Death Investigation Systems, Coroner Partisanship and Reported COVID-19 Mortality

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Abstract

The politicization of the COVID-19 pandemic in the United States has raised questions about the integrity and accuracy of death reporting, particularly in jurisdictions with elected, partisan coroners. Using mortality data from the CDC and manually collected data on county-level death certification systems and coroner party affiliation where applicable, I examine the parallel systems of appointed medical examiners and elected coroners and investigate the effect of partisanship on reported COVID-19 deaths. Cross-sectional comparisons do not seem to suggest counties with coroners report fewer deaths than those with medical examiners, and difference-in-differences specifications reveal limited evidence of a statistically significant but not economically meaningful effect of partisanship on reported COVID death counts.

1 Introduction

Since the initial outbreak of COVID-19 (Coronavirus Disease 2019) in the United States in 2020, measuring deaths that result directly or indirectly from the pandemic has become an essential part of accurately assessing its impact (Zylke & Bauchner, 2020). Challenges abound, and official death numbers are widely believed to be a massive undercount of the true toll (Kiang et al., 2020; Stokes, Lundberg, Bor & Bibbins-Domingo, 2021; Woolf et al., 2020; Woolf et al., 2021). One of those challenges involves the proper attribution of deaths to COVID-19. Public health experts have underscored the importance of death certification in informing the public and guiding the policy response to the pandemic, and identified drawbacks in the current system, such as the lack of COVID testing in many cases and the inadequate training received by death certifiers (Gill & DeJoseph, 2020; Stokes, Lundberg, Bor & Bibbins-Domingo, 2021).

Concerns have been raised, and anecdotal evidence documented, about the fragmented death investigation systems in the United States. The American population is served by a mix of coroner and medical examiner (ME) systems at both state and local levels. Medical examiners are appointed officials who are medical professionals that received specialized training in death certification and forensic autopsy. In contrast, coroners are usually elected and politically partisan, and often laypeople who are neither trained in death certification nor medicine at large (Hanzlick & Combs, 1998; Institute of Medicine (US) Committee for the Workshop on the Medicolegal Death certification System [IOM Committee], 2003). This lack of professional knowledge and training is sometimes assumed to lead to a larger number of unattributed COVID-19 deaths in jurisdictions with coroners compared to those with MEs (Stokes, Lundberg, Bor & Bibbins-Domingo, 2021). Possibly exacerbating the problem is the politicization of the pandemic in the United States, with views clearly divided along party lines among both elected officials and the public. Some media reports and analysis have pointed to possible underreporting of COVID deaths by elected Republican Party coroners out of political motivations (Bergin et al., 2021; "Politics of Death", 2022). A Republican coroner of a Missouri county reportedly said his office "[didn't] do COVID deaths", and attributed no deaths to COVID-19 in 2021 (Bergin et al., 2021). Such reports suggest the possibility that partisan politics may incentivize partisan coroners to manipulate death numbers in their jurisdictions.

Despite the theorizing and anecdotes, to my knowledge, no empirical analysis has been conducted on the effect of coroner partisanship on COVID-19 death reporting. A major obstacle may be data availability on partisanship: states and counties differ vastly in whether and how information about coroner elections and party affiliations is made available, and a comprehensive account of the nationwide picture would require extensive manual data collection and verification. In this paper, I employ an original, manually-collected data set containing such information, combine it with mortality data from the United States Centers for Disease Control and Prevention (CDC) and investigate the effect of death certification systems and partisanship. Taking into consideration the interval-censored nature of the mortality data due to privacy concerns, I estimate both regular, left-censored tobit models and interval-censored models to account for suppressed death counts, and also use the probability of having a low and suppressed death count as a dependent variable in alternative specifications. A cross-sectional comparison of coroner and ME counties does not reveal different levels of underreporting, and my models produce mixed evidence on systemic underreporting in Republican-coroner counties compared to Democratic-coroner ones.

This paper contributes to a nascent and growing literature that seeks to understand the toll of COVID-19 in America. A major focus of the literature is to estimate excess deaths, i.e. the difference between actual and expected numbers of deaths (Zylke & Bauchner, 2020). When performed at subnational levels, these exercises consistently reveal large geographic variations in the percentage of excess deaths not directly attributed to COVID-19, as pointed out by Woolf et al. (2020) and Woolf et al. (2021) at the state level, and by Stokes, Lundberg, Elo, et al. (2021) and Ackley et al. (2022) at the county level, to name a few examples. My work partially aims to test one possible explanation for such disparity. It is also closely related to research that examines the politicization of the COVID-19 pandemic in the United States and its public health consequences, ranging from individual behaviors (e.g. Allcott et al., 2020; Grossman et al., 2020) to local government policy (e.g. Holman et al., 2020) to possible fraudulent death reporting practices (Eutsler et al., 2023). This paper is the first to focus on the death investigation system and seeks to test the theory of political influence on death certifiers.

The rest of the paper is structured as follows. Section 2 reviews the facts and findings on the politicization of COVID-19 in the United States and outlines the country's two main types of death investigation system. Section 3 provides a summary of the data and sample. Section 4 describes the empirical methodology. Section 5 presents the results and offers possible interpretations. Section 6 discusses the findings and Section 7 concludes.

2 Background

Politicization of COVID-19 in the United States

The emergence of COVID-19 in the United States coincided with a period of intense political polarization in the country. Unsurprisingly, the public discourse on the pandemic also largely evolved into a polarized political debate during its course. Ever since the virus's initial outbreak, top-ranking and high-profile Republican Party figures repeatedly downplayed the threat posed by the virus, endorsed conspiracy theories and pseudoscientific treatments, used racist language to refer to the disease, contradicted public health recommendations issued by the CDC, and dismissed Democratic officials' concerns and policy responses as political stunts (Bolsen & Palm, 2022; Halpern, 2020). Both polarization and politicization were also amplified by the news media (Hart et al., 2020); conservative media in particular spread and promoted COVID-related misinformation (Motta et al., 2020).

As a consequence, perceptions, attitudes and behavior among the American public all displayed sharp partisan divisions. Two longitudinal and cross-national studies conducted by Stroebe et al. (2021) show that the extent of such politicization increased over time, and was greater in the U.S. than in a comparison group of countries. Compared to self-identified liberals or Democrats, conservatives or Republicans were less concerned about the health risk posed by COVID-19 and less trusting in mainstream media's reporting on the pandemic and public health recommendations from medical experts (Allcott et al., 2020; Kerr et al., 2021; Rothgerber et al., 2020). They also reported less adherence to health-protection protocols such as hand-washing, quarantining, mask-wearing and social distancing (Allcott et al., 2020; Rothgerber et al., 2020; Kerr et al., 2021; Stroebe et al., 2021), were more skeptical of and less likely to receive COVID vaccines (Bolsen & Palm, 2022), and less supportive of aggressive government policies both in pandemic control and on related public issues (Gadarian et al., 2021). Moreover, Gadarian et al. (2021) find that the partisan divide cannot be fully explained by other correlating variables, such as consumption of conservative media or the local COVID-19 death toll.

The self-reported differences are corroborated by empirical data. Google search data indicates Democrats showed greater interest in social distancing (Grossman et al., 2020). Using mobile phone location data, Allcott et al. (2020), Grossman et al. (2020) and Gollwitzer et al. (2020) all find that residents in U.S. counties with higher Democratic vote shares in the 2016 presidential election were more likely to practice social distancing and comply with stay-at-home orders. These disparities between counties are also subsequently linked to higher COVID-19 infection and death rates in Republican-leaning counties (Gollwitzer et al., 2020).

Furthermore, local policymaking seems to reflect the partisan differences, too. Democratic governors were generally more prompt in adopting a variety of social-distancing policies than Republican ones (Adolph et al., 2021; Grossman et al., 2020). Holman et al. (2020) find the ideological leaning of local populations to be one of the factors that affected how early municipal governments issued stay-at-home orders. There is also evidence on political influences on COVID death reporting: Eutsler et al. (2023) employ Benford's law, a phenomenon observed in naturally occurring numerical data sets, and find evidence of underreporting of COVID-19 deaths; in addition, the extent of such underreporting in a county is related to the county's partisan leaning in the 2016 presidential election vote as well as the party affiliation of the state governor. All of this points to the possibility of a partisan line that divides death investigation systems in America as well.

Death certification during COVID-19 in the United States

Death certificates are a crucial source of information about public health. During the COVID-19 pandemic, data from death certificates formed the basis of the mortality statistics published by the CDC's National Center for Health Statistics, and informed national and subnational monitoring of the pandemic's progression and severity (Gill & DeJoseph, 2020). Normally, natural-cause deaths (such as those resulting from viral infections) that occur in the hospitals or long-term care/hospice facilities are certified by a facility physician (Bhullar et al., 2022; Gill & DeJoseph, 2020). These deaths thus do not require reporting to medical examiner or coroner (ME/C) offices, which together form the medicolegal death investigation system in the United States and are legally mandated to investigate unnatural or unexpected deaths (IOM Committee, 2003). Nevertheless, ME/Cs played an important role in certifying COVID-related deaths during the pandemic for a number of reasons. Firstly, all deaths that take place outside hospitals or care facilities are reportable to ME/C offices (Bhullar et al., 2022), and such deaths accounted for a meaningful portion of COVID deaths (Pathak et al., 2021). More importantly, while statutes that specify the types of death reportable to a ME/C differ by jurisdiction, most jurisdictions require reporting of deaths that involve diseases that may constitute a threat to public health, shifting confirmed and suspected COVID deaths into ME/Cs' purview, and allowing ME/Cs to revise death certificates when necessary (Gill & DeJoseph, 2020; Kiang et al., 2020; National Vital Statistics System, 2023). In practice, ME/Cs are extensively involved in certifying and counting COVID-related deaths (Zavattaro, 2023).

