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Conditional Persistence? Historical Disease Exposure and Government Response to COVID-19

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Conditional Persistence? Historical Disease Exposure and Government Response to COVID-19*

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Abstract

In this paper, we investigate differences in government responses to the COVID-19 pandemic. Drawing on the theory of the Behavioral Immune System and the Parasite Stress Theory, we hypothesize that a higher historical disease exposure leads to a stricter government response to the pandemic, in particular during the first year which was characterized by fundamental uncertainty. Our empirical analysis, using weekly panel data for almost every country in the world, show that a higher historical disease exposure is indeed related to a stronger response to disease dynamics, at least in the first year of the pandemic. The pattern is the same for state-level containment policies within the United States. Our results suggest that the persistence of historical legacies may not be deterministic, but rather time-varying and conditional on circumstances. Cultural norms may matter more in times of crisis and fundamental uncertainty.

Keywords: COVID-19, cultural persistence, pathogen prevalence, containment policy, behavioral immune system

JEL Codes: I18, H12, Z18

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1 Introduction

Why did some countries, like China, pursue draconian lockdowns during the COVID-19 pandemic while others, like Sweden, had much less strict measures? Our paper explores the importance of historical disease exposure for explaining the variation over time across countries in the strictness of government containment policies during the recent COVID-19 pandemic of 2020-22. We propose a conceptual framework in which a comparatively high historical prevalence of infectious disease activated a *behavioral immune system* that resulted in informal behavioral norms characterized by pathogen avoidance and a strong in-group orientation. We demonstrate empirically that the explanatory power of such deep behavioral norms from the past for predicting government policy is strongest in times of *fundamental (Knightian) uncertainty* such as during the early, pre-vaccine stage of the COVID-19 pandemic in 2020, whereas their importance fades when uncertainty is reduced (in the post-vaccine stage of 2021-22). Apart from our analysis of government responses to COVID-19, we contribute to the "persistence literature" in economics by showing that the influence of historically determined behavioral norms on contemporary outcomes, might in fact often be *conditional* on context- and time-specific circumstances, rather than being deterministic.

A large literature in economics has shown that cultural norms and formal institutions play an important role for understanding cross-country differences in economic growth and prosperity (North, 1991; Acemoglu et al., 2005; Nunn, 2012). A typical research design in this "persistence literature" postulates that some historical exogenous condition X has given rise to a cultural or institutional configuration Z, which in turn has caused differences in a contemporary outcome variable Y, for instance in GDP per capita. One famous public health-related example of such a theory is Acemoglu et al. (2001), hypothesizing that exogenous variation in mortality in malaria and yellow fever among colonial settlers (X) led to the adoption of disease environment-specific colonial policies and institutions (where Z is either extractive or inclusive institutions), which in turn had a long-term and persistent effect on GDP per capita. Focusing on former French colonies in Africa, Lowes and Montero (2021) demonstrate that coercive colonial campaigns, aimed at preventing disease like sleeping sickness, led to a persistent culture of mistrust against modern medicine. Another well-known study in this tradition suggests that an historically high exposure to infectious disease led to in-group oriented, collectivist cultural norms that facilitated collective action but which retarded modern innovation (Gorodnichenko and Roland, 2017). Since extractive institutions and collectivist cultural norms have been widely accepted as being harmful for modern technology-driven economic growth, another branch of the field has focused on the reduced-form impact of historical conditions X (such as climatic variation and an early transition to agriculture) with cultural norms Z as an outcome variable (Talhelm et al., 2014; Olsson and Paik, 2016; Giuliano and Nunn, 2021; Ho et al., 2022).

However, this persistence literature has recently been criticized for being overly deterministic and for overstating the importance of historical conditions. For instance, Maseland (2021) shows that the impact of the various proposed historical condition X do not always have a consistently significant effect in line with the underlying hypothesis when the outcome variable is measured over a range of more recent years. Regardless of the validity of the specific empirical criticisms of the persistence literature, there is a looming conceptual question whether it is reasonable to think of historically determined cultures or institutions as having a consistently negative or positive impact on contemporary desirable outcomes or whether the impact of the historical legacy might in fact often be context-specific and time-varying? For instance, a collectivist culture might be beneficial for achieving a high popular acceptance of mobilization during times of war or of restrictions during pandemics, but might also constrain international exchange and hinder the pursuit of drastic innovations that lead to "creative destruction"[1]

In this paper, we develop a conceptual framework where we argue that historical conditions and their associated deep behavioral norms should in general have a stronger impact on

[1]See for instance Gorodnichenko and Roland (2017) for a discussion about the tradeoff between individualism and collectivism when it comes to innovation and public goods-provision which were central for development at different points in time.

contemporary government policy in times of fundamental (or Knightian) uncertainty such as during wars and pandemics, whereas there should be a weaker link to individual behavior and public policy during "normal" times. We further hypothesize that historical legacies will play a greater role during the early phase of major crises when useful knowledge from recent experiences is almost non-existent and uncertainty therefore is so high that the probability distribution of potential outcomes is more or less unknown (Knight, 1921).

We test this hypothesis using the recent COVID-19 pandemic as a natural experiment. We propose that exogenous variation in the historical prevalence of infectious disease should have influenced historical behavior and present policies for disease containment. We ground this hypothesis in the recently developed theory of the *Behavioral Immune System* (BIS) suggesting that a historical high prevalence of pathogens should give rise to pathogen avoiding behavioral rules, including avoidance of strangers and in-group orientation (Murray and Schaller, 2016; Ackerman et al., 2021). All else equal, we propose that countries with a higher level of historical pathogen prevalence (HPP) should adopt stricter government policy (i.e. in terms of lockdowns, school closures, etc) during the 2020-22 COVID-19 pandemic.

The key test of our theory exploits the fact that the first year of the pandemic 2020 was characterized by very high degree of Knightian uncertainty about the transmission and mortality of COVID-19 and the effectiveness of non-pharmaceutical interventions (NPIs) (Ferguson et al., 2020), whereas the rapid development of COVID vaccines during the latter half of 2020 should have reduced Knightian uncertainty during 2021-22.

In our empirical analysis, we create weekly measures of COVID-19 policy strictness and reported deaths in COVID-19 for all countries in the world and for US states during 2020-22 from the OxCGRT database (Hale et al., 2021). After controlling for objective informational input to the government, i.e. the reported mortality per capita in the disease within the country as well in neighboring countries, we study whether (static) measures of *historical pathogen prevalence* (HPP) from Murray and Schaller (2010) have a time-varying impact on policy strictness along the lines hypothesized above. Our panel results indeed show that a higher HPP gave rise to a greater sensitivity to reported deaths during 2020 but that this effect disappeared during 2021-22 among the 182 countries in our sample. A very similar pattern arises when we instead use US states as our sample. The result is robust to using other outcomes variables and proxies for historical disease exposure.

We interpret these results as showing that historical legacies might have time-varying effects on contemporary outcomes and that historical behavioral norms matter in a more profound sense in times of fundamental uncertainty. We argue that this insight of the possibility of *conditional persistence* is novel to the literature and believe that our results might inform a more nuanced future analysis of how historical cultures and institutions affect contemporary policy and economic prosperity.

Our paper is related to several strands of literature. Most obviously, our research is related to the emerging literature on the political economy of government COVID-19 policies. Macro economists have, for instance, contributed to our understanding of the interaction between infection rates, government containment policies, and economic decisions by households (Eichenbaum et al., 2021), something which epidemiological models like Ferguson et al. (2020) often ignore. Other economists have studied the effectiveness of containment policies such as mask requirements for reducing growth rates of confirmed COVID-19 cases and deaths (Chernozhukov et al., 2021), the impact of school closures on future earnings and welfare for children (Fuchs-Schündeln et al., 2022), the effect of COVID-19 on religiosity (Bentzen, 2021), and the inherent tradeoff between short-run restrictive public health measures during pandemics and the long-run impact of such measures for liberal institutions and economic freedom (Geloso et al., 2022; Furton, 2023; Koyama, 2023).

In our paper, we focus on the relationship between historical disease exposure and government containment policies during COVID-19. There are at least three pathways through which historical disease could affect contemporary COVID policies. First, the causal impact might emerge indirectly through its effect on a country's set of cultural norms. Prominent research agendas in psychology argue that there is a strong impact of historical disease environments on the BIS, as discussed further below (Murray and Schaller, 2016). According to the *Parasite Stress Theory* of Thornhill and Fincher (2014), regions with higher historical levels of disease should be more prone to develop collectivist cultural norms, characterized by in-group orientation, aversion to strangers, and a low sense of individual agency. Lowes and Montero (2021) document empirically a specific example of such a mechanism, showing how coercive colonial medical campaigns to fight the spread of sleeping sickness in French Africa gave rise to a culture of persistent mistrust to modern medicine among affected groups. Economists like Enke (2019) have shown that pathogen threat was associated with a stronger kinship tightness among ethnic groups prior to industrialization, whereas Gorodnichenko and Roland (2017) exploit the empirical relationship between historical disease exposure and culture to establish an external source of variation in individualism-collectivism across the world.

Furthermore, a large number of recent papers in social science have shown that there is negative relationship between the strength of individualistic cultural norms and the strictness and/or the speed of implementation of COVID-19 containment policies (Chen et al., 2021; Jiang et al., 2022; Kitayama et al., 2022; Ashraf et al., 2022). Bazzi et al. (2021) and Bian et al. (2022) both study the importance of a historical frontier culture of "rugged individualism" within United States for understanding public and private responses to the COVID-19 pandemic. Using different outcome variables, both studies show that social distancing is less prevalent in more individualistic counties. Bazzi et al. (2021) further find that local governments in counties with a highly individualistic culture tend to enact less of restrictive containment policies.

