How Pandemics Impact Tax Revenues and Inequality: Evidence from the 1918 Spanish Flu*

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Abstract

This paper investigates how state-level characteristics in demographics, economic structure, and non-pharmaceutical interventions (NPIs, the equivalent to today's shelter-in-place, quarantine, and social distancing measures) impacted tax revenues during the 1918 Influenza in the U.S.

Following Correia et al. (2020), Lilley et al. (2020), Goolsbee and Syverson (2020), and Serrato and Zidar (2018), I estimate two difference in differences models with panel data using various ways of constructing treatment and control groups. I also run a cross sectional analysis incorporating more time-invariant variables.

The main preliminary results from the diff-in-diff models are that the intensity of NPI policies seemed to have more dramatic impacts on tax revenue growth than the adoption speed of NPI. As a treatment factor, higher NPI intensity tended to depress tax revenues, especially corporate tax growth rate. Faster speed NPI implementation led to higher corporate tax growth rate than otherwise. Personal tax and total tax did not seem to have been impacted as dramatically as corporate tax.

By creating treatment and control groups based on whether a state had neighbors enacting more intense NPI policies, I observe that treatment states were more likely to also have more intense NPI measures "under peer pressure." However, there were no significant differences in tax revenue growth between the treatment and control group states.

When examining cumulative growth rates from 1918 to 1925, I find that tax revenue growth was mostly driven by the expansion of tax base, both in terms of number of tax returns and amount of net income available for revenue collection, rather than by geographical or demographic factors. The intensity or speed of NPI measures and the mortality rate during the pandemic likely did not have a persistent impact on tax revenue growth when considered over a medium-term (seven-year) window.

Keywords: 1918 Spanish Flu, Public Finance, Tax Policy, Economic Inequality

Honor Pledge: This paper represents my own work in accordance with University regulations.

Disclaimer: This write-up shows some very preliminary results that I have produced over this summer, so both the econometric methodology and theoretical framework still need significant improvement. I would truly appreciate any feedback and criticism, and I would be happy to share my data and code for replication if anyone would like to dive deeper into some of those questions.

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1 Introduction

1.1 Research questions and motivation

Since the outbreak of the Covid-19 crisis, scholars in economics have worked diligently to investigate its effects on various aspects of today's society: asset markets, macroeconomy, household finance, consumer behaviors... We also see a renewed interest in the 1918 Spanish Flu, which is often regarded as the deadliest pandemic in U.S. history.

Much inspired by some of the economic history working papers that were published in the past few months, this research project seeks to examine whether the 1918 flu could inform us anything about fiscal policies or inequality during pandemics. More specifically, how did characteristics in geography, demographics, and economic structure influence public finance during the 1918 pandemic? What was the impact of varying degrees of non-pharmaceutical interventions (NPIs) like various shelter-in-place, quarantine, and social distancing measures on state-level income for households and corporations, size of tax base, and tax revenues during the 1918 flu? By extension, because taxation is closely related to other fiscal variables such personal wealth, number of wage earners, corporate profits, public spending, etc., could we gain any additional insights on inequality and the distributive dynamics of income and wealth during a pandemic based on the study of public finance?

The motivation for examining the above questions is two-fold. First, most papers on the 1918 flu have focused on the flu's impact on economic growth or or asset returns, and there is not much written on the impact on taxation or fiscal policies. For instance, amongst the 48 countries examined by Barro et al. (2020), 1918 flu caused economic declines for GDP and consumption of 6 and 8 percent, respectively, in a typical country. Using municipality-level data from Denmark, Dahl et al. (2020) found that severely affected municipalities experienced short-run declines in income, suggesting that the epidemic led to a V-shaped recession. Using regional data from Sweden, Karlsson et al. (2014) found significant increases in poorhouse rates and negative effects on capital returns. As someone interested in public finance, I thought it'd be worthwhile to examine whether the 1918 flu had any dramatic effect on fiscal outcomes.

The second part of my motivation comes from my interest in the role of inequality throughout economic history, though unfortunately I have not devoted enough time to explore this aspect of the issue. Mostly based on the Black Death, it seems that many people have developed the conception that epidemics are generally inequality-reducing, while the Covid-19 crisis has somewhat surprised us with the opposite effect as billionaires became richer and working-class people struggled more. Since the 1918 flu is often talked about as the most deadly pandemic in American history, it might be worth looking into any misunderstandings we have had about the 1918 flu in terms of distributive dynamics and impacts of fiscal policies on people's income and wealth. These broader questions about inequality and economic history were initially sparked by my podcast interviews and email discussions with Guido Alfani, Leah Boustan, Merle Eisenberg, Branko Milanovic, and Emil Verner, whom I thank deeply for encouraging me to take on this topic.

1.2 Methodology and literature review

The methodology of this paper draws inspiration from a range of recent studies on both the 1918 pandemic and the Covid-19 crisis. First, I follow Correia, Luck, and Verner (2020) (CLV [2020]), who studied the impacts of NPI policies on employment and output in the manufacturing sector in selective cities during the 1918 pandemic. They used a difference-in-differences model controlling for city-level fixed effects. My research will build upon their framework, and instead of examining NPIs' impacts on city-level employment and economic growth, I will focus on tax revenues and other fiscal variables on the state level. Following their framework, NPIs are the treatment factor in my analysis, and I investigate whether states/cities that enacted NPIs more quickly/stringently had their tax revenues impacted more/less.

Then, I follow Goolsbee and Syverson (2020) to estimate a diff-in-diff model for neighboring states that may or may not have implemented different levels of NPI policies and thus arrived at different public finance outcomes. Goolsbee and Syverson (2020) investigated how the shelter-in-place policy during the Covid-19 crisis impacted consumer spending behaviors using much more granular and comprehensive data than what I am able to obtain for 1918. So, I would not say that my analysis is a direct replication of their methodology, but is merely a byproduct inspired by their paper.

To improve the accuracy of my diff-in-diff models, I follow Serrato and Zidar (2018) and constructed indicators for neighboring and similar states based on various state-level characteristics. Because there is no readily available data on state-level NPI policies, I also constructed state-level NPI scores based on the city-level indices provided by CLV (2020). I explain my methods for these data preparation in the data section.

Lastly, I also condensed the full-range panel data set into a cross-sectional data set with cumulative growth rates of most of my time-variant variables. Cross-sectional regressions allow for a wider range of variables to be used than the panel data regressions, so they are in fact quite important to this analysis as they finally allow us to directly observe how time-invariant characteristics in geography, economic structure, and NPI measures influenced the tax outcomes.

In the following sections of my paper, I will present the three frameworks explained above. I also write quite extensively about some of the detailed discussions on econometric frameworks between me and other scholars in case they are helpful.

2 Data

2.1 Data sets and sources

I have constructed a fairly comprehensive data set on state-level public finance, economic characteristics, and public health from around 1917 to 1925 relying on the following sources:

- U.S. statistical census abstracts, from where most of the public finance data both for households and for corporations are directly transcribed by hand. I thank my friend Wenxuan Hao for spending dozens of hours on this boring task with me.

- World Inequality Data Base, from where I downloaded data on the distribution of fiscal and pre-tax income by income class (bottom 90%, top 10%, top 1%, etc.). So far, these data mainly served as controls for examining tax variables, though more analysis should be conducted on them as dependent variables.

- Center for Disease Control (CDC) Mortality Statistics, from where I obtained state-level mortality data specifically due to influenza. Those data have been crossed checked with CLV (2020)'s data set.

- Lilley, Lilley, and Rinaldi (2020) (LLR [2020]), who responded to CLV (2020) in detail and published a replication package on their site. Their data package contained many helpful data points on state-level time-invariant controls such as population count.

- Swanson and Curran (1976), which contains city-level data for business tax, total revenue, and a range of government expenditure items from 1905 to 1930. This data set is kindly provided by Emil Verner. However, I have noticed multiple coding errors in this data set, most fatally that there are abrupt jumps in values across all variables in 1919. Without accurate estimates of the 1919 tax revenues, it has become much harder to investigate how city-level public finance responded to NPI measures. Had the data set been error-free, I could have directly replicated CLV (2020)'s city-level analysis in the context of public finance.

- Lindert (1978), where I obtained data on personal income per capita in 1900, 1910, 1919-21, and 1930 (in 1960 consumer dollars). They serve as useful controls for both the panel and cross-sectional analyses.

2.2 Construction of state-level NPI measures

CLV (2020) constructed a set of city-level Non-Pharmaceutical Interventions (NPIs) speed & intensity measures based on the NPI data from Markel et al. (2007). The methodology is described as such:

> The intensity depends on the "cumulative sum of the number of days where three types of NPIs were activated (school closure, public gathering bans, and quarantine/isolation of suspected cases) in fall 1918."
>
> The speed depends on "the number of days elapsed between when the city death rate exceeded twice its baseline death rate and the first day city officials enforced a local NPI." The researchers multiply the day count by minus one so that higher values indicate a faster response.

In order to replicate CLV (2020)'s diff-in-diff study on a state-level, I would first need to construct a similar set of state-level NPI scores. However, there is currently NO data set for state-level NPI policies implemented in 1918. Therefore, decided to extrapolate CLV (2020)'s data and scores and construct a state-level measure.

The simple outline of my formula is: for each city that has enacted NPI policy, I first calculate the ratio of its 1917 population in relation to the sum of city populations *for cities that I have data for*. I then multiply this ratio (or "weighting") with the city-level NPI intensity & speed indices to arrive at this city's "contribution" to the state-level NPI measure. Lastly, I sum up all such cities in a given state to arrive at the final state-level NPI intensity & speed scores. So, the NPI score for a given state s based on all its cities c can be represented as:

$$NPIScore_s = \sum_{c \in s} \left(\frac{Population_c}{\sum_{c \in s} Population_c} \times NPIScore_c \right) \tag{1}$$

For example, say NYC has NPI intensity of 100 and NPI speed of -50. If NYC is the only city in New York state to enact NPI policies, it would get 100% of the weighting, and the NY state NPI intensity and speed scores would be 100 and -50. In contrast, if Massachusetts have two cities that I have NPI data for - Boston with an intensity score of 50 and population of 70,000; and Cambridge with an intensity score of 80 and population of 30,000 - then I will weight their measures by 70% and 30% respectively, and the Massachusetts intensity score would be:

$$NPIIntensity_{MA} = \left(\frac{70,000}{70,000 + 30,000} \times 50 \right) + \left(\frac{30,000}{70,000 + 30,000} \times 80 \right) = 59.$$

This measure was improved based on Andrew Lilley's advice. It does not directly assign a score to areas that we do not have NPI data for because I do not want to assume that such areas had zero NPI measures. It's not the case that a city or rural area that doesn't appear in the data set of Markel et al. (2007) simply didn't do anything, but rather it's just that NPI data for only a few cities were collected. Under my system, I effectively assume that all other cities in a state adopted the average known measure of that state, but not zero measures. It is not a perfect measure, but should serve as a decent proxy.

2.3 Construction of neighbor states

Here, I follow Serrato and Zidar (2018) and create a "**neighbor state**" indicator for NPI measures. As Serrato and Zidar (2018) noted in their paper, there exist 109 unique state border pairs, including pairs sharing single-point border. My data set contains much fewer state border pairs, since I only have NPI data on 26 states.

I refer the reciprocal of "neighbor" as the "**anchor state**," which is the central state from whose point of view we count its neighbors. For each of the growth rate variables of each those 26 "anchor states," I create a variable *neighbormax_**, which shows the highest value amongst the neighboring states. For example, Rhode Island has a NPI days score of 42, and the score for its neighbor states are 39 (Connecticut) and 51 (Massachusetts). So, $neighbormax\_DaysofNPI = 51$ for Rhode Island.

I then create a dummy indicating whether one of the numerous neighbors of a given state has a higher score than the state itself. In the case of Rhode Island and NPI days, this dummy will equal to 1 since it indeed has a neighbor with a higher value. Amongst the 26 states we have NPI data on, 18 states have neighbors that implemented longer days of NPI, and 8 states did not have neighbors with higher NPI days score. This creates a clear contrast that I could use for treatment and control groups.

2.4 Construction of similar states

I then follow Serrato and Zidar (2018) and use state-level demographic and economic characteristics to construct a measure of state similarity. For each state i in the sample, I fit a linear probability regression to estimate the probability that state $j \neq i$ is state i based on the given characteristics listed. The estimates ($\hat{Y}$ value) from this regression provide a measure of how similar states are to a given state.

Specifically, for each state-year, I estimate the specification:

$$\begin{aligned} \mathbb{1}i = \beta_0 + \beta_1 pretgr1825_i + \beta_2 cnumgr1825_i + \beta_3 ptaxgr1825_i + \\ \beta_4 ctaxgr1825_i + \beta_5 UrbanPopShare_i + \beta_6 percapgr2030_i \end{aligned} \tag{2}$$

where $\mathbb{1}_i$ is a dummy indicator for each state i; and for each state i: *pretgr1825* is the cumulative growth rate for the number of personal tax returns from 1918 to 1925; *cnumgr1825* is the cumulative growth rate for the number of corporations from 1918 to 1925; *ptaxgr1825* is the cumulative growth rate for the amount of personal tax from 1918 to 1925; *ctaxgr1825* is the cumulative growth rate for the amount of corporate tax from 1918 to 1925; *UrbanPopShare* it the urban population share of a state in 1900; and *percapgr2030* is the cumulative growth rate for per capita income from 1920 to 1930.

This is a much simpler proxy for state similarity than what Serrato and Zidar (2018) constructed, and because of the cross-sectional nature of my data set, my measure will not incorporate the time-variant nature of state similarities, which is a big shortcoming of this method.

The list of anchor, neighbor, and similar states is shown in table A1 in the appendix.

3 Panel Data Analysis - The CLV and LLR Approach

3.1 Dynamic difference in differences analysis with fixed effects

As briefly mentioned above, CLV (2020) estimated the impacts of NPIs on employment and output in the manufacturing sector in selective cities. Shortly after the release of CLV's paper, LLR (2020) was published to question CLV's results, essentially claiming that CLV failed to account for population and economic growth trends before the 1918 flu. In LLR's model, after including those pre-1918 trends, CLV's results are no longer significant. Both CLV and LLR use difference-in-differences analysis while controlling for city-level fixed effects.

My analysis below follows CLV and LLR's econometric setup:

$$Y_{s,t} = \alpha_s + \tau_t + \sum_{j \neq 1914} \beta_j NPI_{s,1918} \mathbb{1}_{j=t} + \sum_{j \neq 1914} \gamma X_s \mathbb{1}_{j=t} + \epsilon_{s,t} \tag{3}$$

The LHS dependent variables are the fiscal variables of my interest (e.g. **growth rates of personal, corporate, and total tax revenues**) in a state s in year t. The RHS includes state and year fixed effects α_s and τ_t; $NPI_{s,1918}$ is the NPI treatment variable of interest; X_s are the state-level time-invariant controls (i.e. agricultural employment share in 1910, manufacturing employment share in 1914, urban population share in 1910, income per capita in 1910, and mortality rate in 1918).

One main difference between the CLV and LLR specification is that CLV's time range limits to 1909-1923, whereas LLR includes 1899-1927. Pre-trends could confound the measurement of the effect of NPIs on economic growth, so LLR first run the analysis over CLV's time range, then extend the results to the longer 1899-1927 sample to analyze potential trends that could confound the analysis.

3.2 Discussion on CLV and LLR's econometric setup

I built my analysis for this section based on LLR's Stata do file published on their Github site, and I used their state-level time-invariant controls data. However, there are a few parts of their analysis that I have failed to understand the rationale for and disagree with at this current setup. (Admittedly, much of my confusion here is likely due to my insufficient knowledge in econometrics, and I look forward to better understanding their approaches soon.)

I have been in communications with both Andrew Lilley and Emil Verner throughout my research process, and I sincerely thank them for clarifying up some of my very basic questions about their research. I think it'd be important to lay out my previous confusions and reflection process to justify the changes I made in my setup based on CLV and LLR's model.

