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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 better preserved other inventive factors.

Keywords: Non-pharmaceutical interventions, public health, invention, patents, influenza, localized knowledge spillovers
JEL classifications: I80, 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 promote interactions that recombine 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, forthcoming).

Widespread adoption of non-pharmaceutical interventions (NPIs)[1] by local and national governments to slow the spread of COVID-19 has limited social interactions in cities, potentially reducing the benefits of density. While the extent and duration of NPIs during the COVID-19 pandemic are unprecedented, similar interventions were adopted historically to control the spread of diseases such as the 1918 influenza pandemic. Whether these temporary measures have long-lasting impacts on cities' fortunes, aggregate invention rates, and economic growth is important for evaluating possible future NPI policies and for understanding economic recovery following pandemics.

We shed light on these questions by estimating the effect of NPIs during the 1918 pandemic on local patenting rates in a sample of 50 large US cities. These 50 cities accounted for 21% of the population and 39% of all patent filings in the US in 1910. Surprisingly, we find that cities that adopted longer NPIs did *not* experience a larger decline in patenting rates relative to cities with shorter NPIs. Instead, cities that adopted longer NPIs saw an increase in their patenting rates after the pandemic ended.

Our analysis combines high-frequency, city-level data on patenting rates and NPI durations. We construct a city–month panel of issued patents from the Comprehensive Universe of US Patents (CUSP, Berkes, 2018), which describes the city of each inventor, filing and award dates, technology class, and ownership status for the near-universe of US patents since 1836. We combine these data with the types and lengths of NPIs adopted by 50 large US cities during the 1918 pandemic. We extend the database of city NPIs of Markel et al. (2007) by collecting data for seven additional cities from an updated version the same archival source, the *Influenza Archive 2.0* (2016). The resulting database is a balanced city–month panel, 1910–1926.

We estimate the effect of NPIs on patenting rates (the number of patents issued divided by the city's population) 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

[1]Examples of NPIs include mask mandates, social distancing, school and business closures, public gathering bans, and isolation and quarantine of infected people.

durations of less than 90 days.[2] We find that patenting rates increased in long-NPI cities by 7–12% after the pandemic ended (April 1919) relative to short-NPI cities. During the pandemic (September 1918–March 1919), long NPI cities had smaller and statistically insignificant increases in relative patenting rates.

One identification challenge is that longer NPIs may have been adopted by faster-growing cities that could have experienced larger increases in patenting in the absence of the pandemic. This concern is attenuated by normalizing our outcome variable in per capita terms. Additionally, our data allow tests for differential pre-trends between the two groups of cities. We find no evidence of differences in patenting rates between long- and short-NPI cities in the pre-pandemic years. Our preferred specification includes city–month, Census-region–year, and month–year fixed effects. These fixed effects control for time-invariant factors that may contribute to patenting activity (e.g., universities), national trends, local seasonal trends, and regional shocks. Finally, our results are robust to different specifications, including "leave-one-out" estimates that drop one city at a time. Taken together, the pre-trend analysis and alternative estimates suggest that our results are unlikely to be driven by omitted factors or unobserved trends.

Longer NPIs 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. Previous research suggests that NPIs had small to moderate effects in reducing city mortality rates (Markel et al., 2007; Correia et al., 2020; Chapelle, 2020; Clay et al., 2018; Barro et al., 2020). By saving lives, longer NPIs may have preserved labor inputs into invention. Further, by favoring a coordinated response to the pandemic, longer NPIs may have reduced uncertainty, anchored expectations, and preserved intangible or organizational capital, increasing post-pandemic invention.

We provide additional evidence on potential mechanisms. First, we find that longer NPIs had larger positive effects on patenting rates for grants with multiple inventors and grants owned by external assignees.[3] These types of patents may reflect more complex invention processes and may also rely more on social interactions. Second, we find that patenting rates in emerging fields such as electricity and mechanical engineering benefited most from longer NPIs. Those emerging fields may reflect greater technological and market risk. Our interpretation is that longer NPIs did not reduce capacity for riskier and more complex invention. Instead, by reducing mortality and overall uncertainty, NPIs may have facilitated access to financial resources and preserved the intangible or organizational capital required for invention.

[2] We also estimate directly the impact of NPI duration in days on patenting rates.

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

2 Historical Background and Conceptual Framework

The 1918 influenza pandemic was brief and severe. In the US, the first sporadic 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 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.[4]

In response to the pandemic, US cities adopted a variety of NPIs directed at restricting social interactions to limit the spread of the disease. Markel et al. (2007) classify those measures into three categories: public gathering bans, school closures, and isolation and quarantine of confirmed and suspected cases. The earliest NPIs 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 50 cities sample, the total number of days of NPIs of all types ranged from 28 to 170 days (see Appendix Table A.2).

What was the likely effect of NPIs on invention? NPIs may shift both invention supply and demand, so the sign of their equilibrium effect is theoretically ambiguous and likely depends on the time horizon. In the short run, NPIs might depress patenting rates by reducing labor inputs (inventors work less), capital inputs (businesses invest less), and idea inputs (inventors are deprived of social interactions). However, if NPIs save lives, they might increase labor inputs and preserve intangible or organizational capital in the medium or long run. The effect of NPIs on idea inputs is also unclear since inventors in short-NPI cities might respond by voluntarily limiting social interactions to decrease their risk of infection.

On the supply side, NPIs might reduce short-run labor inputs to inventive activities through quarantine measures but increase them in the medium run by increasing health and reducing mortality. NPIs adopted during the flu pandemic appear to have reduced mortality. Markel et al. (2007) and Correia et al. (2020) find that NPIs reduced peak mortality and cumulative excess mortality. 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. Because death rates for adults between the ages of 18 and 44 during the 1918 influenza were unusually high and the average patentee in this period was 41 years old (Sarada et al., 2019), illness and mortality among likely inventors might have directly

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

incapacitated labor devoted to inventive activities, resulting in fewer patents.[5] By saving lives, NPIs may have increased labor inputs to inventive activities in the medium run.[6]

There is evidence that the 1918 pandemic disrupted the production of intermediate inputs and capital flows. Industrial production and coal production dropped (Velde, 2020). The textile and lumber sectors also declined (Bodenhorn, 2020). Correia et al. (2020) document widespread business disruption and uncertainty during and in the aftermath of the pandemic. Disruption in economic activity may induce a loss of accumulated intangible or organizational capital (Rubenstein, 1962), and overall uncertainty may reduce incentives for business investment. By favoring a coordinated response to the pandemic, NPIs could mitigate this effect by better preserving organizational or intangible capital and stabilizing business expectations.