A second pathway from historical disease exposure to COVID-19 policies runs through formal institutions such as democracy. Thornhill and Fincher (2014), Gorodnichenko and Roland (2017) and Gorodnichenko and Roland (2021) all show strong empirical associations between a high prevalence of infectious disease and autocratic forms of government. In a cross-country analysis of the first wave of the pandemic (up to summer 2020), Saam et al.

(2022) find that there are differences in the reactions of 40 democratic and 40 autocratic governments to COVID-19. Using the same early time window, Sebhatu et al. (2020) show that democracies exerted a slower response to the pandemic and that many countries tended to follow the policies of their neighbors. Toshkov et al. (2022) present evidence that more centralized countries with separate ministers of health, reacted faster to the pandemic.

A third pathway focuses on a more direct link between historical pathogen prevalence and COVID-19 policy responses. Using the HPP data on historical disease from Murray and Schaller (2010), Lu et al. (2021) show that there is a negative correlation between historical pathogen prevalence and the speed with which government implemented mobility restrictions up to August, 2020 among around 150 countries.

While recognizing that cultural norms and formal institutions both are strongly associated with government choices of COVID-19 containment policies, we argue that these are only proximate causes of policy variation and that a more fundamental explanation for policy variation is countries' legacies of historical exposure to infectious disease. In this sense, our paper is also strongly related to the literature on long-run persistence of pathogen loads and health shocks such as Acemoglu et al. (2001); Voigtländer and Voth (2013); Gorodnichenko and Roland (2017) and Jedwab et al. (2022). Unlike these papers, however, we propose that the impact of health shocks might in fact often be conditional on the prevailing circumstances such that governments and individuals will be more prone to turn to historical legacies during crises years with fundamental uncertainty.

In summary, our paper is distinct from and contributes to the existing literature in the following ways: First, our paper is the first to propose a conceptual framework and analyze the empirical relationship between historical disease exposure and the strictness of government COVID-19 containment policies during the whole pandemic 2020-22. Second, our paper is the first to recognize the key difference in terms of the level of Knightian uncertainty between the first versus the second and third years of the COVID-19 pandemic. Third, the paper contributes to the literature on long-run persistence by showing that persistence should be regarded as conditional on whether the period that is studied is characterized by normal conditions or exceptional crisis.

The paper is structured as follows. In chapter 2, we provide a background on the COVID-19 pandemic, whereas section 3 outlines the conceptual framework. Section 4 discusses the data. Section 5 presents the empirical results and section 6 includes a discussion of these findings. Lastly, section 7 lists the conclusions of our study.

2 Background

The novel coronavirus SARS-CoV-2 was first identified in Wuhan, China in December 2019 and its genetic sequence was published already on January 11, 2020. Transmission of the virus is primarily airborne from person to person and the well-known symptoms of its associated infectious disease COVID-19 include fever, coughing and fatigue. In case of a lethal outcome, death usually occurs 3-4 weeks after infection. In terms of government reaction, the pandemic changed character over time. After an arguably slow initial response, China imposed a strict lockdown in the province of Wuhan on January 23. One week later, on January 30, the WHO declared the new coronavirus a public health emergency of international concern (PHEIC). In general the policy response in other countries was limited before the global scale of the pandemic became apparent. In February and March COVID-19 cases and deaths were confirmed in an increasing number of countries around the world. Europe and in particular northern Italy appear to have been particularly hard hit in this early phase. Amidst an overwhelmed healthcare system and an increasing number of deaths, Italy imposed restrictions on internal movement and activities deemed incompatible with social distancing, in various stages starting in late February. The WHO declared the virus, now named COVID-19, a pandemic on March 11. By April, most governments in the world had put in place very strict COVID-19 containment policies including lock-downs, school closures and travel restrictions.

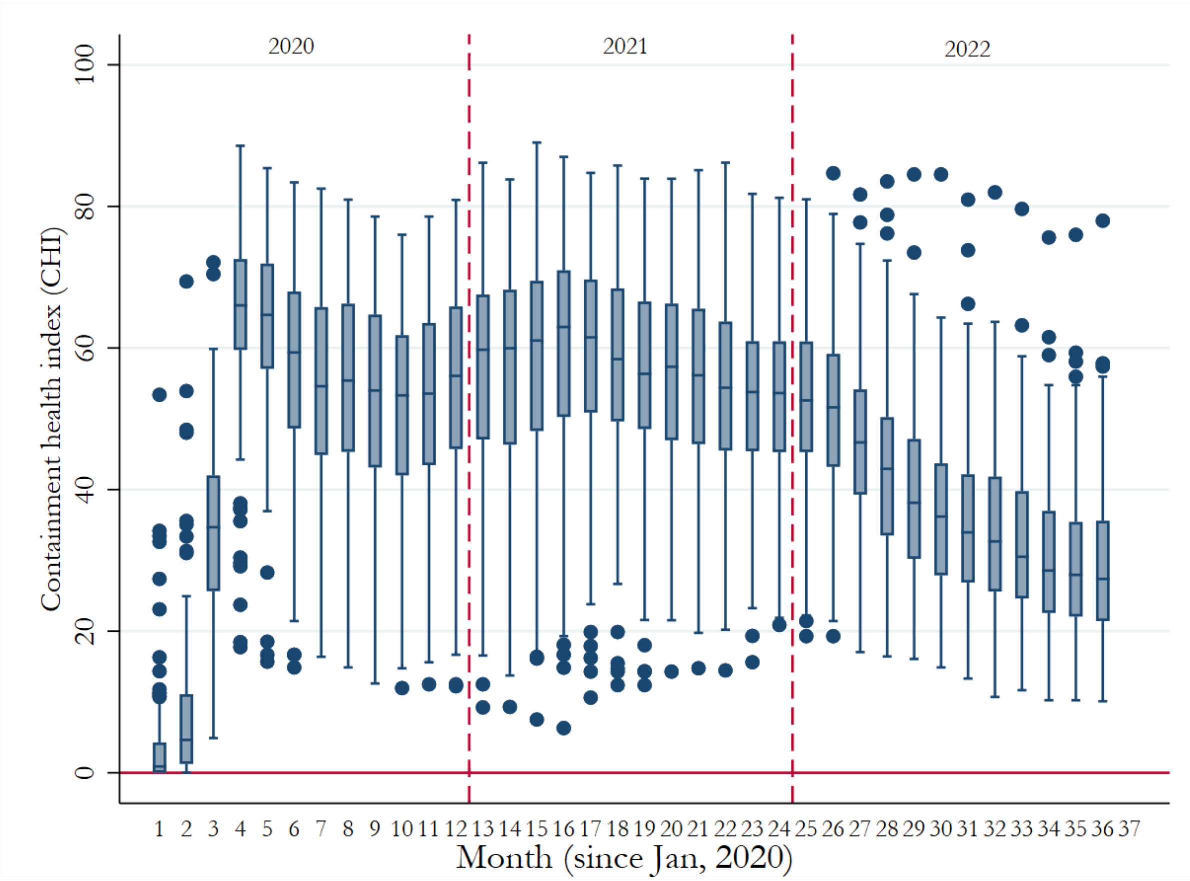
Figure 1 shows the worldwide average strictness of COVID-19 containment polices (measured as CHI from OxCGRT, 2022), with a clear peak in April 2020. Measures included earlier mentioned NPIs such as lockdowns, school closures and travel restrictions, but also less restrictive measures such as testing, contact-tracing and mask mandates. The strength of containment measures declined as the spread of the disease decreased over the summer. A second, lower peak was reached in April, 2021 during a second wave with novel virus variants. After that the average strictness of containment policies gradually fell during the rest of the period. Most countries and international agencies considered the pandemic to be over, and classified COVID-19 as an endemic disease, from winter-spring 2023. By the end of 2022, COVID-19 had caused approximately 6.6 million confirmed deaths according the OxCGRT database.

The first wave during spring 2020 was arguably characterized by *fundamental (Knightian) uncertainty* since no pandemic of similar magnitude had struck in many decades and knowledge about transmission and mortality was limited.[2]. For example it was not known whether transmission could happen when infected persons were asymptomatic, how easily the virus was transmitted by air or how well, if at all, face masks reduced transmission rates (WHO, 2020c,b,a,d). Early influential assessments of the mortality of the pandemic under different containment policy scenarios, such as the famous *Report 9* in the United Kingdom (Ferguson et al., 2020), turned out to be widely off the mark and were later heavily criticized. During spring 2020, no vaccine was yet in sight and the development of a vaccine was believed to take maybe 18 months.

Over time, knowledge about the virus, prevention and treatment improved (even if the debate on the efficiency of containment policies is ongoing). A new stage was arguably reached when it became clear in late 2020 that new vaccines had been successfully developed in record time. On 2 December, the Pfizer-BioNTech vaccine obtained temporary approval

[2] *Knightian uncertainty* refers to a situation when there is a lack of any quantifiable data on the underlying probability distribution of a social phenomenon. Such uncertainty stands in contrast to situations with quantifiable risk which economists usually study (Knight, 1921)

Figure 1: Boxplot distribution of monthly CHI-score for all countries in the world, 2020-22



Note: Boxplot of distribution of monthly CHI-measure for all countries in the world during 36 months 2020-22. Own graph based on data from OxCGRT, 2022

from the UK regulatory agency and by 21 December, the European Union and many other countries had also approved it. A week later, approval was granted in many countries for the Moderna and the Oxford/Astra Zeneca vaccines. The roll-out of the vaccination campaign then started across the world in early 2021. By this time, the fundamental uncertainty regarding the effects of COVID-19 was drastically reduced.