After closely examining LLR's do file, the first question that came to my mind is why their diff-in-diff analysis didn't include the two main effects "post" and "treatment" but only had the "post × treatment" interaction term. In Stata, one would use only one "#" instead of two "#'s" if one merely wishes to include the interaction term and not the main effects, which is what LLR's do file shows. We know that if one only includes the interaction term, Stata is essentially using a linear combination of the two main effects as the main independent variable, rather than actually taking both main effects "post" and "treatment" into consideration. I wonder if this omission would defeat the purpose of running a "diff-in-diff" analysis because there's no differencing taking place between the treatment and control groups.

In response to my first concern above, Andrew graciously provided the following explanation:

> "You have the right intuition for the general case. You should never include an interaction term $X_1 \times X_2$ in a regression without including both the levels of X_1 and X_2 as it means that if there is an average effect of X_1 or an average effect of X_2 you will attribute the average of those effects to the interaction, which changes the interpretation.
>
> "Sometimes, however, you have sufficient controls that you're already accounting for this. A dynamic diff in diff setup is one such example. Notice that we have year fixed effects, denoted τ_t. The issue with including "post" is that it is "spanned by" or "perfectly multicollinear" to "treatment".
>
> ...
>
> "... it is not possible to include "post' once you've included year fixed effects. In effect, "post" is really just collapsing the year fixed effects into fewer varaibles of $t = 0, 1$ vs. $t = 2, 3$."

I have taken Andrew's explanation to heart, and only the main interaction term will be included in my dynamic diff-in-diff regressions shown below.

A second confusion I had was why time-invariant controls are included in a fixed effects regression that will omit these time-invariant variables by construction. As mentioned above, these controls include agricultural employment share in 1910, manufacturing employment share in 1914, urban population share in 1910, income per capita in 1910, and mortality rate in 1918.

Andrew subsequently explained to me that an important component of the econometric setup is $\sum_{j \neq 1914} \gamma X_c \mathbb{1}_{j=t}$, which makes the time-invariant controls vary across the years after they're made to interact with the different years. Therefore, time-invariant controls are used to control for the extent to which the differential growth rates (indexed vs. 1914) every year can be captured by pre-existing differences across cities, rather than the treatment variable.

Sure, the fixed effects regression results would be exactly the same if one simply includes these time-invariant controls as additional independent variables, but in CLV and LLR's context, they're in fact used in a time-variant fashion. I include those controls in the 2nd column of my regressions, in which I use these controls but do not display their coefficients. As one may soon see, the interaction term often no longer remains statistically significant after these time-invariant controls are included.

My third point of confusion about LLR's do file is that in addition to the interaction term before "post × treatment" and the yearly categorical variables "i.Year," LLR also included interactions between city and year "i.city × c.Year," as well as yearly interactions with controls "i.Year × controls" though only with specific years selected out.

Such complex measures of interacting between time-invariant controls and yearly/city categorical variables are mainly used in the their later regressions to control for pre-trends, but I worry they run the danger of over-complicating the regression. Meanwhile, the rationale behind letting only certain years and not the others interact with the controls is not entirely clear, and such arbitrary interaction terms might lead to spurious regression results. At one point, three groups of interaction terms are running: "post × treatment," "year × controls," and "city × year."

Andrew's explanation for their approach is that

> "the point is not that such a regression leads to clear inference when it works, it is more so that it *checks the extent to which a regression result without these controls may be spurious.* Essentially, it is asking 'if I had drawn a straight line through the growth rates for each city, *before the treatment took place*, and then controlled that prior rate of growth for the treatment that occurs afterwards, would the difference in earlier growth rates be as large as this difference I'm measuring after the treatment?'"

My interpretation to Andrew's response is that LLR's econometric setup is more for the purpose of checking for spurious pre-trends that may confound the regression accuracy when such interactions are not included in the regression. The inclusion of those terms is less for the purpose of actually deriving a definitive, causal interpretation for the relationship.

In the context of my analysis, LLR's approach is less applicable since I don't have many years of state-level tax data before 1918 that may help me determine whether the pre-trends confound my analysis. Therefore, I think it's best to not include these interactions between states and time-invariant controls, and I only keep the interactions between time-invariant controls and all years in one of my regression setups. This is a difference between my econometric setup and that of CLV and LLR.

3.3 Results

In the outputs below, column (1) shows the simplest dynamic diff-in-diff setup with no controls. Column (2) shows the LLR replication of the CLV specification with state-level time-invariant controls interacted with yearly categorical variables, as previously explained. In column (3), I run a simple year fixed effects regression with no additional controls. In column (4), I include a series of tax variables to control. For example, if the dependent variable of interest is personal tax growth rate, I include the growth rates of personal tax return number, wages, and net income in the regression. In column (5), I combine (3) and (4) to include both tax variables and year fixed effects.

With NPI intensity as the treatment:

- Table B1 shows that personal tax growth rate is only statistically significant in the "no controls" scenario for the dynamic diff-in-diff model, where higher NPI intensity (enacting social distancing policies for more days) led to a -0.2% difference compared to states with lower intensity. Even when only year fixed effects are implemented, we see no significant variation of personal tax growth rates across the years.

- Unlike personal tax, corporate tax growth rates exhibit statistically significant results across the various regressions in table B2. There seems to be a significant decline of around -0.2% in corporate tax growth rates when NPIs were implemented over a longer period of time versus shorter. After including in the year fixed effects, we see that compared to the growth rate from the base year of 1918, the growth rates for 1919, 1920, and 1921 suffered substantially, declining between -62% to -93% in growth rates (in terms of absolute values, not in relative comparison; so if 1918's growth rate were +100%, then 1919 would be around $100 - 62 = +36\%$).

- The effect of implementing more intense NPIs on total tax revenue is only statistically significant in the "no controls" regression in table B3, where the growth rate for the treatment group states is -2.4% less than the control group.
  As one may have noticed, there are dramatic spikes in the coefficient values when year fixed effects are applied. This is because there was a sudden and significant increase from 1917 to 1918 in total tax revenue across all states. For instance, the total tax revenue of Alabama was around $1.3 million in 1917, but $19 million in 1918 - an almost 15-fold increase; California saw a jump between $24 million to $110 million - slightly less dramatic than AL but nevertheless enough of a jump to skew the regression results.
  I have considered changing the base year for this regression to 1919 and omit the 1918 growth rate data, in which case none of the regressions would exhibit statistically significant results for the main interaction term coefficient. It's not impossible that the more stringent NPI policies indeed caused a -2.4% dent in growth rates, but because of the weird spikes in data, I would not treat this set of regressions with too much significance.

With NPI speed as the treatment:

- Looking directly at the main interaction coefficient, we see that implementing NPIs ten days earlier (which is the benchmark used in this regression) generally did not have any dramatic treatment effect, especially not on personal tax (table B4). The faster NPI speed did positively impact the corporate and total tax growth rates in a statistically significant way, but not in regressions when time fixed effects were included in the regressions (as shown in table B5 and table B6).
- Overall, we may see that in years after 1921, the growth rates for all three tax items fell dramatically compared to the base year 1918. This trend of decline seemed to have ended for personal and total tax revenues after 1922, but corporate tax revenue growth rate continued to decline even in 1925, though less significantly. In other words, tax revenue growth continued to suffer many years after the 1918 flu ended, especially corporate tax, likely also due to the 1920-21 recession.
- The 1918 mortality rate is used in all regressions as a control after being interacted with each year, but it did not seem to have a statistically significant impact on any of the fiscal variables. Likewise for many of the other time-invariant controls that are not outputted here in the regression tables.

3.4 Discussion

Overall, it seems that NPI intensity had more dramatic impacts on tax revenue growth than NPI speed as the treatment factor. Longer NPI intensity tended to depress tax revenues, especially corporate tax growth rate, for many years after the pandemic ended. Faster speed NPI implementation led to higher corporate tax growth rate than otherwise. Personal tax and total revenue collection did not seem to have been impacted as dramatically as corporate tax, likely due to the sensitive nature of businesses to the health of the economy.

However, I would take all the results above with a grain of salt. There are many confounding factors that could have distorted the picture. For one, the 1920-21 economic recession likely depressed corporate tax revenue growth.

Another is that abrupt and often more than ten-fold spikes in total tax revenues across the states from 1917 to 1918. One possible explanation of the spike is the 1918 War Revenue Act, where a one-time excess profits tax was implemented to help with the WWI efforts, thus causing the 1918 total tax revenue spike. This is likely not the cause: according to state-level census data, "war profits and excess tax" was implemented in 1918 but was repealed in 1921; had this item been the reason of the major spike, we should see significant revenue decline back to the pre-WWI level after 1921, which did not happen.

Another possible explanation for the spike is that there could be panic spending during the pandemic: people spent more; business revenues went up; and the total tax revenue went up... However, such phenomenon would've exhibited itself in personal and corporate net income data, which stayed relatively stable throughout the pandemic. I have not come across consumer consumption data, otherwise that would help verify this hypothesis.

A more likely explanation is that the census collection technique simply improved in 1918, so either more items within the broader umbrella of total tax revenues were recorded, or the accuracy of the years after 1918 were dramatically improved. However, we cannot verify this hypothesis because the censuses did not leave any footnote or explanation stating any inaccuracy in years before 1918, so I will assume the complete accuracy of the census data for now.

To further examine whether any other major tax reforms took place, I would need to collect data on actual personal and corporate tax rates during the pandemic in order to see whether these spikes are due to fiscal policy innovations.

4 Diff-in-Diff Analysis for Neighbor and Anchor States

Since cities and states implemented different levels of NPI policy, it would be natural to expect that bordering states influenced each other both in terms of policy stringency and economic activity. Goolsbee and Syverson (2020) explored such an idea for Covid-19, where they implemented difference-in-differences analysis to compare consumer behavior within the same commuting zones but across state boundaries that may have different shelter-in-place policy regimes. This kind of analysis allows one to isolate the effects of legal restrictions from voluntary consumer choices.

In the context of the 1918 flu, I will not exactly replicate Goolsbee and Syverson's model (due to the lack of granular data on consumer behaviors), but I am inspired to conduct some preliminary diff-in-diff analysis to see whether a state's tax revenue was impacted by the neighboring states' policy and societal characteristics.

4.1 Anchor-neighbor interaction on mortality and NPI measures

Before digging into the regressions for tax revenues, I would like to first explore the anchor-neighbor dynamic a bit further. The most natural question to ask would be: do states with more stringent NPI measures tend to cluster with each other? For instance, if California had stringent NPI policies (possibly because of a high ratio of population living in cities), would such measure encourage the neighboring state Nevada to adopt a similar level of stringency even though Nevada is much more rural? I boil down this kind of inquiry into two specific questions we may ask about the relationship between state-level NPI measures and the mortality rate in the context of neighboring states:

1) How did mortality and NPI measures in neighboring states depend on this state's mortality and NPI measures? Which we may answer with the following regression in table 1: the mortality rate and NPI speed of the neighboring states didn't seem to be influenced by any anchor state per se, but the days of NPI are statistically significantly correlated. More specifically, if an anchor state has 1 more point higher on the NPI days score, the maximum NPI days score amongst its neighboring states will go up by 0.566.

Table 1: How neighbor states depend on the anchor state

Dependent variables are the maximum values of neighbor states in respective items as labeled

VARIABLES	(1) Mortality	(2) NPI Days	(3) NPI Speed
Mortality1918	0.330	-0.0432	0.00315
	(0.327)	(0.0532)	(0.00995)
DaysofNPI	-0.719	0.566**	0.0218
	(0.889)	(0.209)	(0.0199)
SpeedofNPI	8.154	0.299	-0.0251
	(5.155)	(1.313)	(0.218)
Constant	513.9**	89.32*	-5.664
	(190.3)	(51.47)	(5.274)
Observations	23	23	23
R-squared	0.092	0.399	0.016

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

2) How did mortality in the anchor state depend on neighboring states mortality and NPI measures? Which we may answer with the following regression in table 2. The results indicate that the 1918 mortality rate of the anchor state will go down by around 1.5 person per 10,000 people when the maximum NPI days score of neighbor states goes up by 1. The 1919 mortality rate in an anchor state is no longer statistically significantly with any neighboring states' characteristics, likely because the flu was gradually dying down. And, like what we saw in the previous regression, if the neighboring states' maximum NPI days score goes up by 1, the anchor state's score will likely go up by 0.628 accordingly.

Table 2: How the anchor state depends on neighboring states

Dependent variables are the values of the anchor state in respective items as labeled

VARIABLES	(1) 1918 Mortality	(2) 1919 Mortality	(3) NPI Days	(4) NPI Speed
(max) neighbor mortality 1918	0.154	0.0226	-0.0348	0.00235
	(0.151)	(0.0205)	(0.0305)	(0.00613)
(max) neighbor NPI days	-1.534**	-0.101	0.628***	0.0500
	(0.602)	(0.0998)	(0.135)	(0.0300)
(max) neighbor NPI speed	2.932	0.452	-0.612	-0.0981
	(4.718)	(0.634)	(0.797)	(0.254)
Constant	657.1***	217.8***	39.59	-14.28***
	(132.2)	(17.74)	(29.10)	(4.609)
Observations	23	23	26	26
R-squared	0.259	0.064	0.472	0.131

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

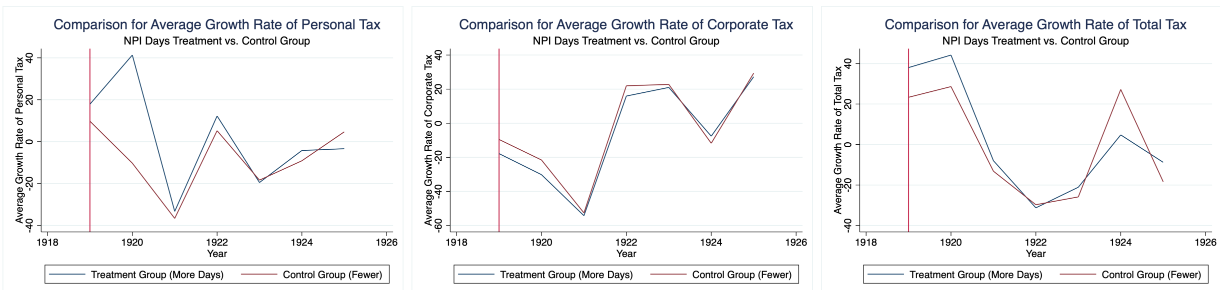
The regression results above make intuitive sense: while the states and their neighbors may not have influenced each other initially in their swiftness to enact NPI measures, it's likely that they will keep the policies for longer if their neighbors don't open up. This means that states with more/less stringent NPI intensity will likely to cluster with each other since they're influenced by their peers.

4.2 Impacts of NPI measures from neighbor states

Certainly, it would be naive to think that some regressions would be able to explain the detailed policy making behind each state's NPI decisions back in 1918, but the anchor-neighbor dichotomy briefly explored in the above subsection may lead us to further inquiries. For example, did a state's tax revenue decline more or less dramatically when it was surrounded by states with higher NPI stringency? More specifically, would a state's tax revenue decrease more drastically when no neighboring states have more stringent NPI measures, which may lead that state's residents and businesses to possibly migrate to those neighboring states with less stringent NPI policies and thus push down tax revenues?

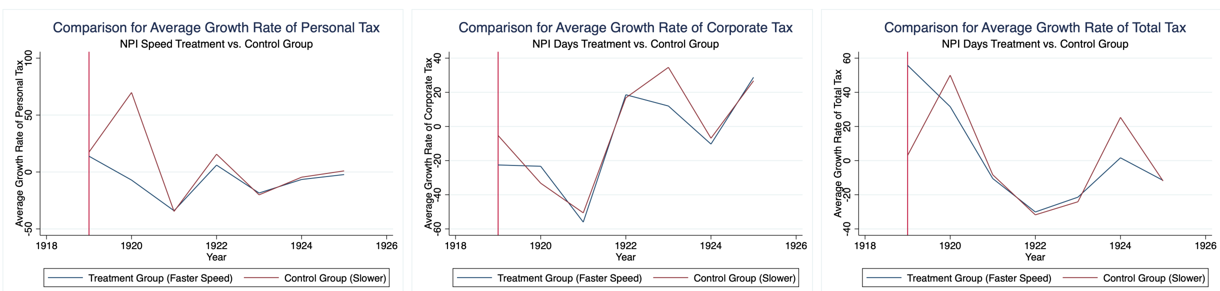
Here, I distinguish the treatment and control groups by whether any of a given state's neighbors have a higher score for NPI days or speed. In other words, if a state has no neighbor with a faster NPI response speed, this state will be in the control group; whereas those with neighbors that acted faster will be in the treatment group. We may arrive at the following set of graphs for tax revenue growth rates over time based on the treatment and control groups. The list of states in the treatment groups is shown in table A2. I graph out the differences between those the treatment and control groups in figures 1 and 2, by NPI days and speed as the treatment factor respectively.

