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Much Ado about Nothing? Counterterrorist Legislation has Few Effects

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Abstract

The events of 9/11 not only caused anger and fear among citizens the world over, but also led to counterterrorist legislation (CTL) in many countries. This paper identifies the most important determinants of passing CTL and the effects of such legislation on the likelihood of future terrorist attacks and on civil liberties. We particularly focus on the interplay between constitutionalized emergency provisions and CTL. We find that constitutional emergency provisions seem unrelated to CTL. It is not newly passed CTL, which drives civil liberties down, but, rather, the terrorist attacks themselves and the immediate and unmediated government responses to them.

Keywords Terrorism · Counterterrorist legislation · State of emergency · Constitutional emergency provisions

JEL Classifications K40 · Z13

Introduction

Although the events of 9/11 occurred more than twenty years ago, they seem to be very present in the minds of many. In the wake of 9/11, governments all over the world passed an unprecedented number of counterterrorist legislation (CTL). This paper analyzes both the causes and the effects of CTL in a broader perspective by relying on a time period covering the years from 1970 until 2014. At least three

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different rationales for passing CTL can be named. (1) It could be passed to reduce the likelihood of future terrorist attacks. (2) It could also be passed as a placebo to reassure nervous citizens that the government is in control of things and that it does not give in to any terrorist threats. (3) Finally, terrorist incidents could serve as a mere pretext for governments to reduce civil liberties. Depending on the relevant rationale, different consequences are to be expected: In the first case, one would expect some changes in the behavior of government officials such as police, border controls, secret service and so forth. It is likely that civil liberties would suffer and at the end of the day, the question would be whether the reductions in civil liberties are offset by the reduced probability of terrorist incidents. A number of scholars have argued that such a trade-off exists and that, normatively speaking, we should accept a reduction in civil liberties in exchange for fewer terrorist incidents occurring (for example Ignatieff 2013). Others have argued that no such trade-off exists and that the reduction in civil liberties does not buy any additional security (for example Walsh and Piazza 2010).

If CTL is passed merely as a placebo, we should not expect any effects: The probability of terrorist incidents should not change, neither should the degree to which civil liberties are upheld. If CTL is passed according to the third rationale, the effects on terror are unclear, whereas we would definitely expect a significant decrease in civil liberties. So, the difference between the first and the third rationale is that in the first, reduced civil liberties are accepted as a "lesser evil," whereas in the third, their reduction is the main goal.

In previous work (Bjørnskov and Voigt 2020; 2022), we have shown that constitutional emergency provisions and their use are relevant for how governments deal with terrorist incidents. The more competences the constitution allocates to the executive under a state of emergency, the more likely a state of emergency is to be called. Once it has been declared, it generally leads to substantially more government repression, while the country is also more likely to suffer from additional terror attacks, thus challenging the effectiveness of states of emergency.

In this contribution, we are particularly interested in the possible interplay between constitutional emergency provisions and CTL, that is, legislation on the level of statutory law. In France, for example, after the Bataclan and Stade de France attacks in November 2015, a state of emergency was declared the following morning. After having been extended a number of times, new CTL came into force in November 2017 and the state of emergency was not prolonged any more. One way to interpret this sequence of choices is that politicians believed that at least some of the additional powers that they hitherto only enjoyed under a state of emergency needed to be made permanent by passing CTL. But this sequence of events is not the only possibility: (1) If the constitution does not contain any emergency provisions, passing CTL seems to be particularly likely; (2) if the constitution makes the declaration of a state of emergency (SOE) easy, then passing additional CTL might be perceived as superfluous as an SOE can easily be called should a need to do so be perceived; and (3) should the declaration of an SOE be followed by the absence of any terrorist incidents, no need to pass any new CTL could be perceived.

In 2006, a survey summarizing empirically validated knowledge concerning the effectiveness of counter-terrorism strategies (Lum et al. 2006, p. 3) had this to say:

"In the course of our review, we discovered that there is an almost complete absence of evaluation research on counter-terrorism strategies. From over 20,000 studies we located on terrorism, we found only seven which contained moderately rigorous evaluations of counter-terrorism programs… Further, from the evidence we were able to locate, it appears that some evaluated interventions either didn't work or sometimes *increased* the likelihood of terrorism and terrorism-related harm." This has somewhat changed in the meantime, but the skepticism regarding the effectiveness of counter-terrorist policies certainly remains. It seems that the side effects of counter-terrorism policies have been analyzed more frequently: In a recent contribution, Shor (2019) lists more than 40 studies, the majority of which find that CTL is detrimental to civil liberties.

To the best of our knowledge, the interplay between constitutional emergency provisions and CTL has never been subject to a cross-country analysis. This is, hence, the main contribution of this paper. We actually find that the existence of constitutionalized emergency provisions as well as their content is irrelevant for the likelihood that a country will pass CTL. As previous studies, we also find a deterioration in civil liberties. However, unlike previous studies, we show that this is not driven by CTL but by the number of terrorist incidents occurring in a country. These are troubling findings as they clearly indicate the limited relevance of both constitutional and of statutory rules for actual government behavior.

The rest of the paper is organized as follows: Section "Reasons for Passing Counterterrorist Legislation" develops a number of hypotheses regarding possible determinants of passing CTL, whereas Section "Effects of Counterterrorist Legislation" develops a number of hypotheses on the possible effects of CTL. Section "Data and Empirical Strategy" describes the data and our estimation approach. Section "Does CTL Have Any Effects on Terrorist Incidents?" reports our results regarding the determinants of CTL. Sections "Does CTL Have Any Effects on Terrorist Incidents?" and "Does CTL Affect Civil Liberties?" contain our analysis of the effects of CTL: Section "Does CTL Have Any Effects on Terrorist Incidents?" is an analysis of the effectiveness of CTL, whereas its side effects are the focus of Section "Does CTL Affect Civil Liberties?". Section "Discussion, Conclusions, and Outlook" concludes and spells out a number of follow-up questions.

Reasons for Passing Counterterrorist Legislation

In this section, a number of factors potentially leading to more CTL being passed are discussed. Our brief discussions here are entirely exploratory and are focused on hypotheses instead of clear theoretical expectations. Public choice scholarship has convincingly shown that politicians—like everyone else—try to maximize their own utility. This does, of course, not exclude the possibility that some public-minded politicians are interested in advancing the public good [see, for example, Brennan and Hamlin 2000]. On the other hand, there is ample evidence that disasters—both natural and man-made such as terrorist attacks—have been used by governments as pretexts to increase their own power to the detriment of the governed; for a book length treatment on such actions with regard to the US, see Higgs (1987). In this

study, we develop a number of hypotheses that allow for both benevolent and less benevolent politicians and propose to let the data speak.

