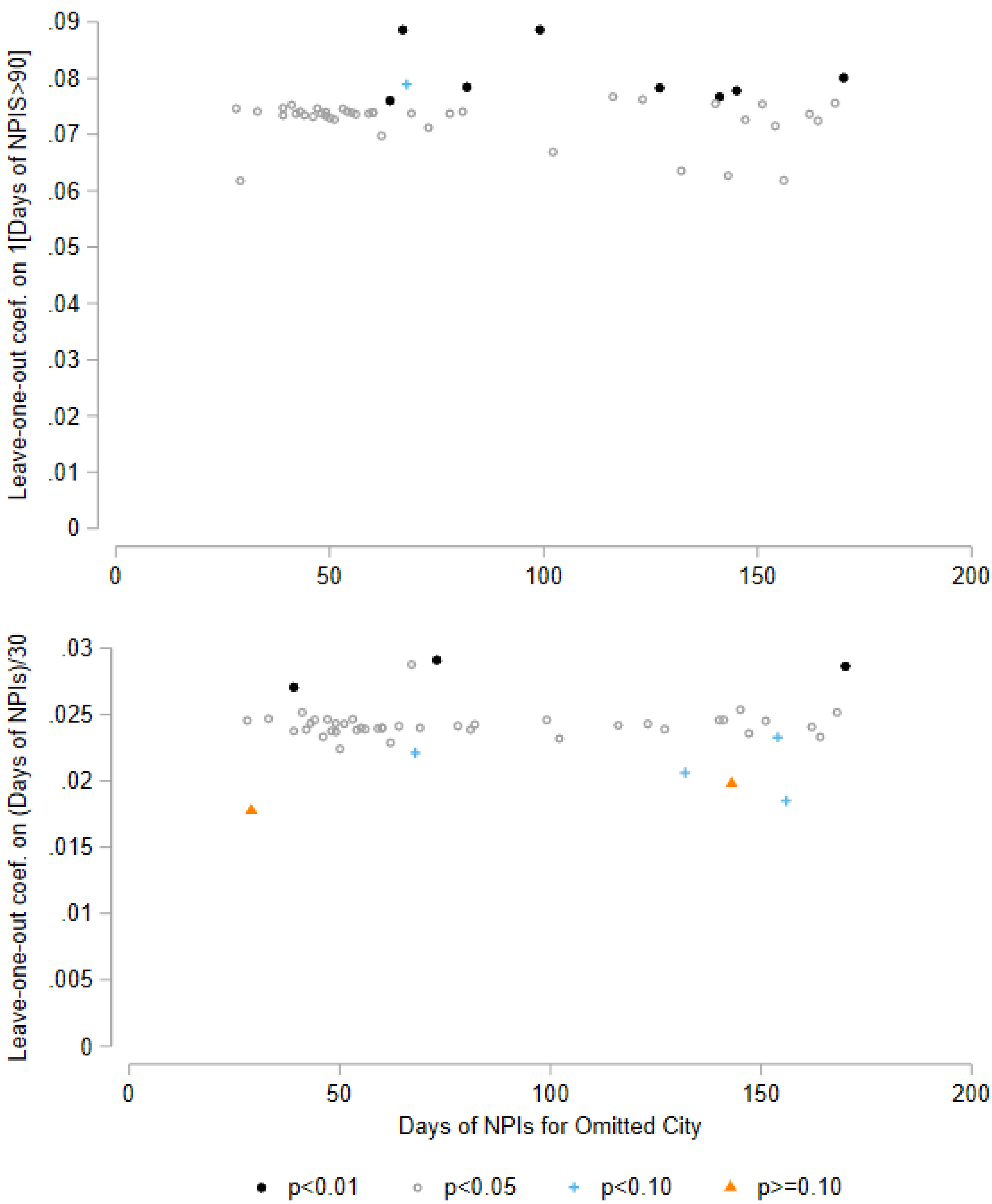


Figure A.2 shows 50 estimated coefficients obtained by alternately leaving one city out of the estimation sample and estimating the DD parameter $\beta$ as in Equation (1). The top panel is for the binary treatment specification and the bottom panel is for the continuous NPI model. In both cases, the specification includes city-by-month of year and Census-region–year fixed effects and uses the data from 1916–1920). The estimates are sorted by the duration of NPI length (in days) for the omitted city. Inference is based on city-level cluster-robust methods.