ME and coroner systems coexist in the United States today, and the type of office overseeing medicolegal death investigation varies by state and county. According to the CDC (2023b), 22 states and the District of Columbia exclusively use ME systems; among them, 16 states and D.C. have a centralized system at the state level, and 6 have a county- or district-based system. The remaining 28 states use coroner systems in at least some parts of the state, with 14 of them using a county- or district-based coroner system, and the other 14 using a county- or district-based system with a combination of MEs and coroners. The coroner system originated in 9th- or 10th-century England, and its current use in the United States is a vestige of the British colonist era (Hanzlick & Combs, 1998). The modern incarnation of the ME systems first emerged in 1877, and it is the consensus among today's public health experts that ME systems are clearly preferable to coroner systems (IOM Committee, 2003), but the latter persist. From the 1960s to the 1980s, a period of rapid transition from coroner systems to ME systems took place nationwide, followed by a "lull in the action" starting in the 1990s (Hanzlick, 2007); in recent years, the pace of conversion seems to be picking up again (Denham et al., 2022).

Two main differences distinguish ME and coroner systems from each other, and both point to reasons for the former's advantage over the latter. The first difference concerns qualification and professionalism. ME offices are held by medical professionals—usually physicians, often pathologists or forensic pathologists—who additionally receive special training and certification in death investigation (Hanzlick & Combs, 2007; IOM Committee, 2003). This type of training is seldom provided in medical schools or healthcare facilities, making MEs more competent death certifiers than other healthcare professionals. On the other hand, neither such qualification nor training is required of coroners, and they are almost always laypeople who need as little as a high school diploma to qualify for the job (Choi & Gulati, 2017; IOM Committee, 2003). The deficiency in knowledge and training makes coroners less capable of the task of investigating deaths: coroner systems have been found to be less efficient and more error-prone than ME systems (Choi & Gulati, 2017; Denham et al., 2022; Flynn, 1955).

The second difference that sets the two systems apart pertains to the method of selection for each type of office. MEs are appointed, whereas coroners are usually elected (IOM Committee, 2003). The perceived electoral mandate of coroners is one of the main arguments made against the conversion to ME systems (Flynn, 1955; IOM Committee, 2003). But the flip side of representing the will of the electorate is that the electoral system provides incentives for coroners to respond to political pressure, and voters' demands may not always align with what is good for society (Choi & Gulati, 2017; "Politics of Death", 2022). In the case of COVID-19, where politics and public health have become so inextricably intertwined, it is conceivable that the conflict of interests can lead elected coroners to make questionable decisions. Given their authority in death certification, coroners may, for instance, choose to omit COVID-19 as a cause of death in the absence of a positive laboratory test, even though CDC guidelines say it should be listed as long as certain clinical criteria are met; they may also remove COVID-19 from a death certificate bowing to pressure from the family of the deceased (Bergin et al., 2021; Bordelon, 2021; "Politics of Death", 2022). In their Benford's law analysis, Eutsler et al. (2023) find descriptive evidence that counties with MEs were less likely to see politically-motivated underreporting than those with coroners. Some descriptive works that estimate excess deaths from COVID-19, such as Paglino et al. (2023), point out that regions with higher discrepancies between reported COVID deaths and estimated excess deaths were more likely to have coroners rather than MEs.

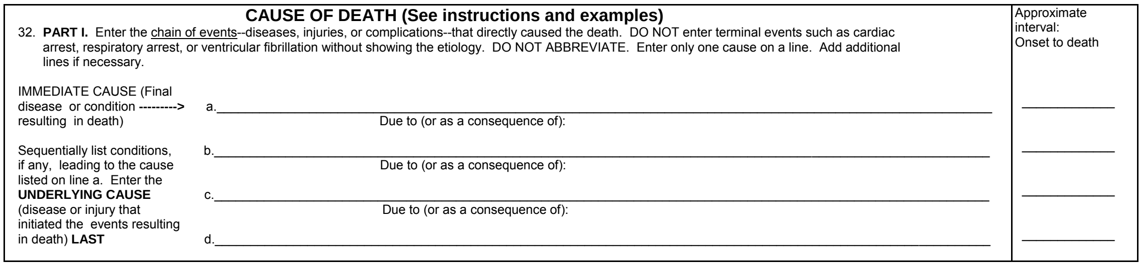
Laws governing ME/C offices vary greatly by state, and the offices differ not only in the manner of selection of ME/Cs, but also in their structure, operation and procedures (CDC, 2023b; Hanzlick, 1993). The United States Department of Justice conducts five-yearly censuses of ME/C countries nationwide. The most recent one, conducted in 2018 (Brooks, 2021), shows that ME/C offices vary in size of jurisdiction, manpower, budget and caseload, all of which are positively correlated. For example, on average, coroner offices serving counties with a population larger than 250,000 have a staff of 15 people, whereas those in counties with a population smaller than 25,000 only employ two people. Employees of the office act on behalf and under the authority of the ME/C, who serves as the primary death investigator and is responsible for completing the death certificate (Hanzlick, 1996). It is customary in the literature examining the effect of death investigation systems to treat the ME/C office as a unitary entity without distinguishing between the roles of the overseeing ME/C and other staff members of the office (e.g. Denham et al., 2022; Klugman et al., 2013), and I adopt the same approach in this paper.

3 Data

The data used for my analysis mainly consists of two parts. Mortality data comes from the Wide-ranging ONline Data for Epidemiologic Research (WONDER) system maintained by the CDC (2023a), and includes monthly all-cause (i.e. total) death and COVID death counts at the county level in 2020 and 2021, along with each county's urban–rural classification according to the National Center for Health Statistics' 2013 scheme (the most recent update to the scheme). While CDC WONDER contains arguably the highest-quality mortality data for the United States, its privacy rules introduce one complication to my analysis. In order to avoid revealing individual identities, CDC wonder suppresses death counts when they are below 10; instead of the actual value, the death count is replaced by a dedicated code. Counts of zero are not suppressed. The suppression hence results in interval-censored data, with death counts between 1 and 9 concealed. I will address this issue when I introduce my empirical models in Section 4.

I include three types of death counts in my data: all-cause deaths, deaths where COVID-19 is listed as a cause of death, and deaths where COVID-19 is listed as the *underlying* cause of death. On the U.S. standard death certificate, up to 20 causes of death can be listed to form a "chain of events that directly cause the death" (see Figure 1). The first listed cause is the immediate one, and the last the underlying one, which initiated the chain. The main analysis focuses on deaths where COVID-19 is mentioned anywhere in the list of causes, i.e. the most broadly-defined COVID deaths based on death certificate data. Death counts with COVID-19 listed as the underlying cause are used for supplemental analysis.

Additionally, I obtained data on types of death investigation office and party affiliations of elected coroners in both 2017 and 2021. This data is generously provided by by Matthew Isbell, a political data analyst. He manually compiled the data from state and local government websites and directories depending on where states store and publish such information, and crosschecked it with multiple sources, including the CDC (2023b). To the best of my knowledge, this is the first time such data has been gathered and used in empirical research:



CAUSE OF DEATH (See instructions and examples)

32. **PART I.** Enter the chain of events--diseases, injuries, or complications--that directly caused the death. DO NOT enter terminal events such as cardiac arrest, respiratory arrest, or ventricular fibrillation without showing the etiology. DO NOT ABBREVIATE. Enter only one cause on a line. Add additional lines if necessary.

		Approximate interval: Onset to death
IMMEDIATE CAUSE (Final disease or condition ---------> resulting in death)	a.________ Due to (or as a consequence of):	________
Sequentially list conditions, if any, leading to the cause listed on line a. Enter the **UNDERLYING CAUSE** (disease or injury that initiated the events resulting in death) **LAST**	b.________ Due to (or as a consequence of):	________
	c.________ Due to (or as a consequence of):	________
	d.________	________

Figure 1: Part of the U.S. standard certificate of death

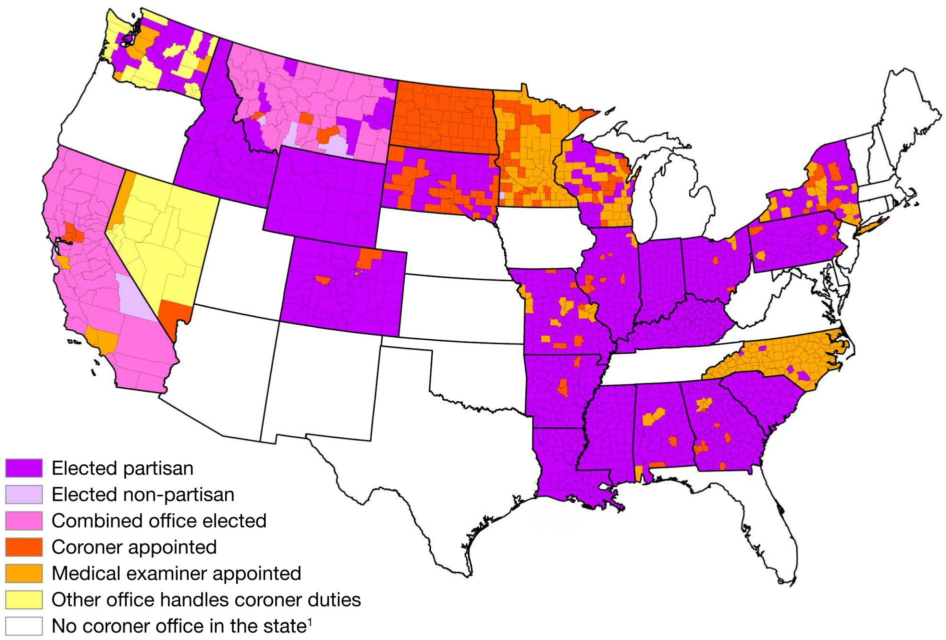


Figure 2: Type of death investigation office and method of selection in 2021

while earlier research on the U.S. death investigation systems at the local level, such as Denham et al. (2022), has utilized data on office type, mine is the first to focus on the effect of coroner partisanship.

