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Source: *The Journal of Legal Studies*, Vol. 41, No. 1 (January 2012), pp. 131-164
Published by: The University of Chicago Press for The University of Chicago Law School
Stable URL: http://www.jstor.org/stable/10.1086/663766
Accessed: 12/02/2014 16:12

# Human Rights Violations after 9/11 and the Role of Constitutional Constraints

**Benedikt Goderis and Mila Versteeg**

**ABSTRACT**

After 9/11, the United States and its allies took measures to protect their citizens from future terrorist attacks. While these measures aim to increase security, they have often been criticized for violating human rights. But violating rights is difficult in a constitutional democracy with separated powers and checks and balances. This paper empirically investigates the effect of the post-9/11 terror threat on human rights. We find strong evidence of a systematic increase in rights violations in the United States and its ally countries after 9/11. When testing the importance of checks and balances, we find that this increase is significantly smaller in countries with independent judicial review (countermajoritarian checks) but did not depend on the presence of veto players in the legislative branch (majoritarian checks). These findings have important implications for constitutional debates on rights protection in times of emergency.

## 1. INTRODUCTION

After 9/11, the governments of the United States and its allies took a range of counterterrorism measures to protect their citizens from future

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[*Journal of Legal Studies,* vol. 41 (January 2012)]
 0047-2530/2012/4101-0005$10.00

terrorist attacks.[1] While these measures aim to increase domestic security, they have often been criticized for violating human rights. Coercive interrogation, ethnic profiling, interception of communications, and preventive arrests and detention may be necessary to fight terrorism but are often in violation of a country's commitment to human rights. A common response to such criticisms is that there is a trade-off between security and liberty and that in times of national emergency, such as the aftermath of 9/11, the security-liberty balance shifts in favor of security (Posner and Vermeule 2008; Posner 2007). Indeed, opinion polls suggest that people have been willing to trade rights against security (Davis and Silver 2004; Cole and Dempsey 2006). Moreover, the extensive media coverage of a few big human rights controversies, such as the indefinite pretrial detentions in Belmarsh prison in London and Guantánamo Bay in Cuba, suggests that rights have been violated after 9/11. But are these isolated incidents or has there been a systematic deterioration in the human rights practices of the West?

Violating human rights is not easy in a constitutional democracy with separated powers and checks and balances. Democratic governments may be constrained by what we will call majoritarian and countermajoritarian checks and balances. Majoritarian checks play out in the relationship between the executive and legislative branches. Such checks have traditionally been thought of as the power that the legislative and executive branches exercise over each other (Hamilton and Madison 1788). However, today, the relationship between political parties within those branches is arguably more important than the constitutional arrangements between the branches as such (Levinson and Pildes 2006; Tsebelis 2002). If there is an opposition party in the legislative branch that is large enough to exercise veto power, it may constitute an effective check on the executive. This veto power is exercised to further the interest of the majority of voters. Political parties, after all, seek electoral

mission Second Workshop of the Network for the Economic Analysis of Terrorism, the Annual Conference of the Netherlands Network of Economics, and the Center for Economic Studies and Ifo Institute Second Workshop on Political Economy for helpful comments.

1. The United States adopted 53 resolutions and 68 acts in the first year after 9/11, including the 2001 PATRIOT Act (Library of Congress 2002). Canada drafted the 186-page Anti-terrorism Act within a month (Roach 2003). Australia introduced 40 pieces of counterterrorism legislation (Australian Human Rights Commission 2008), while the United Kingdom also adopted several comprehensive counterterrorism bills. Germany adopted a security package that amended "nearly one hundred regulations in seventeen different statutes and five statutory orders," and all European Union (E.U.) members incorporated a common definition of terrorism in their criminal laws following the 2002 E.U. Framework Decision on Combating Terrorism (Scheppele 2004).

support and reelection. Therefore, when the majority of voters want to trade rights against security, veto players remain silent. But when the majority opposes counterterrorism measures, veto players are likely to use their vetoes. While majoritarian checks may not prevent a tyranny of the majority, they do prevent tyranny of the executive. One may thus expect that countries with majoritarian checks have seen fewer post-9/11 rights violations than countries without majoritarian checks, but only to the extent that violations were contrary to the wishes of the majority.

Countermajoritarian checks are exercised by the judicial branch. If an independent judicial branch is equipped with the power of judicial review, it may enforce the nation's constitutional precommitments to rights and invalidate laws that violate the constitution. When using this power, the judiciary guarantees a minimum level of rights protection regardless of what the majority wants. It keeps majorities to their precommitments, even when, at a later time, majorities favor security over rights (Elster 1979; Holmes 1988). One may therefore expect that countries with independent judicial review have seen fewer rights violations after 9/11 than countries without such review.

In this paper, we empirically analyze these issues. We use an ordered probit model and difference-in-differences estimation for 152 countries between 1978 and 2006 to investigate the effect of the post-9/11 terror threat on human rights. We find strong evidence of a systematic increase in rights violations in the United States and its allies after 9/11. When testing the importance of checks and balances, we find that this increase is significantly smaller in countries with independent judicial review (countermajoritarian checks) but does not depend on the presence of veto players in the legislative branch (majoritarian checks).

Our findings have implications for constitutional debates on rights protection in times of emergency. After 9/11, scholars have stood divided on whether a nation should stick to its precommitments to human rights (Dworkin 2003; Levinson 2002) or whether flexible security-liberty trade-offs should prevail (Posner 2007; Posner and Vermeule 2008). An important view is that in times of emergency, majorities panic (Ignatieff 2004; Stone 2004; Ackerman 2004) and democracies fail to take account of minority interests (Cole 2003; Sunstein 2004). In these times, it is best to stick to precommitments. The opposite view is that a constitution is "not a suicide pact" (Justice Goldberg in *Kennedy v. Mendoza Martinez* [372 U.S. 144 (1963)]) and that a constitution that "will not bend will break" (Posner 2007). A government may have to compromise rights today to save lives in the future. This paper is the first to empirically establish that, overall, Western countries did not stick to their precommitments after 9/11. Whether the trade-off was real or imagined, rights have been traded against (the perception of) security. While rights deteriorated in the first 4 post-9/11 years, they improved again in 2005 and 2006. This may indicate that the fear of a "ratchet effect" (Posner and Vermeule 2008), or that when bending the constitution, it will never bend back, is not justified.

We also find that in countries with strong judicial review, courts prevented such rights violations in the first place. This finding is important for those commentators who are divided on the appropriate institutional competence of different branches in times of emergency. Those who favor flexible balancing focus on the executive: the executive is best equipped to act fast and deal flexibly with security threats. Courts should not exercise review but defer to the executive (Posner and Vermeule 2008; Yoo 2005). Those who favor precommitments focus on the judiciary: courts should scrutinize security policies and invalidate measures that violate rights (Barak 2002; Koh 2002). Those who favor a middle way focus on the legislature: legislatures should make sure that security measures represent the wishes of the majority of the people. Judicial review should serve only to strengthen this political process (Sunstein 2004; Issacharoff and Pildes 2004). Our results suggest that one's normative position on institutional competence directly implicates rights. Legislatures clearly do worse in protecting rights than the judiciary. And in contrast with claims that judges are unable to exercise review in times of crises (Ackerman 2004; Posner 2007), courts did protect rights after 9/11. Cicero's maxim "silent enim leges inter arma" [during war law is silent] did not apply to constitutional law after 9/11. Yet, judicial review to strengthen legislative involvement, as predominant in the U.S. Supreme Court, is unlikely to protect rights, as legislatures have allowed for significant right violations.[2] The ineffectiveness of legislative checks also tells us something about the executive. Apparently, executives were not trying to take advantage of citizens or pursuing partisan objectives but violated rights on the majority's account. If not, legislatures would have vetoed such a course of action.