Financial markets were also stressed by the 1918 pandemic. For example, heavy claims against life insurance companies led to their exit from the market for high-grade railroad bonds.[7] Long-NPI cities may have better stabilized access to inputs and capital markets.[8] If they did, then inventors in long-NPI cities, particularly in risky technologies, may have had better access to critical factors in the medium run.

The 1918 pandemic likely reduced social interactions. Limits on social interactions may hinder 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. NPIs implemented during the 1918 flu pandemic may have had similar effects by shutting down workplaces, schools, bars, and other social gathering places. However, the types and lengths of NPIs adopted during the 1918 pandemic suggest limited long-run effects on patenting through a social interaction channel. First, 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. Second, the most common types of NPIs—school closures—seem less likely to affect the kind of informal knowledge flows inventors rely on. Third, differences between cities with short and long NPIs might be muted if inventors in cities with short NPIs voluntarily avoided social interactions to reduce their infection risk.

On the demand side, pandemics may reduce demand for invention by reducing income. Barro et al. (2020) find that real incomes declined by 6-8% in the average country during the

[5] Labor scarcity was likely a major contributor to business disruption in 1918 (Correia et al., 2020). Garrett (2009) finds increases in wages in areas with higher mortality, consistent with labor shortages.

[6] Inventors might also have been motivated by non-market factors, such as priority credit or prestige among peers (Merton, 1957; Stephan, 1996). NPIs may have also helped preserve the value of these factors.

[7] Cortes and Verdickt (2020) document that financial difficulties for life insurance companies around the 1918 pandemic were attenuated by increasing demand for life insurance products, larger issuance of equity, and increased prudential regulation.

[8] Correia et al. (2020) find evidence of a positive relationship between the adoption of NPIs and local assets of national banks in the aftermath of the pandemic.

1918 flu pandemic. However, the market for inventions is likely regional or even national in scope. For this reason, it seems unlikely that there were large effects of NPIs on invention through local demand channels.

In sum, NPIs during the 1918 pandemic might have reduced patenting in the short run by reducing labor inputs, capital inputs, and idea inputs. But these negative short-run effects may have been limited by the type and short duration of most NPIs. In the medium run, by increasing health and reducing mortality, NPIs may have preserved inventor labor supply and organizational capital, increasing patenting rates.[9]

3 Data

We construct a new city–month panel on NPIs and patenting rates. We start with data on NPI length for 43 cities from Markel et al. (2007). We extend this database by seven additional cities using the *Influenza Archive 2.0* (2016), an update of the archive that Markel et al. (2007) used to construct their original sample. One virtue of expanding the Markel et al. (2007) sample is that our new panel includes fast-growing cities in the western and southern US (see Appendix Table A.1). Systematic information on the duration of NPIs is limited to the 50 cities in our expanded sample.

We construct a city–month panel of *ever-granted* patents from CUSP (Berkes, 2018). CUSP includes the near-universe of patents issued by the US Patent and Trademark Office. 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–month observation. Importantly, CUSP includes the application filing date for each patent that was subsequently granted, allowing us to assign patents to the month of application versus the month of issue. Thus, we are able to more closely measure the date of invention.

Across our 50-city sample, the average 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.2). This amounts to an 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 270. The share of cities classified as longer NPI cities (treatment group) is 0.36.[10]

Figure 1 shows the mean log monthly patenting rates for long- and short-NPI cities between January 1916 and December 1920.[11] The dashed lines show residualized log monthly patenting

[9] There is no evidence of pandemic-related effects in the number of filed applications or in lengthy discussions of staffing and turnover (see the 1918 or 1919 *Annual Reports of the Commissioner of Patents*).

[10] We also collect and use city total populations, compositions by race and sex, and literacy rates from historical US Censuses 1900–1930. Intercensal values are linearly interpolated between the Aprils of each Census year.

[11] As explained in the introduction, long-NPI cities have cumulative NPI durations of more than 90 days.

after removing city-specific seasonality (with month-of-year 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-NPI cities had similar trends in monthly patenting rates. Monthly patenting rates peaked in early 1917 and declined thereafter until late 1918. These declines may reflect US entry into World War I and the mobilization of resources and labor for the war effort.[12] Both long- and short-NPI cities show sharp rebounds in patenting rates starting around October or November 1918. Short-NPI cities briefly exhibit higher patenting rates in mid-1919. However, through the end of 1920, long-NPI cities show persistent increases in patenting rates compared with short-NPI cities. We explore these patterns in depth next.

4 Empirical Framework

We use difference-in-differences (DD) to estimate the causal effect of NPIs on city patenting rates. Figure 1 shows the timeline of the pandemic and defines the time periods in our analysis. We define before September 1918 as the *Pre-treatment* period. In some specifications, we partition this *Pre-treatment* period 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 divide the *Post-treatment* period into a *During* period, covering the seven months during the pandemic peak (September 1918 through the end of March 1919), and an *After* period, from April 1919 to the end of the sample.

There are two important features of the patenting data that the empirical model needs to account for. First, there are a substantial number of zeros in city–month patenting. Second, because patent filing is relatively rare, there is greater heteroskedasticity in smaller cities with less patenting. To address these issues, we use a Poisson Pseudo-Maximum Likelihood (PPML) estimator. PPML accommodates zero-valued outcomes, efficiently handles heteroskedasticity, and is robust to misspecification (the true density does not need to be Poisson), and inference can easily account for overdispersion and clustering.

The baseline DD specification that compares outcomes pre- and post-pandemic makes the following conditional mean assumption for patenting rates:

Short-NPI cities have cumulative NPI durations of less than 90 days.

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

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

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 $\mathbb{1}[\text{Post } 9/18]$ equals one in the *Post-treatment* period. Since the 1910s and 1920s saw rapid social and economic changes, we test the robustness of our results to alternative choices for the starting and ending years.