Figures 2 and 3 provide a visualization of the data Isbell collected. In these maps, I only highlight states with at least one coroner office; the rest have either statewide or county/district-based ME systems.[1] As of 2021, 1,276 counties across America elected coroners, with all but six conducting the elections in a partisan manner. A small number (176) of counties appoint rather than elect coroners, including the entirety of North Dakota. Repub-

[1] The highlighted states in the maps exclude three states despite their being listed in CDC (2023b) as ones with coroner or mixed ME/C systems. Texas has a mixed system, with medical examiners in some counties and the office of justice of the peace handling coroner duties in others. In Nebraska, county attorneys perform coroner duties. Kansas has a district-based system. These states are excluded from the empirical analysis, as none have strictly-defined, county-based coroners.

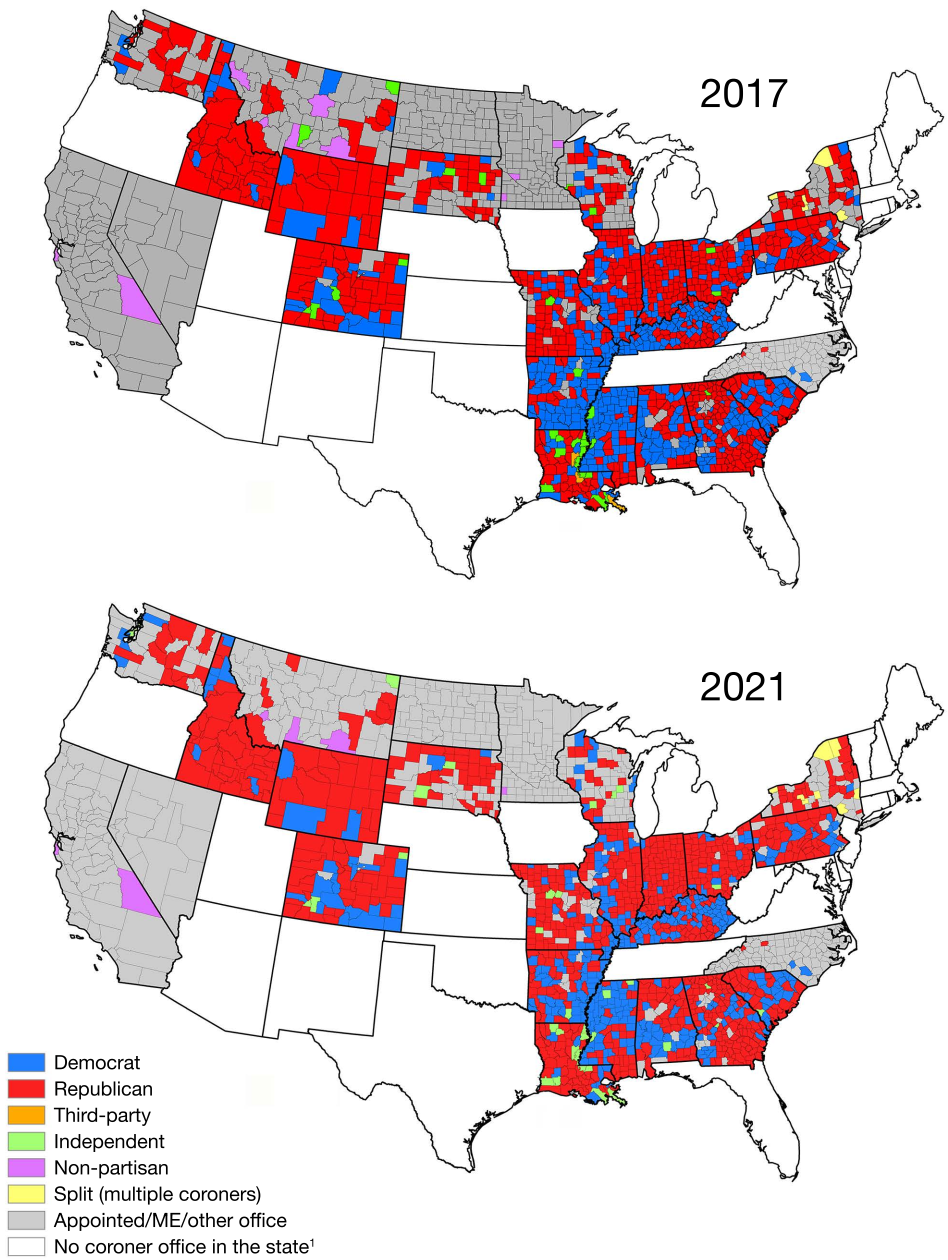


Figure 3: Partisan makeup of death investigation offices in 2017 and 2021

licans held more coroner offices than Democrats in both years, and made moderate gains in the 2020 election. Among coroner offices held by either of the two major parties in both 2017 and 2021, Democrats and Republicans held on to 352 and 725 counties, respectively, after the election; 119 flipped from Democrats to Republicans, and 20 in the opposite direction.

I augment the mortality and ME/C office data with two additional sources. The mid-2020 county resident population estimates from the United States Census Bureau (2022) are used to derive demographic characteristics of counties, including the shares of gender, race and age groups in the population. The 2020 county-level presidential election results are obtained from the MIT Election Data and Science Lab (2018).[2]

My main sample consists of 2,367 counties over 22 months, from March 2020 to December 2021, for a total of 52,074 observations. The counties are from 46 states and the District of Columbia. Three states are excluded as explained in footnote 1. Among the non-ME counties, I only include those with elected partisan coroners, and drop those with appointed coroners, elected non-partisan coroners and combined offices to allow for straightforward comparisons. As can be seen in Figure 2, this selection criterion excludes the entire state of North Dakota (which appoints coroners), a majority of counties in California, Montana (both of which elect combined offices) and Nevada (where coroners' offices are combined with sheriffs' offices [CDC, 2023b]), about half of Washington and South Dakota, a third of Minnesota, and a few counties in eight other states. Some counties in Alaska and Hawaii are also excluded because of non-perfect matching between jurisdiction definitions from the three different sources in my data set. Five counties in the sample reported zero COVID-19 deaths in 2020 and 2021. Among the 2,367 counties, 1,270 (53.7%) have elected partisan coroners, and the remaining 1,097 (46.3%) have MEs. Table 1 shows the partisan breakdown within the coroner counties before and after the 2020 election. As pointed out previously, coroners are more likely to be Republican than Democratic.

Table 2 summarizes and compares characteristics both between ME and coroner counties, and between Democratic-coroner and Republican-coroner counties in 2021. Both pairs of groups are largely similar in terms of age structure and sex ratio, and any significant differences are small in magnitude. The racial composition is different within each pair: ME counties tend to have fewer black residents and more Hispanic ones than coroner counties; Republican-coroner counties have much more white residents and much fewer black ones than Democratic-coroner counties. Politically, as expected, Republican-coroner counties were more in favor of Donald Trump, the incumbent president and Republican presidential

[2]In addition, I manually collected 2020 county coroner election results, where possible, from various state and county sources. However, because a vast majority of coroner races were uncontested, the results are not very informative for my purposes and therefore not included in the data.

		Democrat	Republican	Other/No party	Split
Year	2017	478 (37.6%)	755 (59.4%)	32 (2.5%)	5 (0.4%)
	2021	374 (29.4%)	853 (67.2%)	35 (2.8%)	8 (0.6%)

Table 1: Partisan makeup of elected partisan coroners

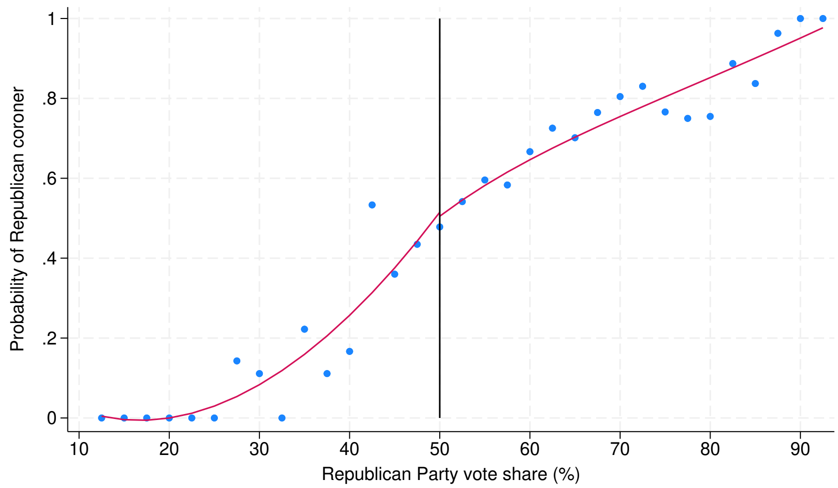


Figure 4: 2020 county Republican vote share and coroner partisanship

nominee, than Democratic-coroner ones in the 2020 election; ME counties were more supportive of Joe Biden, the Democratic nominee, and saw a greater leftwards shift compared to 2016 than coroner counties. Geographically, ME counties are more likely located in the Northwest and less likely in the Midwest; Republican-coroner counties are much more likely to be in the Midwest and much less likely to be in the South. Finally, ME counties are more likely to be metropolitan, defined as being in a metropolitan statistical area. It is also worth pointing out that while the average population is marginally different between Democratic- and Republican-coroner counties, a closer look at their distributions reveals that both include counties of all sizes in roughly similar proportions. This alleviates concerns about different practices between offices in large and small jurisdictions described at the end of Section 2, as any effect of such differences should be balanced out between the two parties.