The rest of this paper is structured as follows. Section 2 describes

2. The U.S. Supreme Court in times of war does not directly enforce constitutional rights but only requires presidential action to be authorized by Congress (Issacharoff and Pildes 2004).

the methodology and data. Section 3 reports the results of estimating the effect of 9/11 on human rights. Section 4 investigates whether this effect depends on checks and balances. Section 5 concludes.

## 2. METHODOLOGY AND DATA

In this section, we describe our econometric model and the variables used in the estimation. The effect of the (perceived) post-9/11 terror threat on human rights is analyzed using the following ordered probit model:

$$y^*_{i,t} = \alpha t_i + \beta p_t + \delta(t_i \times p_t) + \Sigma^l_{k=1}\theta_k y_{i,t-k} + \gamma' z_{i,t} + \varepsilon_{i,t} \tag{1}$$

and

$$y_{i,t} = j \quad \text{if } \lambda_{j-1} < y^*_{i,t} \leq \lambda_j, \tag{2}$$

where the subscripts $i = 1, \ldots, N$ and $t = 1, \ldots, T$ index the countries and years in the panel data set used for estimation. The variable $y_{i,t}$ represents an indicator of human rights violations with an ordinal scale ($j = 1, 2, \ldots, M$), $y^*_{i,t}$ is an underlying latent variable, and the $\lambda_j$ values are cutoff values. The probability that the indicator of human rights violations takes a value of $j$ is the probability that the latent variable $y^*_{i,t}$ takes a value between $\lambda_{j-1}$ and $\lambda_j$.[3]

We evaluate the impact of the post-9/11 terror threat using a difference-in-differences estimator. Hence we identify a treatment group of countries that was exposed to an increased terror threat after 9/11 and a control group that was not exposed. The variable $t_i$ is a treatment-group-specific effect, which is included to account for average permanent differences in rights violations between treatment and control (that is, differences that are unrelated to 9/11). The variable $p_t$ is a period-specific effect, which is included to control for post-9/11 changes in rights violations that are common to the treatment and control groups and hence unrelated to 9/11. The variable $p_t$ takes a value of one for the year 2001 and all subsequent years and zero otherwise.[4] The coefficient $\delta$ of the interaction term between $t_i$ and $p_t$ captures how the effect of the post-9/11 period differs between the treatment and the control group and therefore measures the true effect of the treatment, that is, the true effect of the post-9/11 terror threat on human rights. The aim of our empirical analysis is twofold. We first test the effect of the post-9/11 terror threat

3. We compute robust standard errors clustered by country to account for heteroskedasticity and within-country serial correlation in the error terms.

4. Our results, while less significant, are not very different when using 2002–2006 instead of 2001–2006 as the post-9/11 period.

on human rights; hence we attempt to find a good estimate of $\delta$. We then investigate whether this effect occurs conditional on a country's checks and balances.

Following the empirical human rights literature (for example, Dreher, Gassebner, and Siemers 2010; Hafner-Burton and Tsutsui 2005; Poe and Tate 1994), we also include lags of the dependent variable and a vector $z_{i,t}$ of control variables: log gross domestic product (GDP) per capita, GDP per capita growth, democracy, log population, and civil war.[5] Our data set consists of all countries and years for which data are available and covers 152 countries between 1978 and 2006. Table 1 reports summary statistics and data sources for the variables used in the estimation. Next we discuss how the key components of equations (1) and (2) were constructed.

### 2.1. Measuring Human Rights

In recent decades, political scientists have developed several indicators of government respect for human rights. The most commonly used is the political terror scale, which measures political violence on a 1–5 ordinal scale (Gibney, Cornett, and Wood 2008). The political terror scale contains two indicators that were constructed using the same coding methodology but draw from two independent sources. The first (pters) is based on the annual U.S. State Department country reports on human rights, while the second (ptera) is based on the annual Amnesty International country reports on human rights. The U.S. State Department reports were introduced during the Nixon administration, when Congress adopted the Harkin amendment to the Foreign Assistance Act, which prohibits development assistance to governments engaged in gross violations of human rights (Poe, Carey, and Vazquez 2001). Data collection for the U.S. State Department reports is a collective effort of U.S. embassies around the world, which gather information "from a variety of sources across the political spectrum, including government officials, jurists, armed forces sources, journalists, human rights monitors, academics, and labor activists" (U.S. State Department 2007). The Amnesty International reports are a tool for Amnesty to further respect for human rights through awareness raising and "naming and shaming." Amnesty International collects its information in similar fashion to the U.S. State

5. Our results are robust to the inclusion of additional controls, including trade openness, international war, region dummies, and year dummies. These variables are not included in our baseline specifications because they either were not robustly significant or severely lowered the number of observations.

**Table 1.** Summary Statistics

| | $N$ | Mean | SD | Min | Max | Source |
|---|---|---|---|---|---|---|
| 1. pters | 3,481 | 2.39 | 1.10 | 1 | 5 | Gibney, Cornett, and Wood (2008) |
| 2. ptera | 2,960 | 2.70 | 1.05 | 1 | 5 | Gibney, Cornett, and Wood (2008) |
| 3. physint | 3,199 | 3.12 | 2.25 | 0 | 8 | Cingranelli and Richards (2008) |
| 4. Troops Afghanistan and Iraq (dummy) | 3,581 | .17 | .37 | 0 | 1 | U.S. Central Military Command (2007), fact sheets by the U.S. Department of Defense and NATO |
| 5. Troops Afghanistan or Iraq (dummy) | 3,581 | .33 | .47 | 0 | 1 | U.S. Central Military Command (2007), fact sheets by the U.S. Department of Defense and NATO |
| 6. NATO (dummy) | 3,583 | .12 | .33 | 0 | 1 | Gibler and Sarkees (2004); NATO and 22 U.S.C. 2321k (1996) |
| 7. NATO Plus (dummy) | 3,583 | .32 | .47 | 0 | 1 | Gibler and Sarkees (2004); NATO and 22 U.S.C. 2321k (1996) |
| 8. (2001–2006) (dummy) | 3,583 | .24 | .43 | 0 | 1 | |
| 9. GDP per capita (log) | 3,583 | 7.46 | 1.58 | 4.69 | 10.72 | World Bank, World Development Indicators |
| 10. GDP per capita growth | 3,583 | .02 | .05 | −.63 | .64 | World Bank, World Development Indicators |
| 11. Democracy | 3,583 | 4.75 | 4.17 | 0 | 10 | Polity IV[a] |
| 12. Population (log) | 3,583 | 16.06 | 1.55 | 12.34 | 20.99 | World Bank, World Development Indicators |
| 13. Civil war (dummy) | 3,583 | .07 | .26 | 0 | 1 | Gleditsch (2004)[b] |
| 14. Independent judicial review (ijr) (dummy) | 1,751 | .45 | .50 | 0 | 1 | La Porta et al. (2004) |
| 15. Political constraints (pc) (dummy) | 3,570 | .31 | .46 | 0 | 1 | Henisz (2002)[a] |
| 16. Trade openness (log trade as % of GDP) | 3,437 | 4.16 | .55 | 1.84 | 6.16 | World Bank, World Development Indicators |
| 17. International war (dummy) | 3,583 | .01 | .11 | 0 | 1 | Gleditsch (2004)[b] |

**Note.** NATO = North Atlantic Treaty Organization.

[a]Data for 2005 and 2006 were missing, so we used 2004 scores for these years.