Figure A.3: Estimated Effect of NPIs on Patenting Rates Across Central 80% of Possible Cutoffs for Binary Treatment Definition

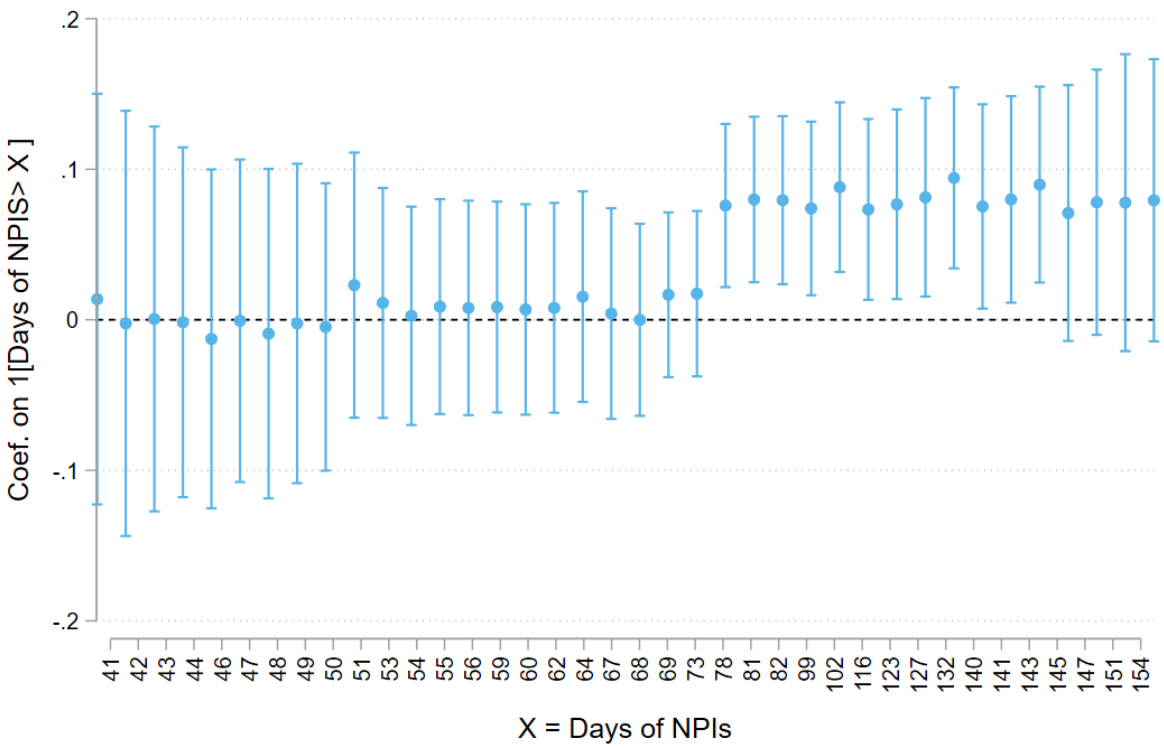


Figure A.3 reports estimates of $\beta$ from Equation (1) from a series of regressions where the threshold in days of NPIs to switch from "shorter" to "longer" varies from 41 days (the 10th percentile in the NPI duration distribution) to 154 days (the 90th percentile in the NPI duration distribution). The underlying regression models include city-by-month of year and Census-region–year fixed effects and are estimated on the 1916–1920 period sample. The vertical bars represent the 95 % confidence intervals from inference based on city-level cluster-robust methods.