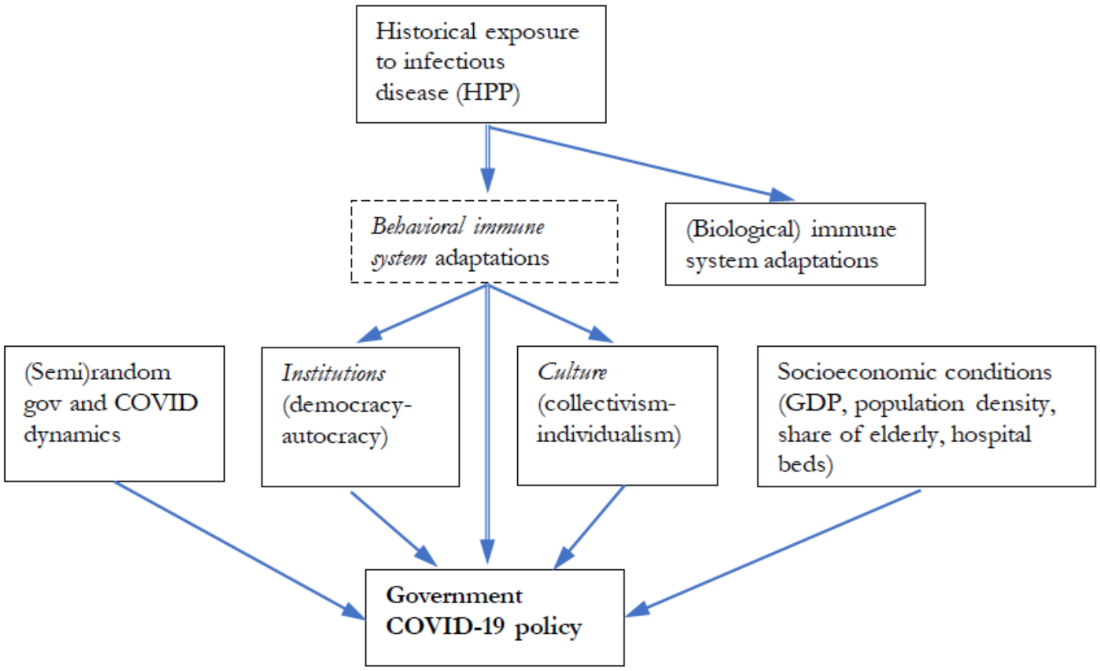
3 Conceptual framework

The conceptual framework for our study is shown in Figure 2 below. Our point of departure is the observation that historical exposure to infectious disease has varied greatly across the world. There are many reasons for this variation, including climatic factors, biogeography, population density, natural shocks, prevalence of disease vectors such as rats and mosquitos, etc. In general, the prevalence of infectious disease tends to be higher in tropical regions close to the equator. The direct cause of such diseases are the transmission of infectious agents (or *pathogens*) such as viruses or bacteria to the human body. Small host animals (i.e. *parasites*) such as helminths or insects can function as disease vectors that transmit the infectious pathogens, but infectious disease might also spread directly between humans, as is the case for airborne diseases.

Populations living in regions with a high pathogen load typically adapt to this environment in two major ways. First, through the process of natural selection, individuals with a genetic disposition for a stronger immune system against locally common pathogens (perhaps due to a mutation) will gradually come to dominate the population in the region. This genetic or evolutionary adaptation of the biological immune system is likely to work only very slowly.

Second, the so called *Behavioral Immune System* (BIS) might also adapt to the pathogen load. The theory of the BIS was developed by psychologists Damian Murray and Mark Schaller (Murray and Schaller, 2016) and has recently gained increased attention in the social science literature. The BIS is defined as a motivational system for parasite and pathogen avoidance that influences behavior in a number of ways. In essence, BIS provides a theory for long-term behavioral adaptations among individuals or groups to the prevailing disease environment where they live. It is important to note that during most of history, people had a very limited understanding of how infectious diseases spread (Koyama, 2023). Still, we know that people understood the importance of cleanliness and reacted to bad smells. We also know that the practice of quarantine was used at least since the Black Death (1347-53). The BIS includes practices for cleanliness and food preparation. A highly activated BIS also fosters restrictive sexual values and behaviors, as well as conformity and obedience to rituals and common practices. In general, it promotes a preference for traditional values,

Figure 2: Conceptual framework of empirical study



Note: The figure describes the conceptual framework and hypothesized causal relationships in our study. Source: Own figure

and suspicion and prejudice towards out-group members who might bring new pathogens (Murray and Schaller, 2016).

In their closely associated *Parasite Stress Theory*, Thornhill and Fincher (2014) put forth the argument that a higher pathogen exposure is also associated with a number of institutional and cultural expressions at the macro level, and also show this empirically. Populations in countries with a high parasite stress (i.e. pathogen load) tend to have more authoritarian types of government and cultural norms favoring collectivism (in-group and family orientation, aversion to strangers, weak sense of personal agency) rather than individualism (out-group orientation, openness to strangers, strong sense of personal agency).[3]

It is thus commonly argued in the literature that there are strong links between the BIS and both institutional and cultural adaptations, as shown in Figure 2. As discussed in the

[3]See Triandis (1995) for a more thorough discussion of individualism-collectivism

introduction, a large emerging literature in social science shows that formal institutions and cultural norms, in turn, have had a strong impact on government policy responses to COVID-19. For instance, Bazzi et al. (2021) and Bian et al. (2022) show that US counties with a stronger individualistic frontier culture tend to engage less with containment measures such as social distancing and mask requirements. Sebhatu et al. (2020) showed that democracies were relatively slow to react in the early phase of the pandemic.

Our argument is that both cultural and institutional variations across countries and regions are primarily *proximate* causes of government COVID-19 policy that in turn are to a large extent founded on deeper behavioral immune system adaptations to historical variations in groups' exposure to pathogen loads and infectious disease. In addition, we argue that there are reasons to believe that the BIS could have independent and direct effects on government COVID-19 policies (see Figure 2). Disease cues may for example give rise to fear and disgust, and promote strong desires to avoid disease (Murray and Schaller, 2016). As will be explained further below, our focus in the empirical analysis will therefore be on proxies for historical disease prevalence rather than on proxies for culture and institutions. We recognize however that any reduced-form empirical relationships between historical disease exposure and contemporary COVID-19 policies could either come into effect with culture and institutions as mediating variables or through an independent direct effect.

There are of course also a large number of other factors that affect the government's response to the COVID-19 pandemic. Most importantly, we expect governments to respond more or less instantaneously to information about the rates of infection and mortality of the disease within their country. We argue that reported deaths in COVID-19 in period t is the most relevant informational input for governments in their choice of containment policy.[4] To a great extent, disease dynamics (such as the location and spread of initial outbreaks, the emergence of new mutations, etc) were largely unpredictable and had a substantial random component.

[4]Also other data such as time since first reported death or number of newly infected people, could certainly have played a role. See sections below for further discussions about this.

Lastly, the stringency of COVID-19 policies will also depend on a number of country characteristics such as the general level of economic development and the level of urbanization. Urbanization is relevant to consider since respiratory infectious diseases spread rapidly in densely populated cities and because the disease tend to be more lethal in environments with poor air quality (Bourdrel et al., 2021). Since COVID-19 fatalities were most common among the elderly, it is also relevant to take into account the share of elderly in the population. The number of hospital beds per capita before the pandemic may be an additional key input for COVID-19 public policy since many governments feared that their health care system would be overwhelmed by COVID-19 patients and potentially took their health care capacity into account when deciding on containment policies. Also the general strength of state capacity could play a role for the strength of government response. Lastly, we hypothesize that recent experience of other corona virus diseases such as SARS and MERS might influence public policy.

4 Data

In this section, we present the data that we use in our study with a particular focus on our proxies for the historical exposure to infectious disease.

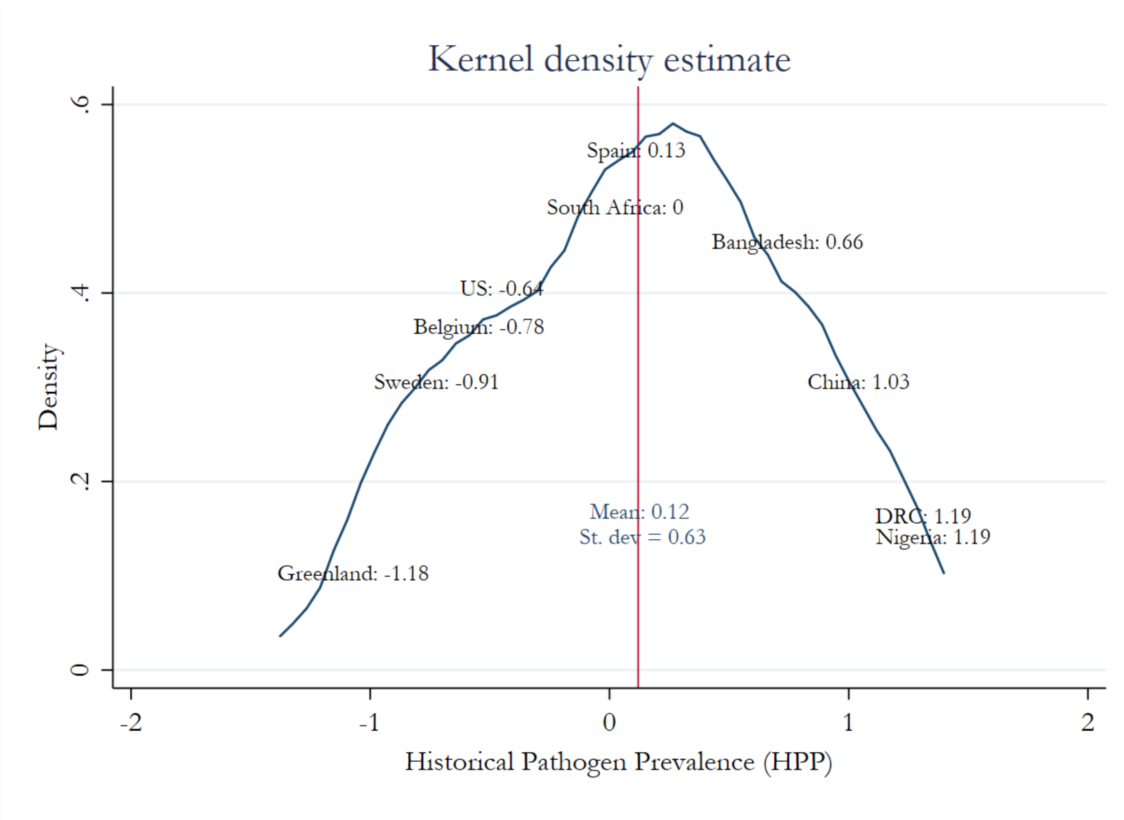
4.1 Historical exposure to infectious disease