[b]Data for 2006 were missing, so we used the 2005 values for this year.

Department but relies on its own employees, local human rights activists, and nongovernmental organizations (NGOs; Amnesty International 1998).

The political terror scale indicators translate the qualitative information from these country reports into five-point ordinal scales. The indicators are available for 183 countries from 1976 to 2006. Each country, in each year, is placed in one of the following categories[6]:

1. Countries are under a secure rule of law, people are not imprisoned for their views, and torture is rare or exceptional. Political murders are extremely rare.
2. There is a limited amount of imprisonment for nonviolent political activity. However, few persons are affected, and torture and beatings are exceptional. Political murder is rare.
3. There is extensive political imprisonment or a recent history of such imprisonment. Execution or other political murders and brutality may be common. Unlimited detention, with or without a trial, for political views is accepted.
4. Civil and political rights violations have expanded to large numbers of the population. Murders, disappearances, and torture are a common part of life. In spite of its generality, on this level terror affects those who interest themselves in politics or ideas.
5. Terror has expanded to the whole population. The leaders of these societies place no limits on the means or thoroughness with which they pursue personal or ideological goals.

Cingranelli and Richards (2008) recently developed an alternative set of indicators, also based on the U.S. State Department and Amnesty International reports. Their Cingranelli and Richards (CIRI) data set contains not only aggregate measures of government repression but also 13 disaggregated measures that capture specific rights. The aggregate physical integrity rights index (physint) captures the type of rights that have most likely been affected by post-9/11 counterterrorism initiatives. While substantively similar to the political terror scale, the physical integrity index is not based on a holistic assessment of a country's degree of state violence but rather assesses the frequency of four types of rights violations: torture, extrajudicial killing, political imprisonment, and disappearance. For each of these components, the indicator takes a value of 0 (no violations), 1 (between one and 50 violations), or 2 (50 or more

6. For more information on the political terror scale, see Gibney, Cornett, and Wood (2008).

violations). The aggregate physical integrity index equals the sum of the four components and ranges from 0 (full government respect for these rights) to 8 (no government respect for these rights). The index is available for 200 countries from 1981 to 2006.[7] In our analysis, we use the two political terror scale indicators (pters and ptera) and the physical integrity index (physint) as alternative measures of human rights violations.[8]

Both the physical integrity index and the political terror scale indicators capture de facto rights rather than de jure rights.[9] Even though the U.S. State Department and Amnesty International both report on human rights laws and policies, the quantitative human rights measures explicitly exclude such laws and policies. Moreover, while the U.S. State Department and Amnesty International report rights violations by the government as well as by private actors, both the physical integrity index and the political terror scale indicators capture only state violence and exclude all instances of human rights abuse by private actors. It is only with respect to extraterritorial rights violations and the rights of foreign nationals that the physical integrity index and the political terror scale indicators take different approaches. The physical integrity index excludes all rights violations conducted beyond a nation's internationally recognized borders. It moreover codes only rights violations against citizens and excludes all rights violations directed against foreign nationals. By contrast, the political terror scale does not exclusively focus on citizens, and, while it mostly reflects political violence within a country's borders, it also takes into account human rights incidents abroad, such as those at Guantánamo Bay and Abu Ghraib.

Descriptive statistics for our estimation sample indicate substantial variation in the three human rights indicators, both across countries and over time. In 30 percent of all pters observations, a country's score differs from its score in the previous year. This percentage is even higher for

7. For more information on the Cingranelli and Richards (CIRI) data, see David Louis Cingranelli alnd David L. Richards, CIRI Human Rights Data Project (http://ciri.binghamton.edu. All CIRI indicators were rescaled so that higher scores correspond to more rights violations.

8. The pairwise correlations are .79 (pters-ptera), .72 (pters-physint), and .64 (ptera-physint).

9. De jure rights refer to human rights legislation, including constitutional laws. By contrast, de facto rights refer to the actual human rights situation in a country regardless of the law on the books.

ptera (38 percent) and physint (62 percent).[10] Most of these year-to-year changes correspond to one level up or down.[11] A large number of U.S. allies also experienced changes in their human rights scores after 9/11. Examples of countries that went from a pters score of 1 in 2000 to a pters score of 2 after 9/11 include Australia, Austria, Canada, the Czech Republic, France, Germany, Japan, Poland, Portugal, and the Slovak Republic. An example of a country that went from 1 in 2000 to 3 in 2002 is Spain. Countries that went from 2 to 3 are Albania, Armenia, Bulgaria, Korea, Moldova, and Romania. The Appendix zooms in on the events in two of these countries: Australia and the United Kingdom. It summarizes the main counterterrorism measures taken, the human rights violations reported by the U.S. State Department and Amnesty International, and the corresponding pters, ptera, and physint scores. It is these types of events that motivate the analysis in this paper.[12]

### 2.2. Identifying Countries at Risk

After 9/11, the U.S. and many of its allies took counterterrorism measures. This was partly due to a belief that terror threats had gone up in all these countries, consistent with Bin Laden's fatwas in which he threatened not just the United States but also its allies. But counterterrorism measures were also taken as part of a broad international support for the U.S.-led War on Terror. The strength of this support already appeared the day after the 9/11 attacks, when the North Atlantic Treaty Organization (NATO) for the first time invoked Article 5 of its charter, declaring that the atrocities were an attack on all 19 member states. Hence, increased terror threats at home and abroad led U.S. allies to adopt counterterrorism laws. New warnings from al-Qaeda in response to the wars in Afghanistan and Iraq further increased fears, especially in countries that participated in the wars.

We use two sets of U.S. ally groups to evaluate the impact of the post-9/11 terror threat on rights. The first includes the United States and its post-9/11 allies during the wars in Afghanistan and Iraq. Since the

10. Maddala and Wu (1999) panel unit root tests reject a unit root for all three variables. This suggests that the series are $I(0)$, and hence running the specification in equations (1) and (2) in levels is appropriate.

11. In 28, 35, and 40 percent of all pters, ptera, and physint observations, respectively, a country's score differs by one level from its previous score.

12. Australia experienced post-9/11 changes in its pters and ptera scores but not in its physint score. The reason is that most of the rights violations in Australia were directed against asylum seekers and therefore, as explained above, show up in the political terror scale indicators but not in the physical integrity index.

terror threat and thus the need for counterterrorism may depend on a country's level of support, we distinguish between countries that deployed troops in Afghanistan and/or Iraq and countries that provided material assistance (equipment, helicopters, fuel, transport, supplies) or nonmaterial assistance (use of airspace, naval bases, strategic support).

Although military support for the wars in Afghanistan and Iraq probably is a good indicator of countries' exposure to terror threats, it may be endogenous. For example, regimes that give low weight to human rights may also be more likely to participate in the wars. However, our discussion of the post-9/11 terror threat suggests a second set of treatment variables that is based on military alliances prior to 9/11 and hence suffers less from endogeneity. In particular, we use several variables that identify U.S. military allies in 2000. Since closer allies may face more severe threats, we distinguish between different levels of military commitment. The countries with the highest commitment to the United States are the NATO members, who agreed to mutual defense in response to an attack by any external party. The second and third groups of pre-9/11 allies include the "major non-NATO U.S. allies" and the members of the Euro-Atlantic Partnership Council (EAPC).[13] Finally, we consider other U.S. formal military alliances.[14]

Figure 1 shows the average human rights scores for one of the indicators, pters, for the balanced sample of 95 countries for which we have data since 1981. The solid and dashed lines correspond to the 32 U.S. allies and the 63 non-U.S. allies in the balanced sample, respectively. Although these simple averages should not be taken as evidence of causality, the average increase in rights violations in U.S. ally countries after 2000 is clearly consistent with an adverse effect of 9/11 on human rights in these countries.