The first couple of hypotheses focus on terrorist events as a direct (or indirect) trigger for passing CTL. We differentiate between domestic terrorist events and those taking place in neighboring countries. The remaining hypotheses take the potential relevance of existing institutions for passing additional CTL explicitly into account. Both the existence and the contents of emergency constitutions play a central role in these hypotheses.

Terrorist incidents remind citizens how vulnerable they are and that their protection by the state is incomplete at best. Terrorists have been credited with the capacity to capitalize on probability neglect, that is, the focus of many on the bad outcome of an event rather than on the (low) probability that it will occur again (Sunstein 2003). Probability neglect is likely to lead to action bias—the urge to do something. In all likelihood, these people will also demand politicians to "do something." Representatives of the state are therefore likely to pass CTL to demonstrate their resolve vis-à-vis their citizens, a behavior in line with the placebo rationale offered in the introduction as one possible explanation for the passing of CTL.[1]

Yet, politicians themselves are also likely to suffer from action bias implying that they might believe that passing additional CTL will, indeed, reduce the likelihood of future terrorist incidents (Patt and Zeckhauser 2000; Zeelenberg et al. 2002).[2] Finally, rational power maximizing politicians can (mis-)use the prevalent action bias to pass CTL that would reduce civil liberties—and expand their own powers to the detriment of both the opposition and citizens at large.[3]

Hypothesis #1a The more terrorist incidents have occurred in state i at time t, the higher the probability of new CTL being passed in state i at t + 1.

We assume that more deadly terrorist attacks increase the salience of the incident and that such attacks have a larger impact on the feeling of being defenseless. To counter these perceptions, legislators are more likely to pass fresh CTL, which leads us to slightly modify our first hypothesis. Here again, any of the three rationales possibly underlying the passing of CTL might be at play.

Hypothesis #1b Holding the number of terrorist incidents constant, the higher the number of fatalities that have occurred in state i at time t as a consequence of these incidents, the higher the probability of new CTL being passed in state i at t + 1.

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[1] Bueno de Mesquita [2007] argues that in response to pressure by their citizens, governments oversupply observable policies at the expense of unobservable ones. Notice that this hypothesis is perfectly in line with public choice scholarship: Politicians may act not because they are convinced that this is the right thing to do but because they hope to conserve (or even increase) their popularity.

[2] A complication is that if politicians honestly believe that CTL is effective in lowering the risk of terrorism, but it is not, this then begs the question why politicians and the political system do not learn from the ineffectiveness.

[3] This effect should be stronger in autocracies than in democracies, since in democracies, opposition politicians might be in power in the future.

The next hypothesis is also concerned with the salience of terrorist attacks but is based on geographical distance. If terrorist attacks have occurred "around the corner," then—due to the high salience of such events—many citizens will reason that something similar could also occur at home. In such a situation, the passing of CTL might be perceived as pro-active government policy and governments are likely to be eager to seize that opportunity. In this case too, legislators may believe that passing CTL reduces the probability of future terrorist incidents domestically. And again, they may also use the passing of CTL in neighboring countries as a justification of reducing civil liberties at home.

Hypothesis #2a The more terrorist incidents occurred in neighboring countries, the higher the probability of new CTL being passed.

A related but different transmission mechanism due to spatial diffusion may also trigger the passing of CTL domestically: Instead of the number of terrorist incidents triggering CTL, it may be that legislative activity of neighboring governments may increase incentives to emulate them.[4]

Hypothesis #2b The higher the number of additional CTL passed in neighboring countries, the more likely additional CTL is to be passed.

The hypotheses described so far dealt with terrorist attacks themselves and potential reactions in neighboring countries. They did not, however, take the domestic institutional landscape explicitly into account. This is what we turn to now. In this paper, we are particularly interested in the potential interplay between (domestic) constitutional emergency provisions and CTL. The following hypotheses therefore deal with this interplay.

If the country's constitution does not provide for additional powers to the executive under a state of emergency, the probability of passing CTL is higher because following one or more terrorist incidents there will be a general feeling that "something needs to be done." Here, CTL thus functions as a substitute for constitutionalized emergency provisions.

Hypothesis #3 All other things being equal, countries with no constitutionalized emergency provisions are more likely to pass CTL than countries having explicit emergency provisions in their constitutions.

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[4] In addition, if politicians believe that passing CTL does, indeed, reduce the likelihood of terrorist attacks, then the passing of CTL in a neighboring country may have negative externalities in the sense that it could increase terrorist events domestically because terrorists might simply move across the border. Neumayer et al. [2014] find some evidence in favor of a closely related hypothesis, namely that diffusion only takes place among countries with similar threat levels. Negative externalities will, however, only materialize if counter-terrorist policies are effective at least to some degree, which is still unclear. Neumayer et al. [ibid.] themselves emphasize that this transmission mechanism only refers to international terrorism and is, hence, limited to a number of topics such as Islamist terror against western civilization.

The next hypothesis delves a bit deeper into the possible interrelation between constitutionalized emergency provisions and CTL. Whereas hypothesis #3 only asks if such provisions exist, the next hypothesis conjectures that if they exist and it is relatively easy to declare an SOE, then the urge to pass additional ones will be perceived as lower.

Hypothesis #4 Countries with emergency constitutions that make the declaration of a state of emergency easy are less likely to pass CTL than countries with emergency constitutions that make the declaration of a state of emergency difficult.

Effects of Counterterrorist Legislation

After having dealt with possible factors driving the passing of counterterrorist legislation in the last section, we now move on and deal with the possible consequences of CTL. We distinguish between an intended effect (the diminution of terrorist incidents) on the one hand, and—possibly unintended—side effects (the diminution of civil liberties broadly conceived).

Assume that CTL is effective in deterring future attacks. CTL could be effective in multiple ways: It could make the logistics of carrying out a terror attack more difficult; it could make it less attractive to become a terrorist by increasing the probability of being caught or by heightening the sentence in case of being convicted. More CTL may thus imply fewer attacks.

Hypothesis #5 The more CTL a parliament has passed, the lower the likelihood of terrorist incidents occurring.

Unfortunately, it is not only the effectiveness of CTL that is uncertain. On top, CTL is likely to have negative side effects. If CTL grants additional powers to the police, border controls, and secret service personnel, a deterioration in civil liberties is a likely side effect. Depending on the actual behavior of these—and other—agents, CTL might even lead to a deterioration in physical integrity rights if, for example, suspects are being tortured.

Hypothesis #6 The more CTL a parliament has passed, the higher the chances to observe a deterioration in civil liberties.

Data and Empirical Strategy

Four groups of data are essential for testing the hypotheses developed in sections "Reasons for Passing Counterterrorist Legislation" and "Effects of Counterterrorist Legislation" above: We need data on (1) counterterrorist legislation, (2) on terrorist events, on (3) civil liberties, and on (4) constitutionalized emergency provisions (including their use). We describe the sources for our data in turn.