Table A.1: Summary Statistics on Patents and NPIs

| | Mean | SD | Min | p25 | p50 | p75 | Max | $N$ |
|---|---|---|---|---|---|---|---|---|
| **Outcome Variables** | | | | | | | | |
| Patenting Rate (per 100,000 population) | 5.19 | 3.17 | 0 | 2.91 | 4.86 | 6.89 | 27.58 | 3000 |
| Patents | 26.95 | 50.58 | 0 | 5.5 | 13 | 25.5 | 392.67 | 3000 |
| Single-inventor patents | 24.8 | 46.89 | 0 | 5 | 12 | 23 | 366 | 3000 |
| Multi-inventor patents | 2.15 | 4.05 | 0 | 0 | 1 | 2.5 | 37 | 3000 |
| No-assignee patents | 15.84 | 29.81 | 0 | 3 | 8 | 15.5 | 258.5 | 3000 |
| Patents with assignees | 11.10 | 21.62 | 0 | 1 | 4.5 | 11 | 172 | 3000 |
| **Treatment Variables** | | | | | | | | |
| Days NPIs | 85.18 | 45.59 | 28 | 49 | 65.5 | 132 | 170 | 50 |
| 1[NPIs > 90] | 0.36 | 0.48 | 0 | 0 | 0 | 1 | 1 | 50 |
| Excess Death Rate (Markel et al. 2007) | 505.5 | 138.2 | 210.5 | 410 | 522.9 | 591.8 | 806.8 | 43 |

Notes: The sample period is 1916-1920. All variables are defined at the city-month level for the 50 cities in the main sample, except the city-level measure of pandemic severity which is only available for the 43 cities in Markel et al. (2007). The outcome variables are patenting rates (per 100,000 population), constructed from all patents filed and subsequently granted, taken from CUSP. Patents are assigned to cities based on the inventors' city of residence. Multi-authored patents are proportionally assigned in a way that reflects the share of inventors residing in each city. The treatment variables are the number of days of NPIs, a binary indicator for NPI periods longer than 90 days, and a city-level measure of the severity of the 1918 pandemic (excess death rate). The NPI variables are observed for the 50 cities of the main sample while the excess death rate is measured in only the 43 cities in Markel et al. (2007). See Appendix Table A.2 for the complete list of 50 cities.