Figure 1: Comparison for Average Growth Rates for Tax – by NPI Days



Left: Personal Tax; Middle: Corporate Tax; Right: Total Tax

Figure 2: Comparison for Average Growth Rates for Tax – by NPI Speed



Left: Personal Tax; Middle: Corporate Tax; Right: Total Tax

In the regression outputs for this framework, as shown in table 3 and 4, all columns display the classic diff-in-diff econometric setup:

$$E_{it} = \beta_0 + \beta_1 PostFlu + \beta_2 Treatment + \beta_3 PostFlu \times Treatment + \epsilon.$$

The results show that when a neighbor state had longer NPI days or faster NPI speed, the anchor state itself did not have more or less tax revenue in a statistically significant way. There was a major difference between the pre- and post-flu periods for both the treatment and control groups, but the difference in differences wasn't significant. For more detailed diff-in-diff regression results under this framework, see table B7 to B12 in the Appendix.

Table 3: When A Neighbor State Had Longer NPI Days

Dependent variable: tax growth rates as labeled

VARIABLES	(1) Personal	(2) Corporate	(3) Total Tax
Post Flu	-20.50***	-24.00***	-225.1***
	(6.315)	(5.687)	(42.55)
=1 if neighb has longer NPI days	8.112	-9.347	39.32
	(12.53)	(8.452)	(56.16)
Post * Longer NPI Days	1.498	6.687	-37.46
	(18.37)	(9.700)	(56.35)
Constant	9.780*	22.04***	219.9***
	(5.081)	(4.746)	(42.76)
Observations	182	208	208
R-squared	0.014	0.049	0.341

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 4: When A Neighbor State Had Faster NPI Speed

Dependent variable: tax growth rates as labeled

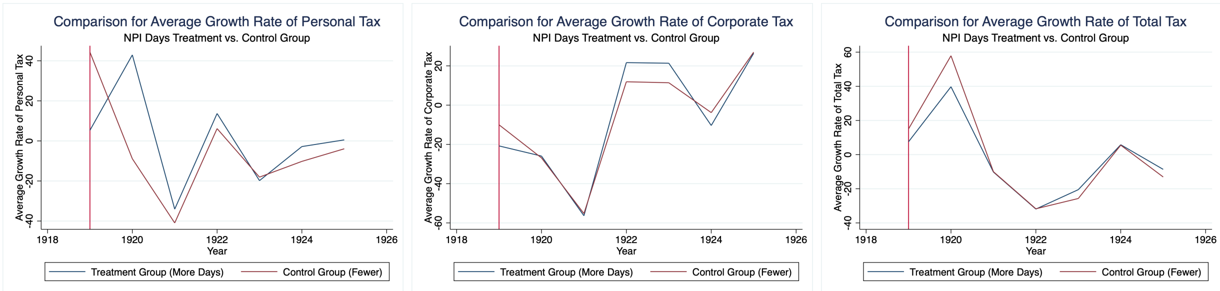
VARIABLES	(1) Personal	(2) Corporate	(3) Total Tax
Post Flu	-12.93	-16.61	-272.5***
	(26.29)	(9.823)	(47.28)
=1 if neighb has faster NPI speed	-3.589	1.828	-43.90
	(18.00)	(10.87)	(58.63)
Post * Faster NPI Speed	-11.32	-4.779	37.30
	(27.48)	(11.97)	(59.23)
Constant	17.47	14.51	272.4***
	(16.53)	(9.202)	(47.09)
Observations	182	208	208
R-squared	0.020	0.046	0.342

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

4.3 Impacts of NPI measures from similar states

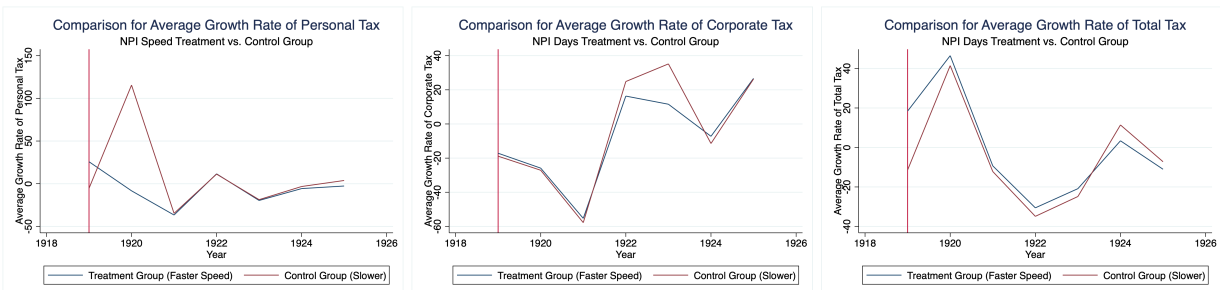
We may run another set of regressions with the same methodology above but now using scores from similar states as the differentiating factor for creating treatment vs. control groups. In other words, a state would be in the treatment group if its "similar states" had a maximum NPI score higher than itself; if not, it would be in the control group. Then, I apply the same diff-in-diff framework from the neighbor state analysis on this group of data. The list of states in the treatment groups is shown in table A2. The graphs for the treatment vs. control group differences are presented in figure 3 and 4.

Figure 3: Comparison for Average Growth Rates for Tax – by NPI Days



Left: Personal Tax; Middle: Corporate Tax; Right: Total Tax

Figure 4: Comparison for Average Growth Rates for Tax – by NPI Speed



Left: Personal Tax; Middle: Corporate Tax; Right: Total Tax

The regressions results from table 5 and 6 show statistically significant but potentially puzzling preliminary results for this diff-in-diff treatment method. The interaction coefficient for the treatment group is positive in value, implying that states that had a similar implementing longer NPI days had much higher tax growth rates than their control group counterparts, whose similar peers did not implement more stringent NPI policy. For NPI speed, it seems that the treatment group states had lower corporate tax growth rate than the control group states, implying that having peers who acted faster was correlated with a corporate tax growth decline.

Table 5: When A Similar State Had Longer NPI Days

Dependent variable: tax growth rates as labeled

VARIABLES	(1) Personal	(2) Corporate	(3) Total Tax
Post Flu	-56.98**	-37.23***	-317.2***
	(21.27)	(10.95)	(41.68)
=1 if similar states have longer NPI days	-39.02*	-21.70*	-108.5*
	(22.33)	(11.94)	(53.29)
Post * Longer NPI Days	51.74*	23.77*	107.0*
	(25.87)	(12.86)	(53.62)
Constant	44.33**	31.30***	314.3***
	(21.21)	(10.46)	(41.08)
Observations	168	192	192
R-squared	0.024	0.064	0.335

Robust standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 6: When A Similar State Had Faster NPI Speed

Dependent variable: tax growth rates as labeled

VARIABLES	(1) Personal	(2) Corporate	(3) Total Tax
Post Flu	17.73	4.739	-276.4***
	(33.08)	(10.30)	(68.27)
=1 if similar states have faster NPI speed	31.18*	31.51***	-48.70
	(17.54)	(10.70)	(74.41)
Post * Faster NPI Speed	-53.72	-35.49***	49.43
	(34.62)	(11.88)	(73.93)
Constant	-5.393	-6.397	272.0***
	(14.42)	(9.327)	(68.77)
Observations	168	192	192
R-squared	0.033	0.082	0.321

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

5 Cross Sectional Analysis

In addition to using the full panel data set to investigate the research questions, an alternative approach is to condense each state's data into a cross-sectional data set, which might give more insight on how certain time-invariant variables may influence the outcomes of interest. For instance, it's not possible to examine how the 1900 employment share in the agricultural sector in each state influenced any time-variant dependent variables (such as tax outcomes) since such a time-invariant variable would simply be omitted due to multicollinearity or differenced out in a fixed effect regression. However, we may construct a time-invariant (cross-sectional) dependent variable such as the cumulative growth rate for personal tax between 1918 and 1925, and this is something that we *can* regress the agricultural employment share on.

With such logic, I create two kinds of growth rate variables: cumulative growth rate and compound annual growth rate (CAGR):

$$Cumulative\ Growth\ Rate = \left(\frac{V_{final} - V_{begin}}{V_{begin}}\right) \times 100\% \tag{4}$$

$$CAGR = \left(\frac{V_{final}}{V_{begin}}\right)^{\frac{1}{t}} - 1 \tag{5}$$

5.1 OLS Approach for Growth Variables

The first econometric setup for cross-sectional analysis is fairly simple, where I begin with OLS regressions:

$$Y_{s,i,j,m,n} = \beta_0 + \sum_i \beta_i TaxControls_{s,i} + \sum_j \beta_j Geo_{s,j} + \sum_m \beta_m Inequality_{s,m} + \sum_s \beta_s NPI_{s,n} + \epsilon \tag{6}$$

The LHS dependent variables are the fiscal variables of my interest (e.g. **growth rates of personal, corporate, and total tax revenues**) in a state s in year t. The RHS includes a wide range of variables:

- $TaxControls_i$ are the growth rate of tax variables from 1918 to 1925 depending on the kind of tax of interest (for personal tax, the controls are personal tax return number, personal income, personal wages, personal tax exemption; for corporate tax, the controls are number of corporations, number of corporate wage earners, corporate net income; for total tax revenue, the controls are the number of personal returns and corporations).

- Geo_j are geographical controls, such as state-level urban population share in 1917, population in 1917, agricultural employment share in 1910, manufacturing employment share 1914, and region category (Northeast, Midwest, South, and West).

- $Inequality_m$ are inequality-related variables, such as the growth rates from 1918 to 1925 of fiscal income of the bottom 90%, top 10%, top 1%, and top 0.1%. I also include state-level per capita income growth rate from 1910 to 1930 and from 1920 to 1930.
- $NPI_{s,n}$ include either NPI values or NPI categories. The values are the actual values of 1918 mortality rate, NPI intensity scores, and NPI speed scores. The categories are three above/below-median indicators created based on the actual values.

5.2 Results

For cumulative growth rates of personal tax (table B19), we see that it's statistically significantly correlated with the growth rates of many personal tax related variables in intuitive ways: when the growth rate of personal return number, wages, or net income went up, tax revenue went up accordingly; and when personal tax exemption went up, tax revenue growth fell. Meanwhile, it seems that personal tax growth was more heavily correlated with the top 1% fiscal income than that of the top 10% of bottom 90%. Geographical locations didn't seem to make a big difference, though being in the South seemed to have impacted tax growth negatively.

The regressions for corporate tax (table B20) exhibit similar but less statistically significant results. Corporate tax growth rate increased around +0.6% as corporate income grew by +1%, but the growth of corporations would actually lead to a decline in corporate tax revenue, which is a puzzling result. Equally puzzling, a higher growth rate for the bottom 90% fiscal income would lead to a larger decline in corporate tax. Geography didn't matter significantly, though being in the South seemed to have hurt corporate tax growth.

Total tax revenue growth (table B21) increased when the number of personal tax returns increased but not when the number of corporations did. Both should be expansions of the broader tax base, though admittedly the increase in corporation number might not directly result in a higher number of corporations paying tax *per se*. Higher mortality rate in 1918 hurt overall tax growth, though NPI measures didn't seem to matter significantly throughout the regressions for any of the tax variables.

Overall, the conclusion from this section is that tax revenue growth was mostly driven by the expansion of tax base, both in terms of number of tax returns and amount of net income available for revenue collection. Geography seemed to play a role only in that the Southern states suffered from slower tax growth, and there was no statistically significant differences between Northeast, West, and the Midwest (which was used as the base region for regressions and seemed to have the fastest tax revenue growth compared to the other regions).

Variables related to inequality measures such as fiscal income grouped by income class didn't seem to significantly affect the tax growth rate, though we do observe the puzzling result that higher income growth from the bottom 90% would actually hurt personal and corporate tax growth. There may be more accurate measures of inequality that could be better incorporated in this analysis, and this warrants further exploration.

The values and categories of NPI and mortality rate in 1918 do not play a statistically significant role in shaping the cumulative growth rate of tax variables. Because the data of interest in this section are the cumulative growth rates from 1918 to 1925, those regressions essentially take a seven-year view on the tax revenue development, and we might thus be able to say that NPI and mortality rate did not have a dramatic impact on tax growth when being considered over a seven-year or medium-term horizon, in which characteristics in geography and tax base played a much larger role.

6 Questions Unanswered

As briefly stated in the "disclaimer" on the front page, this paper is still in quite an early stage, and there are many interesting aspects that I have not gotten the chance to explore. Should anyone wish to carry this topic further, I would be more than happy to provide my data set and code if they could be any helpful.

6.1 On taxation during the 1918 flu

I used binscatter graphs (attached in the appendix) to fully explore the data set, and many interesting points of lever emerged, which I unfortunately have not gotten the chance to fully explore.

CDC only had mortality data for around 30 "registration states," which means that I primarily ran diff-in-diff analysis based on the treatment and control groups created based on those 30 states. However, "non-registration states" had significantly lower tax revenues than registration states because they were the less developed areas like Alabama, Idaho, and Mississippi. So, by not including the "non-registration" states in my analysis, I am losing out on a very important control group that may help one arrive at the conclusion that tax revenues growth during and after the 1918 flu could have had more to do with the urban/rural differences than mortality rate differences. Likewise, there were many other pairs of treatment and control groups that I did not get the chance to run analysis on, which could reveal more about how state-level characteristics impact tax revenues.

The 1918 flu took place during a quite tumultuous time in history - at the end of WWI and just before the 1920-21 economic recession in the U.S. WWI was a period when aggressive fiscal policies and production incentives were implemented, and the 20-21 recession was when households and businesses suffered dramatically in terms of their financial well-being. Therefore, it seems hard to single out the effect of the 1918 flu on public finance. It would be quite likely that much of the tax revenue decline after 1918 flu was due to the 20-21 recession, or it could be that the flu had such a persistent impact - either one could be a plausible explanation, and my framework is not yet sufficient to explain how one theory might be better than the other.

6.2 On inequality during pandemics

As briefly mentioned in the introduction, I started this project in June, 2020 largely because of my interests in public finance and inequality. Back then, Prof. Branko Milanovic posed a challenging question to me: Why did we think (based on the Black Death mostly) that epidemics are inequality-reducing, while Covid has somewhat surprised us with the opposite effect as billionaires became richer and poor people struggled more?

My preliminary research in June led to my brief email communications with Prof. Guido Alfani, who suggested to me that the idea that inequality reduces after pandemics was largely generalized from the Black Death and is wrong. The Black Death did lead to significant and long-lasting inequality reduction, but subsequent plagues did not. Prof. Alfani's words motivated me to think what misunderstandings we may have about the 1918 flu in terms of distributive dynamics and public finance policies, since it returned to the public spotlight after the Covid-19 crisis.

I also had the chance to talk to Dr. Merle Eisenberg, who added insightful commentaries from a historian's perspective. He noted that if one can cite the Black Death as perhaps inequality-reducing, then one can also cite the Antonine Plague, the Cyprian Plague, the Justinianic Plague, Cholera in the 19th century, the 1918 Flu, and Covid-19 (to name just a few) to show how pandemics did not seem to make a difference.

Dr. Eisenberg also supplemented many caveats to the notion that the Black Death was inequality-reducing: first, if a large portion of the population needs to die in order to achieve economic inequality reduced for the next generation (and it didn't last much more than maybe 50 years in the case of Black Death), then this framework probably should not be an appropriate way to look at things in the first place. Meanwhile, even if the Black Death did reduce economic inequality, it did not seem to have changed the upper elite, since there were no political revolutions due to the Black Death, and all the states remained the same with the same rulers, so the power dynamic between the "top 1%" and the general population still stayed the same.