As our main measure of historical exposure to infectious disease, we use the *Historical Pathogen Prevalence* (HPP) measure from Murray and Schaller (2010). This variable measures the historical prevalence of seven common infectious diseases: *leishmanias, schistosomes, trypanosomes, malaria, typhus, filariae*, and *dengue fever.* This data was derived from historical maps from Rodenwaldt and Bader´s (1952-1961) *World Atlas of Epidemic Diseases* and Whayne et al.´s (1944) *Global Epidemiology.*[5]. The diseases were each rated on

[5]This variable has previously featured in a number of economics papers including Gorodnichenko and Roland (2017)

a 4-point scale with 0 implying completely absent or never reported, and 3 implying severe levels of epidemic at least once. The data represent the geographical variation in exposure to infectious disease up to about 1950 (Lu et al., 2021). The measure is normalized to range from very low exposure at around -1.2 to very high exposure at +1.2 with a mean close to 0. The Kernel distribution function of our HPP-variable for the 182 countries in our sample is shown in Figure 3. Selected country examples are indicated in the graph. Not surprisingly, arctic Greenland has the lowest pathogen prevalence in the sample (-1.18) whereas tropical countries such as DRC and Nigeria have the highest prevalence (1.19). Spain is very close to the mean level of HPP: 0.13. The standard deviation is 0.63.

Figure 3: KERNEL DISTRIBUTION OF HISTORICAL PATHOGEN PREVALENCE (HPP)



Note: The figure shows the kernel density plot for our main proxy for historical disease exposure: *HPP*. Selected countries are shown in graph together with their associated HPP score.

A natural alternative proxy for historical pathogen prevalence would be the famous *Log Settler Mortality*-measure from Acemoglu et al (2001). However, this variable is only available for 86 former colonies and only cover two infectious diseases: *malaria* and *yellow fever*. Figure B3 in Appendix B shows a scatter plot of the two variables and the Pearson correlation (0.69) is shown in Table B1. Another closely related variable is HPP_9, which includes all the seven diseases from HPP but adds tuberculosis and leprosy (Murray and Schaller, 2010). This variable is only available for 152 countries but the correlation with HPP in Table B1 is very high (0.97).

Other potential proxies for pathogen prevalence use more contemporary data. More recent data has the advantage of a higher data reliability but also the problem of being further removed from the historical conditions that we want to capture. As our main alternative variable, we use *Non-zoonotic* from Fincher and Thornhill (2012), capturing the modern prevalence of a large number of infectious diseases transmitted from person to person (non-zoonotic transmission) rather than from parasite to human (zoonotic). It has been argued that the BIS was particularly strongly affected by human-to-human transmission (O'Shea et al., 2022) and this variable has the advantage of isolating the impact of such disease transmission. Figure B2 in Appendix B shows the scatter plot of the association between HPP and Non-zoonotic. The Pearson correlation coefficient is 0.83.

As an additional validity check, we also analyze the correlation between HPP and yet another proxy for contemporary pathogen prevalence: *CombPS* from Fincher and Thornhill (2012). The scatter plot and the correlations to the other variables (0.83) are shown in Appendix B. Our overall conclusion from these validity checks is that our preferred main measure HPP has a relatively high coverage, should be a good proxy for historical disease environments, and that it also correlates strongly with other candidate measures.

4.2 Government COVID-19 policy

To measure the government response to the COVID-19 pandemic we use data from the Oxford COVID-19 Government Response Tracker (OxCGRT), which provides daily data on various COVID-19 containment measures for almost every country or territory in the world.

In addition to information on specific measures, such as school closures, mask mandates, travel restrictions, etc., the data include a number of indices on the government response (Hale et al., 2021). In our main estimations we use the *containment and health index* (CHI). The main alternative would be the Stringency Index which include a number of NPIs which reduce (physical) social interaction in the general public (school closings, workplace closings, cancelled public events, restrictions on gathering size, public transport closures, stay at home requirements, domestic travel restrictions and international travel restrictions). The CHI include all NPIs in the Stringency index and a number of additional measures to decrease the spread of the virus, namely public information campaigns, testing policies, contact tracing, mask mandates and vaccination policies. We prefer the CHI over the Stringency index since it is more comprehensive. In particular we prefer an index which includes vaccination policies since we investigate policy responses during the whole pandemic, both before and after vaccines were available. [6] The Stringency index, which can be seen as a better measure of the extent to which individual freedoms are restricted to fight the pandemic, is used as a robustness check. The CHI range between 0 and 100, with equal weight to each of the 14 sub-indices (which also range between 0 and 100). The overall mean is 47.97 and the overall standard deviation 18.80. At any given time the standard deviation is smaller (10.5 to 16.5). Figure 1 in the background section shows the variation in the CHI over time.

4.3 Disease dynamics

We use information on confirmed reported deaths in the country itself and in neighbouring countries to proxy the information on disease dynamics that governments had at their disposal when deciding on containment measures. While government agencies are likely to have collected information from different sources, new deaths neatly summarizes the number of people that has recently been infected and have thereafter actually died, given factors such as physical environment, population structure, health care system etc. in the country. It

[6] We use the version of the CHI which average over policies for vaccinated and un-vaccinated in cases in which these differ.

should thus be highly informative about the current severity of the pandemic in the country. However, infectious diseases do not stop at borders and so it makes sense to also consider disease dynamics in other countries. In particular, disease dynamics in countries with shared land borders may be informative about what to expect in the own country. The main alternative data would be new infections or excess deaths. However, available data on new infection is patchy and, importantly, it also was so when decisions were taken. While excess deaths is a good measure of the overall impact of the pandemic, governments did not have access to these data when making decisions on containment measures.

We measure disease dynamics with the number of new weekly deaths per 100,000 inhabitants in the country. More precisely, we calculate the average of the total cumulative confirmed number of deaths for the week in question minus the average of the total cumulative confirmed number of deaths in the previous week, divided by the population in 2019. The data on confirmed number of total deaths is from OxCGRT. The population in 2019 is from the World Bank's *World Development Indicators*. To compute new deaths in neighbouring countries we use the average of new weekly deaths per 100,000 inhabitants in all countries which share a land border with the country in question. We set new weekly deaths in neighbouring countries to 0 for island nations (and South Korea since we do not have information from its only neighbour, North Korea).

It seems possible that confirmed deaths are not immediately observed and/or that it takes some time to react to information. We therefore compared the estimated response of the CHI to different lags of new weekly deaths in the own and neighbouring countries in Appendix Table A1. While the lag structure does not seem to matter much, the non-lagged new weekly deaths variables seem to overall perform slightly better than 1-week, 2-week or 3-week lags: the F-statistic for the whole period is slightly larger, the R-squared is generally towards the higher end, and coefficients are generally somewhat larger. To not lag also comes with the additional advantage of not losing early observations.

4.4 Additional variables

When we investigate the general response to the global COVID-19 pandemic, independent of disease dynamics in the country and its neighbours, we use a number of control variables. Socioeconomic variables - log GDP per capita, share of urban population, share of elderly population (age 65 or above), all from 2019 - are from the World Development Indicators. We include two variables as proxies for institutional quality: *Government effectiveness* and *Voice and accountability* from the World Bank's *Worldwide Governance Indicators*-dataset (Kaufmann and Kraay, 2022). *Government effectiveness* captures aspects like the quality of public services, independence from political pressures, and the quality of policy formation and implementation. This percentile rank measure ranges between 0-100 with 0 corresponding to lowest rank and 100 to the highest. Similarly, *Voice and accountability* is also a rank percentile measure between 0-100 and measures the extent to which citizens are able to participate in selecting their government, freedom of expression, and free media.

Recent experience of a corona-virus epidemic may have affected the response to COVID-19. For example, South Korea has been credited with a rapid and efficient response in terms of testing, contact-tracing and isolation of infected due to its experience of the MERS-epidemic. We use dummy variables for having been exposed to the 2003 SARS-epidemic and the MERS-epidemic starting in 2012. We set each dummy to 1 if there was more than one reported case per one million inhabitants in a country. Canada, China, Hong Kong, Macao, Taiwan, Mongolia and Singapore have more than one reported SARS case per million inhabitants, and Jordan, Kuwait, Oman, Qatar, South Korea, Saudi Arabia and United Arab Emirates more than one reported MERS case.[7].