In our preferred specification, we include city–month fixed effects to control for time-invariant drivers of inventive activities specific to a city, such as proximity to transportation networks (Perlman, 2016; Agrawal et al., 2017) or the presence of local universities (Kantor and Whalley, 2014; Andrews, 2020), and for possible differences in seasonality of patenting across cities. We also include month–calendar-year fixed effects to control for national trends in patenting, and Census-region–year fixed effects to control for the differential evolution of patenting trends in the North, South, Midwest, and West[13] 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).[14]

The primary treatment is a function $g()$ of the number of days of NPIs imposed by each city. We focus on a binary treatment indicator that compares across cities with shorter and longer NPIs (NPI duration of 90 days or more), but we also report estimates when the treatment is defined as the number of days of NPI 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 β.

Identification of β requires that the error term in Equation (1) is uncorrelated with $g(NPI_c) \times \mathbb{1}[\text{Post } 9/18]$, conditional on the controls and fixed effects included in the regression. This can be interpreted as the standard 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. Therefore, as is common with DD designs, we test the identifying assumption by probing the robustness of the estimates to different sets of fixed effects and city-level time-varying controls that may predict invention rates (e.g., educational attainment trends). We also investigate differences in pre-trends.

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

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

[14] Knowledge flows declined between US and German scientists during World War I (Iaria et al., 2018). One concern is that the resumption of these knowledge flows after the war might be spuriously correlated with the adoption of NPIs, perhaps driven by the local share of residents of German descent. Since immigrants by country of origin are strongly clustered within the US (Abramitzky and Boustan, 2017), our inclusion of Census-region–year fixed effects is likely to absorb this correlation.

presence of treatment effect heterogeneity (Card and Krueger, 1994; Abadie, 2006), unlike DD estimands in more complicated settings (e.g., de Chaisemartin and D'Haultfœuille, 2020; Goodman-Bacon, 2018).

We also report estimates from an extended DD specification that mimics Equation (1) but divides the *Pre-treatment* period into *Before* and *Baseline* periods, as well as the *Post-treatment* period into *During* and *After* periods:

$$\begin{aligned} E\left[\frac{Y_{cmt}}{P_{cmt}}\right] \quad &= \quad exp\,(\delta_{cm} + \delta_{my} + \delta_{ry} + g(NPI_c)\times \\ &\{\beta_P \cdot \mathbb{1}[\text{Pre } 9/17] + \beta_D \cdot \mathbb{1}[9/18 - 3/19] + \beta_A \cdot \mathbb{1}[\text{Post } 4/19]\}) \end{aligned} \tag{2}$$

This specification allows us to directly test for differences in pre-trends and to highlight dynamic effects of longer NPIs on innovation *During* versus *After* the pandemic. While we remain agnostic about the duration window for dynamic effects after the pandemic, we focus on the sample period from 1/1916 to 12/1920 in our preferred specification to avoid possible confounding factors long before or after NPIs were implemented. Nevertheless, we also present estimates varying time windows around the pandemic below.

5 Results

Table 1 reports the estimates of β from Equations (1) and (2). These 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. Columns (1) to (3) introduce different sets of fixed effects in the regression models while columns (4) to (7) consider alternative time windows for the sample. Standard errors clustered by city are reported in parentheses (as in the rest of the empirical analysis).

The results in Panel A show that long-NPI cities had higher patenting rates than short-NPI cities. The simple DD estimates range from 0.056 to 0.093 log points, which we interpret as increases in the monthly patenting rate of 5.6% to 9.3%.[15] All these estimates are statistically significant at the 5% level; the choice of fixed effects and starting and ending years is mostly inconsequential. In our preferred specification in column (3) that includes city–month, Census-region–year, and month–calendar-year fixed effects, the effect of long NPIs on patenting rate is 7.4%, with a 95% confidence interval ranging between 1.7% and 13.1%.[16]

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

[16] Since cities in our sample adopted NPIs of varying duration, the comparison between long-NPI (treatment) and short-NPI cities (control) follows a fuzzy difference-in-differences design (de Chaisemartin and D'Haultfœuille, 2018). In the Appendix, we show that simple fuzzy-DD corrections yield estimates of NPI effects that are larger than those reported in Table 1.

The extended DD estimates in Panel B confirm this result. An added result is that the positive effect on NPI length on invention is largely driven by the period *After* the pandemic ended, versus *During* the pandemic (although this difference is 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 underlying Equations (1) and (2).

Panel C presents the same analysis as in Panel A, but for a specification where NPI length enters linearly in number of days (divided by 30 for ease of interpretation). The results in Panel C confirm the findings in Panel A. 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 cities with differing NPI length and that the NPI effect on patenting rate primarily operates through an impact on patenting rates in the *After*-pandemic period.

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) and (2). Figure 2 reports these results. To proceed, we aggregate the data to an annual frequency and shift 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 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 and positive, and significantly different from zero in about half the years. (Pooling their effects results in estimates similar to those in Table 1).

5.1 Robustness analysis

We briefly discuss the results of robustness analyses reported in Appendix Table A.3. Odd-numbered columns report simple DD model estimates, and even-numbered columns report extended DD model estimates. All models are based on the specification of column (3) in Table 1. Taken together, the evidence in Appendix Table A.3 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 Market et al. (2007) (columns 3 and 4), to the inclusion of linear time trends interacted with city-specific indicators of human capital from the 1910 Census (columns 5 and 6), and to controlling for city-levels indicators of pandemic severity (columns 7 and 8). Adding 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 9 and 10).

We also perform a jackknife-like exercise and report the estimated coefficients for 50 specifications based on Equation (1) where each specification drops one city from the estimation sample. Appendix Figure A.2 shows that the specifications with the binary treatment are stable across jackknife samples and robust to dropping any city, while the specifications with the number of days in NPIs as the treatment variable are also mostly stable and retain statistical significance in all but two cases.

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, respectively). 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.

5.2 Interpretation and Further Results

Table 2 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). This analysis helps characterize possibly heterogeneous effects of NPIs on different types of inventions. Throughout Table 2, we focus only on our preferred specification (city–month, month–calendar-year, and Census-region–year fixed effects, sample period of 1916–1920, and binary long-NPI treatment).