Figure 4 plots the probability of a county coroner being Republican against the county's Republican Party vote share (between the two major parties) in the 2020 presidential election. Coroner party affiliation is highly correlated with presidential vote share, but a regression-discontinuity (RD) style quadratic fit shows no discrete jump at a cutoff of 50% vote share. This indicates ticket splitting between the presidential candidate and the coroner candidate was common, which was in turn likely due to the large proportion of uncontested coroner races. The close relationship between the two variables, shown here and in Table 2, calls for

	Office type			Coroner party (2021)		
	Coroner	ME	Difference (ME – C)	Democrat	Republican	Difference (R – D)
Population, Jul 2020 est.	61,322	165,426	104,104***	70,858	57,926	−12,933*
	(109,287)	(468,183)	(13,545)	(150,140)	(87,539)	(6,848)
Share of age group (%):						
0 to 19	24.658	23.708	−0.950***	24.346	24.819	0.474**
	(3.147)	(3.478)	(0.136)	(2.947)	(3.182)	(0.193)
20 to 29	12.034	11.973	−0.061	12.491	11.813	−0.677***
	(2.822)	(3.024)	(0.120)	(3.175)	(2.629)	(0.174)
30 to 49	23.702	23.627	−0.075	23.669	23.718	0.049
	(2.325)	(2.793)	(0.105)	(2.288)	(2.315)	(0.143)
50 to 64	20.430	20.627	0.196**	20.412	20.445	0.033
	(2.083)	(2.390)	(0.092)	(2.050)	(2.105)	(0.130)
65 to 74	11.447	11.894	0.447***	11.429	11.445	0.015
	(2.230)	(2.874)	(0.105)	(2.075)	(2.292)	(0.138)
75 to 84	5.692	6.002	0.310***	5.646	5.713	0.067
	(1.258)	(1.774)	(0.063)	(1.254)	(1.265)	(0.078)
85 and above	2.037	2.170	0.133***	2.007	2.047	0.040
	(0.621)	(0.704)	(0.027)	(0.588)	(0.632)	(0.038)
Share of male (%)	50.230	50.207	−0.023	49.917	50.327	0.410***
	(2.325)	(2.056)	(0.091)	(2.409)	(2.221)	(0.141)
Share of racial group (%):						
White	82.712	83.794	1.083	73.836	86.988	13.153***
	(18.049)	(15.081)	(0.690)	(23.773)	(12.587)	(1.042)
Black	13.010	8.796	−4.214***	22.145	8.689	−13.456***
	(17.896)	(12.289)	(0.641)	(24.080)	(11.901)	(1.029)
Hispanic	5.754	8.989	3.235***	5.499	5.901	0.402
	(6.867)	(11.337)	(0.380)	(7.302)	(6.763)	(0.430)
2020 pres. vote share (%):						
Democratic (Biden)	28.313	33.315	5.003***	34.963	24.959	−10.004***
	(14.188)	(16.337)	(0.628)	(17.049)	(11.368)	(0.828)
Republican (Trump)	61.256	51.122	−10.134***	53.237	64.959	11.721***
	(17.626)	(18.493)	(0.743)	(19.938)	(15.323)	(1.046)
Shift to GOP vs 2016	−3.728	−7.146	−3.418***	−3.458	−3.999	−0.541
	(10.988)	(11.900)	(0.471)	(9.266)	(11.822)	(0.689)
Region (1 = yes):						
Northeast	0.072	0.105	0.033***	0.048	0.076	0.028*
	(0.258)	(0.306)	(0.012)	(0.214)	(0.265)	(0.016)
Midwest	0.339	0.268	−0.071***	0.217	0.400	0.183***
	(0.474)	(0.443)	(0.019)	(0.412)	(0.490)	(0.029)
South	0.469	0.509	0.039*	0.650	0.390	−0.259***
	(0.499)	(0.500)	(0.021)	(0.478)	(0.488)	(0.030)
West	0.120	0.119	−0.001	0.086	0.134	0.048**
	(0.325)	(0.323)	(0.013)	(0.280)	(0.340)	(0.020)
Metro area (1 = yes)	0.354	0.470	0.117***	0.318	0.374	0.056*
	(0.478)	(0.499)	(0.020)	(0.466)	(0.484)	(0.030)
n	1,270	1,097	—	374	853	—

Levels of significance: *** = .01, ** = .05, * = .10.

Table 2: Descriptive statistics for sub-samples

controlling for the presidential vote in my empirical specification. On the other hand, the lack of a jump at the 50% level precludes the adoption of a fuzzy-RD design.

Before moving on to econometric analysis, I take inspiration from works like Eutsler et al. (2023) and Campolieti (2022) and check for signs of underreported COVID-19 deaths using a mathematical tool. Benford (1938) described the following phenomenon. In many sets of naturally-occurring numbers, the leading digits follow a probability distribution where the smaller numbers occur more often than larger numbers: the probability of the leading digit being $d \in [1, 9]$ equals $P(d) = \log_{10}\left(1 + \frac{1}{d}\right)$, with $P(1) \approx 0.301$, $P(2) \approx 0.176$, ..., and $P(9) \approx 0.046$. Similar distributions also exist for non-leading digits: for example, 0 is the most likely second digit (probability 0.120), and 9 the least likely (probability 0.085). Deviations from the Benford distributions in supposedly naturally-occurring data may indicate data manipulation, and Benford's law has been successfully used to detect fraud in a wide range of contexts (Mebane, 2006; Nigrini, 2012). Eutsler et al. (2023) argue that daily COVID death counts are likely to meet the necessary conditions for the data to satisfy the law, and find higher frequencies for small leading digits in the reported death counts than the theoretical distribution predicts, pointing to possible underreporting. In the case of monthly CDC WONDER data, despite the censoring of death counts below 10, the law should still hold for the remaining data with higher orders of magnitude (Benford, 1938) and, in any case, for the second digits of the death counts as they are not affected by the censoring.

Figure 5 illustrates the comparison of observed frequencies for the leading and second digits with the theoretical Benford's law distributions across subsamples grouped by office type, or by the party affiliation of coroner in each year (for example, the subsample "Democratic 2020" only includes death counts in the year of 2020 in counties with a Democratic coroner in that year). Two patterns stand out from the charts. First of all, for both the first and second digits, low (high) digits appear more (less) frequently in COVID death counts than predicted by the corresponding Benford distribution, but the observed and theoretical distributions seem to match much better for all-cause death counts, confirming the applicability of Benford's law to my data set while indicating possible underreporting of COVID deaths. Secondly, the extent of deviation of COVID death counts from Benford distributions is similar across all subsamples, and there does not appear to be clear visual evidence suggestive of systemic underreporting by either office type or either party in either year.

Nigrini (2012) proposes the use of the mean absolute deviation (MAD) as a testing

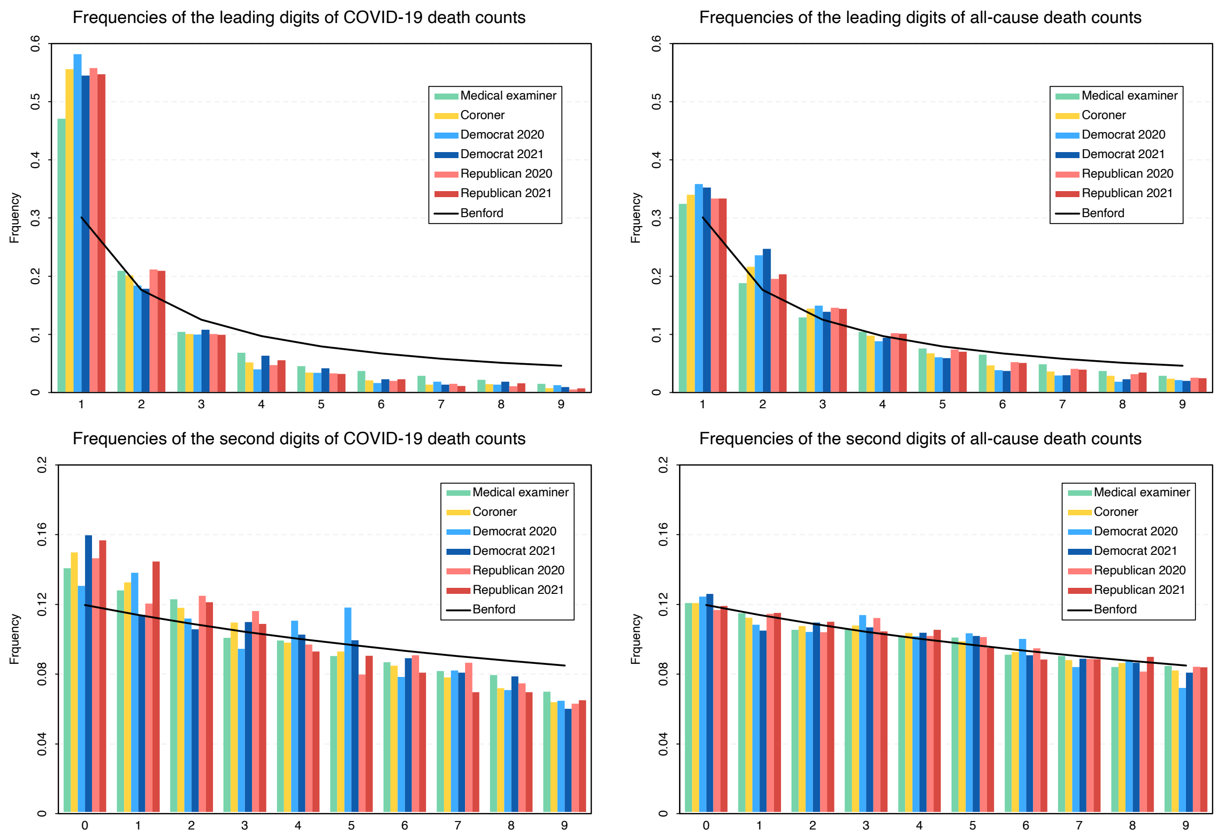


Figure 5: Observed frequencies of leading and second digits vs. Benford's law distributions

statistic for conformity to Benford's law. It is defined as

$$MAD = \frac{1}{D}\sum_{d=1}^{D}|OP_d - TP_d|\,,$$

where D is the total number of possible digits in the position (9 for the leading digit and 10 for all thereafter), and OP_d and TP_d are the observed and theoretical proportions of digit d. The smaller the statistic, the closer the conformity. In Table 3, I calculate the MAD for both the leading and the second digits across all subsamples and compare them to Nigrini's (2012, p.160) critical values for different levels of conformity. The statistical tests reported in panels (a) and (b) confirm my visual observation: the MAD is vastly higher for COVID-19 deaths than for all-cause deaths in every case. Tests for both the leading and second digits also indicate that ME counties are subject to less underreporting than coroner counties. In terms of coroner party affiliation, however, neither party seems to reliably and significantly outperform the other in both years. Panel (c) focuses on the leading digits of COVID death counts in major-party-coroner counties, and calculates the MAD in each year for counties