Our main data are on counterterrorist legislation for which we use the comprehensive dataset on counterterrorist legislation introduced by Shor (2011) and updated until 2017 by Louis and Shor (2019).[5] Drawing on a variety of sources, it covers almost 2,000 pieces of legislation passed after World War II in up to 219 countries and territories. They define counterterrorism as "state policies, which are commonly enacted or implemented in response to oppositional terrorist events/threats, and/or policies declared by states as directed, at least partially, at preventing or limiting future oppositional terrorist acts" (ibid., 5.). The dataset includes information on the subject matter regulated by the legislation. The six areas explicitly coded in the dataset refer to "sanctions on support for terrorist and terrorist organizations," "international terrorism and immigration," "financial counterterrorist legislation," "protection of infrastructure, borders, and transportation," "limitations on weapons," and "repression of civil liberties." We aggregate these areas into two variables: a dummy capturing the extensive margin, that is, whether any CTL was introduced, and a variable capturing the intensive margin by the logarithm to the number of separate pieces of legislation introduced in a year.

However impressive the CTL dataset is, we must nevertheless emphasize an important caveat. The dataset compiled in Shor (2011) and Louis and Shor (2019) covers *additional* and *new* CTL, but does not provide any basis on which to judge the stock of CTL.[6] In the following, we therefore observe the propensity to introduce additional CTL and can only assess the marginal effects of such changes. Essentially, we are in the same situation as someone who starts watching a football match around the 60th minute but who has no way of knowing what the score is at that time. This means that we are logically unable to provide any clean assessment of the efficacy of the CTL framework of any country, but must gauge such questions from simpler indications.

We first attempt to identify the potential drivers leading governments to pass CTL. According to hypothesis 1, one such driver is the number of terrorist incidents in a given year in the country. In defining terrorism, we follow Enders and Sandler (2012) who define it as "the premeditated use or threat to use violence by individuals or subnational groups to obtain a political or social objective through the intimidation of a large audience beyond that of the immediate victims." This is also the operational definition behind the large dataset from the Global Terrorism Database maintained at the University of Maryland (GTD 2019), which we use in the following. While the GTD is not perfect and may for example undersample events in relatively poor and small countries and code too many events as terrorism, it remains the most comprehensive dataset. We also argue that our sample restriction, as outlined below, alleviates most such problems. In order to match the terrorist data with other available data, we cover the years between

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[5] Epifanio [2011] is another such dataset covering, however, far fewer countries over a far shorter period (namely 20 liberal Western democracies from 2001 until 2008).

[6] The dataset goes back to the nineteenth century for a number of countries, but far from all implying that creating a stock variable for all countries is not possible. In addition, we do not really know anything about the content of the legislation. Also, the dataset does not contain any information on legislation that was revoked at some point in time.

1970 and 2014 for which we measure the degree of terrorism as the logarithm (plus one) to the number of events. We count only attacks from domestic groups, as we believe international terrorism does not in the same way give rise to domestic legislation.

To be able to gauge possible side effects of CTL as formulated as hypothesis 6 above, we need measures of civil liberties. As rather broad measures, we rely on the Freedom House (2019) index of civil liberties and political freedoms, and the Cingranelli-Richards (CIRI) index of respect for physical integrity rights (Cingranelli and Richards 1999). We use the indicator by Fariss (2014), which over the last couple of years has become one of the standard choices. Finally, we draw on three measures from the V-Dem database: the protection of physical integrity rights, the degree to which government represses civil society organizations (CSO rights), and the degree to which government censors the media (media censorship) with higher values standing for less repression and censorship. In a sense, one can think of these measures as covering specific aspects of civil liberties. They thus enable us to make more precise inferences regarding the side effects of CTL. In all cases, we rescale the indices to the same 0-1 scale as the V-Dem measure and ensure that higher numbers signify more respect for civil liberties such that our estimates across indicators are directly comparable.

Finally, as we are particularly interested in the potential interplay between CTL and a country's emergency constitution—the formal provisions that explicitly regulate political action during and after states of emergency included in a country's constitution—we need data on these. As documented by Elkins et al. (2009), nine out of ten constitutions today include explicit provisions for how to deal with emergencies, and these provisions are frequently used. Specifically, we use the index of emergency powers (INEP) developed by Bjørnskov and Voigt (2018) based on data from the Comparative Constitutions Project [Elkins et al. 2009], which contains three cost elements and three benefit elements. The cost elements of the INEP capture: (1) the degree to which the right to declare a state of emergency is concentrated in a single person or disbursed across multiple veto players; (2) the extent to which the executive needs the approval of an emergency declaration of other actors within the political system; and (3) the number of different situations that can be used to justify the declaration of a state of emergency. Similarly, the benefit elements capture: (4) whether the executive or other political actors are allowed to suspend fundamental civil and political rights during a state of emergency; (5) whether parliament can be dissolved during a state of emergency; and (6) whether the government is allowed to introduce censorship of the media and expropriate property during an emergency. All six elements are distributed between 0 and 1, such that 0 implies a situation in which political actors have no discretionary power—when, for example, the executive has to pass several veto players before being able to declare a state of emergency and where the government is given no additional powers to dissolve parliament or introduce censorship—while 1 refers to a situation in which the executive has full discretionary powers unchecked by any other political or judicial actors.

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Both overall parts of the INEP are distributed on a scale from 0 to 1 with higher scores implying more discretionary power allocated to the executive.[7]

We aim at parsimonious econometric models. Yet, to alleviate concerns of omitted variable bias, we include a number of both economic and political covariates. For simplicity, most of the covariates will be included in models aiming to explain the passing of CTL as well as in those interested in ascertaining their effects.

Regarding economic covariates, we include the income per capita in log form to control for the possibility that a society's reactions to terrorist threats are also determined by its income level. From previous research (Bjørnskov and Voigt 2020) we know that given a terrorist event had occurred in a country, governments of countries with higher per capita income are significantly less likely to declare an SOE. Passing fresh CTL is another possible way to react to terrorist acts and a reaction similar to that observed regarding the declaration of an SOE appears possible. This is why we include per capita income, which proxies for both state capacity and the capacity of civil society.

We further include a dummy capturing recessions—years in which GDP growth was negative—for two reasons: Previous research (Bjørnskov and Voigt 2020) shows that terrorist events are more likely in recession years. As these events may lead to additional CTL, we control for recession years. In addition, governments may be particularly prone to pass CTL during recessions to demonstrate how eager they are to pursue policies in favor of their citizens. We also include the logarithm to the size of the population, as larger countries are substantially more likely to experience terrorist attacks (Bjørnskov and Voigt 2020). These variables are from the Penn World Tables, mark 10 (Feenstra et al. 2015).