## 3. ESTIMATING THE EFFECT OF 9/11 ON HUMAN RIGHTS

In Tables 2 and 3, we report the results of estimating the ordered probit model described in equations (1) and (2). Table 2, columns 1–3, shows

13. Major non-NATO allies are countries legally designated by the U.S. government as exceptionally close allies that have strategic working relations with American forces. The Euro-Atlantic Partnership Council was created in 1997 for dialogue and consultation on political and security-related issues.

14. The Correlates of War Formal Interstate Alliance Dataset (Gibler and Sarkees 2004) documents bilateral defense alliances with Australia, Canada, Japan, and the Philippines, a bilateral entente alliance with South Korea, and a multilateral defense alliance with all member states of the Organization of American States (OAS).

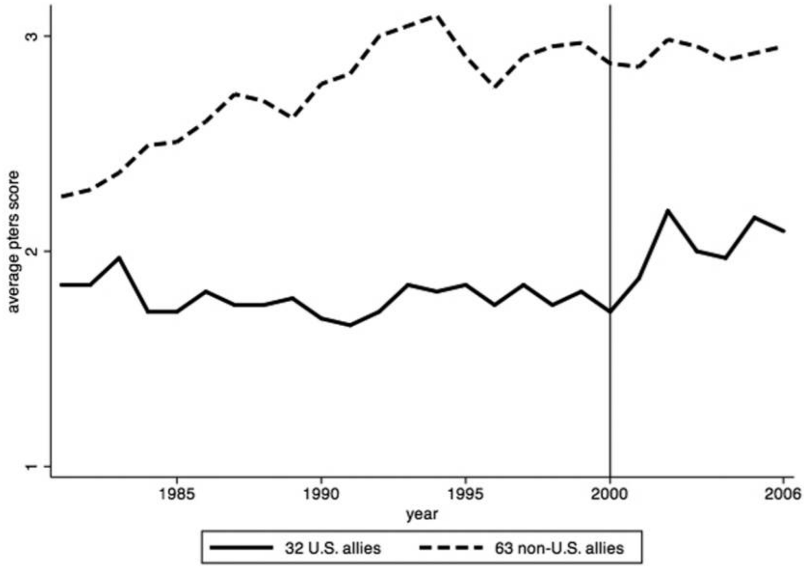


**Figure 1.** Average levels of human rights violations (pters) in balanced panel, 1981–2006

the specifications in which we use the countries that deployed troops in both Afghanistan and Iraq for our three indicators of human rights violations (pters, ptera, and physint). The interaction term of the U.S. Allies indicator and the period-specific effect (2001–2006) enters with a positive sign and is statistically significant at 5 percent in all three specifications. This is consistent with the hypothesis that the post-9/11 terror threat has led to an increase in human rights violations in the countries that supported the United States in the War on Terror. To obtain an estimate of the size of the effect, we also report the change in the probability of each outcome (in percentage points) if the interaction term increases from 0 to 1.[15] Conditional on having been in the

15. Hence we compute the incremental effect $(\Delta \Pr[y_{i,t} = j|x])/[\Delta(t_i \times p_t)]$ for each outcome $j$, which is conditional on the independent variables in the vector $x$. Since our main interest lies in the effect of the post-9/11 terror threat on U.S. ally countries, we set the variables U.S. Allies and (2001–2006) at 1 and the two lagged dependent variables, "Human rights violations$_{t-1}$" and "Human rights violations$_{t-2}$," at the best possible score (1 for pters and ptera and 0 for physint), which for many U.S. allies was the actual score in the year before 9/11. All other regressors were set at their median values for the U.S. ally countries in our sample in 2000. Puhani (2008) has shown that the recommendation of Ai and Norton (2003) to compute the double difference $(\Delta^2 \Pr[y_{i,t} = j|x])/\Delta t_i \Delta p_t$ does not

**Table 2.** The Effect of the Post-9/11 Terror Threat on Human Rights: Post-9/11 U.S. Allies

| | Troops Afghanistan and Iraq | | | Troops Afghanistan or Iraq | | |
|---|---|---|---|---|---|---|
| | pters (1) | ptera (2) | physint (3) | pters (4) | ptera (5) | physint (6) |
| U.S. Allies | −.27* (.12) | −.29* (.12) | −.27* (.11) | −.26** (.08) | −.28** (.08) | −.29** (.08) |
| (2001–2006) | .27** (.05) | .05 (.04) | .08+ (.04) | .24** (.05) | .04 (.05) | .04 (.04) |
| U.S. Allies × (2001–2006) | .27* (.11) | .33* (.15) | .25* (.10) | .21* (.08) | .20* (.10) | .23** (.07) |
| Δ Probability ($y_{i,t}$ = 0) | | | −10.0* | | | −9.0** |
| Δ Probability ($y_{i,t}$ = 1) | −7.9** | −12.1* | 5.1* | −6.6** | −7.4* | 4.4** |
| Δ Probability ($y_{i,t}$ = 2) | 7.7** | 11.3* | 3.6* | 6.4** | 6.9* | 3.3** |
| Δ Probability ($y_{i,t}$ = 3) | .2* | .8* | 1.1* | .2* | .5* | 1.1** |
| Δ Probability ($y_{i,t}$ = 4) | .0 | .0 | .2* | .0 | .0 | .1** |
| Δ Probability ($y_{i,t}$ = 5) | .0 | .0 | .0+ | .0 | .0 | .0* |
| Δ Probability ($y_{i,t}$ = 6) | | | .0 | | | .0+ |
| Δ Probability ($y_{i,t}$ = 7) | | | .0 | | | .0 |
| Δ Probability ($y_{i,t}$ = 8) | | | .0 | | | .0 |
| Human rights violations$_{t-1}$ | 1.02** (.05) | .87** (.05) | .43** (.02) | 1.02** (.05) | .86** (.05) | .43** (.02) |
| Human rights violations$_{t-2}$ | .54** (.04) | .47** (.03) | .22** (.02) | .54** (.04) | .47** (.04) | .21** (.02) |
| GDP per capita (log) | −.11** (.02) | −.06** (.02) | −.12** (.03) | −.10** (.02) | −.05* (.02) | −.10** (.03) |
| GDP per capita growth | −.94+ (.49) | −1.31** (.50) | −.41 (.42) | −.93+ (.49) | −1.28** (.50) | −.40 (.41) |
| Democracy | −.05** (.01) | −.03** (.01) | −.04** (.01) | −.04** (.01) | −.03** (.01) | −.04** (.01) |
| Population (log) | .12** (.02) | .09** (.02) | .13** (.02) | .12** (.02) | .09** (.02) | .13** (.02) |
| Civil war | .63** (.12) | .55** (.10) | .57** (.08) | .66** (.12) | .58** (.10) | .60** (.08) |
| *N* | 3,416 | 2,680 | 2,966 | 3,416 | 2,680 | 2,966 |
| Treatment observations | 150 | 114 | 143 | 306 | 257 | 293 |
| Pseudo-$R^2$ | .48 | .36 | .31 | .48 | .36 | .31 |

**Note.** For each regressor we report the estimated coefficient from the ordered probit model and the robust standard error of the coefficient clustered by country. For the regressor U.S. Allies × (2001–2006), we also report the change in the probability of each outcome (in percentage points) if the regressor increases from zero to one. In columns 1–3, the regressor U.S. Allies refers to the countries that deployed troops in both Afghanistan and Iraq, while in columns 4–6, it refers to the countries that deployed troops in Afghanistan or Iraq or both. GDP = gross domestic product.