(a)

Leading digits:	ME	Coroner	Dem 2020	Dem 2021	Rep 2020	Rep 2021
COVID deaths	0.0452$^{\times}$	0.0623$^{\times}$	0.0642$^{\times}$	0.0547$^{\times}$	0.0650$^{\times}$	0.0621$^{\times}$
All-cause deaths	0.0104**	0.0220$^{\times}$	0.0315$^{\times}$	0.0303$^{\times}$	0.0172$^{\times}$	0.0182$^{\times}$

(b)

Second digits:	ME	Coroner	Dem 2020	Dem 2021	Rep 2020	Rep 2021
COVID deaths	0.0098**	0.0127$^{\times}$	0.0140$^{\times}$	0.0101*	0.0123$^{\times}$	0.0170$^{\times}$
All-cause deaths	0.0019**	0.0020***	0.0059***	0.0036***	0.0033***	0.0020***

(c)

Leading digits:	Dem hold	Dem to Rep	Rep hold	Rep to Dem
2020	0.0605$^{\times}$	0.0807$^{\times}$	0.0654$^{\times}$	0.0581$^{\times}$
2021	0.0603$^{\times}$	0.0800$^{\times}$	0.0601$^{\times}$	0.0339$^{\times}$

Levels of conformity: *** = close conformity, ** = acceptable conformity, * = marginally acceptable conformity, $^{\times}$ = nonconformity

Table 3: Mean absolute deviation (MAD) test for Benford's law conformity

that saw a party switch and those that didn't. The tests again suggest non-conformity across the board, although there now appears to be indicative evidence of a pre-existing gap between switch and no-switch counties: Democratic-coroner counties that flipped Republican already had a higher MAD in 2020 than those that didn't flip, and Republican-coroner ones that flipped had a lower MAD in 2020 than their no-flip counterparts. However, the gaps seem to have persisted in 2021 without growing in size (a smaller MAD in Republican-to-Democratic counties in 2021 is likely due to a small sample size of 110), suggesting the actual party switch did not exacerbate the pre-existing differences. Instead, these differences are probably attributable to factors other than the death investigation system, such as public attitudes towards COVID death certification and prevalent practice in the healthcare facilities.

4 Empirical Methodology

OLS Difference-in-differences framework

I employ a difference-in-differences strategy in my empirical analysis. In order to examine the effect of coroner partisanship on reported deaths, I leverage party flips in coroners' offices in the 2020 election. Because of the minimal number of counties that had third-party/independent coroners or offices with split party control, it becomes impractical to estimate the effect of party flips either to and from these affiliations. Hence I confine the analysis to counties where the coroner belonged to one of the two major parties in both years. This leaves me with 1,216 counties (out of 1,270 coroner counties) for a total of

26,752 observations. The basic diff-in-diff specification is given by the regression model

$$\begin{aligned} y_{it} = \alpha_0 + \alpha_t + \eta \times Post_t + \beta_1 \times DtoR_i + \beta_2 \times DtoR_i \times Post_t \\ + \gamma_0 \times R_i + \gamma_1 \times RtoD_i + \gamma_2 \times RtoD_i \times Post_t + Z_{it}'\theta + \epsilon_{it}, \end{aligned}$$

where y_{it} is the death count in county i in month t; α_t is the month fixed effect; $DtoR_i$ and $RtoD_i$ are indicator variables that equal 1 if the coroner's office in county i flipped from Democratic to Republican, or from Republican to Democratic, in the 2020 election, respectively; $R_i = 1$ if county i had a Republican coroner in 2020; $Post_t = 1$ if month t is January 2021 or later, when newly elected coroners were in office; X_{it} is a vector of covariates, including 2020 presidential election vote shares, the shift towards the Republican Party between 2016 and 2020, population and its polynomials (up to third-order) and logarithm, fixed effects for state, urbanization status, and demographic (age, gender, race) groups, as well as region and month fixed effects and their interaction (to roughly account for the different timing of COVID-19 waves in different U.S. regions). The coefficients of interest are β_2 and γ_2, which capture the effects on reported deaths of a Democratic-to-Republican or a Republican-to-Democratic flip, under the identifying assumption of parallel pre-trends between counties that had coroners from the same party in 2020 but different ones in 2021. Because the coroner's office only has an effect on COVID death counts through its capacity for medicolegal death investigation and does not affect local public health policy, and assuming the latter has been sufficiently controlled for using the presidential election vote shares, these coefficients of interest represent the effects of coroner party changes alone.

Given the structure of my data set, I estimate the equivalent (and more symmetric)

$$y_{it} = \alpha_0 + \alpha_t + \eta Post_t + \sum_{k=1}^{3} (\phi_k Change_{ki} + \delta_k Change_{ki} \times Post_t) + Z_{it}'\theta + \epsilon_{it}, \quad (1)$$

where $Change_{1i}$, $Change_{2i}$ and $Change_{3i}$ are a set of indicator variables for the coroner party change in the 2020 election ($k = 1$ means "Republican hold", 2 means "Democratic to Republican flip", 3 means "Republican to Democratic flip", and "Democratic hold" is the omitted group). In this specification, the coefficients of interest are represented by δ_2 and $\delta_3 - \delta_1$, which are equal to β_2 and γ_2 from Equation (1), respectively. I also allow for dynamic treatment effects by estimating the alternative specification

$$y_{it} = \alpha_0 + \alpha_t + \sum_{k=1}^{3} (\phi_k Change_{ki} + \tau_{kt} Change_{ki}) + Z_{it}'\theta + \epsilon_{it}, \quad (2)$$

where the effect of a Democratic-to-Republican (or Republican-to-Democratic) shift in month t compared to the reference month is captured by τ_{2t} (or $\tau_{3t} - \tau_{1t}$).

Considerations about data censoring

As mentioned in Section 3, CDC WONDER censors low death counts due to privacy concerns, i.e. to avoid the identification of individuals. One straightforward solution is to estimate Equation (2) using a standard tobit model (Tobin, 1958) and treating the dependent variable as left-censored below 10.[3] The model, which is included in common statistical packages, is estimated using maximum likelihood estimation (MLE). Without loss of generality and for the sake of simplicity, I can designate all suppressed death counts y_{it} as equal to 9, a value not observed for y_{it} elsewhere in the data. Then, assuming a conditional normal distribution for the true (latent) death count y_{it}^*, the likelihood function for the sample is

$$\mathcal{L}\left(\beta,\sigma\right) = \prod_{y_{it}>9}\left[\frac{1}{\sigma}\varphi\left(\frac{y_{it}-X_{it}'\beta}{\sigma}\right)\right]\prod_{y_{it}\leq 9}\Phi\left(\frac{9-X_{it}'\beta}{\sigma}\right),$$

and the log-likelihood function is

$$\log\mathcal{L}\left(\beta,\sigma\right) = \sum_{y_{it}>9}\log\left[\frac{1}{\sigma}\varphi\left(\frac{y_{it}-X_{it}'\beta}{\sigma}\right)\right] + \sum_{y_{it}\leq 9}\log\Phi\left(\frac{9-X_{it}'\beta}{\sigma}\right), \tag{3}$$

where $\varphi\left(\cdot\right)$ and $\Phi\left(\cdot\right)$ are the probability density function and cumulative density function of a standard normal distribution, respectively.

There are two main drawbacks to this standard approach. Firstly, since death counts of zero are actually not censored, treating them as such means discarding a large amount of information in the data (out of the 52,074 county-month observations, 12,895, or close to a quarter, had a death count of zero). Secondly, the tobit model assumes a conditional normal distribution for the dependent variable, which may not be the most suitable assumption for death counts. I therefore propose two alternative regression models to address the censoring issue.

The first model is a slight modification of the standard tobit model that allows me to make use of the zero counts. The data is treated (correctly) as interval-censored on $[1,9]$. Additionally, under the assumption of normal distribution, any $y_{it}=0$ can be considered a "censored" value for a true $y_{it}^*<0$. The log-likelihood function then becomes

[3] Censored regression models are only suitable for data where the dependent variable is censored based on a fixed threshold, which is the main reason why I use raw death count rather than death rate as the dependent variable.

$$\log \mathcal{L}(\beta,\sigma) = \sum_{y_{it}>9} \log\left[\frac{1}{\sigma}\varphi\left(\frac{y_{it}-X_{it}'\beta}{\sigma}\right)\right] + \sum_{y_{it}=9} \log\left[\Phi\left(\frac{9-X_{it}'\beta}{\sigma}\right) - \Phi\left(\frac{-X_{it}'\beta}{\sigma}\right)\right] + \sum_{y_{it}=0} \log\Phi\left(\frac{-X_{it}'\beta}{\sigma}\right), \quad (4)$$

and I can estimate the coefficients β and σ using MLE.