With regard to political covariates, we control for whether countries have a democratic or an autocratic government. This may be relevant for both passing CTL and its effects. Assuming that autocratic governments dispose of more de facto competences than democratically elected governments, they should have fewer incentives to pass CTL which would grant them additional competences. Regarding the effects of CTL on civil liberties, they could be more severe under autocratic governments.[8] To tease out the most of this distinction, we follow a new feature in Bjørnskov and Rode's (2020) update of Cheibub et al. (2010) and distinguish between four basic types of systems: (1) fully democratic regimes, defined as countries in which free and fair elections are held at regular intervals and where all political actors accept the results of the elections; (2) regimes with similar formal institutions and elections, but where the elections are either rigged, unrepresentative or not respected by the executive branch, which we term electoral autocracies; (3) single-party regimes, that is, regimes in which elections are held, but only a single party or candidates approved by a single party are allowed to run for office; and (4) regimes without

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[7] In the following, we do not include an indicator of whether a state of emergency was actually declared. As in previous papers, preliminary tests showed that it appears irrelevant to any outcome if an emergency is actually declared or if it is merely a political option to do so.

[8] Bjørnskov et al. [2022] show that autocratic governments are significantly more likely than their democratic counterparts to overstep constitutional constraints in relation to states of emergency.

any regular elections. In the following, we combine categories (3) and (4), which become the comparison category. From the same dataset, we include a dummy capturing whether government power was challenged in a given year by a failed coup attempt, which could provide alternative causes of emergencies as well as increased repression.

Regarding the composition of parliament, we include two variables: parliament ideology and parliament fractionalization (both from Berggren and Bjørnskov 2017). The first reflects the average political ideology of the legislature. This variable is included because the ideological leaning of a parliamentary majority might have an effect on both the propensity to pass CTL and its effects on civil liberties.[9] Parliamentary fractionalization is included because CTL can only be passed with a parliamentary majority. Since the difficulty of securing a parliamentary majority increases in the fractionalization of parliament, we explicitly control for it. The average ideological position in parliament can take on values between − 1 (communist or unreformed socialist parties) and 1 (classical liberal parties). From the same source, we derive the Herfindahl-Hirschmann index of fractionalization in parliament; note that this index is coded between 0 and 1 such that 1 refers to a situation with only one party. From the same source, we also include a dummy for election years based on the assumption that governments may have different incentives immediately before elections. In Bjørnskov and Voigt (2020) we find that subsequent to a terrorist attack, governments are significantly less likely to declare a state of emergency in an election year.

It is well known that the law on the books is frequently very different from the law in action. We try to account for the *de facto* quality of formal institutions by relying on the judicial accountability variable as contained in the V-Dem database (Coppedge et al. 2016). It captures the degree to which the judiciary is accountable to the law. Finally, if a government has declared a state of emergency following a terrorist attack and passed new and additional CTL, it is unclear whether effects should be attributed to the former or the latter. We therefore also add the logarithm (plus one) to the number of terrorist attacks in a country in a given year.

The full data are summarized in Table 1.

Our full sample covers up to 2,552 country-year observations from 82 countries in Europe, the Americas and Australasia that experienced a total of 60,649 episodes of terrorism between 1970 and 2014 (GTD 2019). We limit our sample to these countries because they all have formal constitutional institutions that resemble those of Western democracies. As such, we ensure that the *de jure* political institutions and thus the formal structure of political decision-making processes are similar in all countries within the sample. This allows us to ignore the particular problems associated with absolutist policy-making in, for example, North Korea or Saudi Arabia, and the often unstructured processes in parts of Africa.

We keep our empirical strategy quite simple in order to make all results transparent. For the extensive margin, we employ a random effects panel data logit estimator

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[9] Bjørnskov and Voigt [2020] find that ideologically more conservative governments are significantly less likely to suffer from terrorist events.

Table 1 Descriptive statistics

	Mean	Standard deviation	Observations
Any legislation	.180	.385	3474
#legal changes	.262	.738	3474
Log GDP per capita	9.552	.759	3330
Recession	.207	.405	3274
Log population size	1.738	1.899	3330
Single-party regime	.159	.366	3948
Electoral autocracy	.121	.326	3948
Democracy	.720	.449	3948
Election year	.228	.419	3780
Parliament fractionalization	.439	.242	3204
Parliament ideology	.075	.408	3204
Failed coup	.010	.105	3948
Judicial accountability	2.406	1.046	2989
Physical integrity rights	.766	.271	2989
CSO rights	.741	.283	2989
Media censorship	1.176	1.614	3320
Fariss repression	.824	1.399	3319
Freedom house rights	4.998	3.339	3401
CIRI human rights	5.743	2.059	2195
Log no. of terrorist events	1.099	1.509	3585
Cost INEP	.433	.180	3536
Benefit INEP	.334	.206	3536

in which we add annual fixed effects and fixed effects for broad world regions (Latin America, the Caribbean, Asia and the post-communist countries of Eastern Europe). For the intensive margin, we estimate all results using simple ordinary least squares with the same fixed effects. In all cases, we lag all control variables one year in order not to risk simultaneity bias. As such, throughout the section we estimate specifications as in Eq. 1 in which $Z_{i, t}$ is the outcome variable—CTL legislation or civil liberties—in country I in year t, $X_{*I, t}$ is the set of control variables with β a vector of our main estimates, R_i a set of regional fixed effects, D_t a set of annual fixed effects, and $\varepsilon_{i, t}$ is an error term.

$$Z_{i,t} = \alpha + \beta X_{*i,t} + R_i + D_t + \varepsilon_{i,t} \qquad (1)$$

For the estimates of civil liberties and related effects, we further add a twice-lagged dependent variable, $Z_{i, t-2}$. While this choice induces Nickell bias, with 35 years of data the bias is small, and the inclusion of a twice-lagged dependent comes with the substantial benefit of making our estimates dynamic and less likely to be subject to endogeneity. Although not entirely removing the problem, the potential past effects of changes in civil liberties on terrorist attacks are likely to be subsumed in the lagged dependent variable. The logic of this choice, which follows previous