Appendix tables A2 and A3 show summary statistics for the main sample and the sample of US states.

[7]SARS cases are from WHO available at https://www.who.int/publications/m/item/summary-of-probable-sars-cases-with-onset-of-illness-from-1-november-2002-to-31-july-2003. MERS cases are from FAO available at https://www.fao.org/animal-health/situation-updates/mers-coronavirus

5 Empirical analysis

5.1 Empirical strategy

According to our conceptual framework, historical disease exposure influences how the government respond to the COVID-19 pandemic. The main equation that we estimate is the following weekly panel data regression:

$$C_{it} = \alpha D_{it} + \beta D_{nt} + \gamma D_{it} * HPP_i + \eta_i + \epsilon_{it} \tag{1}$$

In this expression, C_{it} is the average of the CHI of country i in week t, D_{it} is the increase in confirmed deaths in COVID-19 per 100,000 people between week $t-1$ and t, D_{it} is the increase in confirmed deaths per 100,000 people between week $t-1$ and t in countries that share a land border with country i, HPP_i is the historical pathogen prevalence of country i, η_i is a country fixed effect capturing all observed and unobserved time-invariant differences between countries, such as their level of income, their health care capacity and their political institutions. Similarly, the possible impact of HPP_i which does not depend on disease dynamics is absorbed by the fixed effect η_i. ϵ_{it} is the error term. Our main parameter of interest is γ, which shows how the response to new confirmed COVID-19 deaths depends on historical disease exposure. In line with the discussion above, our main hypothesis is that $\gamma > 0$, at least during the first year 2020 characterized by fundamental uncertainty. We estimate equation 1 for all weeks jointly and separately for each year, where the initial response during 2020 was during a time of fundamental uncertainty, while the situation was less uncertain in 2021 and 2000, not the least since vaccines were then available.

To formally test if the influence of historical disease exposure on the government response differ between the first year of fundamental uncertainty and the consecutive years we also estimate

$$C_{it} = \alpha D_{it} + \beta D_{nt} + \gamma D_{it} * HPP_i + \lambda \mathbf{I}_{2020} * D_{it} * HPP_i + \eta_i + \epsilon_{it}, \tag{2}$$

where $\mathbf{I}_{2020} = 1$ for observations from 2020 and zero for 2021-22. In accordance, a $\lambda > 0$ indicates that the response is different during the first year.

A key feature of infectious diseases is the spatial diffusion between locations. Hence, in order to understand pandemic outcomes in a certain country, it is of particular importance to carefully take into account spatial autocorrelation.[8] One straightforward strategy for achieving this is to control for death rates in neighboring countries, as shown above. Additionally, we report both standard errors clustered at the country level and standard errors corrected for temporal and spatial autocorrelation, using the Stata *acreg*-command, developed by Colella et al. (2020) on the basis of Conley (1999). Importantly, since we use weekly observations, both types of standard errors accommodate autocorrelation over time in a country. The latter additionally adjusts for correlation in the COVID-19 response of geographically close countries within a certain range. Put differently, this procedure should account for unobserved similarities between nearby countries, resulting in similarities in government COVID-19 responses over time. Both in the cross-country and cross-state analysis of the United States, we use a radius of 1000 km for assessing spatial autocorrelation. [9]

Historical disease exposure may also impact the general response to the global COVID-19 pandemic, independent of disease dynamics in the own and neighbouring countries. To investigate this possibility, we run regressions with the estimated fixed effects in equation (1) as the dependent variable. Hence, in a second stage we estimate

$$\hat{\eta}_i = \omega + \delta HPP_i + \theta X_i + \nu_{it} \tag{3}$$

where X_i includes time-invariant country characteristics that could matter for the response to the pandemic, i.e. log GDP per capita in 2019, urban population, population

[8]See Kelly (2019) for an argument that many of the papers in the persistence literature do not provide adequate tests of spatial autocorrelation and hence produce biased results. See Voth (2021) for counter arguments against this claim.

[9]The specific Stata procedure that we employ computes heteroskedasticity-autocorrelation-consistent (HAC) standard errors with a linear temporal decay in time for up to 10 weeks. Results for other geographical ranges are shown in the appendix and are available upon request.

above age 65, government effectiveness, voice and accountability and whether the country were hit by the earlier SARS or MERS epidemics. We also control for absolute latitude since it is correlated with HPP.

5.2 Main results

Our main results are presented in Table 1. In column (1) we estimate a common interaction term between new deaths and historical disease exposure for all three years, as in equation (1). In column (2) we test if the influence of historical disease exposure on the response to new deaths was different in the first year of fundamental uncertainty compared to in the two later years, in accordance with equation (2). The main coefficient of interest is that of the triple interaction term between new deaths, HPP and a year 2020 dummy. In columns (3)-(5) we estimate equation (1) separately for each year.

New deaths in both the country itself and in neighbouring countries are statistically significantly associated with the strictness of the policy response. One additional confirmed death per 100,000 people in the country increases the CHI with about 1.6 in the full sample as well as in 2020 and in 2022. In 2021 the increase is 0.9. A difference in 1.6 would, for example, approximately correspond to required school closings at all levels rather than at some levels or to recommended school closings rather than no school closing policies at all, other policies being equal.[10] Countries appear to respond more to new deaths in neighbouring countries than to new deaths in the own country, the exception being in 2021. One reason could be that governments try to be forward-looking and, conditional on deaths in the own country, expect that an increased spread of COVID-19 in neighbouring countries should soon affect the own country.

In the regressions for all years and for 2020, the response to new deaths in the own country is stronger in countries with higher historical disease exposure. In the full sample, a one unit increase of the HPP increases the response to new deaths in the own country with

[10] In standardized terms, a one standard deviation increase in new deaths in the own country increase the CHI with about 1.7 standard deviations in the full sample.

Table 1: Within-estimates of CHI Index in weekly cross-country panel 2020-22

	DV: CHI Index				
	(1)	(2)	(3)	(4)	(5)
	All years		2020	2021	2022
D_{it}	1.584	1.679	1.660	0.920	1.614
	(0.232)***	(0.237)***	(0.361)***	(0.166)***	(0.474)***
	[0.173]***	[0.174]***	[0.377]***	[0.131]***	[0.401]***
D_{nt}	3.414	3.464	3.568	0.794	5.261
	(0.350)***	(0.351)***	(0.416)***	(0.274)***	(0.670)***
	[0.273]***	[0.276]***	[0.528]***	[0.218]***	[0.509]***
$D_{it} * HPP_i$	1.108	0.938	1.830	0.016	0.497
	(0.306)***	(0.338)***	(0.471)***	(0.245)	(0.668)
	[0.268]***	[0.298]***	[0.536]***	[0.204]	[0.286]
$D_{it} * HPP_i * I_{2020}$		0.733			
		(0.372)*			
		[0.345]**			
I_{2020} (dummy)		3.312			
		(0.658)***			
Constant	44.568	43.398	46.663	54.112	35.686
	(0.222)***	(0.323)***	(0.266)***	(0.287)***	(0.258)***
Country FE	Yes	Yes	Yes	Yes	Yes
Observations	26,164	26,164	8,352	9,048	8,764
R-squared	0.13	0.14	0.08	0.09	0.20
Countries	175	175	174	174	175

Notes: The dependent variable is average CHI index observed in during one week, 2020-22. Country-week observations during 156 weeks in 174-175 countries are shown in column (1), country-week observations for 2020, 2021 and 2022 respectively in columns (2)-(4). *** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered at the country level in parentheses (). Standard errors corrected for spatial and temporal autocorrelation in brackets [], using a spatial cutoff of 1000 km.

more than 70%. In 2020 it more than doubles the response. The regression in column (2) suggests historical disease exposure to influence the response to new deaths throughout all years, but more so in 2020 compared to in the later years. However, there is no statistically significant impact in the separate 2021 and 2022 samples (columns (4) and (5)). Hence, while results are inconclusive with regard to the possible influence of historical disease exposure later in the pandemic, when vaccines were available and knowledge about e.g. transmission and mortality was higher, there is a clear impact in the first year of fundamental (Knightian) uncertainty. The coefficient of the 2020 dummy also shows stricter policies overall in the first, uncertain, year.

While the strictness of the policy response reacts to disease dynamics, as measured by new deaths in the own and neighbouring countries, the impact is still small in comparison to the constant, i.e. the average strength of the policy response without any new deaths in the own or in neighbouring countries. In Table 2 we investigate correlates of the general response to the global COVID-19 pandemic which did not depend on country-specific disease dynamics. We do so by regressing the fixed effects from the weekly panel estimations in Table 1 on a set of explanatory variables, for all years (using the fixed effects from the regression in column (1) in Table 1 1) and separately for each year.

Controlling for absolute latitude and socioeconomic and institutional variables, there is a statistically significant association between the historical disease exposure and the general strictness of the COVID response, for all years, in 2020 and in 2021, but not in 2022. A one unit increase in the HPP increases the CHI with about 4.6, 7.6 and 5.4. Absolute latitude is positively correlated with the strength of the policy response in 2020, but negatively so in 2022. Contrary to expectations, countries with a larger share of older people had a weaker policy response during the first year. The urban share of the population is not statistically significant. Policies are stricter in richer countries.