+Denotes significance at the 10% level.

*Denotes significance at the 5% level.

**Denotes significance at the 1% level.

**Table 3.** The Effect of the Post-9/11 Terror Threat on Human Rights: Pre-9/11 U.S. Allies

| | NATO | | | NATO Plus | | |
|---|---|---|---|---|---|---|
| | pters (1) | ptera (2) | physint (3) | pters (4) | ptera (5) | physint (6) |
| U.S. Allies | −.50* (.22) | −.47* (.22) | −.49** (.16) | −.29** (.09) | −.23** (.08) | −.25** (.08) |
| (2001–2006) | .28** (.05) | .04 (.04) | .09* (.04) | .26** (.05) | .04 (.05) | .07 (.05) |
| U.S. Allies × (2001–2006) | .27+ (.16) | .53* (.22) | .16+ (.10) | .20* (.08) | .20* (.10) | .15* (.07) |
| Δ Probability ($y_{i,t}$ = 0) | | | −6.4+ | | | −5.8* |
| Δ Probability ($y_{i,t}$ = 1) | −6.8+ | −19.4* | 3.8 | −6.1* | −7.4* | 2.7+ |
| Δ Probability ($y_{i,t}$ = 2) | 6.7+ | 18.1* | 2.0+ | 5.9* | 6.9* | 2.2* |
| Δ Probability ($y_{i,t}$ = 3) | .1 | 1.3* | .5 | .2* | .5* | .7* |
| Δ Probability ($y_{i,t}$ = 4) | .0 | .0 | .1 | .0 | .0 | .1* |
| Δ Probability ($y_{i,t}$ = 5) | .0 | .0 | .0 | .0 | .0 | .0+ |
| Δ Probability ($y_{i,t}$ = 6) | | | .0 | | | .0+ |
| Δ Probability ($y_{i,t}$ = 7) | | | .0 | | | .0 |
| Δ Probability ($y_{i,t}$ = 8) | | | .0 | | | .0 |
| Human rights violations$_{t-1}$ | 1.02** (.05) | .86** (.05) | .43** (.02) | 1.02** (.05) | .87** (.05) | .43** (.02) |
| Human rights violations$_{t-2}$ | .53** (.04) | .47** (.04) | .21** (.02) | .54** (.04) | .47** (.04) | .21** (.02) |
| GDP per capita (log) | −.10** (.02) | −.06* (.02) | −.10** (.03) | −.09** (.02) | −.05* (.02) | −.10** (.02) |
| GDP per capita growth | −1.08* (.49) | −1.36** (.50) | −.46 (.42) | −.99* (.48) | −1.34** (.50) | −.36 (.42) |
| Democracy | −.04** (.01) | −.03** (.01) | −.04** (.01) | −.05** (.01) | −.03** (.01) | −.04** (.01) |
| Population (log) | .13** (.02) | .09** (.02) | .15** (.02) | .13** (.02) | .09** (.02) | .13** (.02) |
| Civil war | .66** (.13) | .57** (.10) | .60** (.08) | .65** (.12) | .56** (.10) | .58** (.08) |
| $N$ | 3,418 | 2,680 | 2,968 | 3,418 | 2,680 | 2,968 |
| Treatment observations | 96 | 68 | 101 | 299 | 256 | 292 |
| Pseudo-$R^2$ | .48 | .36 | .31 | .48 | .36 | .31 |

**Note.** For each regressor we report the estimated coefficient from the ordered probit model and the robust standard error of the coefficient clustered by country. For the regressor U.S. Allies × (2001–2006), we also report the change in the probability of each outcome (in percentage points) if the regressor increases from zero to one. In columns 1–3, the regressor U.S. Allies refers to North Atlantic Treaty Organization (NATO) members in 2000, while in columns 4–6, it refers to either NATO or Euro-Atlantic Partnership Council members, major non-NATO U.S. allies, or bilateral U.S. allies (all in 2000).

+Denotes significance at the 10% level.

*Denotes significance at the 5% level.

**Denotes significance at the 1% level.

regime with the lowest degree of rights violations (1 for pters and ptera and 0 for physint) in the previous year, the probability of a U.S. ally country staying in that regime falls by 7.9 (pters), 12.1 (ptera), or 10.0 (physint) percentage points for each of the post-9/11 years. This changed probability for each year indicates a substantial increase in the number of cases in which rights violations in U.S. ally countries went up during the period 2001–2006. Hence, the effect is not only significant but also sizeable. The lower probability of being in the best rights regime is for the most part offset by a higher probability of being in the next best rights regime, with the next one to three regimes taking up the residual. This indicates that the effect is mostly explained by changes from one regime to the next.

In Table 2, columns 4–6, we reestimate the specifications of columns 1–3 but we now use the countries that deployed troops in Afghanistan or in Iraq or in both. This more than doubles the number of treatment observations [that is, observations for which the variable U.S. Allies × (2001–2006) equals 1]. Again, the interaction term of the U.S. Allies indicator and the period-specific effect (2001–2006) enters positive in all three specifications and is significant at 5 percent for pters and ptera and at 1 percent for physint. The size of the coefficients and the marginal effects are slightly smaller than in columns 1–3. To investigate whether the 9/11 effect depends on a country's level of support for the wars in Afghanistan and Iraq, we reestimated these specifications with separate treatment variables for troop deployment in both Afghanistan and Iraq and troop deployment in either Afghanistan or Iraq. In all three specifications, the difference between the coefficients for both groups was insignificant, suggesting that pooling the two groups is appropriate.[16] In the remainder of this paper we therefore use countries that deployed troops in Afghanistan or in Iraq or in both as the post-9/11 group of U.S. allies.[17]

apply to nonlinear difference-in-differences models in which the interest lies in the estimation of a treatment effect. This is because the treatment effect is not captured by the double difference but instead is represented by the incremental effect $(\Delta \Pr[y_{i,t} = j|x])/[\Delta(t_i \times p_t)]$, which we calculate here. However, we also computed effects according to Ai and Norton (using the Stata command "predictnl" instead of "mfx") and found that they were similar to the effects presented here.

16. Using the number of troops deployed in Afghanistan or Iraq per million of inhabitants (data from the Stockholm International Peace Research Institute's Multilateral Peace Operations Database and the World Bank's World Development Indicators), we also investigated whether the effect of 9/11 is significantly larger for U.S. allies that deployed more troops but again found no evidence that this is the case.

17. We found no effect of 9/11 in countries that did not send troops but provided material or nonmaterial assistance. We therefore excluded these countries from our treatment group.