My second model is a modification of the standard Poisson regression model. I assume that the dependent variable y_{it}^* follows a Poisson instead of normal distribution, a more accurate assumption for death counts (Scott, 1981), and that the logarithm of its conditional expectation is a linear combination of the covariates, i.e. $\log E\left(y_{it}|\, X_{it}\right) = X_{it}'\beta$, or $E\left(y_{it}|\, X_{it}\right) = \exp\left(X_{it}'\beta\right)$. Because the probability mass function of a Poisson distribution with expectation λ is

$$f(k;\lambda) = \frac{\lambda^k e^{-\lambda}}{k!},$$

in the standard Poisson regression model, which is also estimated using MLE, the likelihood function would be given by

$$\mathcal{L}(\beta) = \prod_{i,t} \frac{\exp\left(y_{it}X_{it}'\beta\right)\exp\left(-e^{X_{it}'\beta}\right)}{y_{it}!},$$

and the log-likelihood function

$$\log \mathcal{L}(\beta) = \sum_{i,t} \left[y_{it}X_{it}'\beta - \exp\left(X_{it}'\beta\right) - \log\left(y_{it}!\right)\right].$$

Now I modify the Poisson regression model to allow for interval censoring. When y_{it} is interval-censored on $[1,9]$ and designated a value of 9, the likelihood function becomes

$$\mathcal{L}(\beta) = \prod_{y_{it}\neq 9} \frac{\exp\left(y_{it}X_{it}'\beta\right)\exp\left(-e^{X_{it}'\beta}\right)}{y_{it}!} \prod_{y_{it}=9} \sum_{k=1}^{9} \frac{\exp\left(kX_{it}'\beta\right)\exp\left(-e^{X_{it}'\beta}\right)}{k!},$$

which gives the log-likelihood function

$$\log \mathcal{L}(\beta) = \sum_{y_{it}\neq 9} \left[y_{it}X_{it}'\beta - \exp\left(X_{it}'\beta\right) - \log\left(y_{it}!\right)\right] + \sum_{y_{it}=9} \log\left(\sum_{k=1}^{9} \frac{\exp\left(kX_{it}'\beta\right)\exp\left(-e^{X_{it}'\beta}\right)}{k!}\right). \quad (5)$$

I can then estimate the parameter β using MLE. I prefer this interval-censored Poisson model to the interval-censored tobit model because of the more realistic distribution assumption.

5 Estimation Results

ME counties vs. coroner counties

I begin by presenting comparisons between counties with different types of death investigation offices. It is important to stress from the outset that the following results are only descriptive, since my identification strategy does not extend to ME–coroner comparisons. Instead of diff-in-diff, these are simple-difference regressions with the same set of controls as in my main specifications and with the ME indicator interacted with the month indicators. Figure 6 displays the expected deaths by month based on a standard tobit (T) regression, an interval-censored tobit (T-IC) regression, and an interval-censored Poisson (P) regression, counterparts of Equations (3), (4) and (5), respectively. Each regression is run twice, with COVID-19 deaths and all-cause deaths as the dependent variable. Standard errors are clustered at the state level. The plots display point estimates and 95% confidence intervals.

I make a few observations from these estimates. Firstly, the shapes of the curves, even after controlling for covariates, closely follow the monthly COVID death tolls in the U.S., which saw its first three peaks of mortality in April 2020, January 2021 and September 2021. Secondly, the three sets of results have clear qualitative similarities, but the standard tobit model produces very large standard errors whereas estimates from the Poisson model are the most precise, with the exception of March and April, 2020, when the U.S. outbreak was concentrated in a small region. Thirdly, although results from the COVID-19 death regressions seem to suggest ME and coroner counties sometimes see statistically significant gaps in either direction in the number of reported COVID deaths in certain months, the same is true of all-cause deaths, with the gap often similar in sign and often at least as large in magnitude, and neither measure shows consistent underreporting by one type of county compared to the other. This indicates that both sets of differences are almost certainly driven by other county characteristics which are not sufficiently accounted for. Therefore, the results underscore the fact that these comparisons are correlative and descriptive and should not be used to derive causal conclusions about the effect of death investigation system types.

Democratic-coroner counties vs. Republican-coroner counties

I now move to the main analysis of the paper and examine the effect on reported deaths of party switches in the 2020 election. In the first four columns of Table 4, I report coefficient estimates from counterparts of Equation (1), the single-period diff-in-diff specification. I use both the interval-censored tobit and Poisson models, with either COVID or all-cause deaths as the dependent variable. Figures 7 and 8 illustrate the dynamic effects from counterparts

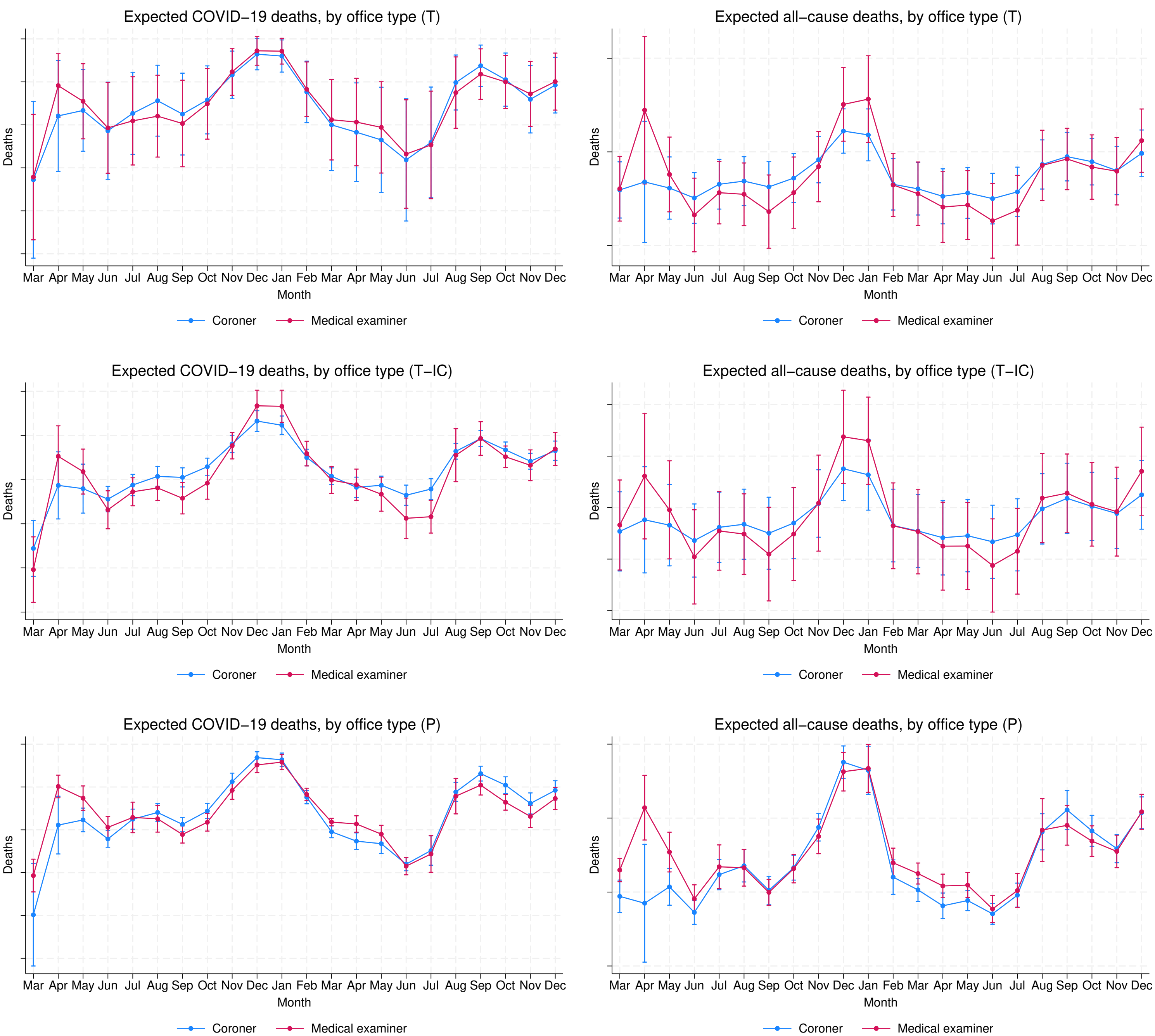


Figure 6: Comparison of reported COVID-19 and total death counts between ME and coroner counties

of Equation (2), showing point estimates and 95% confidence intervals. December 2020 is the reference month in Figure 8. Standard errors are clustered at the state level. In order to more accurately capture any change in death counts immediately before and after the party change in January 2021, and to strip out the volatile initial outbreak in Spring 2020, the models reported in Table 4 restrict the sample to the 10-month period between August 2020 and May 2021, leaving me with slightly less than half of my coroner partisanship subsample.

I first examine the results from the simple diff-in-diff models, with one set of $Change_{ki} \times Post_t$ interaction terms to capture the effect of party changes. In Table 4, the coefficients of interest are δ_2, which represents the effect of a switch from a Democratic to a Republican coroner, and $\delta_3 - \delta_1$, which measures the effect of the opposite switch. In both the interval-censored tobit model and my preferred Poisson model, the point estimates for these coefficients in the COVID death regression have signs that are consistent with the theory of political influences on Republican coroners ($\delta_2 < 0$ and $\delta_3 - \delta_1 > 0$). The tobit model produces no statistically significant estimate out of all four (two coefficients each for COVID and all-cause deaths), but three from the Poisson model are significant at the 1% level.

The fact that estimates for both δ_2 and $\delta_3 - \delta_1$ are highly significant with all-cause deaths as the dependent variable should raise concerns, as it suggests that a change in coroner partisanship has an effect on the overall number of deaths in a county, which seems unlikely and points to issues with model specification. On the other hand, it should be noted that these point estimates are much larger in magnitude in the COVID death regressions than in those for all-cause deaths. In a Poisson regression, a coefficient on an independent variable is interpreted as the marginal expected effect of the variable on the *log*, not actual, dependent variable; as all-cause deaths are, by definition, larger in value than corresponding COVID deaths, the difference in magnitude is to be expected even if the true marginal effects are in fact the same on the pre-log death count. Nevertheless, it turns out the larger estimates in the COVID death regressions do translate to a larger effect on death counts, which can be seen from the bottom four panels in Figure 8.[4] Here, I give one possible interpretation of these results. Although I am unable to perfectly control for correlating factors that affect the difference in all-cause death counts between counties before and after the party change, such differences in COVID deaths move in the same direction but to a much larger extent, implying an increase in COVID deaths as a share of all-cause deaths.[5] Given that the estimated δ_2 in the COVID death regression is significant and $\delta_3 - \delta_1$ is not, this interpretation would

[4]Normally, I can use the `margins` command in STATA to explicitly calculate the marginal effect of independent variables on the pre-log dependent variable. But the command fails to work properly in this case, most likely due to empty cells.