Table 2 Main determinants of CTL

	Any legislation		#of newly passed laws	
	1	2	3	4
Log GDP per capita	− .170 (.207)	.008 (.222)	.741** (.354)	.683 (.449)
Recession	− .114 (.174)	− .128 (.184)	− .237 (.401)	− .343 (.448)
Log population size	.276*** (.071)	.281*** (.075)	.284*** (.111)	.300** (.119)
Electoral autocracy	.626 (.611)	–	.871 (.946)	–
Democracy	.435 (.681)	–	1.956** (.916)	–
Election year	− .185 (.122)	− .265** (.119)	− .087 (.237)	− .017 (.234)
Parliament fractionalization	.078 (.533)	.232 (.605)	2.663*** (.988)	1.489 (1.017)
Parliament ideology	− .204 (.314)	− .296 (.381)	.945 (.581)	1.488** (.676)
Failed coup	− .008 (.502)	− .162 (.588)	− .139 (1.562)	.000 (1.449)
Judicial accountability	.209 (.122)	.242* (.137)	− .369** (.148)	− .417** (.168)
Log no. of terrorist events	.149** (.069)	.135* (.074)	.047 (.129)	.059 (.158)
Cost INEP	− .285 (.519)	− .408 (.517)	.530 (.791)	.495 (.848)
Benefit INEP	.013 (.484)	.147 (.500)	− .604 (.659)	− .638 (.754)
Regional FE	Yes	Yes	Yes	Yes
Annual FE	Yes	Yes	Yes	Yes
Observations	2552	2193	544	500
Countries	70	67	68	65
Wald Chi-squared	1568.57	1382.98	–	–
Log likelihood	− 1088.537	− 984.271	− 486.361	− 454.739

*** (**) [*] denote significance at $p < .01$ ($p < .05$) [$p < .10$]; regions are Latin America, the Caribbean, Asia, Communist countries, and formerly communist countries. Numbers in parentheses are robust standard errors

research in Bjørnskov and Voigt (2020; 2022), is therefore that we observe the state of civil liberties the year before one or more terrorist attacks, as well as the state the year *after* the events. We therefore effectively observe the *change* in civil liberties following more intensive terrorist activity while making sure that what we observe is not terrorist reactions to an already given degree of civil liberty.

Does Terror Induce More CTL?

We report our main estimates in Table 2 where we first estimate the determinants of observing any legislative change to CTL, the extensive margin (columns 1 and 2). We first find that larger countries introduced CTL substantially more often than smaller ones. Although one could suspect this result to be driven by the USA, which is both the largest country in our sample and one of the countries with the most

dramatic events, this is not the case.[10] We also find indications that countries in which blackletter law and legal reality converge (as proxied by the variable judicial accountability) were somewhat more likely to introduce CTL, and clear evidence that the democracies in our sample were substantially less likely to do so in election years. Calculating odds ratios indicates that the introduction of CTL is about 30% more likely in non-election years. As such, we reconfirm previous findings in the terrorism literature that indicate that terrorist events as well as the political activism such as CTL are not popular with most voters (see Gassebner et al. 2008; Bjørnskov and Voigt 2020).[11] Finally, and in line with our expectations, we observe that CTL is more likely in the year after terrorist events occurred. The implied odds ratios indicate that doubling the number of terrorist events increases the probability of CTL by about 15%. If we plug in the number of fatalities in the place of the number of terrorist incidents following hypothesis 1b, we find that this number is not significantly correlated with the extensive margin, but it is with the intensive margins (Table 6 in Appendix). In other words, all other things being equal, the number of incidents is driving whether or not additional legislation is passed, whereas the death toll more clearly drives the number of legislative pieces passed.

Conversely, we find no evidence of economic or political differences: Richer countries are not more likely to introduce new CTL, recessions do not appear to make governments more likely to react, and neither the ideology nor the fractionalization of parliament matters to the extensive margin. Most importantly, we find no indications to suggest that the design of the emergency constitution in any way affects the probability of introducing further CTL. In other words, our findings indicate that the constitutional provisions regarding events such as terrorist attacks are entirely detached from responses to such events in statutory law.[12]

When we turn to the intensive margin, that is, how many separate laws were introduced when we observe any change, we again find that more populous countries introduced more CTL. In column 3, we also find evidence that countries with less fractionalized parliaments (a higher fractionalization score) introduced more CTL. Yet, the estimates in column 4, in which we exclude all autocracies, indicate that this result is entirely driven by autocracies such that autocracies that do not allow political opposition introduce more CTL.

We also find that countries in which laws are actually implemented as one would expect according to the letter of the law imply *fewer* changes when we observe any change. In other words, countries with actually enforced institutions are more likely

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[10] Although not shown, we have performed a full country jackknife in which we have removed one country at a time and rerun our main regressions. From this set of results, we can easily observe that the exclusion of the USA does not appreciably change any estimates.

[11] In a set of additional robustness tests in which we lag all control variables, we even find that new CTL is significantly *more* likely in the year after an election. We believe this finding reinforces our interpretation that these events are unpopular. The results are available upon request.

[12] We refrain from testing hypothesis #3, which posits that countries without emergency constitutions are more likely to pass CTL than countries with extensive constitutionalized emergency provisions for the simple reason that our sample only contains three such countries, namely Iceland, New Zealand, and the UK.

to introduce CTL, but typically do so in a *single* law instead of throughout several pieces of legislation. When focusing on the democratic subsample in column 4, we also observe that right-wing parliaments are substantially more likely to introduce more pieces of legislation whenever they act. Conversely, we again find no indications that the emergency constitution is in any systematic way reflected in changes to statutory law.

The results regarding the spatial diffusion effects discussed as hypotheses 2a and 2b above are depicted in Table 3. The results are pretty clear: Diffusion does not occur as a consequence of terrorist incidents having taken place in neighboring countries but is observed as a consequence of CTL passed in neighboring countries.

In further tests (not shown), we find that these results are robust to a number of potential problems.[13] We thus find a small but very robust set of determinants of CTL.

Does CTL Have Any Effects on Terrorist Incidents?

Although this is one of the major questions in the entire literature on terrorism, we cannot say anything definitive about the efficacy of counterterrorist legislation because the data do not provide actual information about the strength of the legislative framework. The problem is, as stressed in the data description above, that we can observe *changes* to legislation, but do not know any details of the already existing legislation. We also do not have any information about the degree to which specific legislation such as the one on counterterrorism is actually enforced, which prevents us from making any claims about effectiveness.

However, we can provide some indication by comparing how likely countries were to introduce additional CTL in the years prior to the 9/11 attacks in 2001 to the number of terrorist incidents thereafter. In other words, we compare the growth of CTL prior to 2000, which we think of as a proxy for the stock of functional legislation before the 9/11 attacks, to how likely it appears to have been that that stock deterred terrorism. Our data are summarized in Fig. 1, which depicts the change in the number of terrorist attacks between the 16 years prior

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[13] For example, attempting to separate different types of CTL appears to yield identical results. Similarly, following the separation of terrorist events in Bjørnskov and Voigt [2020], in events against the military or police, against government and government installations, and against public infrastructure, we also find practically identical results. In addition, we also do not find that our results depend on whether a state of emergency was called as a response to the terrorist events. In a set of additional tests, we used the Louis and Shor [2019] dataset to separate legislation that devotes at least one section or article directly to terrorism and legislation that mentions terrorism in the body of the law from other legislation. Our presumption was that the former kind may be more severe, which to some extent is confirmed by the structure of the data: legislation that revolves around compensation to victims, intentions to fight terrorism, or that is simply an amendment to existing law and ratification of international law is often a separate category. However, we find absolutely no differences in the determinants of legislation events between the two categories. These results are available upon request. Finally, we have also performed a country jackknife in which single countries were dropped from the sample; this test also revealed that our main results are not driven by single countries.