The simple DD estimates indicate that long NPIs increased invention rates across most categories of inventor status and patent ownership. However, this pattern is substantially stronger for teams of multiple inventors. Column (2) reports a 20% higher patenting rate for multiple inventor patents overall (irrespective of patent ownership) in long-NPI cities, compared with 6.2% for single inventor patents. The estimates in Columns (7) and (8) further show a larger effect for multiple-inventor patents with an assignee.

Though the extended DD results are not as precisely estimated, a few observations bear noting. First, we once again fail to reject the null hypothesis of no pre-trend differences between long- and short-NPI cities. Second, invention dynamics vary between single- and multiple-inventor patents. For single-inventor patents, long NPIs increase patenting rates primarily

After the pandemic. For multiple-inventor patents, long NPIs tend to increase patenting rates both *During* and *After* the pandemic, with larger effects *During* the pandemic (although the differences between *During* and *After* are not statistically significant).

These results provide preliminary evidence on the channels that link NPI length with increased patenting during and after the flu pandemic. Because multiple-inventor patents were not negatively affected in long-NPI cities *During* the pandemic, reductions in social-interaction inputs to patenting do not seem to have been so large as to produce, on net, a decline in invention. Instead, our results suggest that NPIs may have better preserved inventive factors important for coordinating inventing teams or organizations. Collaborative invention remained heightened *After* the pandemic in long-NPI cities.

The results by assignee status further support the interpretation that coordination or organizational factors may have been better preserved in long-NPI cities. Long NPIs led to nearly twice the increase in patents with an assignee compared with patents without an assignee. This suggests that NPIs may have reduced business uncertainty or preserved organizational capital. In fact, the increasing effect sizes moving to the right in Columns (5)–(8) of Table 2 Panel A 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.

Table 2 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 effect of NPI length on invention varies across technology classes. We find statistically significant positive impacts of longer NPIs in the baseline DD models for Class D (Textiles and paper), Class F (which includes Mechanical engineering, Lighting, and Heating), and Class H (Electricity). For the other classes, the simple DD estimates are not statistically significant. The extended DD results broadly reflect similar patterns, although with reduced precision (in particular for classes with lower average monthly patenting rates).

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) (Berkes et al., 2020). Although there are several possible interpretations of this finding, a plausible hypothesis is that longer NPIs may have reduced business uncertainty and stabilized 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.[17] Correia et al. (2020) survey contemporaneous news, documenting that the pandemic generated considerable stress on the financial markets. However, they 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.

6 Discussion and conclusions

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

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., 2020, Lilley et al., 2020). Analysis of longer-run impacts of the pandemic is confounded by a deflationary recession in 1920–1921. In contrast, we study patenting rates, which link current activity to future productivity growth (Kelly et al., forthcoming). In this sense, our findings provide the first evidence of the effects of NPIs on factors affecting long-run economic growth.

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 of patenting rates (Andrews, 2020). 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

[17] 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.

of exposure.[18] 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 reducing mortality, decreasing uncertainty, and preserving intangible and organizational capital, without disproportionately hindering knowledge flows in cities.

Although both the 1918 flu pandemic and the 2020 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. The sign of the combined effect is ultimately determined by the behavioral, economic, and public policy forces that shape the relative strength of those channels.

[18] 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 seem to have played a more important role in reducing mobility.

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

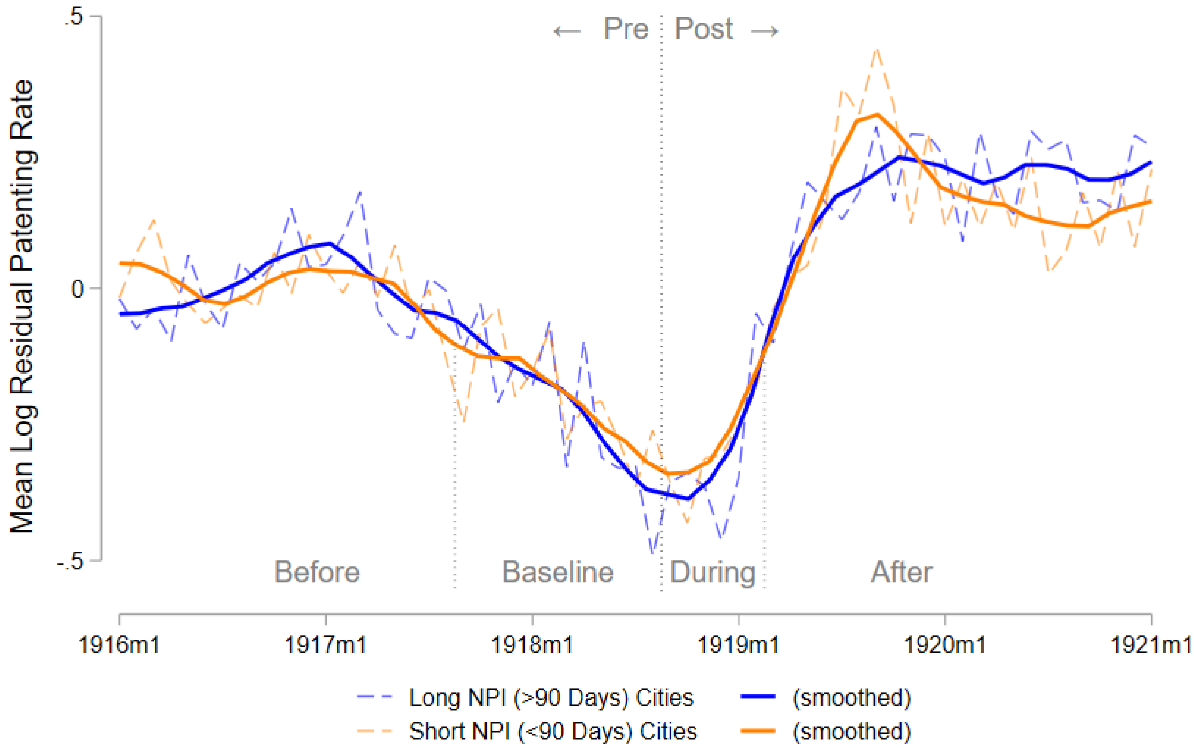

Figure 1 reports residualized log monthly patenting rates after removing city-specific seasonality with month-of-year fixed effects (dashed lines). The average residualized monthly patenting rate for long- (short-)NPIs cities is shown in blue (orange). The solid lines are smoothed values of the residualized 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)