In Table 3, we use treatment variables that are based on military alliances prior to the 9/11 attacks. Columns 1–3 show the specifications in which we use just the countries that were members of NATO in 2000, while columns 4–6 show the specifications in which we use not only NATO member countries but also countries that were either EAPC members, major non-NATO U.S. allies, or bilateral U.S. allies in 2000.[18] Our finding that the post-9/11 terror threat has led to an increase in human rights violations in U.S. ally countries is robust to using these alternative treatment variables. The interaction term of the U.S. Allies indicator and the period-specific effect (2001–2006) again enters with a positive sign in all six specifications and is statistically significant at 5 percent in four of them and significant at 10 percent in the other two. We reestimated the specifications of Table 3, columns 4–6, with separate treatment variables for each of the groups but did not find any significant difference between the effect for NATO members and the effects for the other three groups. In the remainder of the paper we therefore use their common aggregate as the pre-9/11 group of U.S. allies (NATO Plus).[19]

We now turn to the other variables in Tables 2 and 3. First, the treatment group specific effect, captured by the coefficient of the variable U.S. Allies, is always negative and significant, indicating that U.S. allies on average have lower levels of rights violations than other countries. The period-specific effect, captured by the coefficient of the variable (2001–2006), is always positive, and in half of the specifications it significant, which suggests that rights violations on average have gone up after 2000. The two lags of the dependent variable enter positive and are always statistically significant at 1 percent, consistent with the notion that human rights are relatively persistent over time.[20] The other control variables also enter with the expected signs, while the coefficients are almost always highly significant. In particular, higher levels and growth

18. The pre-9/11 and post-9/11 U.S. ally groups partly overlap. The correlation between NATO membership and troop deployment in Afghanistan and Iraq is .48, while the correlation between the larger group of pre-9/11 U.S. allies and troop deployment in Afghanistan and/or Iraq is .78.

19. We did not find any 9/11 effect in member countries of the only other formal U.S. alliance in 2000, the OAS. This is not surprising as the OAS collective defense treaty was last invoked by Argentina during the 1982 Falklands War. At the time, the United States did not respond and aligned itself with the United Kingdom instead, effectively turning the treaty into a dead letter. We exclude the OAS members from our treatment group.

20. Our lag order selection was based on Akaike's Information Criterion and Schwarz's Bayesian Information Criterion. Yet our results are robust to including only one lag (as is common in the empirical human rights literature) or five lags (the ones that enter statistically significant).

rates of GDP per capita are associated with lower degrees of rights violations. More democratic countries also tend to have more respect for human rights. By contrast, countries with larger populations and countries that experience civil war have significantly worse rights regimes.

A possible concern with these results is that the interaction of the U.S. Allies indicator and the period-specific effect (2001–2006) might be endogenous. More formally, this means that the variable $t_i \times p_t$ in equation (1) might be correlated with the error term $\varepsilon_{i,t}$ (a violation of the parallel-trend assumption) and as a result our estimate of the true effect of 9/11 ($\delta$) might be biased. This problem occurs, for example, if there are omitted factors that affect human rights and are correlated with $t_i \times p_t$. One such factor might be the incidence of domestic terrorist attacks in U.S. ally countries, such as in Madrid in 2004 and in London in 2005 (see Dreher, Gassebner, and Siemers 2010). Using data from the Global Terrorism Database (LaFree and Dugan 2008), we construct dummy variables for more than zero, more than 10, more than 20, and more than 50 terrorism fatalities in a country in a given year and add these variables to the specifications in Tables 2 and 3. While we find evidence of a negative effect of domestic terrorist attacks on rights, the estimated effect of 9/11 on rights is equally strong once we control for domestic terrorism.

Another omitted factor might be the participation of U.S. allies in the wars in Afghanistan and Iraq. Since these wars started after 9/11, it is difficult to separate a possible human rights effect of these wars from an effect of the post-9/11 terror threat. However, if one can identify another time interval during which similar events occurred, it is possible to investigate the likelihood that these events drive the results. In the case of the wars in Afghanistan and Iraq, the First Gulf War in Iraq in 1990 and 1991 represents such a time interval. To investigate whether war participation affects human rights, we reestimated the specifications in Tables 2 and 3, but instead of the post-9/11 variables, we now include a dummy for troop deployment in Iraq in 1990 and/or 1991, a dummy for the period 1990–1991, and an interaction term of both dummies. The coefficient of the interaction term was never statistically significant, indicating that troop deployment in the First Gulf War did not affect human rights. It therefore seems unlikely that the estimated effect of the post-9/11 terror threat is explained by participation in the wars in Afghanistan and Iraq.

To control for time-invariant omitted variables, we also experimented with the fixed-effects ordered logit estimator developed by Ferrer-i-Carbonell and Frijters (2004).[21] Our results in Tables 2 and 3, columns 4–6, are robust to the inclusion of country fixed effects. In particular, the effect of 9/11 on rights violations is always positive and is significant at 1 percent in four specifications and significant at 5 and 10 percent in the other two. While our results are robust to including fixed effects, these fixed-effects estimations are not our preferred specifications. This is due to the inability of the fixed-effects ordered logit estimator to predict marginal effects. In addition, it collapses the ordinal dependent variables into binary ones and thus does not take account of the five-point (pters and ptera) and nine-point (physint) ordinal scales of the human rights indicators.[22]

Finally, we considered the possibility that the human rights indicators are subject to reporting bias. As human rights have received more attention after 9/11, observers may have become more skeptical toward the United States and its allies and may closer scrutinize the rights practices of these countries. Higher post-9/11 scores for the rights indicators could therefore reflect this increased scrutiny rather than an increase in rights violations. However, we believe it is unlikely that all three of our indicators (pters, ptera, and physint) are biased. The indicators draw on two independent sources (the U.S. State Department and Amnesty International) and were constructed as part of two independent data projects (the Political Terror Scale and the CIRI Human Rights Data Project). Increased scrutiny may be reflected in the reports of Amnesty International, an international NGO whose sole objective is to improve human rights protection. But it is unlikely to have affected the reports of the U.S. State Department, as the State Department had little reason to increase its scrutiny of rights practices by its own allies. Given that we find similar results for the indicators that are (partly) based on the Amnesty International reports (ptera and physint) and the indicator that is solely based on the U.S. State Department reports (pters), it seems unlikely that biases in the human rights reports affected our findings.

21. We thank Ada Ferrer-i-Carbonell and Paul Frijters for generously sharing the codes.

22. We also experimented with simpler fixed-effects specifications by replacing the dependent variables in Tables 2 and 3, columns 4–6, with dummies that take a value of one for deteriorations in the corresponding rights indicator and zero otherwise and including country and year fixed effects. Focusing on deteriorations only inevitably means excluding any subsequent years in which the relevant country may maintain its higher level of rights violations. Nevertheless, using both ordinary least squares and probit analysis, we find that the effect of 9/11 on rights violations is always positive and is significant in five out of the six specifications.

### 3.1. Related Questions

Having found that 9/11 led to a systematic and sizable increase in human rights violations in U.S. ally countries, we now turn to some related questions. First, we investigate possible effects in Muslim states, as some of them took counterterrorism measures to prevent terrorists from using their territories as safe havens, and in autocratic states, as it is sometimes argued that their leaders used the War on Terror as a justification for repressive legislation that curtails civil liberties. We do not find any evidence of such effects.[23]

Second, using the four subindices of the physical integrity rights index (torture, extrajudicial killings, political imprisonment, and disappearances), we find that the 9/11 effect is mainly driven by torture and to a lesser extent political imprisonment, while disappearances and extrajudicial killings seem less important. Given these findings, we constructed a new physical integrity rights index that equals the sum of the subindices for torture and political imprisonment only. The index ranges from 0 (full government respect for these two rights) to 4 (no government respect for these rights). In the next section, we use this index (physint_tp) instead of the variable physint as one of our three indicators of human rights violations (the other ones being pters and ptera).