[5]Ideally, I would test this using the share of COVID deaths as the dependent variable in regressions, but data censoring makes this impossible.

	COVID-19 deaths (T-IC)	COVID-19 deaths (P)	All-cause deaths (T-IC)	All-cause deaths (P)	COVID deaths ≤ 10	All-cause deaths ≤ 10
$Post = 1$	5.212	0.256	−0.115	0.000553	−0.0455	−0.0439**
	(4.676)	(0.588)	(1.624)	(0.0301)	(0.0319)	(0.0165)
Party flip (*Change*):						
Rep hold (ϕ_1)	−1.729*	−0.111***	−5.648**	−0.0785***	−0.0116	0.0114
	(0.889)	(0.0370)	(2.420)	(0.0118)	(0.0180)	(0.0168)
D–R flip (ϕ_2)	−0.0743	0.0141	−1.220	−0.00627	0.000862	−0.00220
	(0.763)	(0.0589)	(1.776)	(0.0188)	(0.0207)	(0.0176)
R–D flip (ϕ_3)	−3.077	−0.0934	−9.149	−0.0608**	0.00950	0.0123
	(2.945)	(0.0741)	(8.280)	(0.0296)	(0.0359)	(0.0386)
Interactions between *Change* & *Post* = 1:						
R hold (δ_1)	0.844	-0.0339	1.124	-0.00885	0.0421*	−0.00469
	(0.643)	(0.0508)	(0.764)	(0.0120)	(0.0207)	(0.00886)
D–R flip (δ_2)	−0.612	−0.163***	0.629	−0.0334***	0.0130	0.0113
	(0.498)	(0.0534)	(0.792)	(0.0117)	(0.0267)	(0.0172)
R–D flip (δ_3)	1.389	0.0920	1.141	0.0342***	−0.0461	−0.0449*
	(2.861)	(0.0657)	(2.785)	(0.0118)	(0.0275)	(0.0219)
$\delta_3 - \delta_1$	0.545	0.126	0.0162	0.0431***	−0.0882**	−0.0402*
	(3.006)	(0.0786)	(2.832)	(0.0116)	(0.0385)	(0.0223)
Population ('00,000)	6.872***	−0.152**	89.03***	−0.0592*	−0.325***	0.648***
	(1.969)	(0.0660)	(7.363)	(0.0331)	(0.0399)	(0.0527)
Population^2	0.402	0.0191*	−1.989	0.00788	0.0489***	−0.100***
	(0.442)	(0.0103)	(2.049)	(0.00502)	(0.00811)	(0.0114)
Population^3	−0.0318	−0.000783	0.00926	−0.000331	−0.00219***	0.00464***
	(0.0227)	(0.000482)	(0.122)	(0.000237)	(0.000470)	(0.000725)
log (population)	5.949***	1.056***	8.568***	1.020***	−0.0937***	−0.406***
	(0.641)	(0.0372)	(1.775)	(0.0222)	(0.0192)	(0.0244)
2020 pres. vote share (%):						
Republican	0.227***	0.0189***	0.598***	0.0106***	−0.00385***	−0.00331**
	(0.0395)	(0.00277)	(0.113)	(0.00124)	(0.000516)	(0.00128)
Shift to GOP	−0.135**	−0.0101*	−0.278	−0.00534	0.00209*	0.00306*
	(0.0682)	(0.00565)	(0.301)	(0.00380)	(0.00116)	(0.00150)
Share of age group (%):						
0 to 19	−1.473**	−0.288***	−6.698***	−0.161***	0.0335***	−0.00291
	(0.607)	(0.0380)	(1.960)	(0.0282)	(0.00678)	(0.0205)
20 to 29	−1.570**	−0.297***	−6.851***	−0.152***	0.0357***	−0.00694
	(0.634)	(0.0339)	(1.996)	(0.0271)	(0.00665)	(0.0214)
30 to 49	−1.697***	−0.301***	−7.372***	−0.155***	0.0340***	−0.00632
	(0.621)	(0.0351)	(1.939)	(0.0278)	(0.00834)	(0.0223)
50 to 64	−1.526**	−0.295***	−6.334***	−0.144***	0.0321***	−0.00550
	(0.678)	(0.0329)	(2.043)	(0.0279)	(0.00523)	(0.0224)
65 to 74	−1.475***	−0.320***	−4.618***	−0.148***	0.0298***	−0.000189
	(0.569)	(0.0369)	(1.660)	(0.0267)	(0.00954)	(0.0212)
75 to 84	−0.998	−0.227***	−7.432***	−0.108**	0.0389***	−0.0337
	(0.749)	(0.0630)	(2.815)	(0.0494)	(0.0108)	(0.0302)
Share of male (%)	−0.0210	−0.00641	0.132	−0.0174**	0.000744	0.0112***
	(0.106)	(0.0120)	(0.366)	(0.00695)	(0.00240)	(0.00319)
Share of racial group (%):						
White	−0.0731	0.00878	−0.266	0.00176	0.00120	0.000598
	(0.0560)	(0.00849)	(0.267)	(0.00436)	(0.000870)	(0.000966)
Black	0.123*	0.0228***	0.220	0.0111***	−0.00151	−0.00264***
	(0.0655)	(0.00828)	(0.308)	(0.00422)	(0.00124)	(0.000611)
Hispanic	0.123**	0.00579	0.164	0.00207	−0.00127	−0.00136
	(0.0612)	(0.00362)	(0.263)	(0.00212)	(0.000781)	(0.00127)
Intercept	161.3***	30.08***	770.9***	19.47***	−2.412***	−0.190
	(61.64)	(3.442)	(176.5)	(2.797)	(0.648)	(2.186)
Fixed effects	State, region, month, region × month, urbanization					
n	12,160					

Levels of significance: *** = .01, ** = .05, * = .10.

Table 4: Diff-in-diff regression estimates

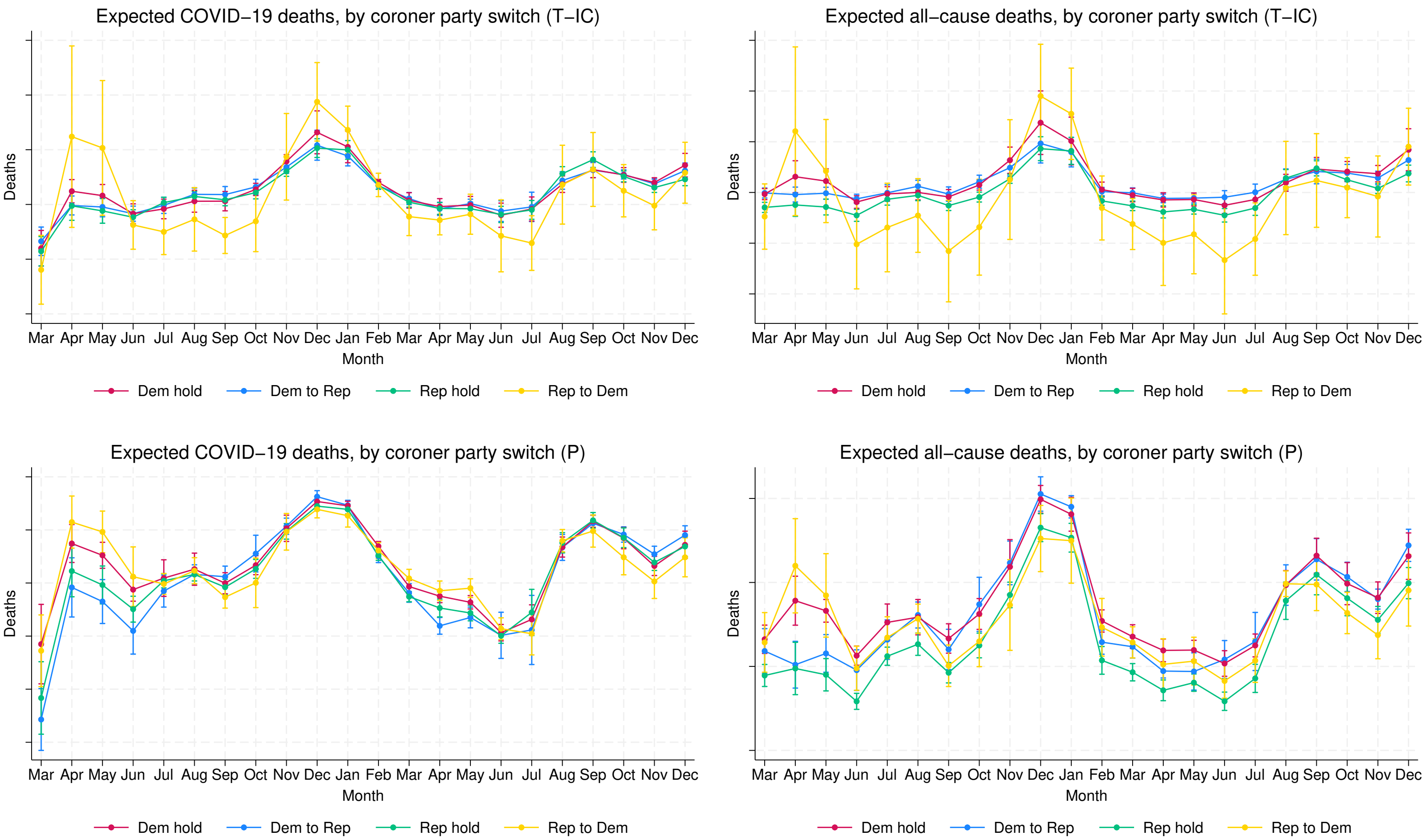


Figure 7: Comparison of reported COVID-19 and total death counts, by coroner party switch

mean that a county's switch from a Democratic to a Republican coroner led to a decrease in reported COVID deaths compared to counties held by Democratic coroners, but the reverse party flip had no such effect in the opposite direction.