Table A.2: NPI Duration and Population by City

| City | Days of NPIs | Pop. (1910) | Source |
|---|---|---|---|
| Albany, NY | 47 | 100,253 | Markel et al. (2007) |
| Atlanta, GA | 46 | 154,839 | Influenza Archive 2.0 |
| Baltimore, MD | 43 | 558,485 | Markel et al. (2007) |
| Birmingham, AL | 48 | 132,685 | Markel et al. (2007) |
| Boston, MA | 50 | 670,585 | Markel et al. (2007) |
| Buffalo, NY | 49 | 423,715 | Markel et al. (2007) |
| Cambridge, MA | 49 | 104,839 | Markel et al. (2007) |
| Charleston, SC | 69 | 58,833 | Influenza Archive 2.0 |
| Chicago, IL | 68 | 2,185,283 | Markel et al. (2007) |
| Cincinnati, OH | 123 | 363,591 | Markel et al. (2007) |
| Cleveland, OH | 99 | 560,663 | Markel et al. (2007) |
| Columbus, OH | 147 | 181,511 | Markel et al. (2007) |
| Dallas, TX | 41 | 92,104 | Influenza Archive 2.0 |
| Dayton, OH | 156 | 116,577 | Markel et al. (2007) |
| Denver, CO | 151 | 213,381 | Markel et al. (2007) |
| Des Moines, IA | 56 | 86,368 | Influenza Archive 2.0 |
| Detroit, MI | 29 | 465,766 | Influenza Archive 2.0 |
| Fall River, MA | 60 | 119,295 | Markel et al. (2007) |
| Grand Rapids, MI | 62 | 112,571 | Markel et al. (2007) |
| Indianapolis, IN | 82 | 233,650 | Markel et al. (2007) |
| Kansas City, MO | 170 | 248,381 | Markel et al. (2007) |
| Los Angeles, CA | 154 | 319,198 | Markel et al. (2007) |
| Louisville, KY | 145 | 223,928 | Markel et al. (2007) |
| Lowell, MA | 59 | 106,294 | Markel et al. (2007) |
| Milwaukee, WI | 132 | 373,857 | Markel et al. (2007) |
| Minneapolis, MN | 116 | 301,408 | Markel et al. (2007) |
| Nashville, TN | 55 | 110,364 | Markel et al. (2007) |
| New Haven, CT | 39 | 133,605 | Markel et al. (2007) |
| New Orleans, LA | 78 | 339,075 | Markel et al. (2007) |
| New York City, NY | 73 | 4,766,883 | Markel et al. (2007) |
| Newark, NJ | 33 | 347,469 | Markel et al. (2007) |
| Oakland, CA | 127 | 150,174 | Markel et al. (2007) |
| Omaha, NE | 140 | 124,096 | Markel et al. (2007) |
| Philadelphia, PA | 51 | 1,549,008 | Markel et al. (2007) |
| Pittsburgh, PA | 53 | 533,905 | Markel et al. (2007) |
| Portland, OR | 162 | 207,214 | Markel et al. (2007) |
| Providence, RI | 42 | 224,326 | Markel et al. (2007) |
| Richmond, VA | 60 | 127,628 | Markel et al. (2007) |
| Rochester, NY | 54 | 218,149 | Markel et al. (2007) |
| Salt Lake City, UT | 141 | 92,777 | Influenza Archive 2.0 |
| San Antonio, TX | 81 | 96,614 | Influenza Archive 2.0 |
| San Francisco, CA | 67 | 416,912 | Markel et al. (2007) |
| Seattle, WA | 168 | 237,194 | Markel et al. (2007) |
| Spokane, WA | 164 | 104,402 | Markel et al. (2007) |
| St Louis, MO | 143 | 687,029 | Markel et al. (2007) |
| St Paul, MN | 28 | 214,744 | Markel et al. (2007) |
| Syracuse, NY | 39 | 137,249 | Markel et al. (2007) |
| Toledo, OH | 102 | 168,497 | Markel et al. (2007) |
| Washington, DC | 64 | 331,069 | Markel et al. (2007) |
| Worcester, MA | 44 | 145,986 | Markel et al. (2007) |

Table A.3: Baseline Characteristics of Long and Short-NPI Cities

| | Mean if Long NPIs | Mean if Short NPIs | Difference without Region FEs | Difference with Region FEs | *N* |
|---|---|---|---|---|---|
| | (1) | (2) | (1) - (2) | | |
| Patenting rate (per 100,000 population), average monthly rate in 1910 | 7.13 | 5.35 | 1.78* | 0.78 | 50 |
| | [2.91] | [2.50] | (0.81) | (0.92) | |
| Population (April 1910) | 259660 | 478080 | -218420 | -192813 | 50 |
| | [158372] | [897326] | (163593) | (223114) | |
| Maximum excess death rate during 1918 pandemic (per 100,000, from Market et al., 2007) | 437.1 | 550.2 | -113.1** | -19.6 | 43 |
| | [106.8] | [139.8] | (37.7) | (57.9) | |
| Share of population in school (April 1910) | 0.182 | 0.194 | -0.012* | -0.004 | 50 |
| | [0.019] | [0.017] | (0.005) | (0.009) | |
| Share of population that is literate (April 1910) | 0.971 | 0.937 | 0.034*** | 0.007 | 50 |
| | [0.011] | [0.033] | (0.006) | (0.006) | |
| Share of population of German descent (April 1910) | 0.050 | 0.035 | 0.015 | 0.006 | 50 |
| | [0.037] | [0.029] | (0.010) | (0.014) | |
| Share of population with German as mother tongue (April 1910) | 0.009 | 0.004 | 0.005+ | 0.005 | 50 |
| | [0.011] | [0.004] | (0.003) | (0.003) | |
| Share of population aged 20–60 (April 1910) | 0.612 | 0.578 | 0.034** | 0.005 | 50 |
| | [0.039] | [0.025] | (0.010) | (0.010) | |
| Average age (April 1910) | 28.8 | 27.9 | 0.9* | 0.3 | 50 |
| | [1.3] | [1.3] | (0.4) | (0.5) | |