Third, we investigate whether the adverse effect of 9/11 varies across years. The results indicate that rights violations substantially increased during the years 2001–4 but decreased again in 2005 and 2006. A possible explanation for these findings is that the perceived threat of new terrorist attacks went down after reaching its peak in the 4 years following 9/11, thus lowering the need for counterterrorism. But it is also possible that violations in the first 4 years after 9/11 eroded popular support for counterterrorism. In both cases, however, the effect of 9/11 has not necessarily died out. Since many of the counterterrorism laws are still in place, new systematic rights violations may occur in the future, especially in the unfortunate event of new terrorist attacks.

Fourth, we investigate whether the 9/11 effect is larger in relatively less developed U.S. allies, as it is sometimes argued that the United States and Western Europe export their human rights "dirty work" to these

23. We found no effect of 9/11 in countries where at least 25, 50, or 75 percent of the population is Muslim (data from Barro and McCleary 2005), and if anything, rights improved rather than deteriorated in autocratic states (states with a minimum score of either 6, 7, 8, or 9 on the Polity IV autocracy scale).

countries. However, using three alternative measures of economic development,[24] we find no evidence that this is the case.

Finally, using data from the Global Terrorism Database (LaFree and Dugan 2008), we investigate whether the 9/11 effect is larger for U.S. allies that experienced one or more major domestic terrorist attacks (50 or more fatalities) between 2001 and 2006 (the United States, South Korea, Phillipines, Russia, and Spain). With the exception of the United States, we find no evidence that this is the case. Clearly, as 9/11 occurred on U.S. territory, its effect on rights was larger in the United States than elsewhere. Since this difference is unlikely to be explained by checks and balances, we exclude the United States from our estimation samples in the next section.[25]

## 4. DO CHECKS MATTER?

Having established that the post-9/11 terror threat led to a significant increase in human rights violations, we now investigate whether this increase was smaller in countries with stronger checks and balances.

### 4.1. Countermajoritarian Checks

To test the effect of countermajoritarian checks, we first divide the countries in our sample according to whether or not they have independent judicial review, that is, a constitutional or supreme court that is politically independent and has the power to invalidate laws that violate the constitution. As an indicator of independent judicial (or constitutional) review, we construct a dummy variable based on indicators of judicial review and judicial independence. The indicator of judicial review is based on Maddex (2007) and is an update of the indicator used by La Porta et al. (2004). The indicator identifies three categories: full judicial review, limited judicial review, and no judicial review.[26] The indicator

24. A dummy for Organisation for Economic Co-Operation and Development membership during the period 2001–6; a dummy for countries with a 2000 level of real per capita gross domestic product (adjusted for purchasing power parity) above the median level for U.S. allies (data from Penn World Tables); and a dummy for countries that in 2000 were classified as "high income" by the World Bank.

25. Pters already excludes the United States as its State Department does not report on its own country.

26. Maddex (2007) identifies two additional categories. The first, de facto review, includes Angola and Bhutan, which are not in our sample owing to missing data. The second, "technically no review," includes Israel, New Zealand, and the United Kingdom. As the constitution in these countries is hardly more than an ordinary law that can be changed by parliament, we classify these countries as having no judicial review. However, results are robust when reclassifying them as having (full or limited) judicial review.

of judicial independence was taken from La Porta et al. (2004) and was computed as the normalized sum of three variables. The first two variables capture the tenures of supreme court judges and administrative court judges. These variables take a value of zero if tenure is less than 6 years, one if tenure is more than 6 years but not lifelong, and two if tenure is lifelong. The third variable captures case law and is a dummy that takes a value of one if judicial decisions are a source of law and zero otherwise. La Porta et al. (2004) argue that a lifelong tenure makes judges both less susceptible to political pressure and less likely to have been selected by the government currently in office. In addition, case law also increases judicial independence, as the binding power of prior judicial decisions limits judicial discretion and thus the ability of governments to influence judges. Using the indicators of judicial review and judicial independence, we construct a dummy variable for independent judicial review. This dummy takes a value of one if a country has full judicial independence (that is, lifelong tenure for supreme court and administrative court judges, as well as case law) and limited or full judicial review and zero otherwise.[27] For the 191 treatment observations in the common sample of Tables 2 and 3 for which we have data on judicial review and independence, the dummy is one in 78 cases and zero in 113 cases. Figure 2 shows the average human rights scores for one of the indicators, pters, for the balanced sample of 27 U.S. allies for which we have data on independent judicial review and pters scores since 1981. The solid and dashed lines correspond to the 11 U.S. allies with and the 16 U.S. allies without independent judicial review, respectively. Both groups of U.S. allies experienced an average increase in rights violations after 2000, but the increase seems to have been somewhat larger for allies without independent judicial review. However, these are simple averages only, and we next turn to regression analysis to more systematically estimate the impact of independent judicial review on the 9/11 effect we identified in the previous section.

Table 4, column 1, augments the specification of Table 2, column 4, with the independent judicial review dummy (ijr) by itself and interacted with each of the variables U.S. Allies, (2001–2006), and U.S. Allies × (2001–2006). The coefficient of the variable U.S. Allies × (2001–2006),

27. We include countries with both full and limited judicial review, as limited powers of judicial review are not necessarily an obstacle to an independent court wishing to exercise review. Most famously, the U.S. Supreme Court decided in *Marbury v. Madison* (5 U.S. 137 [1803]) that it could exercise review, while the Constitution was arguably silent on this power.


**Figure 2.** Average levels of human rights violations (pters) for U.S. allies with and without independent judicial review (ijr) in balanced panel, 1981–2006.

which now captures the effect of 9/11 in countries with no independent judicial review, is positive and statistically significant at 1 percent. The size of the coefficient is considerably larger than before, which suggests that countries with no independent judicial review have seen a more severe increase in rights violations than other countries. The coefficient of the interaction term between U.S. Allies × (2001–2006) and independent judicial review corresponds to the difference between the 9/11 effect in countries without independent judicial review and the 9/11 effect in countries with independent judicial review. The coefficient is negative and statistically significant at 1 percent, which indicates that the effect of 9/11 on human rights is significantly smaller in countries with independent judicial review. The linear combination of the coefficients of U.S. Allies × (2001–2006) and its interaction with independent judicial review points to a statistically insignificant net effect of −.17 in countries with independent judicial review. This suggests that independent judicial review fully mitigated the adverse effect of 9/11 on human rights. Table 4, columns 2 and 3, reports the results for subsamples without and with independent judicial review, respectively. The results