Prof. Leah Boustan was the third scholar who questioned the notion that pandemics are inequality-reducing. She told me that the idea that disease outbreaks can increase wages for the poor and reduce inequality comes from episodes that killed many people, thereby lowering labor supply and raising wages (at least temporarily). If judging strictly from that angle, Covid-19 has certainly not achieved the same level of dramatic alterations in population and labor structures - since the death toll has been much lower than in historical plagues and unfortunately much more concentrated among the elderly.

Overall, the consensus from all the insightful scholars I have talked to seems to be that 1) the past is likely not a good guide for today, as the economic structures differ dramatically; 2) the idea that pandemics are inequality-reducing might only fit in cases where a significant portion of the society pass away (like the Black Death), but should not be generalized.

Returning to Prof. Milanovic's original question to me on why we think pandemics are inequality - perhaps, we may view it from a more "meta" angle and concentrate on the *why* part. Where did that idea initially come from? From what data did scholars initially derive that conclusion? Why did we ignore many of historical plagues that did not seem to reduce inequality (as listed by Dr. Eisenberg)? How can we derive useful and accurate lessons from history without naively extrapolating certain facts or relying on over-generalizing narratives? Such may be the questions more suitable for a historian or sociologist to answer than for an economist, and this project has certainly not explored these ideas.

Though this paper is still in quite a preliminary stage, I ultimately hope to contribute to our understanding on how pandemics impact public finance and distributive dynamics. I look forward to continuing working on those issues going forward, and I sincerely thank everyone who has so graciously helped me wander in this fascinating field.

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Appendix

A. Data Appendix

Based on the various sources briefly described in the data section of my main write-up, I have collected data for the following variables (at the state level unless otherwise note):

- Personal tax: number of personal tax returns, amount of personal tax exemption, wages, net income, and total personal income tax revenue.
- Corporate tax: number of corporations, amount of corporate wages, net income, and total corporate tax revenue.
- Total revenue: special war profit tax and total internal revenue collection.
- Fiscal & pre-tax income: fiscal income in absolute value, and pre-tax income by share percentage in total economy; both variables are categorized by income class (bottom 90%, top 10%, top 1%, top 0.1%, and top 0.01%).
- Per capita income: personal income per capita in 1900, 1910, 1919-21, and 1930 (in 1960 consumer dollars).
- Demographics: state population in 1900, 1910, and 1917; density per square mile; urban vs. rural population share; agricultural vs. industrial sector employment share.
- Public health: mortality rate 1911-1920 for 26 registration states and 43 registration cities; city-level NPI intensity and speed measures calculated by CLV (2020).

Table A1: List of neighbor and similar states

State	Neighbor States	Similar States
AL	FL, GA, MS, TN	WA, TN
AK		
AZ		
AR		
CA	AZ, NV, OR	CO, NJ
CO	AZ, KS, NE, NM, OK, UT, WY	CA, NJ
CT	MA, NY, RI	MA, RI
DE		
DC	MD, VA	no similar states
FL		
GA		
HI		
ID		
IL	IN, IA, MI, KY, MO, WI	PA, TN, WA
IN	IL, KY, MI, OH	IL, PA, WI
IA		
KS	CO, MI, NE, OK	CA, CO, NY
KY	IL, IN, MO, OH, TN, VA, WV	MN, MO, OR
LA	AR, MS, TX	OH
ME		
MD	DE, PA, VA, WV	VA, WI
MA	CT, NH, NY, RI, VT	CT, NY
MI	IL, IN, MN, OH, WI	CT, RI
MN	IA, MI, ND, SD, WI	KY, OR
MS		
MO	AR, IL, IA, KS, KY, NE, OK, TN	KY, VA
MT		
NE		
NV	AZ, CA, ID, OR, UT	no similar states
NH		
NJ	DE, NY, PA	CA, CO
NM		
NY	CT, MA, NJ, PA, RI, VT	KS, MA
NC		
ND		
OH	IN, KY, MI, PA, WV	LA
OK		
OR	CA, ID, NV, WA	KY, MN
PA	DE, MD, NJ, NY, OH, WV	IL, IN, WA
RI	CT, MA, NY	CT, MI
SC		
SD		
TN	AL, AR, GA, KY, MS, MO, NC, VA	AL, WA
TX		
UT		
VT		
VA	KY, MD, NC, TN, WV	MD, MO
WA	ID, OR	AL, IL, TN
WV		
WI	IL, IA, MI, MN	IN, MD, PA
WY		

Note: States without NPI data are left blank. DC and NV's similarity scores are too far from every other state with NPI data.

Table A2: List of anchor states with neighbor/similar states with higher NPI scores

	Neighbor longer days	Neighbor faster speed	Similar state longer days	Similar state faster speed
	AL	IL	AL	CA
	CA	IN	CA	CO
	CO	KS	CT	IL
	CT	KY	IL	IN
	IL	MA	IN	KS
	IN	MO	KY	KY
	MD	NV	LA	MA
	MI	NY	MD	MI
	MN	OH	MA	MO
	MO	OR	MN	NY
	NV	PA	MO	OH
	NJ	RI	NJ	OR
	OH	TN	NY	RI
	OR	VA	PA	TN
	PA	WI	RI	VA
	RI		TN	WA
	TN		VA	WI
	VA			
Count:	18	15	17	17
Count of counterparts:	8	11	9	9

B. Regression Outputs

1. Diff-in-diff regressions for the CLV & LLR approach

With NPI intensity as the treatment:

Table B1: NPI Days - Personal Tax

Dependent variable: personal tax growth rate

VARIABLES	(1) No controls	(2) W/controls	(3) Year FE	(4) W/tax var	(5) Year FE+tax var
c.PostFlu#c.DaysofNPI	-0.211*	0.164	-0.198	-0.109	-0.199
	(0.103)	(0.597)	(0.170)	(0.167)	(0.192)
Year = 1920			28.11		33.43
			(46.37)		(50.78)
Year = 1921			-31.62		-37.91
			(19.36)		(30.91)
Year = 1922			12.75		8.791
			(17.52)		(23.47)
Year = 1923			-16.48		-15.45
			(18.46)		(19.38)
Year = 1924			-3.062		-9.200
			(18.43)		(27.21)
Year = 1925			1.777		-17.96
			(18.26)		(53.74)
Personal tax return growth rate				-0.241	-0.467
				(0.282)	(0.803)
Personal wage growth rate				0.451	0.141
				(0.421)	(0.129)
Personal income growth rate				0.0748	0.0472
				(0.0472)	(0.0364)
Constant	15.16*	-10.74	15.40	5.033	19.65
	(8.008)	(44.39)	(10.37)	(14.37)	(12.33)
Observations	182	133	182	182	182
R-squared	0.013	0.163	0.086	0.029	0.090
Number of State	26	19	26	26	26

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table B2: NPI Days - Corporate Tax

Dependent variable: corporate tax growth rate

VARIABLES	(1) No controls	(2) W/controls	(3) Year FE	(4) W/tax var	(5) Year FE+tax var
c.PostFlu#c.DaysofNPI	-0.186***	0.0832	-0.0782	-0.173***	-0.0338
	(0.0498)	(0.223)	(0.122)	(0.0325)	(0.0769)
Year = 1919			-61.62***		-110.3***
			(11.49)		(8.044)
Year = 1920			-66.75***		-72.58***
			(17.62)		(10.65)
Year = 1921			-92.96***		-73.51***
			(15.16)		(11.60)
Year = 1922			-21.44		-117.4***
			(18.59)		(13.07)
Year = 1923			-17.70		-67.56***
			(17.60)		(10.48)
Year = 1924			-48.11***		-60.28***
			(16.99)		(11.21)
Year = 1925			-11.34		-62.39***
			(15.57)		(10.65)
Corporate number growth rate				-0.465	-0.0639
				(0.282)	(0.0471)
Corporate income growth rate				0.634***	1.086***
				(0.0470)	(0.0856)
Constant	13.77***	23.46	46.38***	11.28***	68.94***
	(3.402)	(17.32)	(9.029)	(2.505)	(6.554)
Observations	208	152	208	208	208
R-squared	0.044	0.740	0.627	0.385	0.844
Number of State	26	19	26	26	26

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table B3: NPI Days - Total Tax Revenue

Dependent variable: total tax growth rate

VARIABLES	(1) No controls	(2) W/controls	(3) Year FE	(4) W/tax var	(5) Year FE+tax var
c.PostFlu#c.DaysofNPI	-2.385***	-0.440	-0.867	-0.502	-0.819
	(0.262)	(0.777)	(0.620)	(0.354)	(0.575)
Year = 1919			-427.2***		
			(62.25)		
Year = 1920			-342.3***		73.78**
			(86.08)		(35.17)
Year = 1921			-391.2***		45.24
			(86.59)		(46.65)
Year = 1922			-412.4***		19.68
			(87.02)		(23.30)
Year = 1923			-404.2***		22.20
			(87.65)		(30.64)
Year = 1924			-370.0***		64.84
			(85.77)		(40.61)
Year = 1925			-393.4***		61.66*
			(86.27)		(36.06)
Personal tax return growth rate				0.432***	0.523*
				(0.0716)	(0.257)
Number of corporations				0.00142	0.000261
				(0.00112)	(0.000664)
Personal income growth rate				-0.0671	-0.0427
				(0.0672)	(0.0360)
Corporate income growth rate				-0.311***	0.00344
				(0.0723)	(0.269)
Constant	221.8***	25.73	460.7***	26.82	20.60
	(17.92)	(62.76)	(47.98)	(25.10)	(16.53)
Observations	208	152	208	182	182
R-squared	0.327	0.853	0.711	0.187	0.288
Number of State	26	19	26	26	26

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

With NPI speed as the treatment:

Table B4: NPI Speed - Personal Tax Revenue

Dependent variable: personal tax growth rate

VARIABLES	(1) No controls	(2) W/controls	(3) Year FE	(4) W/tax var	(5) Year FE+tax var
c.PostFlu#c.SpeedofNPI	0.424		-2.119	-0.260	-2.026
	(2.002)		(2.535)	(2.564)	(2.722)
Year = 1920			-5.012		-2.485
			(24.28)		(32.33)
Year = 1921			-64.74***		-74.44**
			(14.89)		(35.49)
Year = 1922			-20.37		-26.67
			(15.49)		(29.64)
Year = 1923			-49.60***		-51.14**
			(14.85)		(24.24)
Year = 1924			-36.18**		-44.62
			(15.61)		(32.42)
Year = 1925			-31.34**		-52.78
			(15.11)		(57.60)
Personal tax return growth rate				-0.240	-0.398
				(0.294)	(0.713)
Personal wage growth rate				0.502	0.117
				(0.417)	(0.141)
Personal income growth rate				0.0923*	-0.0180
				(0.0536)	(0.0609)
Constant	1.297	-34.01	15.40	-5.508	22.92*
	(12.21)	(46.39)	(10.18)	(16.56)	(13.17)
Observations	182	133	182	182	182
R-squared	0.000	0.179	0.090	0.026	0.092
Number of State	26	19	26	26	26

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table B5: NPI Speed - Corporate Tax Revenue

Dependent variable: corp prate tax growth rate

VARIABLES	(1) No controls	(2) W/controls	(3) Year FE	(4) W/tax var	(5) Year FE+tax var
c.PostFlu#c.SpeedofNPI	1.570*		0.304	1.270*	-0.205
	(0.794)		(0.990)	(0.688)	(0.720)
Year = 1919			-61.62***		-110.5***
			(11.49)		(7.894)
Year = 1920			-71.72***		-77.10***
			(9.382)		(6.229)
Year = 1921			-97.93***		-77.91***
			(10.44)		(6.923)
Year = 1922			-26.40***		-122.3***
			(9.199)		(11.00)
Year = 1923			-22.66**		-72.29***
			(10.41)		(8.000)
Year = 1924			-53.08***		-64.82***
			(8.087)		(5.873)
Year = 1925			-16.31*		-67.12***
			(9.512)		(7.332)
Corporate number growth rate				-0.485	-0.0656
				(0.288)	(0.0527)
Corporate income growth rate				0.630***	1.091***
				(0.0484)	(0.0919)
Constant	9.422**	26.15*	46.38***	6.332*	69.03***
	(4.236)	(14.36)	(9.041)	(3.667)	(6.486)
Observations	208	152	208	208	208
R-squared	0.029	0.741	0.626	0.367	0.844
Number of State	26	19	26	26	26

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table B6: NPI Speed - Total Tax Revenue

Dependent variable: total tax growth rate

VARIABLES	(1) No controls	(2) W/controls	(3) Year FE	(4) W/tax var	(5) Year FE+tax var
c.PostFlu#c.SpeedofNPI	18.11***		-0.778	0.0679	-6.578
	(3.209)		(2.698)	(0.546)	(5.213)
Year = 1919			-427.2***		
			(62.25)		
Year = 1920			-426.8***		-56.54
			(58.29)		(63.72)
Year = 1921			-475.8***		-87.45
			(58.21)		(53.48)
Year = 1922			-497.0***		-115.2
			(58.73)		(78.85)
Year = 1923			-488.8***		-110.8
			(59.94)		(67.88)
Year = 1924			-454.6***		-68.33
			(58.24)		(60.22)
Year = 1925			-477.9***		-73.64
			(58.96)		(69.34)
Personal tax return growth rate				0.569***	0.631*
				(0.139)	(0.320)
Number of corporations				0.000552*	0.00156
				(0.000285)	(0.00145)
Personal income growth rate				0.00286	-0.242
				(0.0301)	(0.185)
Corporate income growth rate				-0.231*	0.0494
				(0.131)	(0.297)
Constant	155.5***	16.51	460.7***	-3.783	17.41
	(17.12)	(22.86)	(48.47)	(4.719)	(17.48)
Observations	208	152	208	182	182
R-squared	0.177	0.855	0.703	0.097	0.296
Number of State	26	19	26	26	26

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

2. Complementary diff-in-diff regressions for neighbor states

Here, I provide some additional diff-in-diff regressions for various tax variables using the neighboring state treatment/control group approach. The regression outputs provided in the main write-up (as shown in table 3 and 4) are essentially condensed versions of the tables below.

In the output tables B7 to B12 below, Column (1) and (2) illustrate an OLS regression of the tax growth rate on the pre- and post-period dummy variable using the data only for the treatment group or only for the control group. The regression runs like this:

$$E_{it} = \beta_0 + \beta_1 PostFlu + \epsilon,$$

where we cluster our standard errors by state.

Column (3) displays the classic diff-in-diff econometric setup:

$$E_{it} = \beta_0 + \beta_1 PostFlu + \beta_2 Treatment + \beta_3 PostFlu \times Treatment + \epsilon.$$

Column (4) shows another fixed effect regression using the "areg" function, where we see that the coefficient for the treatment dummy variable has been omitted in the regression. That is because in the "areg" regression, the treatment dummy gets omitted due to multicollinearity since the information of state is already captured by the fixed effect regression after we type "a(state)" command in Stata.