There is no statistically significant impact of experience from previous corona virus epidemics, except for in 2022. The statistically significant positive coefficient for SARS experience in 2022 is driven by China, which stood out in maintaining very strict COVID policies until in the autumn in 2022. The lack of an impact of previous coronavirus epidemic experiences could be because such experience mattered more for the type of policies implemented and their efficiency than for the overall strictness of the policy response. It could also be because none of these two pandemics reached a scale comparable with COVID-19.

Government efficiency is positively related to the policy response in the full sample, but not in any single year (though it remain comparable in size in all estimations). There is no statistically significant correlation between voice & accountability, our measure of democracy, and the strictness of the response when controlling for all of the above variables.

Table 2: Explaining the fixed effects

	DV: Country fixed effects from weekly panel			
	(1)	(2)	(3)	(4)
	All years	2020	2021	2022
HPP	4.577**	7.550***	5.388**	1.242
	(1.772)	(1.999)	(2.335)	(2.105)
Abs. latitude	-0.050	0.162**	-0.041	-0.273***
	(0.069)	(0.078)	(0.091)	(0.082)
Share pop. 65+	-0.438**	-0.626***	-0.314	-0.144
	(0.203)	(0.227)	(0.266)	(0.242)
Share pop. urban	-0.074	-0.012	-0.117*	-0.079
	(0.047)	(0.053)	(0.062)	(0.056)
Log GDP pc 2019	3.389***	3.483**	6.070***	1.432
	(1.243)	(1.396)	(1.631)	(1.477)
SARS in country	6.726*	3.525	2.083	12.876***
	(3.686)	(4.118)	(4.810)	(4.381)
MERS in country	0.274	0.514	-0.834	0.251
	(3.880)	(4.336)	(5.064)	(4.611)
Government efficiency	0.115**	0.080	0.106	0.092
	(0.053)	(0.059)	(0.069)	(0.063)
Voice & accountability	-0.045	-0.045	-0.031	-0.059
	(0.039)	(0.044)	(0.051)	(0.046)
Constant	-24.018***	-30.995***	-46.259***	-1.321
	(7.902)	(8.831)	(10.314)	(9.390)
Observations	164	163	163	164
R-squared	0.26	0.22	0.27	0.28

Notes: The dependent variable is the country fixed effects from the weekly panel estimations in Table 1, columns (1), (3), (4) and (5). *** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered at the country level in parentheses ().

5.3 Robustness checks

In the previous section, we interacted HPP for country i with new deaths in country i. However, COVID-19 containment policies seem to respond more to new deaths in neighbouring countries than to new deaths in the own country. In Table B2 we therefore interact the HPP with new deaths in neighbouring countries as well as with new deaths in the own country. When we estimate common interaction terms for all years in column (1), the two interaction terms are of about the same size, and both are statistically significant when we use clustered standard errors. When we use spatial correlation adjusted standard errors, only the interaction term with new deaths in the own country is statistically significant.

When we allow for a different interaction term in 2020 compared to in the later years, historical disease exposure appear to increase the response to new deaths in the own country throughout the pandemic, while it increases the response to new deaths in neighbouring countries in 2020 only. Still, in the estimation using the 2020 sample the interaction term between HPP and deaths in neighbouring countries is not statistically significant (even if it is about the same size as the statistically significant coefficient of the interaction term with new deaths in the own country). There are no statistically significant interaction terms in the 2021 or 2022 samples, but the 2021 coefficient on the interaction term with deaths in the own country is quite large. Overall, historical disease exposure seem to matter for how countries respond to disease dynamics as measured by new deaths in the own country and possibly for how they respond to new deaths in neighbouring countries, in particular in the first year. The two interaction terms are of course highly correlated and, even if the interaction term with new deaths in the country itself appears to be stronger, we do not believe that our estimated results permit us to draw any firm conclusions regarding possible differences in how historical disease exposure influence the response to the two different disease measures over the years.

In the data section we mentioned a few alternative measures of disease prevalence. In Table 3, we replace HPP with the measure of non-zoonotic diseases from Fincher and Thornhill (2012). This measure may be of particular interest if we believe that pathogens that transmit between humans are of particular importance for the behavioral immune system responses which ought to underlie the stronger policy response to the COVID-19 pandemic in countries with higher historical disease prevalence. Overall, results are very similar to those for HPP in Table 1.

In Table 4 we replace the CHI with the Stringency Index as the dependent variable, which can be seen as a measure of the extent to which countries are willing to compromise individual rights to curb the pandemic. Again, results are very similar to in the main estimations in Table 1.

In appendix tables B3 and B4, we check the robustness of the results in Table 1 by using different assumptions in our calculation of standard errors that correct for spatial autocorrelation. In Table B3, we use the coordinates for country capitals rather than country centroids. Such an alternative assumption might be reasonable if one thinks that it is rather distance between country capitals that matters for the correlation in government responses in geographically proximate countries. In general, standard errors are quite similar to those in Table 1. A similar pattern is found in B4 where we double the distance to nearby countries that are assumed to affect spatial autocorrelation from 1000 to 2000 km. Our conclusion is that our results are not sensitive to alternative assumptions along these dimensions.

5.4 Panel with US States

While various countries adopted region-specific policies as a response to differences in disease dynamics across space, policies were typically decided at the central level. However, in the United States the states decided on policies within their jurisdiction. This allows us to investigate the extent to which historical disease exposure can explain variation in the strictness of COVID-19 policies within a single country. Our measure of disease exposure in US states is Fincher and Thornhill (2012)'s Parasite Stress (PS) measure which summarize infectious disease cases tracked by the Centre for Disease Control (CDC) between 1993 and 2007. While this data is more recent than the HPP used for the cross-national analysis, the high correlation between the HPP and the Parasite stress measure documented in the data section illustrate the persistence in disease exposure over time. OxCGRT has data

Table 3: Robustness I: Non-zoonotic measure replacing HPP as interaction variable

	DV: CHI Index				
	(1)	(2)	(3)	(4)	(5)
	All years		2020	2021	2022
D_{it}	1.661	1.718	1.831	0.915	2.173
	(0.072)***	(0.278)***	(0.498)***	(0.159)***	(0.885)**
	[0.195]***	[0.192]***	[0.431]***	[0.130]***	[0.805]***
D_{nt}	3.356	3.477	3.722	0.791	5.245
	(0.097)***	(0.340)***	(0.372)***	(0.278)***	(0.675)***
	[0.271]***	[0.271]***	[0.499]***	[0.219]***	[0.510]***
$D_{it} * NZ_i$	0.405	0.298	0.846	-0.004	0.627
	(0.036)***	(0.150)**	(0.244)***	(0.078)	(0.443)
	[0.111]***	[0.114]***	[0.248]***	[0.063]	[0.402]
$D_{it} * NZ_i * I_{2020}$		0.371			
		(0.132)***			
		[0.142]***			
I_{2020} (dummy)		3.300			
		(0.61)***			
Constant	44.554	43.378	46.495	54.117	35.650
	(0.660)***	(0.317)***	(0.232)***	(0.287)***	(0.266)***
Observations	26,164	26,164	8,352	9,048	8,764
R-squared		0.14	0.09	0.09	0.21
Countries	175	175	174	174	175

Notes: The dependent variable is average CHI index observed in during one week in 2020-22. Country-week observations during 156 weeks in 174-175 countries are shown in columns (1)-(2), country-week observations for 2020, 2021 and 2022 respectively in columns (3)-(5). *** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered at the country level in parentheses (). Standard errors corrected for spatial and temporal autocorrelation in brackets [] using a spatial cutoff of 1000 km

Table 4: Robustness II: Stringency Index replacing CHI as dependent variable

	DV: Stringency Index				
	(1)	(2)	(3)	(4)	(5)
	All years		2020	2021	2022
D_{it}	1.807	2.203	1.852	1.255	2.033
	(0.095)***	(0.089)***	(0.376)***	(0.245)***	(0.583)***
	[0.222]***	[0.174]***	[0.408]***	[0.189]***	[0.495]***
D_{nt}	4.374	4.592	3.701	1.674	6.375
	(0.132)***	(0.123)***	(0.481)***	(0.361)***	(0.828)***
	[0.361]***	[0.276]***	[0.607]***	[0.304]***	[0.618]***
$D_{it} * HPP_i$	1.430	1.366	1.598	0.053	1.145
	(0.144)***	(0.150)***	(0.530)***	(0.340)	(0.811)
	[0.344]***	[0.298]***	[0.611]***	[0.299]	[0.677]
$D_{it} * HPP_i * I_{2020}$		1.022			
		(0.225)***			
		[0.345]***			
I_{2020} (dummy)		16.528			
		(0.259)***			
Constant	39.542	33.822	51.053	48.135	23.122
	(0.717)***	(0.722)***	(0.298)***	(0.382)***	(0.312)***
Country FEs	Yes	Yes	Yes	Yes	Yes
Observations	26,178	26,178	8,352	9,048	8,778
R-squared			0.07	0.11	0.20
Countries	175	175	174	174	175

Notes: The dependent variable is average Stringency index observed in during one week in 2020-22. Country-week observations during 156 weeks in 174-175 countries are shown in columns (1)-(2), country-week observations for 2020, 2021 and 2022 respectively in columns (3)-(5). *** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered at the country level in parentheses (). Standard errors corrected for spatial and temporal autocorrelation in brackets [] using a spatial cutoff of 1000 km on confirmed COVID-19 deaths and containment and health polices at the US state level. Hence, our dependent variable is again the CHI.