Table 3 Spatial spill-overs and constitutional conditionality, any legislation

	1	2	3	4	5	6
Log GDP per capita	.008 (.222)	.013 (.225)	.028 (.218)	.008 (.216)	.008 (.222)	.016 (.216)
Recession	−.128 (.184)	−.103 (.184)	−.137 (.187)	−.143 (.189)	−.128 (.184)	−.109 (.185)
Log population size	.281*** (.075)	.269*** (.075)	.277*** (.076)	.279*** (.075)	.281*** (.074)	.281*** (.072)
Election year	−.265** (.119)	−.265** (.119)	−.264** (.119)	−.265** (.119)	−.265** (.119)	−.263** (.119)
Parliament fractionalization	.232 (.605)	.286 (.609)	.228 (.598)	.247 (.595)	.232 (.604)	.283 (.604)
Parliament ideology	−.296 (.381)	−.253 (.385)	−.299 (.382)	−.314 (.375)	−.295 (.379)	−.289 (.370)
Failed coup	−.162 (.588)	−.182 (.574)	−.131 (.593)	−.134 (.599)	−.161 (.587)	−.191 (.586)
Judicial accountability	.242* (.137)	.232* (.137)	.218 (.141)	.211 (.141)	.242* (.137)	.235* (.134)
Log no. of terrorist events	.135* (.074)	.115 (.112)	.154* (.073)	.157 (.074)	.135* (.074)	.136 (.074)
Cost INEP	−.408 (.517)	−.020 (.584)	−.317 (.554)	.086 (.832)	−.408 (.517)	.121 (.539)
Benefit INEP	.147 (.500)	−.414 (.616)	.004 (.517)	.227 (.807)	.148 (.499)	.675 (.584)
Log neighbor terrorism			−.080 (.074)	.034 (.124)		
Neighbor CTL					.024 (.274)	1.251** (.542)
Log terrorism * cost INEP		−.315 (.277)				
Log terrorism * benefit INEP		.421* (.234)				
Neighb. Terr. * cost INEP				−.197 (.308)		
Neighb. Terr. * Benefit INEP				−.115 (.271)		
Neighb. CTL * cost INEP						−1.772 (1.218)
Neighb. CTL* Benefit INEP						−1.751 (1.168)
Regional FE	Yes	Yes	Yes	Yes	Yes	Yes
Annual FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2193	2193	2193	2193	2193	2193
Countries	67	67	67	67	67	67
Wald Chi-squared	1382.98	5959.79	1314.30	1276.11	1512.03	1747.10
Log likelihood	−984.271	−982.421	−983.372	−982.763	−984.266	−979.891

*** (**) [*] denote significance at $p < .01$ ($p < .05$) [$p < .10$]; regions are Latin America, the Caribbean, Asia, Communist countries, and formerly communist countries. Numbers in parentheses are robust standard errors

to 2001 and the 16 years *after* 2000 (until 2016). Likewise, the figure also depicts the change in the number of years with terrorist attacks (the extensive margin)

between the same time periods. To draw some inference from these numbers, we split our sample into countries that had passed little CTL between 1984 and 1999 (below median activity) and those with much CTL (above median). We thus compare the risk of observing terrorist attacks before and after the 9/11 attacks across countries that had already introduced substantial CTL in the preceding 16 years and countries that had not.

We observe that countries that had already introduced much new CTL in the years between 1984 and 1999—amounting on average to four new pieces of CTL and therefore a priori would be better protected against terrorism—on average saw a doubling of the number of attacks after 9/11, while those with relatively few changes (about .5 changes)—that one would think were less protected if CTL worked as intended—saw a 40% decline. Yet, this difference is not statistically significant ($p < .32$) and turns out to be driven entirely by events in Ukraine following the Russian invasion of the country's eastern regions in 2014. Conversely, when looking at the number of years with terrorist events, the former group of countries—the more legislatively active group prior to 2000—saw an average drop of 1.4 years with at least one terrorist attack versus 3.4 years in the latter ($p < .03$) although the two groups were not significantly different prior to 2000 (8.5 event years versus 9.5; $p < .32$). In terms of changes to CTL, the two groups even became more similar after 2000, as the latter introduced an average of 4.7 new laws, while the former introduced 3.3 ($p < .002$).

As such, while we can hardly claim that this is definitive evidence, the indications summarized in Fig. 1 do not indicate that CTL is likely to be particularly effective in combating terrorism. If so, one would strongly have suspected that the more vigilant political traditions, as judged by activity between 1984 and 1999, would already have had stronger legislative safeguards against terrorism and thus have observed a larger decline in terrorist activity than those countries more hesitant to adopt additional legislation. What we observe is the exact opposite pattern, which thus questions the efficacy of CTL.[14]

Does CTL Affect Civil Liberties?

It has often been argued that fresh CTL is bad news for civil liberties (Shor 2019 references more than 40 such papers). Our answer to that question is contained in Tables 4 and 5, which uses the same specification as previous tables but also includes a twice-lagged dependent variable.

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[14] This finding is also in line with Bjørnskov and Voigt [2020] where we find that declaring a state of emergency does not reduce the likelihood of more terrorist incidents occurring.

Fig. 1 Reduction in terrorist events, many or few legislative changes prior to 2000
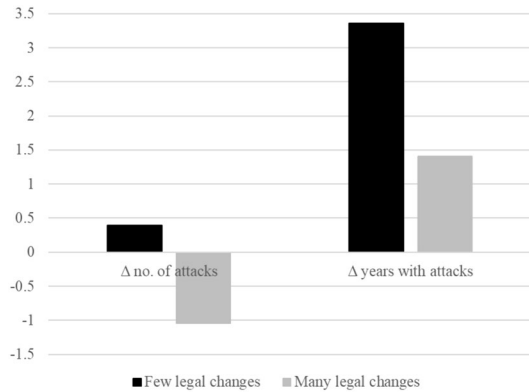
When we simply ask if a country introduced any CTL in a given year, we generally do not see a significant correlation with any of the dependent variables.[15] The only exception is the positive correlation between CTL and media freedom reported in column 6 of Table 4, although the coefficient has an unexpected sign. However, this result is fragile and does not remain significant when correcting for multiple test bias or excluding single countries; a similar problem applies to the couple of findings that are significant at the 10% level. Three of the 12 estimates in the lower panels in Tables 3 and 4, in which we use the number of newly passed CTL, are significant but also not as robust as we would prefer. So, for our sample, we cannot confirm that CTL has been detrimental to civil rights. The number of terrorist events, however, is negatively correlated with civil liberties throughout all dependent variables. It thus seems that government reactions to terrorism cause a deterioration in civil liberties and not the passing of new CTL. This observation might help to reconcile all the studies that observed a deterioration of civil liberties subsequent to the passing of fresh CTL: They might simply not have sufficiently controlled for the number of terrorist incidents.