Table B7: When A Neighbor State Had Longer NPI Days - Personal Tax Revenue

Dependent variable: personal tax growth rate

VARIABLES	(1) Treatment	(2) Control	(3) Diff-in-diff	(4) areg
Post Flu	-19.00	-20.50**	-20.50***	-20.50***
	(17.33)	(6.625)	(6.315)	(6.789)
=1 if neighb has longer NPI days			8.112	
			(12.53)	
Post * Longer NPI Days			1.498	1.498
			(18.37)	(19.75)
=1 if neighb has longer NPI days = o,				-
Constant	17.89	9.780	9.780*	15.40
	(11.51)	(5.330)	(5.081)	(11.15)
Observations	126	56	182	182
R-squared	0.007	0.105	0.014	0.104

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table B8: When A Neighbor State Had Longer NPI Days - Corporate Tax Revenue

Dependent variable: corporate tax growth rate

VARIABLES	(1) Treatment	(2) Control	(3) Diff-in-diff	(4) areg
Post Flu	-17.31**	-24.00***	-24.00***	-24.00***
	(7.899)	(5.966)	(5.687)	(6.054)
=1 if neighb has longer NPI days			-9.347	
			(8.452)	
Post * Longer NPI Days			6.687	6.687
			(9.700)	(10.33)
=1 if neighb has longer NPI days = o,				-
Constant	12.69*	22.04***	22.04***	15.57***
	(7.030)	(4.978)	(4.746)	(4.563)
Observations	144	64	208	208
R-squared	0.037	0.066	0.049	0.071

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table B9: When A Neighbor State Had Longer NPI Days - Total Tax Revenue

Dependent variable: total tax growth rate

VARIABLES	(1) Treatment	(2) Control	(3) Diff-in-diff	(4) areg
Post Flu	-262.5***	-225.1***	-225.1***	-225.1***
	(37.13)	(44.64)	(42.55)	(45.30)
=1 if neighb has longer NPI days			39.32	
			(56.16)	
Post * Longer NPI Days			-37.46	-37.46
			(56.35)	(59.98)
=1 if neighb has longer NPI days = o,				-
Constant	259.2***	219.9***	219.9***	247.1***
	(36.60)	(44.85)	(42.76)	(22.94)
Observations	144	64	208	208
R-squared	0.338	0.348	0.341	0.376

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table B10: When A Neighbor State Had Faster NPI Speed - Personal Tax Revenue

Dependent variable: personal tax growth rate

VARIABLES	(1) Treatment	(2) Control	(3) Diff-in-diff	(4) areg
Post Flu	-24.25***	-12.93	-12.93	-12.93
	(8.119)	(26.99)	(26.29)	(28.26)
=1 if neighb has faster NPI speed			-3.589	
			(18.00)	
Post * Faster NPI Speed			-11.32	-11.32
			(27.48)	(29.55)
=1 if neighb has faster NPI speed = o,				-
Constant	13.88*	17.47	17.47	15.40
	(7.195)	(16.97)	(16.53)	(11.10)
Observations	105	77	182	182
R-squared	0.159	0.002	0.020	0.104

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table B11: When A Neighbor State Had Faster NPI Speed - Corporate Tax Revenue

Dependent variable: corporate tax growth rate

VARIABLES	(1) Treatment	(2) Control	(3) Diff-in-diff	(4) areg
Post Flu	-21.39***	-16.61	-16.61	-16.61
	(6.916)	(10.09)	(9.823)	(10.46)
=1 if neighb has faster NPI speed			1.828	
			(10.87)	
Post * Faster NPI Speed			-4.779	-4.779
			(11.97)	(12.74)
=1 if neighb has faster NPI speed = o,				-
Constant	16.34**	14.51	14.51	15.57***
	(5.854)	(9.450)	(9.202)	(4.574)
Observations	120	88	208	208
R-squared	0.056	0.032	0.046	0.071

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table B12: When A Neighbor State Had Faster NPI Speed - Total Tax Revenue

Dependent variable: total tax growth rate

VARIABLES	(1) Treatment	(2) Control	(3) Diff-in-diff	(4) areg
Post Flu	-235.2***	-272.5***	-272.5***	-272.5***
	(36.10)	(48.55)	(47.28)	(50.33)
=1 if neighb has faster NPI speed			-43.90	
			(58.63)	
Post * Faster NPI Speed			37.30	37.30
			(59.23)	(63.05)
=1 if neighb has faster NPI speed = o,				-
Constant	228.5***	272.4***	272.4***	247.1***
	(35.34)	(48.35)	(47.09)	(22.91)
Observations	120	88	208	208
R-squared	0.382	0.308	0.342	0.376

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

3. Complementary diff-in-diff regressions for similar states

Here, I attach more detailed diff-in-diff regression outputs that are complementary to the combined outputs as shown in table 5 and 6.

Table B13: When A Similar State Had Longer NPI Days - Personal Tax Revenue

Dependent variable: personal tax growth rate

VARIABLES	(1) Treatment	(2) Control	(3) Diff-in-diff	(4) areg
Post Flu	-5.231	-56.98**	-56.98**	-20.50***
	(14.79)	(22.52)	(21.27)	(6.789)
=1 if similar states have longer NPI days			-39.02*	
			(22.33)	
Post * Longer NPI Days			51.74*	
			(25.87)	
=1 if neighb has longer NPI days = o,				-
Post * Longer NPI Days				1.498
				(19.75)
Constant	5.309	44.33*	44.33**	15.40
	(7.003)	(22.46)	(21.21)	(11.15)
Observations	119	49	168	182
R-squared	0.000	0.332	0.024	0.104

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table B14: When A Similar State Had Longer NPI Days - Corporate Tax Revenue

Dependent variable: corporate tax growth rate

VARIABLES	(1) Treatment	(2) Control	(3) Diff-in-diff	(4) areg
Post Flu	-13.47*	-37.23**	-37.23***	-24.00***
	(6.766)	(11.60)	(10.95)	(6.054)
=1 if similar states have longer NPI days			-21.70*	
			(11.94)	
Post * Longer NPI Days			23.77*	
			(12.86)	
=1 if neighb has longer NPI days = o,				-
Post * Longer NPI Days				6.687
				(10.33)
Constant	9.597	31.30**	31.30***	15.57***
	(5.790)	(11.08)	(10.46)	(4.563)
Observations	136	56	192	208
R-squared	0.023	0.141	0.064	0.071

Robust standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table B15: When A Similar State Had Longer NPI Days - Total Tax Revenue

Dependent variable: total tax growth rate

VARIABLES	(1) Treatment	(2) Control	(3) Diff-in-diff	(4) areg
Post Flu	-210.2***	-317.2***	-317.2***	-225.1***
	(33.89)	(44.13)	(41.68)	(45.30)
=1 if similar states have longer NPI days			-108.5*	
			(53.29)	
Post * Longer NPI Days			107.0*	
			(53.62)	
=1 if neighb has longer NPI days = o,				-
Post * Longer NPI Days				-37.46
				(59.98)
Constant	205.9***	314.3***	314.3***	247.1***
	(34.10)	(43.50)	(41.08)	(22.94)
Observations	136	56	192	208
R-squared	0.286	0.399	0.335	0.376

Robust standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table B16: When A Similar State Had Faster NPI Speed - Personal Tax Revenue

Dependent variable: personal tax growth rate

VARIABLES	(1) Treatment	(2) Control	(3) Diff-in-diff	(4) areg
Post Flu	-35.99***	17.73	17.73	17.73
	(10.26)	(35.03)	(33.08)	(35.55)
=1 if similar states have faster NPI speed			31.18*	
			(17.54)	
Post * Faster NPI Speed			-53.72	-53.72
			(34.62)	(37.21)
=1 if similar states have faster NPI speed = o,				-
Constant	25.78**	-5.393	-5.393	16.69
	(10.03)	(15.27)	(14.42)	(11.11)
Observations	119	49	168	168
R-squared	0.222	0.002	0.033	0.117

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table B17: When A Similar State Had Faster NPI Speed - Corporate Tax Revenue

Dependent variable: corporate tax growth rate

VARIABLES	(1) Treatment	(2) Control	(3) Diff-in-diff	(4) areg
Post Flu	-30.75***	4.739	4.739	4.739
	(5.950)	(10.90)	(10.30)	(10.96)
=1 if similar states have faster NPI speed			31.51***	
			(10.70)	
Post * Faster NPI Speed			-35.49***	-35.49**
			(11.88)	(12.64)
=1 if similar states have faster NPI speed = o,				-
Constant	25.12***	-6.397	-6.397	15.93***
	(5.270)	(9.875)	(9.327)	(4.117)
Observations	136	56	192	192
R-squared	0.108	0.003	0.082	0.102

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table B18: When A Similar State Had Faster NPI Speed - Total Tax Revenue

Dependent variable: total tax growth rate

VARIABLES	(1) Treatment	(2) Control	(3) Diff-in-diff	(4) areg
Post Flu	-226.9***	-276.4***	-276.4***	-276.4***
	(28.53)	(72.28)	(68.27)	(72.65)
=1 if similar states have faster NPI speed			-48.70	
			(74.41)	
Post * Faster NPI Speed			49.43	49.43
			(73.93)	(78.68)
=1 if similar states have faster NPI speed = o,				-
Constant	223.3***	272.0***	272.0***	237.5***
	(28.54)	(72.81)	(68.77)	(22.59)
Observations	136	56	192	192
R-squared	0.353	0.278	0.321	0.355

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

4. Cross sectional analysis with cumulative growth rates

Table B19: Cross Sectional Analysis for Personal Tax

Dependent variable: personal tax cumulative growth rate 1918-1925

VARIABLES	(1) Tax	(2) Geo	(3) Inequality	(4) NPI Value	(5) NPI Category	(6) All
UrbanPopShare		-1.846				-0.475
		(1.794)				(0.478)
region = 2, NE		-73.38				-34.32*
		(68.89)				(18.58)
region = 3, S		84.60*				-48.58**
		(47.53)				(17.82)
region = 4, W		-19.50				-6.050
		(55.83)				(15.84)
State Population 1917		-7.23e-07				
		(9.00e-06)				
Agriculture Employment Share 1910		-222.2				
		(246.2)				
Manufacturing Employment Share 1914		380.5				
		(389.5)				
Personal return growth rate 1918-25	1.869*					0.797***
	(0.991)					(0.204)
Personal wages growth rate 1918-25	1.353***					
	(0.203)					
Personal exemption growth rate 1918-25	-1.413***					
	(0.502)					
Personal income growth rate 1918-25	0.0790*					
	(0.0397)					
Bottom 90% fiscal income growth rate 1918-25			-0.698***			
			(0.117)			
Top 10% fiscal income growth rate 1918-25			0.0304			
			(0.178)			
Top 1% fiscal income growth rate 1918-25			1.695***			
			(0.277)			
Top 0.1% fiscal income growth rate 1918-25			-0.325**			
			(0.136)			
Per capita income growth rate 1920-30			-0.157			-0.158
			(0.174)			(0.708)
Mortality1918				-0.0517		0.0837
				(0.0416)		(0.0490)
DaysofNPI				-0.238		-0.286
				(0.149)		(0.194)
SpeedofNPI				-0.395		-0.244
				(0.851)		(0.638)
=1 if above-median mortality = 1					-9.363	
					(14.86)	
=1 if in longer NPI days category = 1					-16.37	
					(14.00)	
=1 if in faster NPI speed category = 1					8.277	
					(14.21)	
Constant	15.17	14.07	-71.44***	7.389	-31.47**	-13.18
	(34.20)	(189.5)	(2.166)	(33.64)	(13.69)	(37.81)
Observations	50	49	49	23	23	23
R-squared	0.916	0.127	0.989	0.163	0.109	0.712

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table B20: Cross Sectional Analysis for Corporate Tax

Dependent variable: corporate tax cumulative growth rate 1918-1925

VARIABLES	(1) Tax	(2) Geo	(3) Inequality	(4) NPI Value	(5) NPI Category	(6) All
UrbanPopShare		-1.027				-0.957*
		(1.768)				(0.455)
region = 2, NE		-38.22				-8.289
		(67.88)				(15.81)
region = 3, S		77.90				-30.95*
		(46.84)				(15.84)
region = 4, W		-43.34				15.08
		(55.02)				(13.44)
State Population 1917		-6.70e-06				
		(8.87e-06)				
Agriculture Employment Share 1910		-320.9				
		(242.6)				
Manufacturing Employment Share 1914		-84.12				
		(383.8)				
Corporate number growth rate 1918-25	-0.124*					0.576*
	(0.0628)					(0.287)
Corporate income growth rate 1918-25	0.588***					
	(0.0125)					
Bottom 90% fiscal income growth rate 1918-25			-1.291***			
			(0.332)			
Top 10% fiscal income growth rate 1918-25			0.160			
			(0.507)			
Top 1% fiscal income growth rate 1918-25			0.977			
			(0.788)			
Top 0.1% fiscal income growth rate 1918-25			-0.136			
			(0.386)			
Per capita income growth rate 1920-30			0.174			-0.527
			(0.495)			(0.604)
Mortality1918				-0.0271		0.0425
				(0.0261)		(0.0390)
DaysofNPI				-0.0592		-0.179
				(0.0934)		(0.165)
SpeedofNPI				0.205		0.0756
				(0.535)		(0.556)
=1 if above-median mortality = 1					-2.218	
					(9.278)	
=1 if in longer NPI days category = 1					-1.810	
					(8.737)	
=1 if in faster NPI speed category = 1					-3.251	
					(8.869)	
Constant	-61.57***	139.8	-74.22***	-38.78*	-57.82***	-31.58
	(2.777)	(186.7)	(6.163)	(21.13)	(8.549)	(32.53)
Observations	50	49	49	23	23	23
R-squared	0.987	0.133	0.909	0.065	0.017	0.410

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table B21: Cross Sectional Analysis for Total Tax

Dependent variable: total tax cumulative growth rate 1918-1925

VARIABLES	(1) Tax	(2) Geo	(3) Inequality	(4) NPI Value	(5) NPI Category	(6) All
UrbanPopShare		-0.0492				-1.441
		(0.759)				(1.077)
region = 2, NE		-36.36				-3.259
		(29.14)				(36.60)
region = 3, S		38.10*				-46.13
		(20.11)				(37.09)
region = 4, W		2.245				47.38
		(23.62)				(32.07)
State Population 1917		4.50e-07				
		(3.81e-06)				
Agriculture Employment Share 1910		40.12				
		(104.1)				
Manufacturing Employment Share 1914		218.6				
		(164.7)				
Personal return growth rate 1918-25	0.534***					0.464
	(0.101)					(0.402)
Corporate number growth rate 1918-25	0.176					0.471
	(0.149)					(0.665)
Bottom 90% fiscal income growth rate 1918-25			0.132			
			(0.340)			
Top 10% fiscal income growth rate 1918-25			0.993*			
			(0.519)			
Top 1% fiscal income growth rate 1918-25			-0.930			
			(0.806)			
Top 0.1% fiscal income growth rate 1918-25			0.468			
			(0.395)			
Per capita income growth rate 1920-30			-0.638			-1.037
			(0.506)			(1.398)
Mortality1918				-0.0959*		0.0162
				(0.0554)		(0.100)
DaysofNPI				-0.309		-0.650
				(0.198)		(0.392)
SpeedofNPI				-0.512		-0.343
				(1.133)		(1.292)
=1 if above-median mortality = 1					-6.920	
					(19.69)	
=1 if in longer NPI days category = 1					-31.18	
					(18.54)	
=1 if in faster NPI speed category = 1					-7.731	
					(18.82)	
Constant	-30.77***	-99.57	-33.26***	51.28	-5.355	84.05
	(7.595)	(80.15)	(6.303)	(44.78)	(18.14)	(76.72)
Observations	50	49	49	23	23	23
R-squared	0.499	0.156	0.498	0.182	0.137	0.432

Standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

5. Additional cross sectional regressions with CAGR

Here, I attach some additional cross sectional regressions using CAGR as the calculation method for the various dependent and independent variables rather than cumulative growth rates. CAGR is much less in absolute values than cumulative growth rates, but as one may see, they usually share the same signs and help us arrive at the same conclusions as we did with cumulative growth rates.