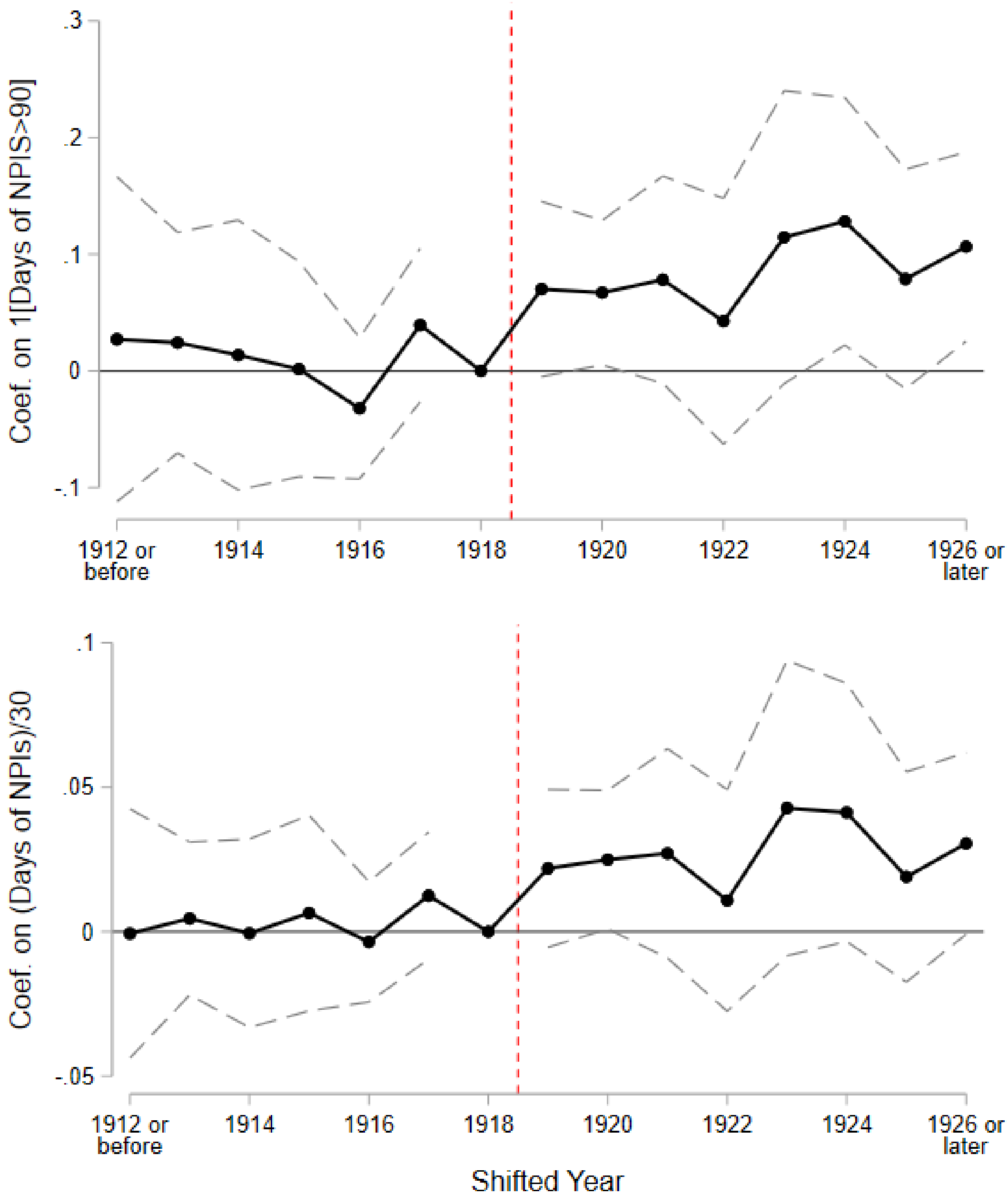

Figure 2 reports estimates from event-study variants of Equation (1) and (2). The city-month data on NPIs and patenting rates is aggregated to an annual frequency represented by shifted years (where the start of each year is shifted by four months to the left (e.g., so that 1918 includes September of 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 based on city-level clustering.

Table 1: Effect of NPI Length on Patenting Rate

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
NPI Length = 1[NPIs > 90 Days]							
Panel A. Simple DD							
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 B. Extended DD							
Before Pandemic × NPI Length	0.013	0.007	0.012	0.017	0.014	0.018	0.032
	(0.027)	(0.027)	(0.033)	(0.036)	(0.032)	(0.036)	(0.038)
During Pandemic × NPI Length	0.049	0.040	0.058	0.063	0.068+	0.070+	0.069+
	(0.041)	(0.041)	(0.040)	(0.039)	(0.040)	(0.039)	(0.039)
After Pandemic × NPI Length	0.068*	0.076*	0.088*	0.085*	0.106*	0.106*	0.117**
	(0.033)	(0.032)	(0.035)	(0.035)	(0.050)	(0.049)	(0.045)
NPI Length = Days of NPIs ÷ 30							
Panel C. Simple DD							
Post Pandemic × NPI Length	0.017+	0.020*	0.024*	0.022*	0.032+	0.031+	0.030+
	(0.010)	(0.010)	(0.011)	(0.011)	(0.018)	(0.017)	(0.017)
Panel D. Extended DD							
Before Pandemic × NPI Length	0.008	0.006	0.008	0.009	0.010	0.009	0.009
	(0.010)	(0.010)	(0.011)	(0.013)	(0.011)	(0.014)	(0.015)
During Pandemic × NPI Length	0.016	0.011	0.018	0.020	0.023+	0.023+	0.023+
	(0.014)	(0.014)	(0.013)	(0.013)	(0.014)	(0.013)	(0.013)
After Pandemic × NPI Length	0.023+	0.026*	0.032*	0.032*	0.040+	0.040+	0.040*
	(0.013)	(0.013)	(0.016)	(0.016)	(0.022)	(0.022)	(0.020)
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 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.1). The dependent variable is the patenting rate (patents filed/population) in a city-month. 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 clustered by city. + 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	(1) Single Inventor	(2) Multiple Inventors	(3) No Assignee	(4) With Assignee	(5) Single Inventor, No Asgn.	(6) Single Inventor, W/ Asgn.	(7) Multiple Inventors, No Asgn.	(8) Multiple Inventors, W/ Asgn.
Simple DD								
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)
Extended DD								
Before Pandemic × NPI Length	0.012	0.011	0.046	-0.037	0.048	-0.040	0.033	-0.022
	(0.034)	(0.125)	(0.031)	(0.056)	(0.034)	(0.061)	(0.116)	(0.214)
During Pandemic × NPI Length	0.039	0.271**	0.053	0.069	0.040	0.040	0.235	0.337
	(0.044)	(0.102)	(0.059)	(0.067)	(0.059)	(0.066)	(0.148)	(0.227)
After Pandemic × NPI Length	0.078*	0.188+	0.092+	0.081+	0.085	0.067	0.169+	0.218
	(0.035)	(0.109)	(0.051)	(0.045)	(0.055)	(0.041)	(0.102)	(0.216)
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	(1) Class A Human Necessit.	(2) Class B Operat.; Transport.	(3) Class C Chemist.; Metal.	(4) Class D Textiles; Paper	(5) Class E Fixed Construct.	(6) Class F Mech. Engr.	(7) Class G Physics	(8) Class H Electric.
Simple DD								
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)
Extended DD								
Before Pandemic × NPI Length	0.069	-0.023	0.032	0.099	0.239*	-0.080	-0.150+	0.264
	(0.058)	(0.060)	(0.136)	(0.213)	(0.112)	(0.076)	(0.085)	(0.179)
During Pandemic × NPI Length	0.153+	-0.001	-0.010	0.098	0.207	0.201+	-0.247*	-0.080
	(0.081)	(0.057)	(0.166)	(0.266)	(0.157)	(0.103)	(0.114)	(0.179)
After Pandemic × NPI Length	0.023	0.055	-0.079	0.344+	0.046	0.069	0.170	0.494***
	(0.079)	(0.067)	(0.146)	(0.192)	(0.125)	(0.082)	(0.145)	(0.122)
Mean of Dep. Variable	4.76	9.43	1.57	1.11	1.86	5.00	2.52	2.56