Whereas the passing of CTL does not have systematic effects on civil liberties, constitutional emergency provisions do. The easier ("less costly") it is for government to call a state of emergency, the lower the respect for physical integrity rights. On the other hand, the more competences government enjoys under a state of emergency according to the constitution, the higher its respect for both physical integrity and NGO rights. Looked at from the other side, this finding seems to make complete sense: The fewer additional competences a government is constitutionally granted under a state of emergency, the more likely it is not to respect both physical integrity rights and NGO rights outside of emergencies.

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[15] Some readers might wonder how that can be given that "repression of civil liberties" is one component of the Counterterrorism Legislation dataset that we rely upon. Whereas that measure refers to *de jure* legislation, the civil liberties measures used as dependent variable in this section are all *de facto* measures. If there is no significant correlation between the two this could, hence, be due to the imperfect implementation of CTL.

Beyond these central findings, a number of additional results are noteworthy: First of all, economic confounders are significantly correlated with both the respect for human rights as coded by Fariss (2014) and media censorship, implying that recessions are bad news for civil liberties. The correlations with the political covariates are mostly in line with our expectations: Democracies generally fare better. This is, however, not the case with regard to media freedom (as in Bjørnskov and Voigt 2021). Finally, election years are good news for physical integrity as well as NGO rights since during these years, these rights tend to improve.

In sum, we have been unable to establish any robust negative effects of CTL on civil liberties. This is an important non-result as many papers claim the existence of such effects (cf. Shor et al. 2018). We should, however, remind the reader that this study covers a specific set of countries, namely those that have a Western style constitution aiming at high levels of the rule of law, at least according to the letter of the constitution. Putting these results into the context of the three possible rationales for passing fresh CTL named in the introduction, we find no unambiguous evidence for the third rationale, namely that terrorist events are generally misused as a welcome pretext to introduce legislation that can be used to curtail civil rights. To the degree that politicians follow the first rationale, namely to reduce the likelihood of more terrorist events occurring, they seem to be rather unsuccessful, as shown in Section "Does CTL Affect Civil Liberties?" above. Given that the passing of CTL has little effects on both reducing terrorism and civil liberties, the second rationale—that the passing of CTL is primarily a political placebo—seems to fit best with our findings.

Discussion, Conclusions, and Outlook

In this paper, we ask two questions: What are the factors determining the passing of counterterrorist legislation (CTL)? And: What are the effects of passing such legislation both on the likelihood of terrorist incidents and on civil liberties? We ask these two questions explicitly taking the respective constitutionalized emergency provisions into account. Separating an extensive from an intensive margin, that is, separating the issue whether any CTL was introduced from the issue how many such laws were introduced, we find a small but robust set of determinants. Regarding the extensive margin, we find that the number of terrorist events is an important factor in inducing CTL, while governments are considerably less likely to pass CTL in election years. Regarding the intensive margin, we find that less fractionalized legislatures are likely to pass more new laws as are more conservative governments. When countries characterized by high-quality institutions pass CTL, they are likely to do so in a single piece of legislation. Our most relevant finding is that passing statutory CTL is entirely unconnected to a country's constitutionalized emergency provisions.

With regard to the question whether CTL leads to a reduction in terrorist incidents, we are unable to provide a convincing fact-based answer because the dataset on CTL does not contain any coding on the severity of measures contained in the respective pieces of legislation. The educated guess presented in Section "Does CTL Affect Civil Liberties?" in which we group countries into legislatively active

Table 4 Effect of CTL on civil liberties

	Physical integrity rights		CSO rights		Media censorship	
	1	2	3	4	5	6
Twice-lagged dependent variable	.827*** (.031)	.814*** (.039)	.765*** (.039)	.711*** (.054)	.851*** (.031)	.858*** (.035)
Log GDP per capita	.022*** (.008)	.026** (.011)	.024** (.011)	.029** (.120)	.106** (.042)	.097** (.047)
Recession	−.004 (.008)	−.008 (.007)	−.000 (.008)	−.010* (.006)	.026 (.047)	−.074** (.037)
Log population size	−.003** (.002)	−.003 (.002)	−.002 (.002)	−.000 (.002)	−.001 (.011)	.005 (.009)
Electoral autocracy	.018 (.018)		.042 (.027)		−.140 (.161)	
Democracy	.079*** (.027)		.120*** (.042)		−.006 (.176)	
Election year	.009*** (.003)	.009*** (.003)	.009** (.004)	.012*** (.004)	.007 (.014)	.019 (.014)
Parliament fractionalization	.056*** (.019)	.084*** (.028)	.055*** (.019)	.080*** (.023)	.088 (.087)	.156 (.089)
Parliament ideology	−.009 (.009)	−.009 (.011)	−.008 (.009)	−.012 (.013)	.129** (.066)	.081 (.061)
Failed coup	−.014 (.039)	.003 (.045)	−.002 (.031)	−.009 (.036)	.110 (.124)	.039 (.094)
Judicial accountability	.003 (.004)	.002 (.004)	.005 (.004)	.007 (.005)	.036 (.023)	.029 (.021)
Log no. of terrorist events	−.005* (.002)	−.009*** (.003)	−.005* (.003)	−.009*** (.003)	−.022 (.014)	−.035*** (.010)
Cost INEP	−.028** (.013)	−.029** (.014)	−.023 (.014)	−.027 (.018)	−.091 (.077)	−.053 (.063)
Benefit INEP	.028** (.011)	.025* (.014)	.040*** (.013)	.050*** (.018)	.125 (.084)	.094 (.075)
Any new CTL	−.001 (.002)	−.000 (.002)	.001 (.003)	.001 (.003)	.019 (.014)	.029** (.014)
Regional FE	Yes	Yes	Yes	Yes	Yes	Yes
Annual FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2508	2156	2508	2156	2552	2199
Countries	70	67	70	67	70	67
Wald Chi-squared	690098.88	–	936514.38	–	344189.99	–
Within R-squared	.798	.552	.825	.553	.781	.565
Results using log # of newly passed laws						
CTL legislation	−.004 (.003)	−.003 (.003)	−.001 (.003)	−.000 (.003)	−.019 (.016)	.026 (.016)