Table B22: Cross Sectional Analysis for Personal Tax

Dependent variable: personal tax CAGR 1918-1925

VARIABLES	(1) Tax	(2) Geo	(3) Inequality	(4) NPI Value	(5) NPI Category	(6) All
UrbanPopShare		-0.125				-0.0734
		(0.140)				(0.0918)
region = 2, NE		-8.182				-7.725**
		(5.390)				(3.544)
region = 3, S		12.08***				-9.446**
		(3.719)				(3.283)
region = 4, W		0.590				-2.136
		(4.369)				(2.871)
State Population 1917		9.31e-07				
		(7.04e-07)				
Agriculture Employment Share 1910		-19.86				
		(19.26)				
Manufacturing Employment Share 1914		60.01*				
		(30.47)				
Personal return CAGR 1918-25	2.744*					1.082***
	(1.502)					(0.273)
Personal wages CAGR 1918-25	0.192					
	(0.295)					
Personal exemption CAGR 1918-25	-2.050					
	(1.262)					
Personal income CAGR 1918-25	0.499***					
	(0.130)					
Bottom 90% fiscal income CAGR 1918-25			0.0519***			
			(0.0150)			
Top 10% fiscal income CAGR 1918-25			1.000***			
			(0.251)			
Top 1% fiscal income CAGR 1918-25			0.0394			
			(0.555)			
Top 0.1% fiscal income CAGR 1918-25			1.069***			
			(0.304)			
Per capita income CAGR 1910-30			6.994			
			(27.46)			
Per capita income CAGR 1920-30			37.92*			38.42
			(22.34)			(76.45)
Mortality1918				-0.00711		0.0195*
				(0.00810)		(0.00913)
DaysofNPI				-0.0532*		-0.0489
				(0.0290)		(0.0335)
SpeedofNPI				-0.0123		0.0365
				(0.166)		(0.121)
=1 if above-median mortality = 1					-0.656	
					(2.906)	
=1 if in longer NPI days category = 1					-3.135	
					(2.737)	
=1 if in faster NPI speed category = 1					1.021	
					(2.778)	
Constant	2.515	-18.13	-13.67***	0.721	-6.581**	-4.572
	(9.285)	(14.83)	(0.654)	(6.549)	(2.678)	(7.106)
Observations	50	49	37	23	23	23
R-squared	0.712	0.376	0.983	0.171	0.110	0.735

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table B23: Cross Sectional Analysis for Corporate Tax

Dependent variable: corporate tax CAGR 1918-1925

VARIABLES	(1) Tax	(2) Geo	(3) Inequality	(4) NPI Value	(5) NPI Category	(6) All
UrbanPopShare		0.0142				-0.313**
		(0.146)				(0.135)
region = 2, NE		-0.937				-2.567
		(5.593)				(4.755)
region = 3, S		4.666				-8.861*
		(3.859)				(4.573)
region = 4, W		-7.143				4.157
		(4.533)				(3.881)
State Population 1917		-3.18e-07				
		(7.31e-07)				
Agriculture Employment Share 1910		-42.71**				
		(19.99)				
Manufacturing Employment Share 1914		-65.28**				
		(31.62)				
Corporate number CAGR 1918-25	-0.0527					1.584*
	(0.121)					(0.767)
Corporate income CAGR 1918-25	1.025***					
	(0.0478)					
Bottom 90% fiscal income CAGR 1918-25			-0.139**			
			(0.0562)			
Top 10% fiscal income CAGR 1918-25			0.0551			
			(0.945)			
Top 1% fiscal income CAGR 1918-25			-0.896			
			(2.085)			
Top 0.1% fiscal income CAGR 1918-25			1.651			
			(1.142)			
Per capita income CAGR 1910-30			-70.14			
			(103.2)			
Per capita income CAGR 1920-30			20.17			-51.97
			(83.98)			(103.3)
Mortality1918				-0.00973		0.0120
				(0.00802)		(0.0112)
DaysofNPI				-0.0226		-0.0484
				(0.0287)		(0.0452)
SpeedofNPI				0.127		0.0797
				(0.164)		(0.165)
=1 if above-median mortality = 1					-1.199	
					(2.866)	
=1 if in longer NPI days category = 1					-0.818	
					(2.699)	
=1 if in faster NPI speed category = 1					-1.697	
					(2.740)	
Constant	-13.68***	19.56	-15.38***	-4.905	-11.78***	-3.291
	(0.595)	(15.38)	(2.459)	(6.483)	(2.641)	(9.861)
Observations	50	49	37	23	23	23
R-squared	0.938	0.238	0.724	0.108	0.050	0.475

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table B24: Cross Sectional Analysis for Total Tax

Dependent variable: total tax CAGR 1918-1925

VARIABLES	(1) Tax	(2) Geo	(3) Inequality	(4) NPI Value	(5) NPI Category	(6) All
UrbanPopShare		0.0659				-0.190
		(0.110)				(0.184)
region = 2, NE		-4.667				0.791
		(4.223)				(6.226)
region = 3, S		4.790				-6.614
		(2.914)				(6.140)
region = 4, W		0.552				8.877
		(3.423)				(5.233)
State Population 1917		1.33e-07				
		(5.52e-07)				
Agriculture Employment Share 1910		11.92				
		(15.09)				
Manufacturing Employment Share 1914		27.47				
		(23.88)				
Personal return CAGR 1918-25	0.562***					0.340
	(0.148)					(0.479)
Corporate number CAGR 1918-25	0.293					0.410
	(0.249)					(1.005)
Bottom 90% fiscal income CAGR 1918-25			-0.0347			
			(0.0536)			
Top 10% fiscal income CAGR 1918-25			2.389**			
			(0.900)			
Top 1% fiscal income CAGR 1918-25			-5.166**			
			(1.987)			
Top 0.1% fiscal income CAGR 1918-25			3.186***			
			(1.088)			
Per capita income CAGR 1910-30			57.84			
			(98.37)			
Per capita income CAGR 1920-30			-35.27			-99.28
			(80.03)			(135.2)
Mortality1918				-0.0143		-0.00318
				(0.00918)		(0.0170)
DaysofNPI				-0.0509		-0.115*
				(0.0328)		(0.0609)
SpeedofNPI				-0.00789		0.0417
				(0.188)		(0.217)
=1 if above-median mortality = 1					0.321	
					(3.182)	
=1 if in longer NPI days category = 1					-4.844	
					(2.996)	
=1 if in faster NPI speed category = 1					-2.671	
					(3.041)	
Constant	-6.098***	-20.58*	-6.100**	7.093	-2.165	15.38
	(1.455)	(11.62)	(2.343)	(7.417)	(2.931)	(13.12)
Observations	50	49	37	23	23	23
R-squared	0.337	0.156	0.494	0.151	0.148	0.397

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

C. Intuition Graphs for State-level Data

1. Findings summary

- I constructed state-level scores for NPI intensity and speed based on existing city-level scores. The relationship between NPI measures and tax revenues is not as obvious as expected. For instance, states with longer NPI days seem to have lower tax revenues throughout the years, not just during the flu period (1918-1919). In terms of growth rate, there are insignificant differences between states with more/less NPI intensity or speed.

- There may be noteworthy spikes and free-falls, but I have double-checked the accuracy of the data, so these dramatic swings are accurate representations of the fluctuation in tax revenues.

- States with higher urban populations generally have higher tax revenues and more dramatic tax revenue fluctuations than the less urban states.

- I do not see clear correlation between the states with higher mortality rate and higher/lower tax revenue growth during and after the 1918 flu. All in all, it seems that tax revenues have less to do with the mortality rate, but more with the urban/rural differences.

- Examining state-level fiscal income by income class breakdown (bottom 90%, top 10%, top 1%, top 0.1%, and top 0.01%) shows that the higher income classes seem to have their incomes fluctuate more dramatically during and after the 1918 flu. For instance, the top 1% had income fall more dramatically percentage-wise than the bottom 90% during the 1918 flu or the 1920-21 recession, but they also regained their wealth more quickly after the crises.

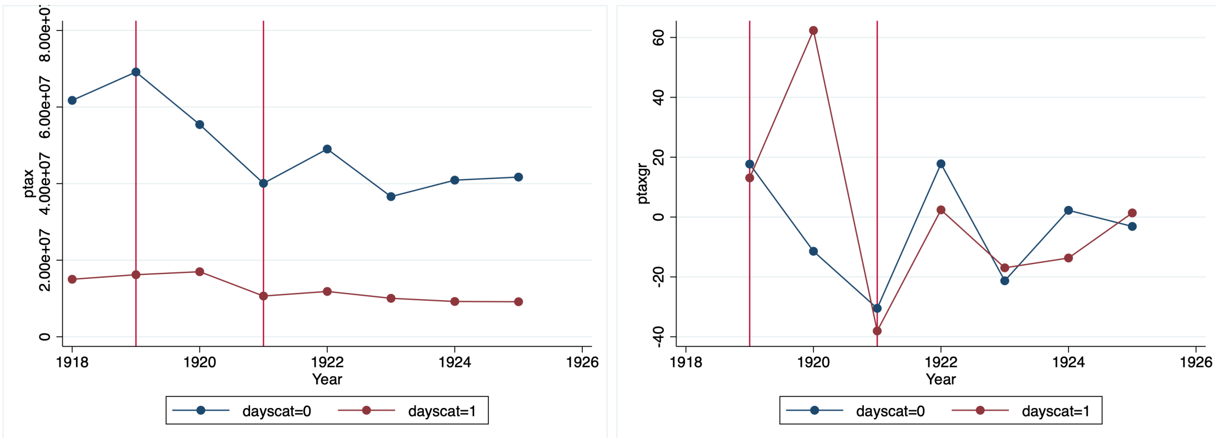
- Motivated by the different behaviors by income class, I ran preliminary yearly fixed effects regressions for income growth rates for the bottom 90% and top 1%. I find that for the bottom 90%, the "Post Flu" and most "i.Year" coefficients aren't statistically significant. On the other hand, for the top 1%, these coefficients are significant and positive. This might warrant further exploration.

The following subsections present graphs in which the absolute value and growth rate of various tax variables are grouped by contrasting categories.

2. By NPI intensity and speed

To clarify the data label: "dayscat==1" indicates more intense NPI; "speedcat==1" indicates faster adoption of NPI. Essentially, the states are divided based on more vs. less intense NPI and faster vs. slower adoption of NPI, and then their tax revenues and growth rates are plotted.

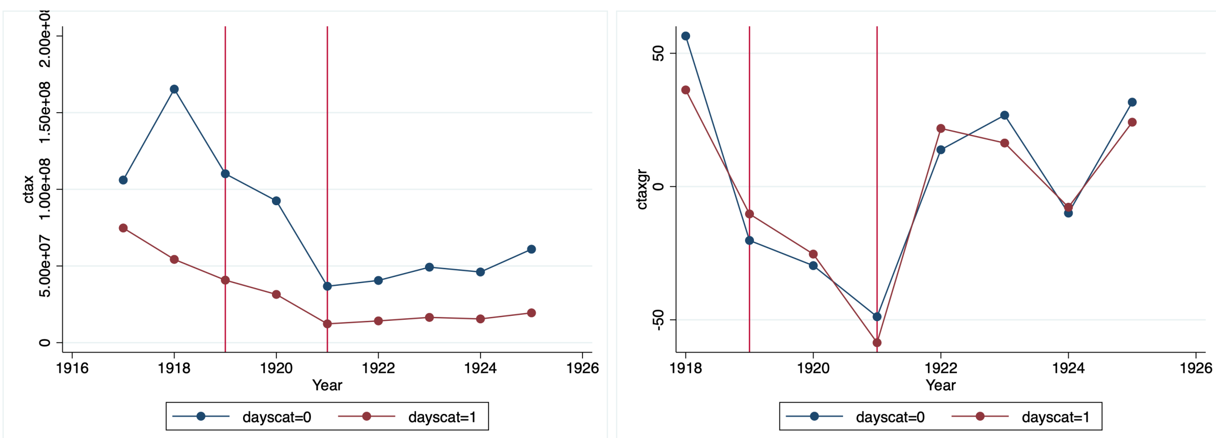
Figure C1: Personal tax revenue by NPI intensity



Left: absolute value; Right: growth rate

States with longer NPI days seem to have lower tax revenues throughout the years, not just during the flu period (1918-1919). States with longer NPI days did significantly better during the recession, though not too dramatically different during the flu.

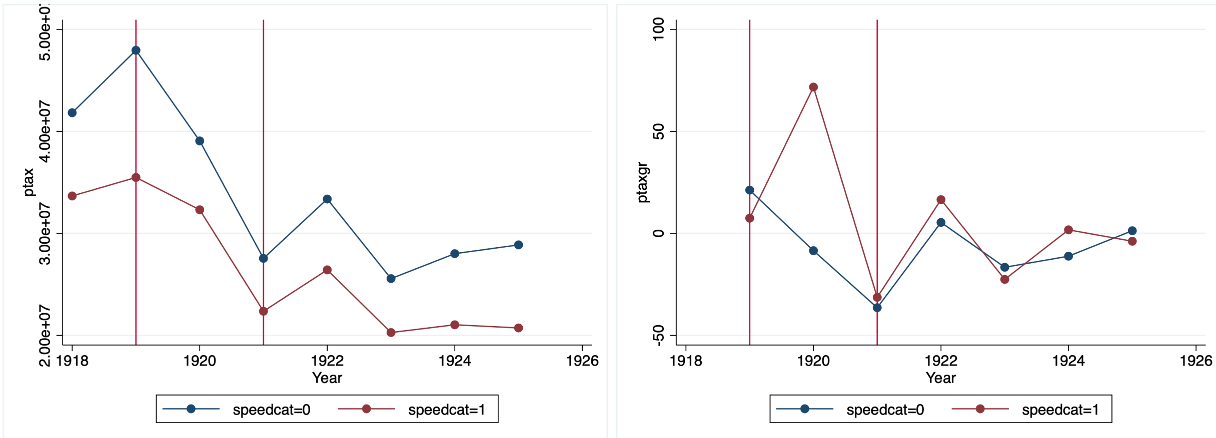
Figure C2: Corporate tax revenue by NPI intensity



Left: absolute value; Right: growth rate

States with longer NPI days seem to have lower tax revenues throughout the years, not just during the flu period. In terms of corporate tax growth rate, there are insignificant differences between states with more/less NPI intensity.

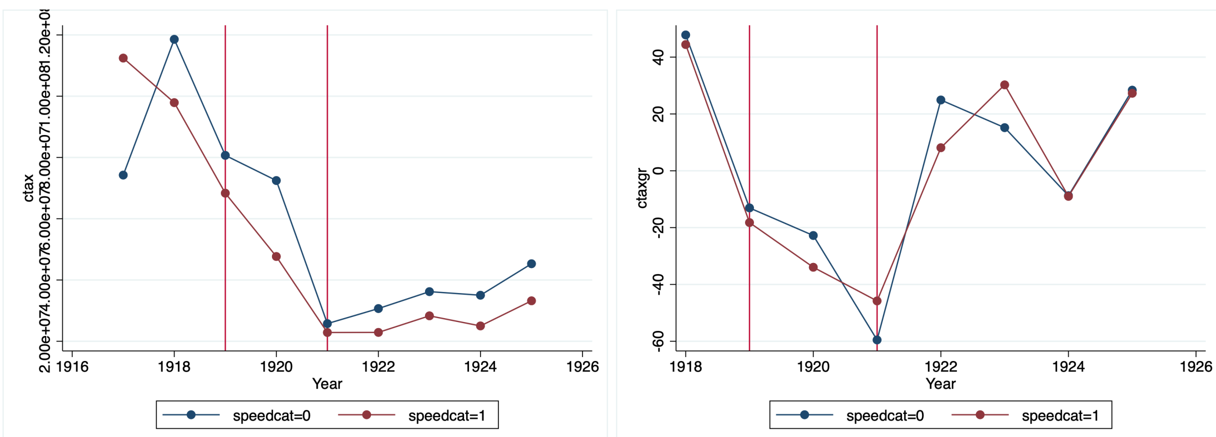
Figure C3: Personal tax revenue by NPI speed



Left: absolute value; Right: growth rate

States with slower NPI response speeds seem to have higher tax revenues throughout the years. In terms of growth rates, states with faster NPI response did significantly better during the recession. Overall, there is no dramatic differences during the Flu period.