All variables are as of the 1910 Census except the patenting rate and the excess death rate during the 1918 pandemic. The patenting rate is calculated as the average per capita patenting rate across all months in 1910; excess death rate is as reported in Markel et al. (2007). The share of population of German descent is calculated as the share of people that reported Germany (or one of its regions) as birth place. German as mother tongue also includes its dialects (e.g., Austrian). Average age is across all city residents in 1910 census. Standard deviations in brackets. Robust standard errors of difference tests in parentheses. + $p<0.10$, * $p<0.05$, ** $p<0.01$, *** $p<0.001$.

# Table A.4: Robustness Analysis

| | Preferred Specification (Column 3 in Table 1) | | Only 43 Markel et al. (2007) cities | | With pandemic severity (43 cities) | | With literacy & schooling controls | | With age controls | | With share German controls | | With time trends | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| **NPI Length=1[NPIs>90 Days]** | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) | (13) | (14) |
| Post Pandemic × NPI Length | 0.074* | | 0.065* | | 0.066* | | 0.062* | | 0.060* | | 0.076* | | 0.074+ | 0.063 |
| | (0.029) | | (0.028) | | (0.029) | | (0.029) | | (0.029) | | (0.036) | | (0.039) | (0.045) |
| Before Pandemic × NPI Length | | 0.012 | | 0.011 | | 0.017 | | 0.018 | | 0.019 | | 0.010 | | |
| | | (0.033) | | (0.033) | | (0.031) | | (0.033) | | (0.033) | | (0.036) | | |
| During Pandemic × NPI Length | | 0.058 | | 0.064 | | 0.061 | | 0.054 | | 0.053 | | 0.059 | | |
| | | (0.040) | | (0.040) | | (0.038) | | (0.039) | | (0.039) | | (0.042) | | |
| After Pandemic × NPI Length | | 0.088* | | 0.074* | | 0.080* | | 0.079* | | 0.077* | | 0.090* | | |
| | | (0.035) | | (0.033) | | (0.033) | | (0.035) | | (0.035) | | (0.036) | | |
| Time Trend × (Days of NPIs)/30 | | | | | | | | | | | | | -0.000 | |
| | | | | | | | | | | | | | (0.005) | |
| City Time Trends | - | - | - | - | - | - | - | - | - | - | - | - | - | X |
| *N* | 3000 | 3000 | 2580 | 2580 | 2580 | 2580 | 3000 | 3000 | 3000 | 3000 | 3000 | 3000 | 3000 | 3000 |

The dependent variable is the patenting rate (patents filed/population) in a city-month. All specifications are estimated using PPML with the exposure variable set to a linear interpolation of city population; coefficients can be interpreted as representing a percentage change in the patenting rate. All models include month-by-year, city-by-month of year, and region-by-year fixed effects, and all samples begin January 1916 and end December 1920. Pandemic severity is the log maximum excess death rate as reported in Markel et al. (2007), interacted with the post period in Column (5) and with *Before*, *During*, and *After* periods in Column (6). Literacy and schooling controls are the share of the population that could both read and write and the share of the population that was enrolled in school, respectively, in the 1910 Census, interacted with a linear time trend. Age controls are share aged 20–60 and average age in the 1910 Census interacted with linear time trend. Share German controls are the share of the population of German descent and the share of the population speaking German as their mother tongue in the 1910 Census interacted with linear time trend. Standard errors clustered by city. + p<0.10, * p<0.05, ** p<0.01, *** p<0.001.