*** (**) [*] denote significance at $p < .01$ ($p < .05$) [$p < .10$]; regions are Latin America, the Caribbean, Asia, Communist countries, and formerly communist countries. Numbers in parentheses are robust standard errors

Table 5 Effect of CTL on civil liberties, non-V-Dem measures

	Fariss repression		Freedom House rights		CIRI human rights	
	1	2	3	4	5	6
Twice-lagged dependent variable	.916*** (.018)	.908*** (.019)	.721*** (.034)	.666*** (.044)	.616*** (.039)	.633*** (.042)
Log GDP per capita	.007 (.004)	.006 (.005)	.042*** (.010)	.055*** (.014)	.039** (.017)	.042** (.020)
Recession	−.004 (.003)	−.002 (.002)	.006 (.007)	−.000 (.005)	−.015* (.008)	−.006 (.009)
Log population size	−.004*** (.001)	−.005*** (.002)	−.004* (.002)	−.004 (.003)	−.016*** (.005)	−.013*** (.005)
Electoral autocracy	.004 (.010)		.018 (.029)		−.001 (.033)	
Democracy	.025** (.011)		.097** (.039)		.068* (.036)	
Election year	.001 (.001)	.002 (.001)	−.001 (.003)	.0029 (.003)	.002 (.006)	.007 (.006)
Parliament fractionalization	.006 (.011)	.019 (.013)	.049*** (.018)	.074*** (.020)	.059** (.032)	.054 (.041)
Parliament ideology	.001 (.004)	−.004 (.005)	.004 (.011)	−.005 (.014)	.005 (.017)	−.014 (.019)
Failed coup	−.011 (.011)	−.012 (.012)	−.017 (.034)	−.009 (.040)	−.056 (.035)	−.036 (.050)
Judicial accountability	.003 (.002)	.004* (.002)	.015** (.005)	.018*** (.007)	.007 (.009)	.008 (.009)
Log no. of terrorist events	−.004*** (.001)	−.004*** (.001)	−.005* (.002)	−.006*** (.002)	−.025*** (.004)	−.024*** (.005)
Cost INEP	−.006 (.007)	−.010 (.008)	−.011 (.017)	−.009 (.021)	−.068** (.027)	−.054** (.027)
Benefit INEP	.017** (.007)	.025*** (.009)	.019 (.015)	.014 (.021)	.033 (.033)	.019 (.036)
Any new CTL	−.002 (.001)	−.003* (.002)	−.003 (.003)	−.006* (.003)	−.004 (.006)	−.006 (.006)
Regional FE	Yes	Yes	Yes	Yes	Yes	Yes
Annual FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2485	2145	2456	2133	1779	1597
Countries	70	67	70	67	70	67
Wald Chi-squared	338782.54	–	162202.28	–	6531.92	–
Within R-squared	.881	.840	.793	.452	.261	.214
Results using log # of newly passed laws						
CTL legislation	−.004** (.002)	−.004** (.002)	−.005 (.003)	−.007** (.003)	−.000 (.006)	−.002 (.006)

*** (**) [*] denote significance at $p < .01$ ($p < .05$) [$p < .10$]; regions are Latin America, the Caribbean, Asia, Communist countries, and formerly communist countries. Numbers in parentheses are robust standard errors

vs. non-active ones prior to 2001 and then ask whether the legislatively active ones have been hit by fewer terrorist events post 2001 would make us think that CTL has been rather ineffective. This finding is in line with our previous finding (Bjørnskov and Voigt 2020) that declaring a state of emergency subsequent to a terrorist attack does not significantly reduce the likelihood of suffering from yet another such attack. It is also consistent with our final result that CTL does not seem to affect the protection of citizens' civil liberties, which may suggest that it is mostly implemented for purely political reasons. Given the many heated debates preceding the passing of CTL, we can with reasonable certainty assume that they are much ado about nothing.

Our finding that the details of constitutionalized emergency provisions are in no way determining whether new CTL is passed and if so, how much of it, is unexpected. In previous papers, we found that the emergency provisions matter in a number of ways, sometimes in unexpected ones. One rather nasty way to interpret this combination of findings is that the changes to statutory law that we see here do tend to follow terrorist attacks, but they are meant to signal political will and competence, and not to be effective in any real way. This is in line with the second rationale in favor of CTL mentioned in the introduction, namely that it serves primarily as a placebo. At the same time, it can be interpreted as a refutation of the two other rationales named there, namely to fight terrorism and to serve as a pretext for governments to make their own position more secure by curtailing civil liberties. With regard to this last point, it is worth reminding the reader that we are here comparing the most democratic countries of the world. In other world regions, we might, hence, find completely different results.

Appendix

See Appendix Table 6.

Table 6 Alternative terrorism measure, no. of fatalities

| | Any legislation | | #legal changes | |
	1	2	3	4
Log GDP per capita	−.122 (.207)	.074 (.243)	.755** (.330)	.612* (.370)
Recession	−.224 (.189)	−.265 (.207)	−.048 (.381)	−.156 (.419)
Log population size	.462*** (.084)	.440*** (.089)	.307*** (.109)	.306** (.121)
Electoral autocracy	.759 (.782)	–	.943 (.959)	–
Democracy	.191 (.835)	–	1.862** (.865)	–
Election year	−.166 (.146)	−.243* (.142)	−.105 (.252)	−.037 (.255)
Parliament fractionalization	.019 (.530)	.442 (.645)	2.235*** (.855)	1.120 (.946)
Parliament ideology	.120 (.349)	−.151 (.399)	.965 (.567)	1.517**(.680)
Failed coup	.246 (.569)	.260 (.575)	−.316 (1.367)	−.185 (1.191)
Judicial accountability	.252* (.152)	.268** (.171)	−.333** (.126)	−.345*** (.124)
Log no. of fatalities	−.043 (.124)	−.065 (.143)	.176** (.083)	.189* (.097)
Cost INEP	−.014 (.638)	−.319 (.636)	.247 (.737)	.099 (.746)
Benefit INEP	.059 (.572)	.461 (.566)	−.165 (.604)	−.114 (.617)
Regional FE	Yes	Yes	Yes	Yes
Annual FE	Yes	Yes	Yes	Yes
Observations	1986	1652	443	402
Countries	70	67	66	61
Wald Chi-squared	596.22	734.11	3015.98	2830.66
Log likelihood	−838.617	−743.611	−401.963	−375.348

Regions are Latin America, the Caribbean, Asia, Communist countries, and formerly communist countries. Numbers in parentheses are robust standard errors

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