Figure C4: Corporate tax revenue by NPI speed



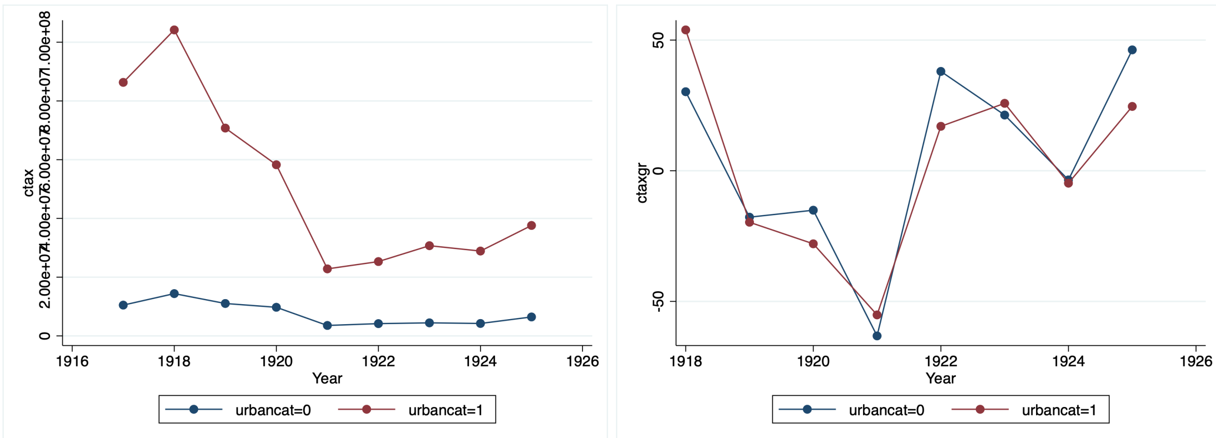
Left: absolute value; Right: growth rate

States with slower NPI response speeds seem to have higher tax revenues throughout the years. There is overall insignificant differences in terms of corporate tax growth rate between states with fast/slow NPI speed.

3. By urban density

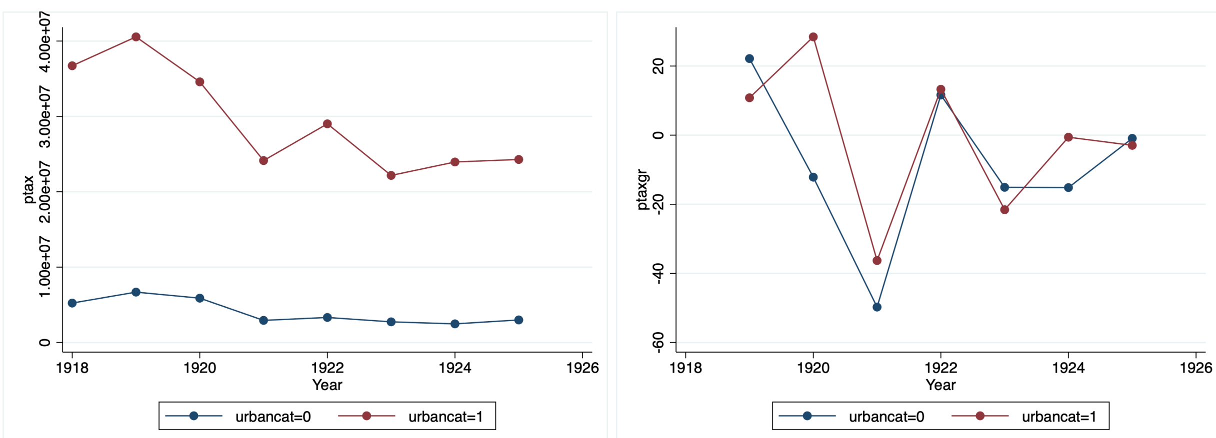
I create three categories of urban density: = 2 if the share of population in major cities makes up for more than 50% of the entire state population (e.g. NY, NJ, RI) – I call these "superurban" states; = 1 if the share of urban population in general makes up for more than 35% of the entire state population (e.g. PA, OR, UT); and = 0 otherwise, which are mainly predominantly rural states left (e.g. AL, WV).

Figure C5: Corporate tax revenue by urban density



From the absolute values graph, we see that superurban states had dramatically more severe decline in tax rev after 1918. The graph on the right shows that log growth rate for more urban states are consistently higher than that of rural areas. This is in line with my intuition because it's likely that urban regions had more dramatic increases and declines of tax revenues before and after the flu.

Figure C6: Income tax revenue by urban density



Similar observations for personal income tax: superurban states had more severe decline in tax rev after 1918; higher degrees of fluctuations; the log growth rates for more urban states are consistently higher.

Figure C7: Tax return number

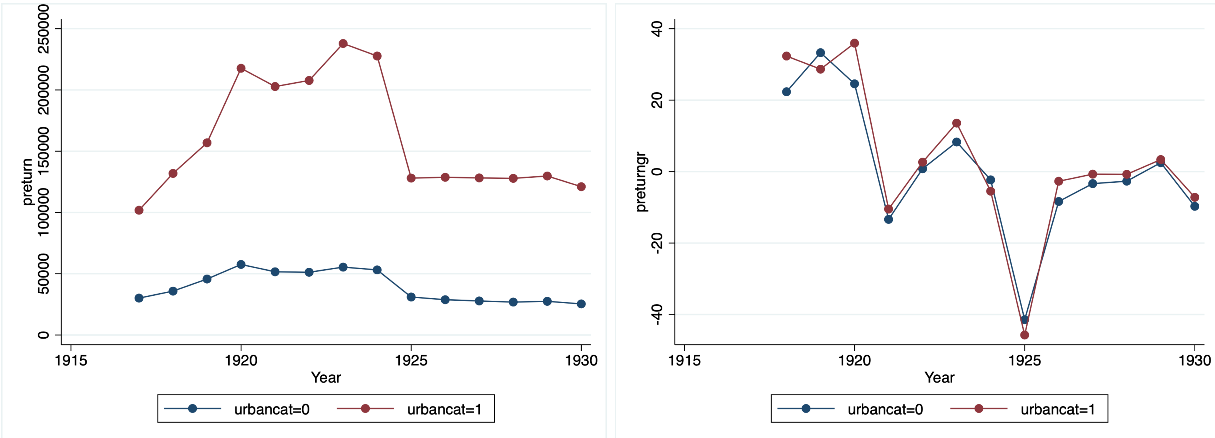
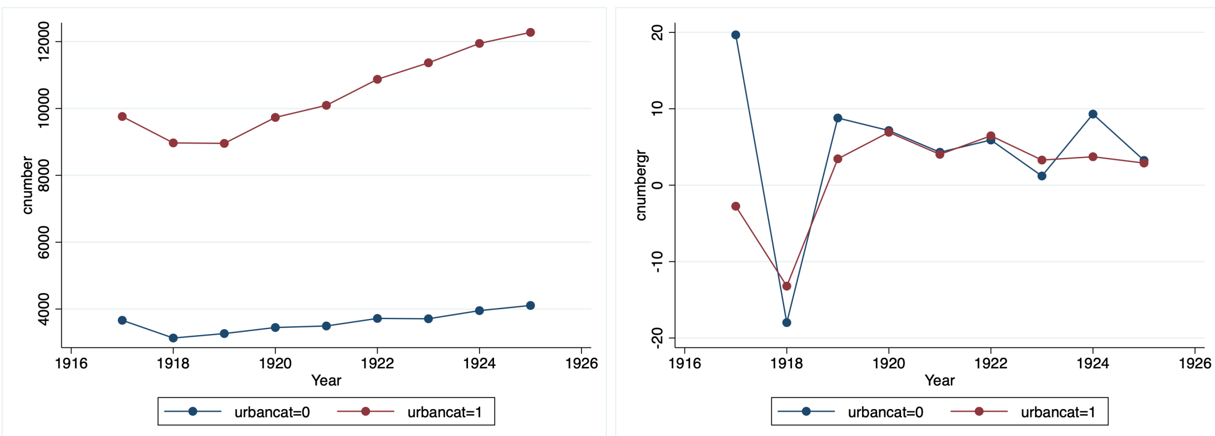


Figure C8: Corporation number



4. By mortality rate

Figure C9: Mortality rate in 1918

```
. summ Mortality1918, d

                        Mortality1918
-------------------------------------------------------------
      Percentiles      Smallest
 1%        334.5          334.5
 5%        389.3          334.5
10%       398.05          334.5       Obs                 540
25%          476          334.5       Sum of Wgt.         540

50%       537.55                      Mean           577.8267
                        Largest       Std. Dev.      145.1966
75%        726.7          883.1
90%       768.55          883.1       Variance       21082.06
95%        803.6          883.1       Skewness        .319764
99%        883.1          883.1       Kurtosis       2.020952
```

By doing a quick tabulation of each states' mortality rate in 1918, we can see a rough division and can create different "mortality categories" accordingly. First, I create four levels of mortality rate by every 25% increase (i.e. =1 if less than 476 per 10,000, =2 if 476-538, =3 if 538-727, and =4 if greater than 727).

Figure C10: Personal income tax by mortality categories

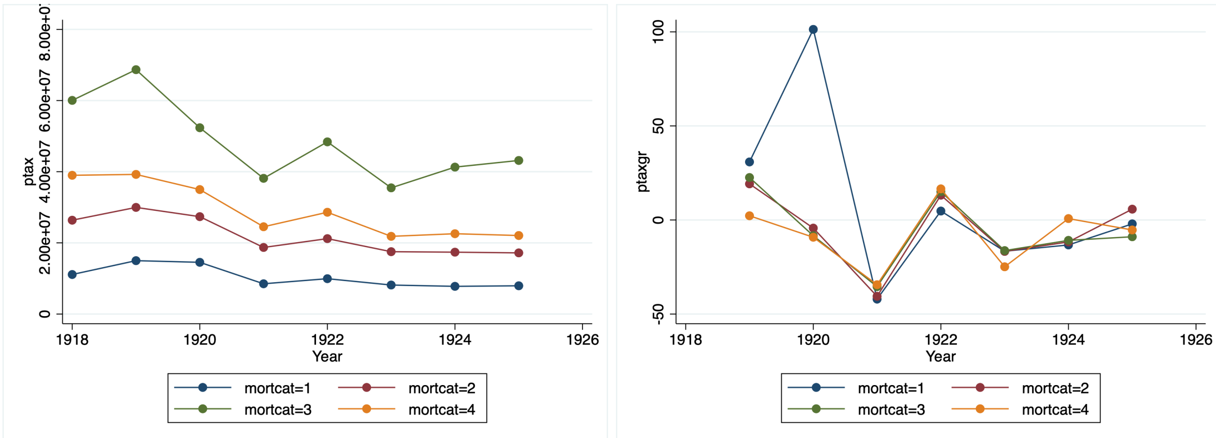
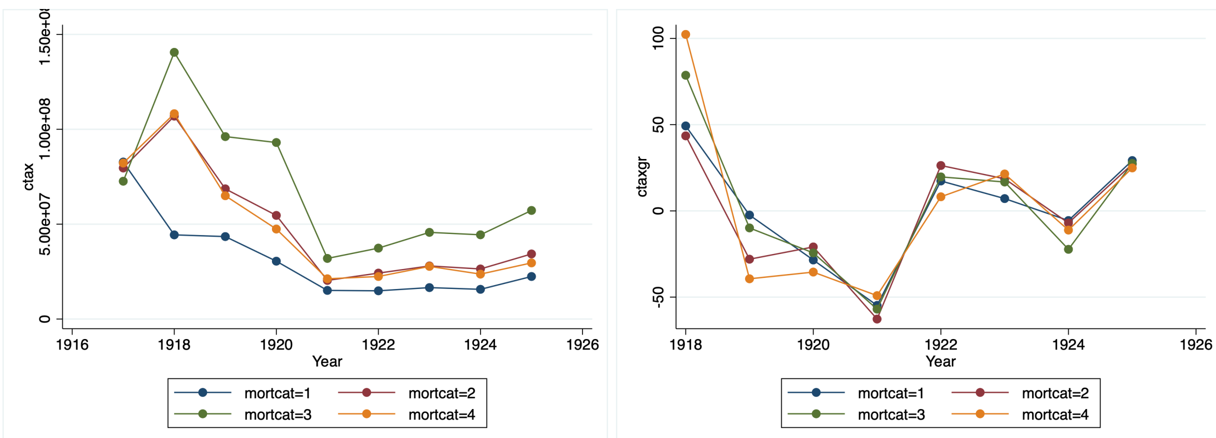


Figure C11: Corporate tax by mortality categories

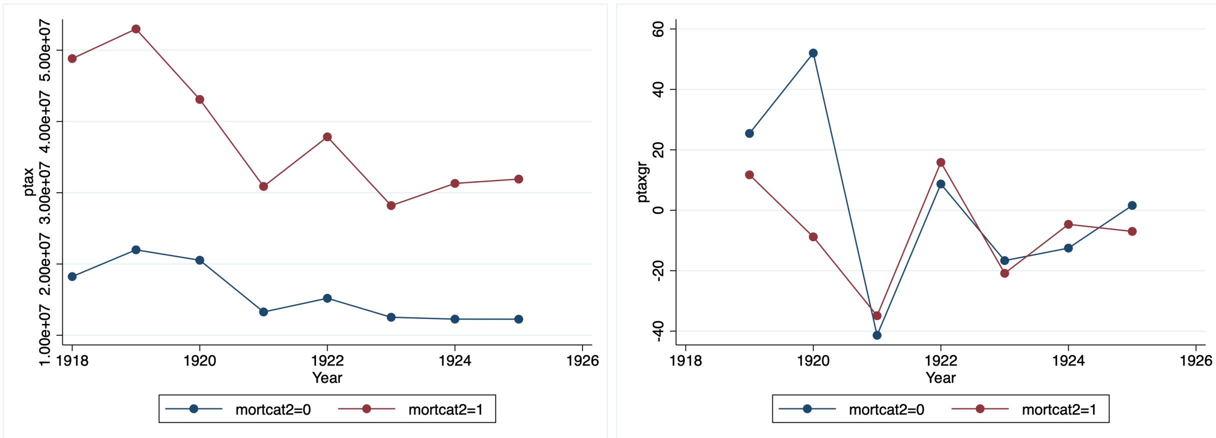


These graphs do not show a linear progression of lower tax revenues when mortality rate goes up – category 3 states have higher revenues than category 4 states, for example. Overall, the revenues and growth rates are quite close amongst different categories.

What is not shown in the above graphs are states that didn't have mortality rate data back in 1918. CDC refer them as the "non-registration states" which are usually rural and less developed areas, such as Alabama, Idaho, Mississippi, etc.. They had significantly lower tax revenues compared to "registration states" with mortality rate data, but unfortunately, I would not be able to run analysis on them based on NPI scores or mortality rate. Their tax revenue trends would be better captured by the urban/rural population categorization or by geographical division.

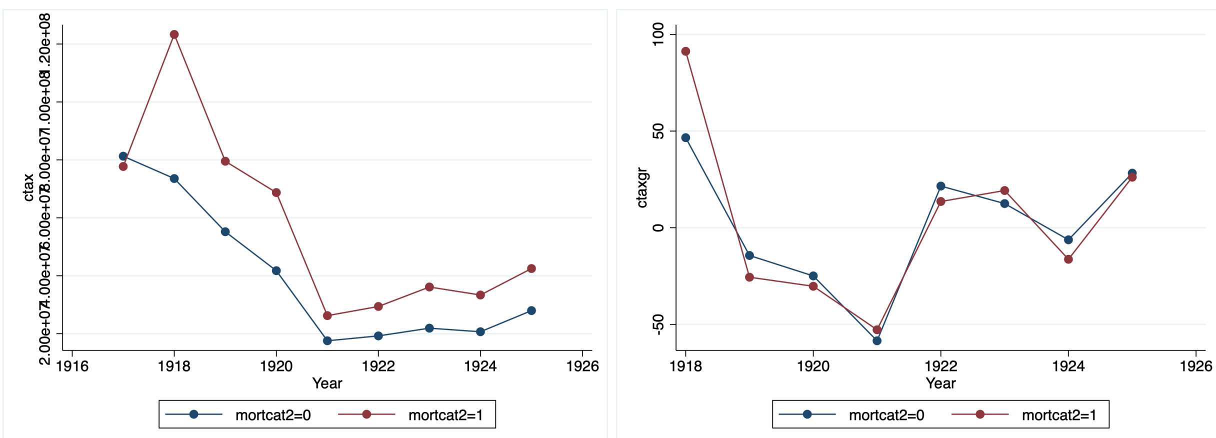
Another way of categorizing states by mortality level is by above/below the median mortality rate 538 per 10,000, and we may arrive at the following set of graphs:

Figure C12: Personal income tax by mortality categories



Above and below median mortality rate

Figure C13: Corporate tax by mortality categories



Again, there is no dramatic difference between states that have high/low mortality rates. States with above-median mortality rate had higher corporate and personal income taxes.

However, non-registration states had significantly lower tax revenues than registration states, and because the non-registration states were the less developed areas, it seems that tax revenues had more to do with the urban/rural differences than mortality rate differences.