In the last two columns of Table 4, I estimate OLS regressions where the dependent variable is an indicator for death counts smaller than 10, i.e. either counts of zero or suppressed counts. The estimates for $\delta_3 - \delta_1$ are significant and negative in both the COVID and all-cause regressions, and those for δ_2 are not significant in either. The results seem to suggest that a switch from a Republican to a Democratic coroner makes a county less likely to have such small COVID death counts (a larger effect than on all-cause deaths) than it otherwise would have. But because the threshold of 10 is completely arbitrary, I refrain from drawing stronger conclusions based on these results.

I now consider the dynamic effects illustrated in Figures 7 and 8. Similar to the preceding subsection on ME/C comparisons, estimates from the interval-censored tobit model are much less precise relative to their magnitude, as Figure 8 most clearly demonstrates. Both figures also show that estimates involving Republican-to-Democrat switches are less precise in general, due to the small number of such flips in the 2020 election.

The Poisson regression plots in Figure 8 (bottom four panels) reveal some interesting patterns. Focusing on the immediate vicinity of the party switch (August 2020–May 2021), estimates from the COVID death regressions indicate statistically significant effects of party changes in both directions. Compared with December 2020, COVID death counts were not statistically different between switch and no-switch counties between August and November, indicating parallel pre-trends. Death counts between the two groups of counties started to significantly diverge in February or March 2021. Democratic-coroner counties that switched Republican began to see a decrease in reported COVID deaths compared to their counterparts that stayed Democratic, and Republican-coroner counties that switched saw higher death counts. The effect lasted for several months before mostly tapering off through the rest of the year. Whereas qualitatively similar trends can be observed in the all-cause death plots, the estimates are less precise and have smaller magnitude just like in the single-period diff-in-diff regression in Table 4. This is the strongest evidence yet from this analysis that supports the theory of politically incentivized death reporting.

Finally, I repeat the analysis in this subsection after replacing my death measure with deaths where COVID-19 was the *underlying* cause, instead of those with COVID-19 listed as a cause of death. The results are almost identical, only with slightly larger standard errors of the estimated coefficients of interest. This is to be expected: the two death count measures turn out to be highly correlated, but the underlying death count is smaller than or equal to the more widely defined measure, leading to slightly more suppressed counts. As they do not affect my analysis, I omit those results.

6 Discussion

Do partisan coroners manipulate reported COVID-19 death counts for political expediency? The analysis of mortality and coroner party affiliation data yields evidence that is decidedly mixed. On one hand, both the single-period and dynamic diff-in-diff regressions produce estimates that seem consistent with the concerns raised about Republican coroners. On the other hand, a few countervailing factors cast doubt on the validity of such theories. The main issue is that I find non-zero effects in my intended placebo tests that use all-cause deaths as the dependent variable, although these are much smaller in magnitude and often less precise. In addition, the dynamic effects I find were relatively short-lived compared to the duration of the COVID pandemic, suggesting any effects from party switches dissipated soon into the new coroner's term. Perhaps more importantly, my most precise estimates

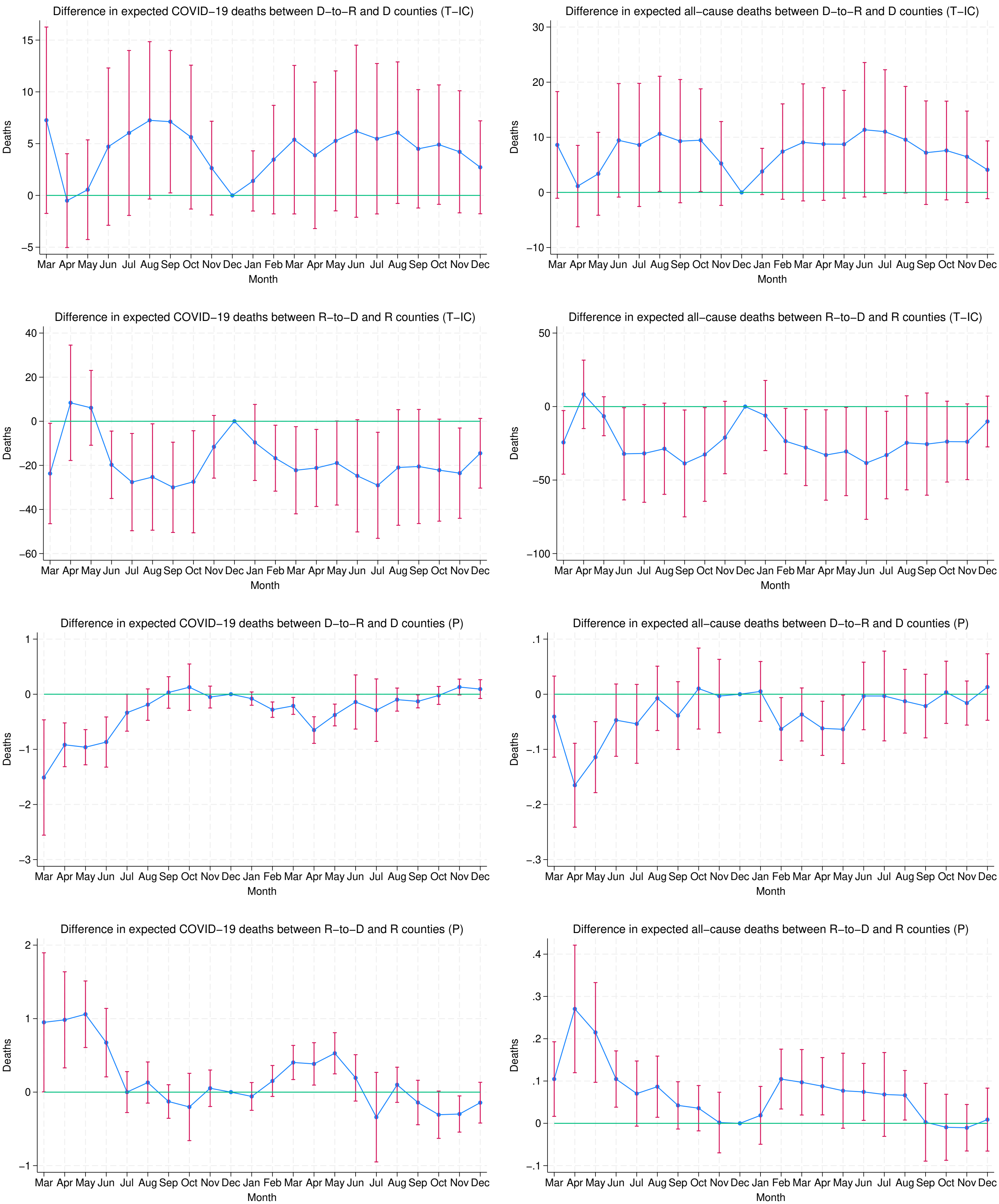


Figure 8: Dynamic diff-in-diff effect estimates

(from the Poisson model) are too small to be economically meaningful, and do not point to large-scale, systemic underreporting of COVID-19 deaths by Republican coroners.

It may be helpful to consider the matter of politically-motivated underreporting in the bigger context of COVID death reporting in general. As mentioned in the introduction, there were immense challenges in properly certifying COVID-19 deaths. During the first two years of the pandemic, estimates put excess deaths in the United States at over 1.1 million (Paglino et al., 2023; Rossen et al., 2022), while the officially reported COVID-19 death toll was around 950,000, meaning over 15% of excess deaths were not attributed to the disease. Some of these deaths will have been due to undocumented or unrecognized COVID infections (Woolf et al., 2020). As my Benford's law test in Figure 5 shows, there is strong indication of underreporting of COVID deaths across death investigation office types, coroner party affiliations and years. It is conceivable that any politically-motivated manipulation paled beside such inevitable underreporting and was masked by it. Tactics that would be used to manipulate death certificates, such as skipping autopsies or requiring positive COVID-19 laboratory tests (Bergin et al., 2021; "Politics of Death", 2022), may resemble constrained testing and investigation capacity faced by ME/C offices across the country.

The significant yet small results may also reflect the small number of coroners engaged in malpractice, the limited scope coroners have for manipulating death certificates, and/or the relatively small share of COVID-19 deaths passing through ME/C offices as opposed to being handled in the healthcare system. While I am not equipped to determine the role of these factors, my findings do suggest that coroners of all affiliations seem to have performed their duties during the pandemic better than many have feared.

7 Conclusion

Using public-use mortality data from the CDC and original data on medicolegal death investigation office types and elected coroners' partisanship, I use a number of tools to test the effect of death investigation on COVID-19 reporting. I find suggestive evidence that there is widespread underreporting in COVID-19 deaths, but a cross-sectional comparison between medical examiner counties and coroner counties does not reveal differing extents of such underreporting. Employing econometric specifications in a difference-in-differences framework, I find some evidence that counties reported fewer deaths when they switched from a Democratic coroner to a Republican one after the 2020 election, and vice versa. However, the magnitude of these differences is too small to have had any meaningful impact in relation to the scale of the underreporting problem during the pandemic.

Considerable accommodation has had to be made due to the data censoring adopted by the CDC, which results in more than half of my observations containing a suppressed death count. If manipulation did happen to a marginal degree as my findings seem to indicate, having low-death-count observations in the data would probably help unveil a much clearer picture of such practice. Therefore, it will be worthwhile to carry out follow-up analysis if and when such data can be obtained. It might also be possible to obtain measures of coroner partisanship that can augment party affiliations per se: Farris and Holman (2023), for example, use a survey to gauge county sheriffs' agreement with right-wing extremist ideology and examine its correlation with their strictness in enforcing mask mandates.

Some empirical research has been done to compare the performance of ME and coroner systems. The political aspect of coroner systems, however, remains overlooked and under-researched. As the United States continues to become more politicized and polarized, the subject may merit further research, in areas including infectious disease and beyond, such as crimes, mental health issues and the opioid crisis. Unfortunately for America, the COVID-19 pandemic is unlikely to be the last time that the integrity of the death investigation system is put under the microscope. Future research endeavors should aim to strengthen the system's resilience against potential biases and maintain public trust in the work it produces.

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