Table A.5: Effects of NPI Length Accounting for Prohibition

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| **NPI Length = 1[NPIs > 90 Days]** | | | | | | |
| *Panel A. Simple DD* | | | | | | |
| Post Pandemic × NPI Length | 0.058* | 0.062* | 0.067* | 0.071** | 0.076** | 0.076** |
| | (0.027) | (0.027) | (0.027) | (0.027) | (0.029) | (0.028) |
| Alcohol Prohibited (18th Amend.) | 0.022 | | 0.017 | | 0.030 | |
| | (0.023) | | (0.023) | | (0.027) | |
| Alcohol Prohibited (WPA) | | 0.041 | | 0.042 | | 0.046 |
| | | (0.033) | | (0.033) | | (0.037) |
| *Panel B. Extended DD* | | | | | | |
| Before Pandemic × NPI Length | 0.012 | 0.011 | 0.007 | 0.005 | 0.010 | 0.008 |
| | (0.028) | (0.028) | (0.027) | (0.028) | (0.033) | (0.032) |
| During Pandemic × NPI Length | 0.049 | 0.049 | 0.040 | 0.039 | 0.059 | 0.056 |
| | (0.041) | (0.041) | (0.041) | (0.041) | (0.040) | (0.040) |
| After Pandemic × NPI Length | 0.070* | 0.074* | 0.078* | 0.082* | 0.088** | 0.088** |
| | (0.033) | (0.034) | (0.033) | (0.033) | (0.034) | (0.032) |
| Alcohol Prohibited (18th Amend.) | 0.022 | | 0.018 | | 0.030 | |
| | (0.023) | | (0.023) | | (0.026) | |
| Alcohol Prohibited (WPA) | | 0.042 | | 0.044 | | 0.046 |
| | | (0.033) | | (0.033) | | (0.036) |
| Fixed Effects | | | | | | |
| City | X | X | - | - | - | - |
| Month-Year | X | X | X | X | X | X |
| City-Month | - | - | X | X | X | X |
| Region-Year | - | - | - | - | X | X |
| *N* | 3000 | 3000 | 3000 | 3000 | 3000 | 3000 |

Notes: Table A.5 reports DD estimates of the effect of NPI length on patenting rates with controls for prohibition. The sample includes the 50 cities for which we have information on NPI length (see Appendix Table A.2). The dependent variable is the patenting rate (patents filed/population) in a city-month. The treatment variable is a binary indicator for NPI period longer than 90 days. Alcohol Prohibited (18th Amend.) uses the date the 18th Amendment went into effect (Jan. 17, 1920) as the date that all not-yet-dry cities prohibited alcohol sales; Alcohol Prohibited (WPA) instead uses the date the Wartime Prohibition Act went into effect (July 1, 1919). All specifications are estimated using a Poisson Pseudo-Maximum Likelihood (PPML) with the exposure variable set to a linear interpolation of city population. The estimated coefficients can be interpreted as a percentage change in the patenting rate. Standard errors are clustered by city. + p<0.10, * p<0.05, ** p<0.01, *** p<0.001.

Table A.6: Robustness to Alternative Assignments of Multi-Inventor Patents

| Weight Assigned to Inventors on Multi-Inventor Patents: | $\frac{1}{\text{No. of Inventors.}}$ | First Inventor Only=1 | Each Inventor=1 |
|---|---|---|---|
| | (1) | (2) | (3) |
| **NPI Length = 1[NPIs > 90 Days]** | | | |
| *Panel A. Simple DD* | | | |
| Post Pandemic × NPI Length | 0.074* | 0.076* | 0.085** |
| | (0.029) | (0.030) | (0.027) |
| *Panel B. Extended DD* | | | |
| Before Pandemic × NPI Length | 0.012 | 0.016 | 0.014 |
| | (0.033) | (0.033) | (0.034) |
| During Pandemic × NPI Length | 0.058 | 0.061 | 0.075* |
| | (0.040) | (0.041) | (0.038) |
| After Pandemic × NPI Length | 0.088* | 0.093** | 0.099** |
| | (0.035) | (0.036) | (0.036) |
| Fixed Effects | | | |
| Month-Year | X | X | X |
| City-Month | X | X | X |
| Region-Year | X | X | X |
| *N* | 3000 | 3000 | 3000 |