5. Event study

The graphs below are produced following Stata codes for the events study in Serrato and Zidar (2018).

Figure C14: Corporate tax event study

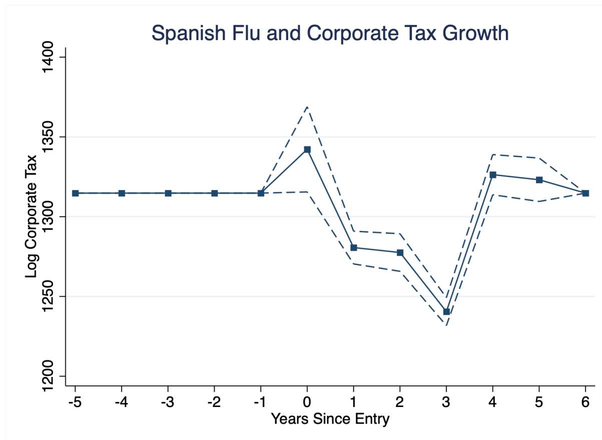
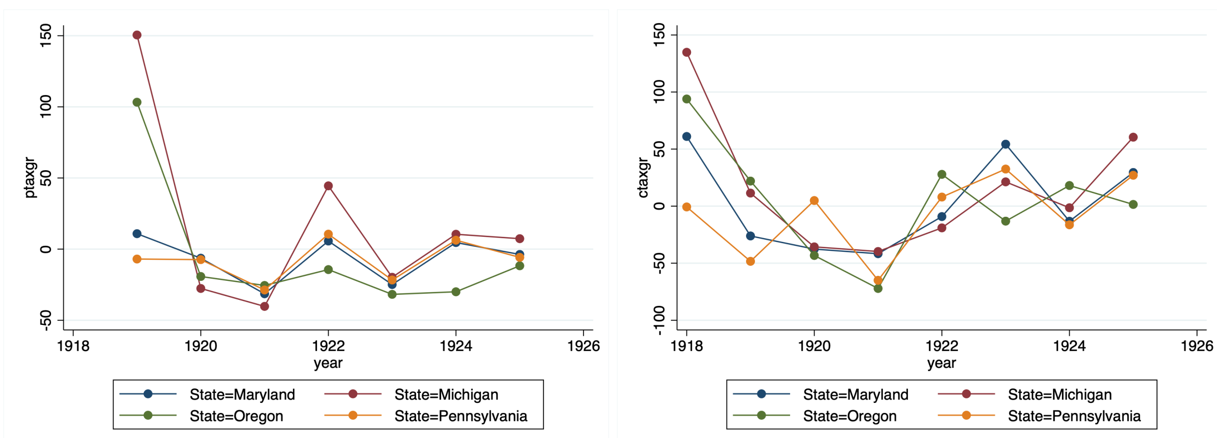


Figure C15: Personal and corporate tax in 4 states - by mortality



Here, I select four states – two states with the highest mortality: Maryland and Pennsylvania; and two with the lowest mortality: Oregon and Michigan. Again, we do not see clear correlation between the states with higher mortality rate and higher/lower tax revenue growth during and after the 1918 flu. All in all, it seems that tax revenues have less to do with the mortality rate, but more with the urban/rural differences.

Figure C16: Personal and corporate tax in 4 states - by NPI intensity

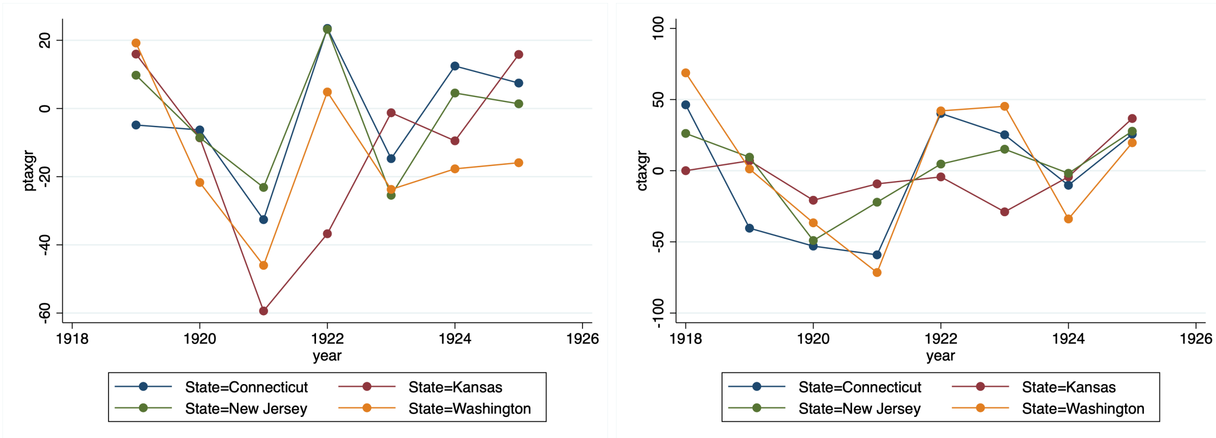
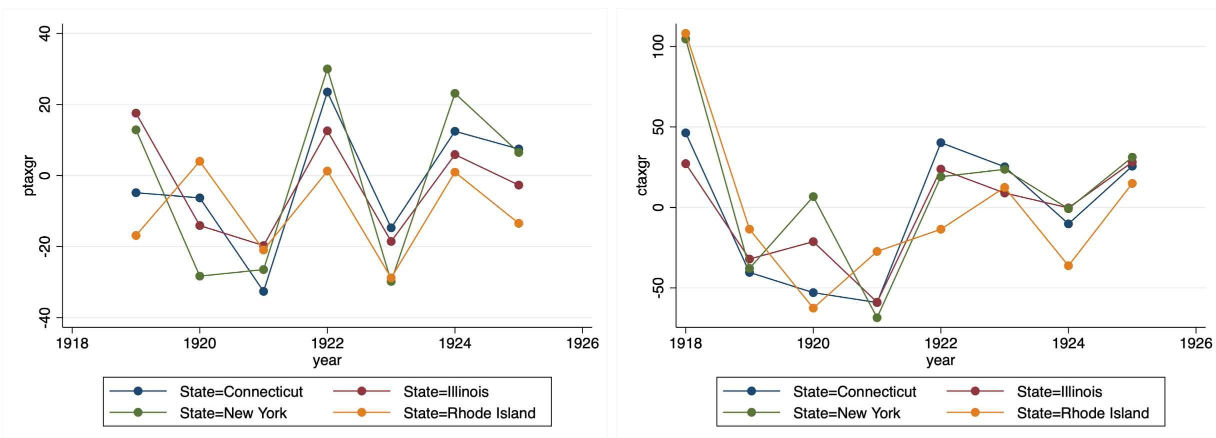


Figure C17: Personal and corporate tax in 4 states - by NPI speed

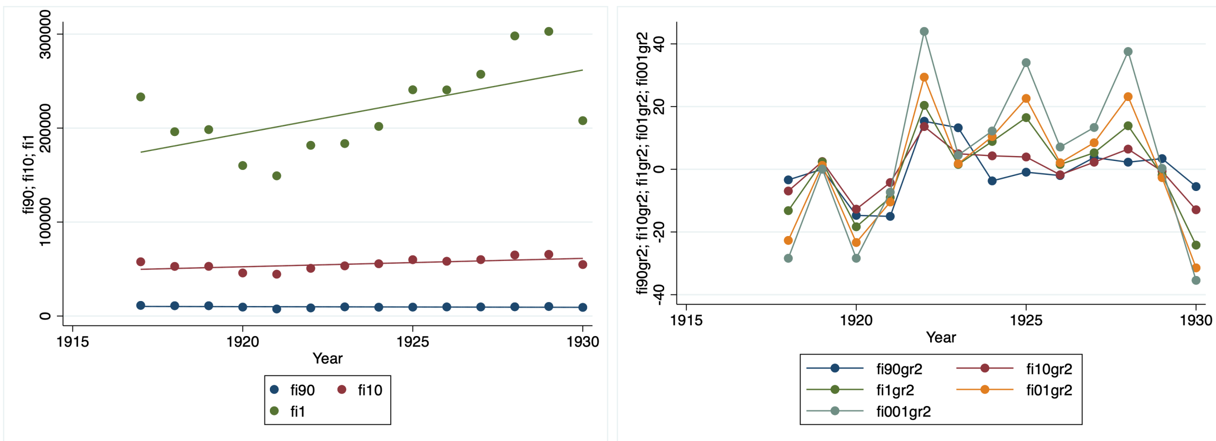


I then select two states with the most stringent NPI measures (Kansas and Washington) and two with the least stringent (New Jersey and Connecticut). And, I select two states with the fastest NPI adoption speed (Connecticut and Minnesota) and two with the slowest (New York and Illinois). Again, we do not see clear correlation between NPI measures and higher/lower tax revenue growth during and after the 1918 flu.

6. Fiscal income and pre-tax income

Here, I plot out state-level fiscal income by various income classes (bottom 90%, top 10%, top 1%, top 0.1%, and top 0.01%).

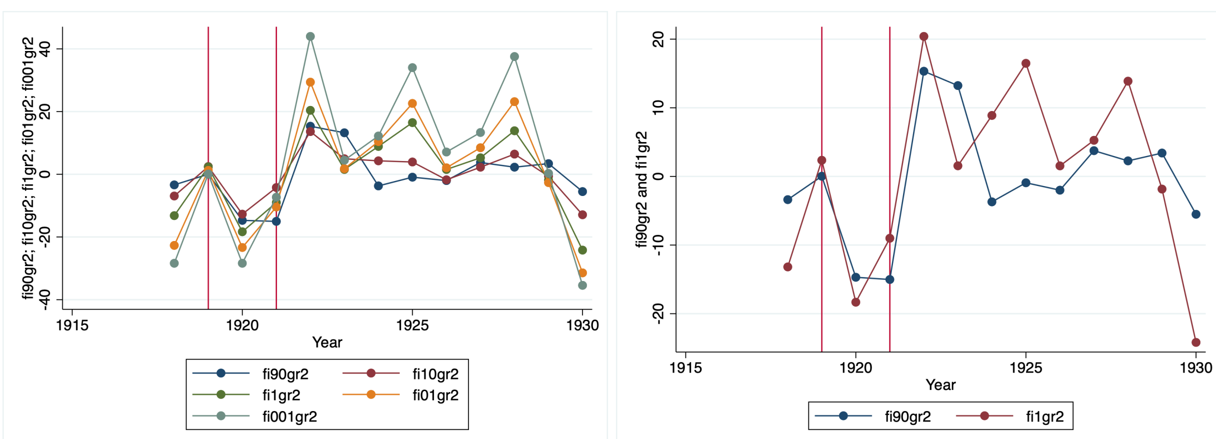
Figure C18: Fiscal income across all income classes



The left graph shows the absolute value of fiscal income for the bottom 90%, top 10%, and top 1%. By stacking all three levels, we can see that the rich's assets both declined and grew more dramatically compared to lower levels during and after the 1918-1921 period. The right graph shows that log rates go up by income class, and they stay consistent over the years. This implies that the rich both suffered and benefited more dramatically than the poor.

To capture the variation and how much variation happens over time, instead of just general trends, I then graph the growth rates of the fiscal income series over time:

Figure C19: Fiscal income growth rate by income class



Left: all income class; right: bottom 90% and top 1%

We can see that the richer income classes saw more dramatic declines in income than the bottom 90%, but their income also grew more dramatically after the flu and the 20-21 recession.

Motivated by this rough finding, I run a few fixed effects regressions for the growth rates of the fiscal income of bottom 90% and top 1%:

Table C1: Time fixed effects regression fiscal income for bottom 90%

VARIABLES	(1) fi90gr2	(2) fi90gr2	(3) fi90gr2
Year = 1919	3.408		5.502
	(4.512)		(4.542)
Year = 1920	-11.32**		-9.223**
	(4.512)		(4.542)
Year = 1921	-11.59**		-9.502**
	(4.542)		(4.557)
Year = 1922	18.75***		20.84***
	(4.542)		(4.557)
Year = 1923	16.66***		18.75***
	(4.542)		(4.557)
Year = 1924	-0.283		1.811
	(4.542)		(4.557)
Year = 1925	2.500		4.593
	(4.542)		(4.557)
Year = 1926	1.421		3.514
	(4.542)		(4.557)
Year = 1927	7.176		9.269**
	(4.542)		(4.557)
Year = 1928	5.688		7.781*
	(4.542)		(4.557)
Year = 1929	6.831		8.925*
	(4.542)		(4.557)
Year = 1930	-2.093		
	(4.542)		
Post Flu		2.995	-2.093
		(3.542)	(4.542)
Year = 1930, omitted			-
Constant	-3.414	-3.343	-3.414
	(3.197)	(3.400)	(3.197)
Observations	653	653	653
R-squared	0.133	0.001	0.133
Number of State	51	51	51

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

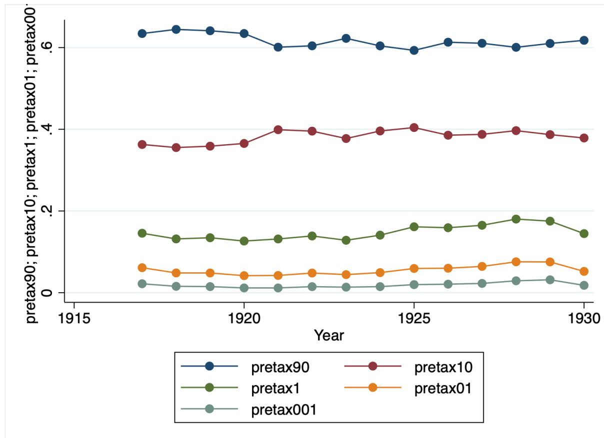
Table C2: Time fixed effects regression fiscal income for top 1%

VARIABLES	(1) fi1gr2	(2) fi1gr2	(3) fi1gr2
Year = 1919	15.54***		26.86***
	(2.234)		(2.249)
Year = 1920	-5.139**		6.181***
	(2.234)		(2.249)
Year = 1921	3.859*		15.18***
	(2.249)		(2.256)
Year = 1922	33.27***		44.59***
	(2.249)		(2.256)
Year = 1923	14.41***		25.73***
	(2.249)		(2.256)
Year = 1924	21.75***		33.07***
	(2.249)		(2.256)
Year = 1925	29.35***		40.67***
	(2.249)		(2.256)
Year = 1926	14.40***		25.72***
	(2.249)		(2.256)
Year = 1927	18.13***		29.45***
	(2.249)		(2.256)
Year = 1928	26.75***		38.07***
	(2.249)		(2.256)
Year = 1929	11.02***		22.34***
	(2.249)		(2.256)
Year = 1930	-11.32***		
	(2.249)		
Post Flu		14.21***	-11.32***
		(2.485)	(2.249)
Year = 1930, omitted			-
Constant	-12.94***	-12.85***	-12.94***
	(1.583)	(2.385)	(1.583)
Observations	653	653	653
R-squared	0.590	0.052	0.590
Number of State	51	51	51

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

With year FE regressions, we essentially compare how things change over time. If coefficients for most of the years variables aren't statistically significant, then it implies that years didn't have effects on the income. For the bottom 90%, we see that most coefficients aren't significant, whether we control for "i.Year" or "Post Flu." On the other hand, for the top 1%, most coefficients are significant and positive. Interestingly, if we do not control for "i.Year," the "Post Flu" coefficient is significant and positive (hinting that the rich became richer after the flu); but after controlling for "i.Year," the coefficient is significant and negative.

Figure C20: Pre-tax income by income class



Like fiscal income, we have bottom 90%, top 10%, top 1%, top 0.1%, and top 0.01%

World Inequality Database (where the data for this graph were downloaded from) provided pre-tax income distribution by percentage, where the bottom 90% and top 10% would together make up 100% of the pre-tax income in a given state. The percentage allows us to directly observe the share of total income being taken by each income class, and there seems to be little fluctuation during the 1918 pandemic years. The 1920-21 economic recession caused the top 10% to concede a slight amount of income share to the bottom 90%, but the overall dynamic did not see any dramatic shift.