Notes: Table 2 reports DD estimates of the effect of NPI length on patenting rates by co-inventors and by assignee status (Panel A) and by patenting class (Panel B). 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. 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 clustered by city. + 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.2.

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.3 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.

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.1). 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).[19] The top panel shows jackknife replicates for binary treatment sorted by the duration of NPI length (in days) 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

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

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 β 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 β 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

Columns (5) and (6) in Appendix Table A.3 estimate the same set of models as Table 1 using the binary specification of treatment, 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 results in Appendix Table A.3 indicate that adding these measures of education does not meaningfully alter our baseline results. There are two main findings: First, the coefficients on NPI length in columns (5) and (6) are very similar to the baseline estimates (columns (1) and (2)) and remain statistically significant at the 5% level. Second, the coefficient estimates on the trends in education are themselves not statistically significant (estimates not reported). This suggests that cities with relatively higher levels of education before the pandemic were not already on a path of increased invention.

Columns (7) and (8) in Appendix Table A.3 are based on the preferred specification in Table 1, but include an additional interaction between an indicator pandemic severity 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). To measure pandemic severity at the city level, we use the log of 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). Therefore the estimation sample is based on the 43 cities in Markel et al. (2007), not the 50 cities sample that underlies Table 1.[20] The results in Appendix Table A.3 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).

Finally, we provide two tests that control for heterogeneous trends in patenting rates across cities. Specifically, in columns (9) and (10) of Appendix Table A.3 we add a linear time-trend interacted with city-specific NPI length (column (9)) and city-specific linear time trends (column (10)). 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 (10) is due to the fact that allowing for city specific time trends adds 49 additional coefficients to be estimates, relative to the model in column (1). Importantly, however, the magnitudes of the point estimates in columns (9) and (10) are very similar to those in column (1), mitigating concerns about differential trends across cities contaminating the baseline estimates.

A2.3 A Fuzzy Difference-in-Differences Interpretation

All cities included in our sample implemented NPIs which varied in duration. Because the binary measure of treatment splits cities with strictly positive NPI durations into two groups (short-

[20] Unfortunately, the weekly mortality data in Markel et al. (2007) does not extend past February 1919.

and long-NPIs), our approach is similar to a fuzzy difference-in-differences design (de Chaisemartin and D'Haultfœuille, 2018).[21] Put differently, both the treatment group (long-NPI cities) and the control group (short-NPI cities) are exposed to NPIs, but exposure is longer in the treatment group. A simple approach to tackle fuzzy DD designs is to use a Wald-like estimator of treatment effects where the DD estimate for the outcome is divided by the DD estimate for the treatment.[22]

In our setting, this Wald-like DD estimator would estimate the effect of maximum observed NPI length relative to no NPIs as:

$$\frac{\hat{\beta}}{\frac{1}{170}\left(\mathbb{E}[NPI|NPI \geq 90] - \mathbb{E}[NPI|NPI < 90]\right)},$$

where $\hat{\beta}$ is one of the standard DD estimates based on the binary measure of treatment (e.g., as reported in Table 1) and 170 is the maximum observed NPI length in our sample. In the sample of 50 cities we obtain the following quantities: $\mathbb{E}[NPI|NPI \geq 90] = 141.1$ and $\mathbb{E}[NPI|NPI < 90] = 53.7$, so the denominator of the Wald-like difference-in-differences estimator is 0.514. Applying this scaling factor to the unadjusted DD estimates in Table 1, would essentially double our estimates of the impact of long NPIs on patenting rates.

However, more structure is required for this simple approach to be valid in our context. We highlight two key considerations. First, the local average treatment effect must be the same in the treatment and control groups. In our setting this means that the average effect of an extra day of NPI in the long-NPI cities (treatment group) is the same the average effect of an extra day of NPI in the short-NPI cities (control group). While this is untestable, we can provide suggestive evidence for the assumption by documenting the stability of the Wald-like DD estimator on subsets of our sample. To proceed, we compute the same Wald-like DD estimator as above, but exclude cities with NPI length between 50 and 90 days.[23] We expect that this estimate—which is based on a comparison between cities in the upper and lower terciles of the NPI length distribution—should be larger than our baseline estimate. Indeed, this alternative estimate of $\hat{\beta}^*$ based on the sample that omits the middle tercile of the NPI length distribution is 0.117 (with a standard error of 0.039). This provides evidence for the monotonicity of the estimated treatment effect, which implies that the local average treatment effect is roughly equivalent between treatment and control groups.