In Table 5, we use a weekly panel of US states and run the same regressions as in the cross-national analysis (Table). State-level COVID-19 policies in US states does not appear to respond to new deaths in the own state, but it does respond to new deaths in neighbouring states. One reason for the comparatively larger influence of new deaths in neighbouring states in USA, compared to that of neighbouring countries in the cross-national sample, might be that within-country borders between states are on average more porous than between-country borders. The positive and significant coefficient for the triple interaction term in column (2), shows that policy response to own deaths is stronger in states with higher PS values in 2020 but not in the later years (and not in the all-years estimation). A one unit increase in the PS index more than doubles the policy response to new own deaths.

6 Discussion

We show that historical disease exposure matters for the strictness of government COVID-19 policies both across countries and across states within the United States. Countries and states with a higher historical disease exposure responded to the COVID-19 pandemic with stricter containment policies. This is in line with the theory of the behavioral immune system (BIS) and the parasite stress theory (PST), which posit that a higher historical disease exposure bring about pathogen avoiding behaviors, more collectivist rather than individualistic cultural norms, and more authoritarian rather than democratic political institutions. Hence, a higher historical disease exposure may be the underlying reason behind results in studies that have found differences in COVID-19 policies between more or less individualistic places (Ashraf et al., 2022; Bazzi et al., 2021; Bian et al., 2022; Chen et al., 2021; Jiang et al., 2022; Kitayama et al., 2022) and between authoritarian regimes and democracies (Sebhatu et al., 2020) or more or less centralised states (Toshkov et al., 2022).

Table 5: Weekly panel regression of CHI Index among US States

	DV: CHI				
	(1)	(2)	(3)	(4)	(5)
	All years		2020	2021	2022
D_{it}	0.261	0.254	0.357	0.196	0.118
	(0.162)	(0.143)*	(0.393)	(0.179)	(0.081)
	[0.122]	[0.113]**	[0.303]	[0.131]	[0.054]**
D_{nt}	1.672	1.769	2.471	0.505	0.562
	(0.183)***	(0.158)***	(0.398)***	(0.207)**	(0.110)***
	[0.237]***	[0.237]***	[0.547]***	[0.250]**	[0.083]***
$D_{it} * HPP_i$	0.213	0.046	0.840	0.106	-0.000
	(0.130)	(0.119)	(0.277)***	(0.132)	(0.094)
	[0.155]	[0.152]	[0.435]*	[0.135]	[0.061]
$D_{it} * HPP_i * I_{2020}$		0.481			
		(0.174)***			
		[0.202]**			
I_{2020} (dummy)		8.003			
		(0.623)***			
		[1.444]***			
State FE	Yes	Yes	Yes	Yes	Yes
Observations	7,296	7,296	2,304	2,496	2,496
R-squared	0.11	0.21	0.16	0.04	0.26
States	48	48	48	48	48

Notes: The dependent variable is average CHI index observed during one week in 2020-22 among 48 US states. State-week observations during 156 weeks are shown in columns (1)-(2), state-week observations for 2020, 2021 and 2022 respectively in columns (3)-(5). *** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered at the state level in parentheses (). Standard errors corrected for spatial and temporal autocorrelation in brackets [] using a spatial cutoff of 1000 km

A forward-oriented government should primarily respond to the information it has on disease dynamics. Hence, in our main specification we estimate how the historical disease exposure influences the response to time-varying disease dynamics, measured by new confirmed deaths in the country/US state. However, historical disease exposure may also influence the general response to the global pandemic, independent of disease dynamics in the country and its vicinity. We find that the historical disease exposure matters both for how strongly containment policy react to disease dynamics in the country and for strictness of the general response to the global pandemic.

In contrast to much of the earlier literature, we study the whole COVID-pandemic from early 2020 to the end of 2022 and show that historical disease exposure matters primarily in the first year of fundamental Knightian uncertainty. This suggest that policy-makers may fall back on deep cultural norms during times of crisis and fundamental uncertainty, when there is very limited knowledge and experience to guide policies. This has implications for how we should think about cultural and institutional persistence. Persistence of historical factors is typically modeled as being stable, even if some decay over time may be be taken into account. Our findings, in contrast, suggest that the importance of historical factors may both increase and decrease over time depending on context. A stronger influence of historical factors during times of Knightian uncertainty also has implications for how we should think about policy-making under uncertainty.

Regarding the possible channels through which historical disease exposure may affect COVID-19 containment policies, the stricter response during the first period of Knightian uncertainty may suggest a cultural or behavioral channel rather than an institutional one. Put differently, it may suggest that policy-makers and/or their constituencies fall back to individualist or collectivist cultural norms or to behavioral gut reactions, to disease cues to a larger extent when there is limited knowledge and experience to base decisions on. However, we do not explicitly investigate the different intermediate channels, and a different policy response during Knightian uncertainty in autocracies compared to in democracies is also possible.

Another delineation in our study is that we do not model the political process underlying the COVID-19 containment policy response. In particular we are silent on the extent to which policy responses reflect the preferences of the constituency or not. We also do not investigate behavioral responses in the general public. We believe these topics are natural areas for future research in an agenda focusing on the implications of the behavioral immune system.

7 Conclusions

On the basis of a conceptual framework informed by the theory of the behavioral immune system, we have investigated differences in government responses to COVID-19. More specifically, we have analyzed whether a high historical exposure to infectious disease has contributed to stricter containment policies in a weekly panel study including almost all countries in the world and all US states. We found that a greater historical exposure strongly influenced government policy during the first year of fundamental uncertainty and that it had a much lower or no impact after vaccines had been introduced from 2021, holding constant objective risk factors such as death rates in the country in question and among neighboring countries. This differential impact of historical legacies puts previous findings in the persistence literature in a new light and potentially contributes to informing future government policy in times of deep crisis.

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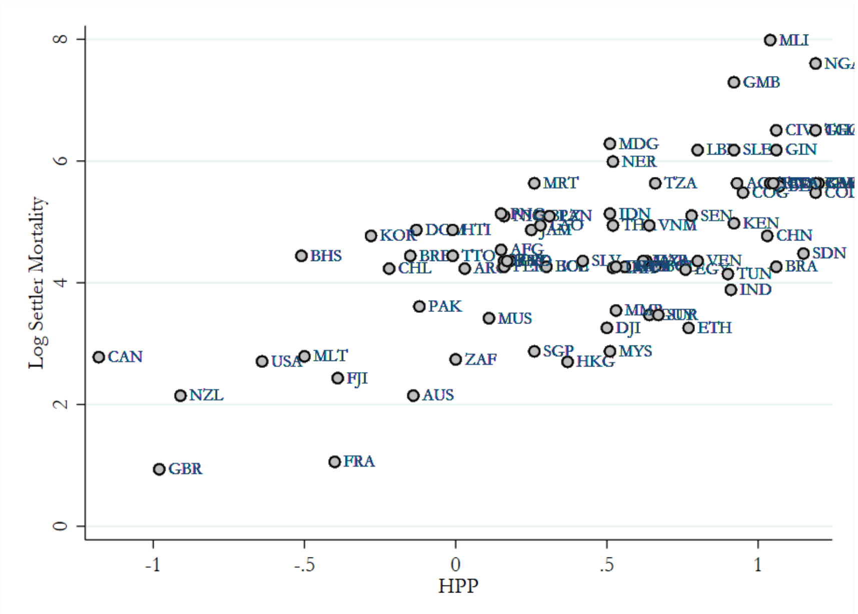
Appendix A. Data

Appendix B. Additional figures and tables

Figure B1: Scatter plot: HPP and Log Settler Mortality



Notes: The figure shows a scatter plot of our main measure HPP and the related measure Settler Mortality for 86 countries. Individual countries are identified by three-letter codes.

Table A1: Lag structure of new deaths and prediction of the CHI

	(1) All years	(2) 2020	(3) 2021	(4) 2022
Panel A: No lags				
D_{it}	1.362***	0.976***	0.917***	1.534***
	(0.213)	(0.282)	(0.161)	(0.433)
D_{nt}	3.324***	3.466***	0.792***	5.212***
	(0.339)	(0.372)	(0.271)	(0.648)
Observations	26,164	8,352	9,048	8,764
R-squared	0.12	0.08	0.09	0.20
F-stat	160	83.07	38.65	70.60
Panel B: One week lag				
$L.D_{it}$	1.314***	1.024***	0.906***	1.492***
	(0.209)	(0.244)	(0.162)	(0.406)
$L.D_{nt}$	3.190***	3.134***	0.886***	4.971***
	(0.332)	(0.344)	(0.260)	(0.607)
Observations	25,990	8,178	9,048	8,764
R-squared	0.12	0.07	0.10	0.20
F-stat	159.2	92.57	42.94	74.14
Panel C: Two week lag				
$L2.D_{it}$	1.251***	0.973***	0.878***	1.472***
	(0.205)	(0.214)	(0.161)	(0.392)
$L2.D_{nt}$	3.028***	2.756***	0.970***	4.688***
	(0.326)	(0.303)	(0.251)	(0.571)
Observations	25,816	8,004	9,048	8,764
R-squared	0.11	0.06	0.10	0.19
F-stat	156.7	100.4	46.50	77.56
Panel D: Three week lag				
$L3.D_{it}$	1.170***	0.898***	0.837***	1.412***
	(0.201)	(0.182)	(0.158)	(0.384)
$L3.D_{nt}$	2.840***	2.282***	1.038***	4.370***
	(0.322)	(0.262)	(0.244)	(0.541)
Observations	25,642	7,830	9,048	8,764
R-squared	0.10	0.05	0.10	0.19
F-stat	152.2	100.6	49.05	79.03
Countries	175	174	174	175

Notes: Standard errors clustered by country in parentheses (). *** p<0.01, ** p<0.05, * p<0.1