Notes: Table A.6 reports DD estimates of the effect of NPI length using different assignment rules for multi-inventor patents. The sample includes the 50 cities for which we have information on NPI length (see Appendix Table A.2). The dependent variable is the patenting rate (patents filed/population), with multi-inventor patent inventors given a weight of one over the total number of inventors in Column (1) (our preferred measure), only the first inventor receiving a weight of one in Column (2), and each inventor receiving a weight of one in Column (3). The treatment variable is a binary indicator for NPI period longer than 90 days. All samples begin January 1916 and end December 1920. All specifications are estimated using a Poisson Pseudo-Maximum Likelihood (PPML) with the exposure variable set to a linear interpolation of city population. The estimated coefficients can be interpreted as a percentage change in the patenting rate. Standard errors are clustered by city. + p<0.10, * p<0.05, ** p<0.01, *** p<0.001.

Table A.7: Robustness to Population Measures

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| **NPI Length = 1[NPIs > 90 Days]** | | | | | | | |
| *Panel 0. Baseline (patenting rate with imputed intercensal population)* | | | | | | | |
| Post Pandemic × NPI Length | 0.056* | 0.065* | 0.074* | 0.067* | 0.093* | 0.088* | 0.086* |
| | (0.027) | (0.027) | (0.029) | (0.032) | (0.044) | (0.041) | (0.044) |
| *Panel A. Exclude population* | | | | | | | |
| Post Pandemic × NPI Length | 0.071* | 0.080** | 0.067** | 0.054 | 0.080* | 0.069 | 0.055 |
| | (0.029) | (0.029) | (0.025) | (0.040) | (0.037) | (0.044) | (0.073) |
| *Panel B. Include interpolated population as control* | | | | | | | |
| Post Pandemic × NPI Length | 0.062* | 0.068* | 0.062* | 0.077+ | 0.083* | 0.091* | 0.108 |
| | (0.031) | (0.030) | (0.027) | (0.041) | (0.035) | (0.045) | (0.072) |
| *Panel C. Use 1910 population as rate* | | | | | | | |
| Post Pandemic × NPI Length | 0.071* | 0.080** | 0.067** | 0.054 | 0.080* | 0.069 | 0.055 |
| | (0.029) | (0.029) | (0.025) | (0.040) | (0.037) | (0.044) | (0.073) |
| *Panel D. Use 1910 population as rate & interpolated population as control* | | | | | | | |
| Post Pandemic × NPI Length | 0.062* | 0.068* | 0.062* | 0.077+ | 0.083* | 0.091* | 0.108 |
| | (0.031) | (0.030) | (0.027) | (0.041) | (0.035) | (0.045) | (0.072) |
| Fixed Effects | | | | | | | |
| City | X | - | - | - | - | - | - |
| Month-Year | X | X | X | X | X | X | X |
| City-Month | - | X | X | X | X | X | X |
| Region-Year | - | - | X | X | X | X | X |
| Sample coverage | | | | | | | |
| begins January of | 1916 | 1916 | 1916 | 1913 | 1916 | 1913 | 1910 |
| ends December of | 1920 | 1920 | 1920 | 1920 | 1923 | 1923 | 1926 |
| $N$ | 3000 | 3000 | 3000 | 4800 | 4800 | 6600 | 10200 |

Notes: Table A.7 shows robustness of DD estimates of the effect of NPI length on patenting rates to alternative population measures. The sample includes the 50 cities for which we have information on NPI length (see Appendix Table A.2). The dependent variable is patents in a city-month. The treatment variable is a binary indicator for NPI period longer than 90 days. All specifications are estimated using a Poisson Pseudo-Maximum Likelihood (PPML). Panel 0 uses imputed intercensal population to define the dependent variable as patenting rate, as is used in all other specifications in the paper. Panel A excludes population. Panel B include interpolated population as a control variable (the default model uses interpolated population as the exposure variable, which is like fixing its coefficient to 1). Panel C sets the exposure variable to 1910 population. Panel D combines the specifications of Panels B and C. Standard errors are clustered by city. + p<0.10, * p<0.05, ** p<0.01, *** p<0.001.