Second, one must assume that there is no discrete change in expected outcomes based on

[21] A fuzzy difference-in-differences model is akin to a standard difference-in-differences model except that the treatment increases in both the treatment and control groups in the post period, but it increases more in the treatment group.

[22] The fuzzy DD estimator proposed by (de Chaisemartin and D'Haultfœuille, 2018) cannot be applied in our setting due to the nonlinear nature of our DD estimator (Poisson Pseudo-Maximum Likelihood).

[23] Note that $\mathbb{E}[NPI|NPI < 50] = 41.2$.

a small "ϵ" increase above the minimum value of the support of the treatment variables (here, zero days of NPIs). This is unlikely in our setting: NPIs involve substantial civic effort and probably effect the salience of the disease environment. A shutdown of even one day is likely much more similar to a shutdown of two days than of zero days.

We conclude from this exercise that some extrapolation of our baseline results outside the range of observable NPI lengths is not unreasonable. Applying a standard Wald-like DD estimator to account for the fuzzy nature of our treatment and control group definitions produces a corrected DD estimate that is slightly larger, but qualitatively similar to preferred estimates in column (3) of Table 1.

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

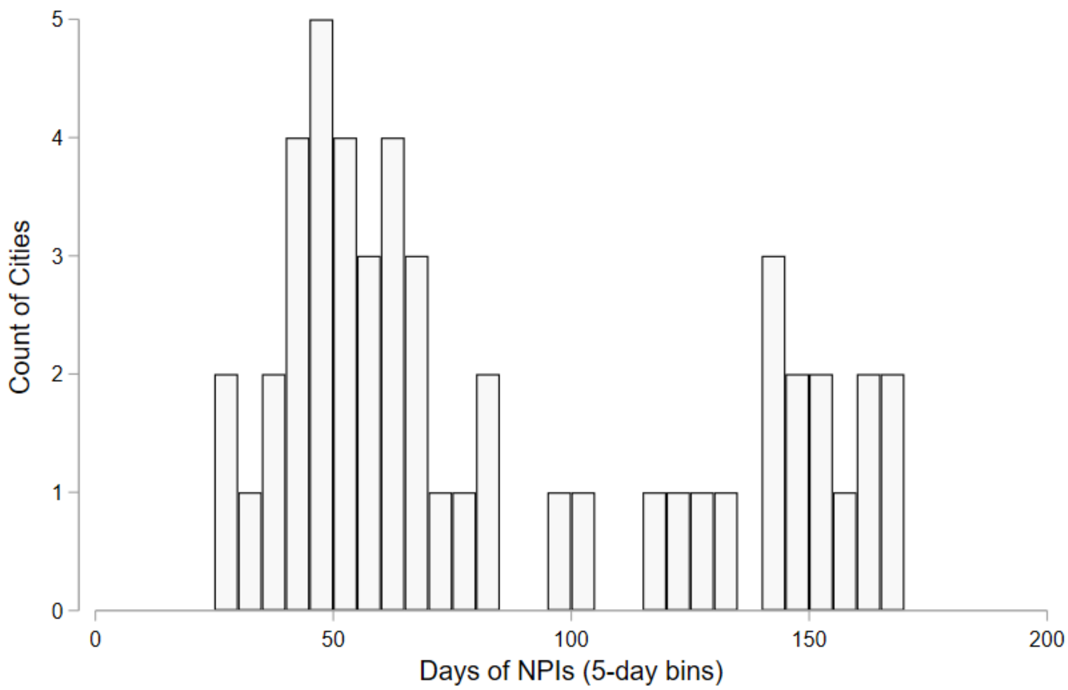

Figure A.2: Leave-One-Out Estimates of the Effect of NPI Length on Patenting Rates