Table A2: Summary statistics

	Obs.	Mean	Std. dev.	Min	Max
Time-varying variables					
D_{it}	26,164	0.844	1.946	-8.101	45.698
D_{nt}	26,164	0.7455	1.445	-1.811	14.068
CHI	26,164	48.291	18.523	0	92.429
Stringency Index	26,164	44.207	23.945	0	100
Time-invariant variables					
HPP	182	0.107	0.628	-1.180	1.200
HPP_9	152	0.139	0.659	-1.310	1.170
CombPS	172	0.346	2.857	-3.615	6.184
Nonzoon	181	0.264	1.974	-2.481	4.728
Absolute latitude	182	26.434	17.147	0.0236	71.707
Population above 65	181	9.467	6.896	1.172	35.621
Urban population	181	61.825	23.345	13.456	100
Log GDP per capita	167	8.741	1.483	5.338	11.618
SARS experience	182	0.0385	0.193	0	1
MERS experience	182	0.038	0.1932	0	1
Government efficiency	180	50.248	29.410	0.481	100
Voice & accountability	180	48.484	28.909	0.483	100

Table A3: Summary statistics US states

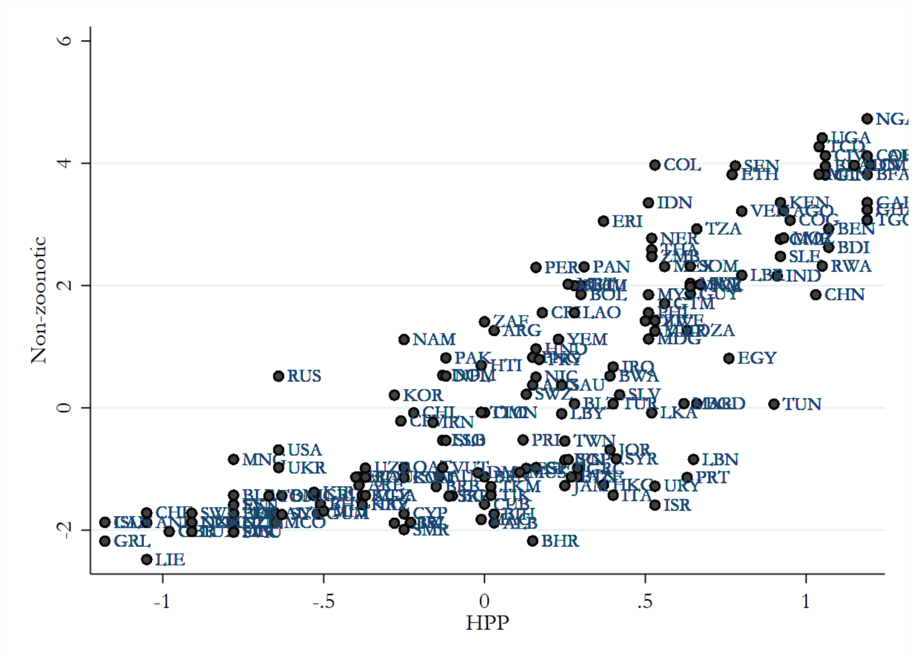
	Obs.	Mean	Std. dev.	Min	Max
Time-varying variables					
D_{it}	7,296	2.151	2.611	-11.362	35.756
D_{nt}	7,296	2.172	2.129	-3.647	18.747
CHI	7,296	48.087	12.776	7.740	80.814
Time-invariant variable					
PS	50	-0.004	0.910	-1.464	2.635

Table B1: Pearson correlation among proxies for infectious disease exposure in cross-country sample

	Correlation matrix				
	(1)	(2)	(3)	(4)	(5)
	HPP	HPP_9	LSM	NZ	CombPS
HPP	1.00				
	(0.000)	1.00			
	182				
HPP_9	0.979	1.00			
	(0.000)				
	152	152			
Log Settler Mortality	0.679	0.742	1.00		
	(0.000)	(0.000)			
	86	75	86		
Non-zoonotic	0.826	0.832	0.667	1.00	
	(0.000)	(0.000)	(0.000)		
	181	152	86	181	
CombPS	0.821	0.858	0.724	0.974	1.00
	(0.000)	(0.000)	(0.000)	(0.000)	
	172	146	85	172	172

Notes: The table shows the Pearson correlation coefficients in the first row for each variable, p-value for Bonferroni significance at 0.01-level on the second row, and number of country observations on the third row.

Figure B2: Scatter plot: HPP and Non-zoonotic



Note: The figure shows a scatter plot of our main measure HPP and the related measure Non-zoonotic for 181 countries. Individual countries are identified by three-letter codes.

Figure B3: Scatter plot: HPP and CombPS

Note: The figure shows a scatter plot of our main measure HPP and the related measure CombPS for 172 countries. Individual countries are identified by three-letter codes.

Table B2: Robustness III: Double interactions

	DV: CHI				
	(1)	(2)	(3)	(4)	(5)
	All years		2020	2021	2022
$D_{it} * HPP_i$	0.807	0.837	1.228	0.119	0.653
	(0.134)***	(0.152)***	(0.507)**	(0.318)	(0.821)
	[0.287]***	[0.323]***	[0.516]**	[0.247]	[0.683]
$D_{nt} * HPP_i$	0.719	0.377	1.142	-0.279	-0.453
	(0.185)***	(0.208)*	(0.796)	(0.511)	(1.115)
	[0.460]	[0.531]	[0.825]	[0.392]	[0.894]
$D_{it} * HPP_i * I_{2020}$		0.090			
		(0.265)			
		[0.452]			
$D_{nt} * HPP_i * I_{2020}$		0.979			
		(0.324)***			
		[0.482]**			
I_{2020}		3.319			
		(0.203)***			
		[0.719]***			
D_{it}	Yes	Yes	Yes	Yes	Yes
D_{nt}	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes
Observations	26,164	26,164	8,352	9,048	8,764
R-squared			0.08	0.09	0.20
Countries	175	175	174	174	175

Notes: The dependent variable is average CHI index observed in during one week in 2020-22. Country-week observations during 156 weeks in 174-175 countries are shown in columns (1)-(2), country-week observations for 2020, 2021 and 2022 respectively in columns (3)-(5). The variables D_{it} and D_{nt} have been included in all regressions but with unreported estimates. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Standard errors clustered at the country level in parentheses (). Standard errors corrected for spatial and temporal autocorrelation in brackets [] using a spatial cutoff of 1000 km.

Table B3: Robustness IV: Alternative coordinates for spatial autocorrelation in Table 1

	DV: CHI Index				
	(1)	(2)	(3)	(4)	(5)
	All years		2020	2021	2022
D_{it}	1.584***	1.679***	1.660***	0.920***	1.614***
	(0.177)	(0.178)	(0.386)	(0.131)	(0.418)
D_{nt}	3.414***	3.464***	3.568***	0.794***	5.261***
	(0.277)	(0.280)	(0.529)	(0.219)	(0.566)
$D_{it} * HPP_i$	1.108***	0.938***	1.830***	0.016	0.497
	(0.268)	(0.299)	(0.538)	(0.204)	(0.550)
$D_{it} * HPP_i * I_{2020}$		0.733**			
		(0.345)			
I_{2020}		3.312***			
		(0.729)			
Country FE	Yes	Yes	Yes	Yes	Yes
Observations	26,164	26,164	8,352	9,048	8,764
R-squared	0.13	0.14	0.08	0.09	0.20
Countries	175	175	174	174	175

Notes: The table is identical to Table 1 except for a different calculation of reported standard errors based on the coordinates of country capital cities instead of standard (centroid) coordinates for countries. The dependent variable is average CHI index observed in during one week, 2020-22. Country-week observations during 156 weeks in 174-175 countries are shown in column (1), country-week observations for 2020, 2021 and 2022 respectively in columns (2)-(4). A constant with unreported estimates has been included in all regressions. *** p<0.01, ** p<0.05, * p<0.1. Standard errors corrected for spatial and temporal autocorrelation in brackets (), using coordinates for capital cities and a spatial cutoff of 1000 km.

Table B4: Robustness V: Alternative distance cutoff for spatial autocorrelation in Table 1

	DV: CHI Index				
	(1)	(2)	(3)	(4)	(5)
	All years		2020	2021	2022
D_{it}	1.584***	1.679***	1.660***	0.920***	1.614***
	(0.176)	(0.176)	(0.389)	(0.129)	(0.401)
D_{nt}	3.414***	3.464***	3.568***	0.794***	5.261***
	(0.309)	(0.312)	(0.621)	(0.227)	(0.544)
$D_{it} * HPP_i$	1.108***	0.938***	1.830***	0.016	0.497
	(0.279)	(0.313)	(0.581)	(0.209)	(0.557)
$D_{it} * HPP_i * I_{2020}$		0.733**			
		(0.349)			
I_{2020}		3.312***			
		(0.936)			
Country FE	Yes	Yes	Yes	Yes	Yes
Observations	26,164	26,164	8,352	9,048	8,764
R-squared	0.13	0.14	0.08	0.09	0.20
Countries	175	175	174	174	175

Notes: The table is identical to Table 1 except for a different assumption of distance cutoff in calculation of standard errors corrected for spatial autocorrelation. The dependent variable is average CHI index observed in during one week, 2020-22. Country-week observations during 156 weeks in 174-175 countries are shown in column (1), country-week observations for 2020, 2021 and 2022 respectively in columns (2)-(4). A constant with unreported estimates has been included in all regressions. *** p<0.01, ** p<0.05, * p<0.1. Standard errors corrected for spatial and temporal autocorrelation in brackets (), using standard coordinates for countries and a spatial cutoff of 2000 km.