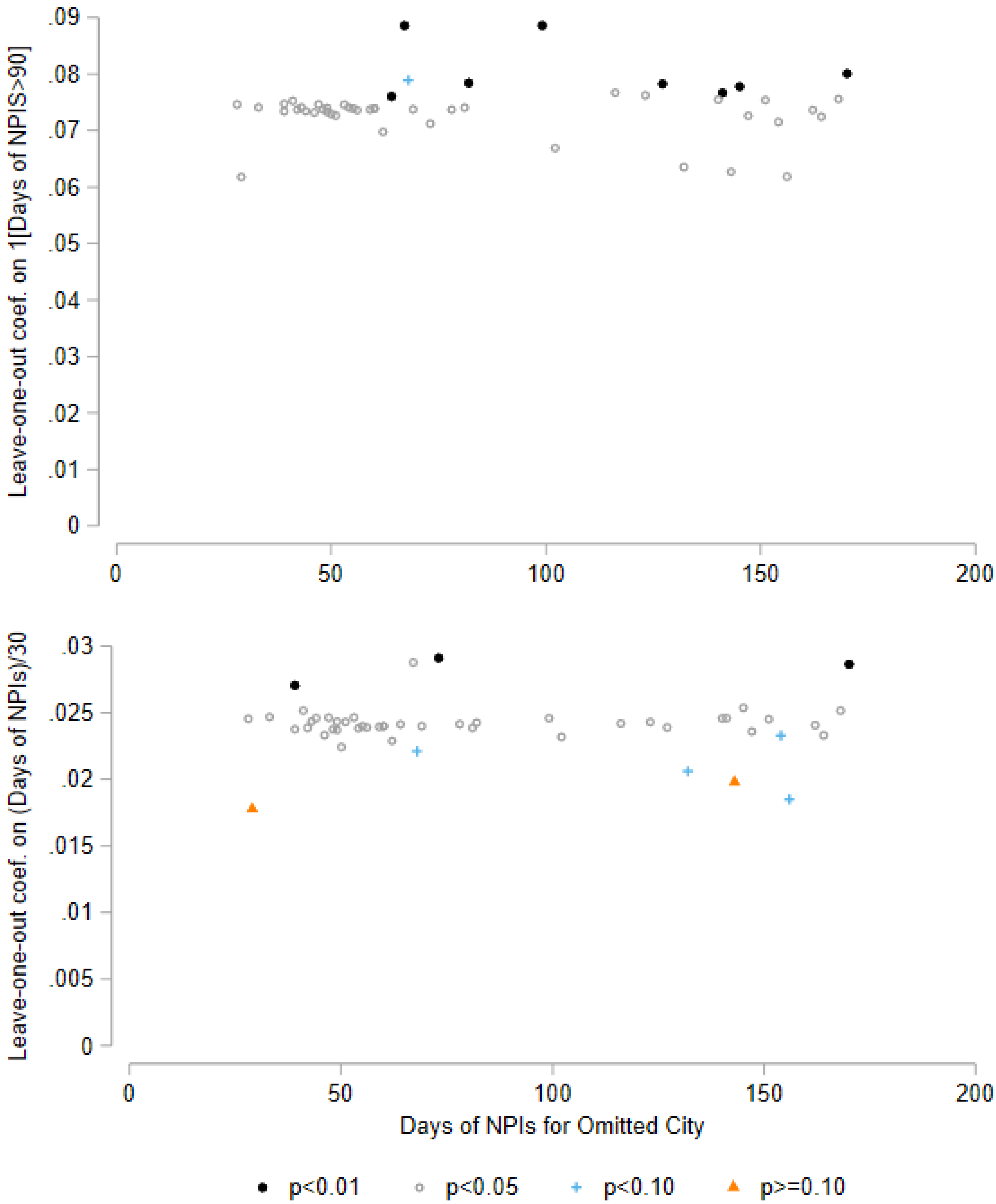

Figure A.2 shows 50 estimated coefficients obtained by alternately leaving one city out of the estimation sample and estimating the DD parameter β as in Equation (1). The top panel is for the binary treatment specification and the bottom panel is for the continuous NPI model. In both cases, the specification 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

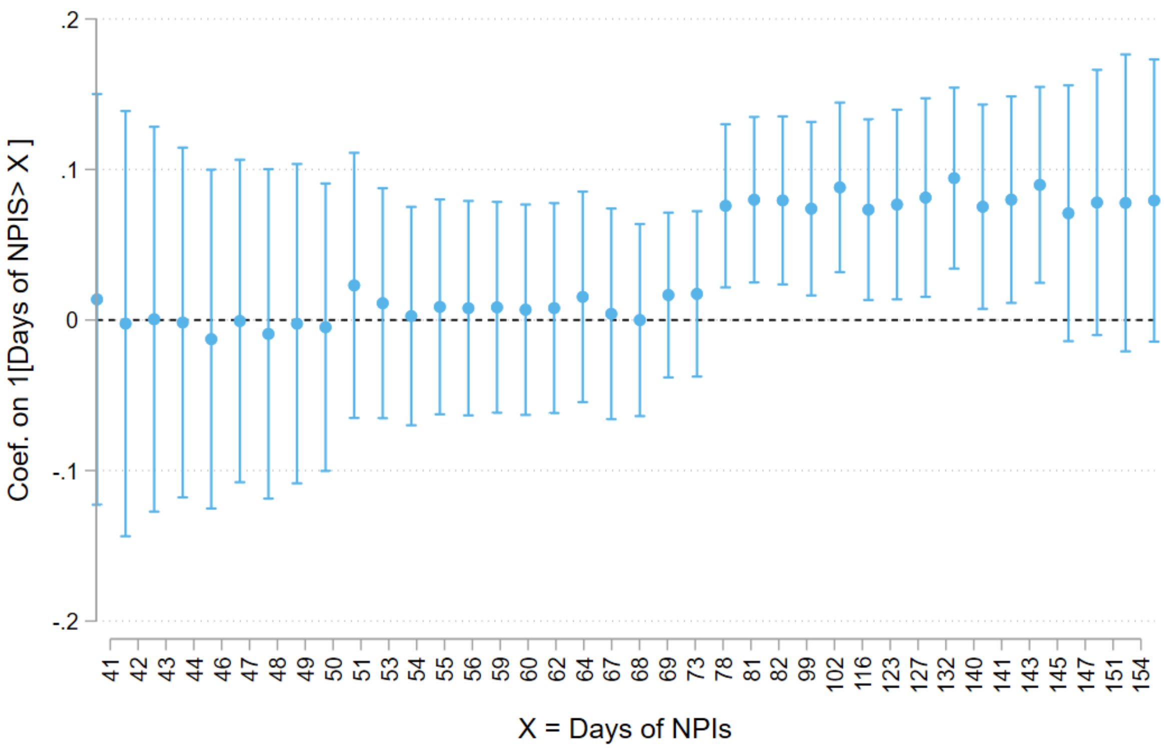

Figure A.3 reports estimates of β from Equation (1) from a series of regressions where the threshold in days of NPIs to switch from "shorter" to "longer" varies from 41 days (the 10th percentile in the NPI duration distribution) to 154 days (the 90th percentile in the NPI duration distribution). The 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: 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.2: 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	1232	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.1 for the complete list of 50 cities.

Table A.3: Robustness Analysis

	Preferred Specification (Column 3 in Table 1)		Only 43 Markel et al. (2007) cities		Add literacy & schooling controls		Add pandemic severity (43 cities)		Add time trends	
NPI Length = 1[NPIs > 90 Days]	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Post Pandemic × NPI Length	0.074*		0.058*		0.063*		0.066*		0.074+	0.063
	(0.029)		(0.028)		(0.029)		(0.029)		(0.039)	(0.045)
Before Pandemic × NPI Length		0.012		0.008		0.017		0.017		
		(0.033)		(0.027)		(0.033)		(0.031)		
During Pandemic × NPI Length		0.058		0.051		0.054		0.061		
		(0.040)		(0.042)		(0.039)		(0.038)		
After Pandemic × NPI Length		0.088*		0.076*		0.080*		0.080*		
		(0.035)		(0.032)		(0.035)		(0.033)		
Time Trend × (Days of NPIs)/30									-0.000	
									(0.005)	
City Time Trends	-	-	-	-	-	-	-	-	-	X
N	3000	3000	2580	2580	3000	3000	2580	2580	3000	3000

Outcome variable is a weighted measure of patents filed in a city in a month with the characteristics given for each column. 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. 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. 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. Standard errors clusterd by city. + p<0.10, * p<0.05, ** p<0.01, *** p<0.001.