**Table 4.** The Effect of the Post-9/11 Terror Threat Conditional on Countermajoritarian Checks

| | Troops Afghanistan or Iraq | | | NATO Plus | | |
|---|---|---|---|---|---|---|
| | pters (1) | pters (2) | pters (3) | pters (4) | pters (5) | pters (6) |
| U.S. Allies | −.51** (.15) | −.48** (.14) | −.05 (.18) | −.52** (.17) | −.52** (.19) | −.09 (.16) |
| (2001–2006) | .14 (.11) | .16 (.12) | .45** (.11) | .20+ (.12) | .22+ (.13) | .41** (.11) |
| U.S. Allies × (2001–2006) | .50** (.18) | .46* (.19) | −.16 (.16) | .40* (.19) | .34+ (.20) | −.05 (.16) |
| U.S. Allies × (2001–2006) × ijr | −.67** (.22) | | | −.46+ (.24) | | |
| ijr | −.24* (.10) | | | −.16+ (.09) | | |
| U.S. Allies × ijr | .52* (.22) | | | .41+ (.23) | | |
| (2001–2006) × ijr | .29* (.15) | | | .19 (.16) | | |
| Human rights violations$_{t-1}$ | 1.18** (.07) | 1.14** (.09) | 1.20** (.11) | 1.18** (.07) | 1.14** (.09) | 1.20** (.11) |
| Human rights violations$_{t-2}$ | .63** (.06) | .62** (.09) | .63** (.09) | .62** (.07) | .60** (.09) | .63** (.09) |
| GDP per capita (log) | −.12** (.04) | −.11* (.05) | −.18** (.06) | −.09* (.03) | −.06 (.05) | −.18** (.05) |
| GDP per capita growth | −2.27** (.88) | −3.86** (1.48) | −1.08 (.97) | −2.28** (.88) | −3.77** (1.42) | −1.13 (.96) |
| Democracy | −.03** (.01) | −.03* (.01) | −.02 (.02) | −.03** (.01) | −.04* (.02) | −.01 (.02) |
| Population (log) | .11** (.04) | .17** (.04) | .03 (.07) | .12** (.04) | .19** (.05) | .03 (.07) |
| Civil war | .52** (.16) | .66** (.19) | .55* (.23) | .50** (.16) | .62** (.19) | .55* (.22) |
| ijr | 0 or 1 | 0 | 1 | 0 or 1 | 0 | 1 |
| $N$ | 1,686 | 934 | 752 | 1,688 | 936 | 752 |
| Treatment observations with ijr = 1 | 66 | ... | 66 | 66 | ... | 66 |
| Treatment observations with ijr = 0 | 102 | 102 | ... | 95 | 95 | ... |
| Pseudo-$R^2$ | .56 | .57 | .54 | .56 | .57 | .54 |

**Note.** For each regressor we report the estimated coefficient from the ordered probit model and the robust standard error of the coefficient clustered by country. In columns 1–3, the regressor U.S. Allies refers to North Atlantic Treaty Organization (NATO) members in 2000, while in columns 4–6, it refers to either NATO or Euro-Atlantic Partnership Council members, major non-NATO U.S. allies, or bilateral U.S. allies (all in 2000). ijr = independent judicial review.

+Denotes significance at the 10% level.

*Denotes significance at the 5% level.

**Denotes significance at the 1% level.

again imply that the increase in rights violations occurred only in countries without independent judicial review. We next repeat the specifications of Table 4, columns 1–3, but using the pre-9/11 instead of the post-9/11 allies. The results, reported in Table 4, columns 4–6, are very similar, and the coefficients are again significant, although at lower levels than before.

Table 5 shows the estimation results for all three dependent variables (pters, ptera, and physint_tp). To save space, we show only the coefficients of the variables of interest and their levels of significance. For comparison, the first three columns in the top of Table 5 repeat the results of Table 4, columns 1–3, while the first three columns in the bottom of Table 5 repeat the results of Table 4, columns 4–6. The next three columns then report the results when using ptera instead of pters as the dependent variable, while the final three columns in each panel report the results when using physint_tp instead. As can be seen, the pters results from Table 4 are robust to using ptera or physint_tp as the dependent variable, both for the full-sample specifications (ijr = 0|1) with the triple interaction term U.S. Allies × (2001–2006) × independent judicial review (ijr) and for the subsample specifications (ijr = 0 and ijr = 1). In all six full-sample specifications (ijr = 0|1) of Table 5, the effect of 9/11 in countries with no independent judicial review [captured by the coefficient of the variable U.S. Allies × (2001–2006)] is positive and significant. The size of the coefficients is always considerably larger than before. Depending on the specification, the probability of a U.S. ally country with no independent judicial review staying in the best human rights regime falls by 10.9, 12.2, 8.4, 8.8, 11.5, or 11.2 percentage points for each of the post-9/11 years. The difference between the 9/11 effects in countries without independent judicial review and countries with independent judicial review (captured by the coefficient of the triple interaction term) is always negative and is significant in five out of six specifications. Hence, the effect of 9/11 is significantly smaller in countries with independent judicial review. In fact, the net effect of 9/11 in these countries (captured by the linear combination of the coefficients of U.S. Allies × (2001–2006) and its interaction with independent judicial review) is never significant. This again suggests that independent judicial review fully mitigated the 9/11 effect on human rights. The results for the subsample specifications (ijr = 0 and ijr = 1) of Table 5 are consistent with these findings. While the 9/11 effect [captured by the coefficient of the variable U.S. Allies × (2001–2006)] is always positive and is significant in five out of six specifications for

**Table 5.** The Effect of the Post-9/11 Terror Threat Conditional on Countermajoritarian Checks: Independent Judicial Review

| | pters | | | ptera | | | physint_tp | | |
|---|---|---|---|---|---|---|---|---|---|
| | ijr = 0\|1 | ijr = 0 | ijr = 1 | ijr = 0\|1 | ijr = 0 | ijr = 1 | ijr = 0\|1 | ijr = 0 | ijr = 1 |
| Troops Afghanistan or Iraq: | | | | | | | | | |
| U.S. Allies × (2001–2006) | .50** | .46* | −.16 | .37* | .41** | −.16 | .25⁺ | .23 | .14 |
| U.S. Allies × (2001–2006) × ijr | −.67** | | | −.55** | | | −.18 | | |
| NATO Plus: | | | | | | | | | |
| U.S. Allies × (2001–2006) | .40* | .34⁺ | −.05 | .34* | .37* | −.23 | .32* | .30* | .03 |
| U.S. Allies × (2001–2006) × ijr | −.46⁺ | | | −.59** | | | −.39⁺ | | |

**Note.** Results are from estimating three variants of the specifications of Tables 2 and 3, columns 4–6.

⁺Denotes significance at the 10% level.

*Denotes significance at the 5% level.

**Denotes significance at the 1% level.

countries without independent judicial review (ijr = 0), it is always considerably smaller and never significant for countries with independent judicial review (ijr = 1).[28]

To investigate whether these results are driven by either judicial independence or judicial review, rather than the combination of both, we reestimated the full-sample specifications in Table 5 (ijr = 0 | 1), but we added a dummy for countries with judicial independence and without judicial review and interactions of this dummy with each of the variables U.S. Allies, (2001–2006), and U.S. Allies × (2001–2006). The results indicated that judicial independence mitigates the adverse effect of 9/11 only if it is combined with judicial review. We repeated this exercise with a dummy for countries with review but without independence and found that judicial review mitigates the 9/11 effect only if it is combined with judicial independence. Hence both judicial review and judicial independence are necessary and our results are driven by the combination of both.[29]

A possible concern with these results is that independent judicial review might be endogenous. It could, for example, be the case that, after 9/11, some countries amended their constitutions to reduce the strength of independent judicial review, thereby enabling the government to adopt farther-reaching counterterrorism measures and opening the way to more severe rights violations. However, such constitutional amendments to independent judicial review seldom occur in practice (La Porta et al. 2004). In fact, an analysis of the post-2000 constitutions of all our sample countries revealed that only three countries implemented judicial reforms after 2000 (the non-U.S. allies Chile, Indonesia, and Iraq).[30] Dropping these three countries from the samples in Table 5 did

28. We reran the specifications in Table 5 measuring judicial independence in terms of life-long tenure for supreme court and administrative court judges only (so no longer including case law). While the coefficients generally have the same sign, they are no longer significant in the ptera and physint_tp specifications and in some cases are also less significant in the pters specifications. This may suggest that case law, in addition to life-long tenure, increases judicial independence and helped constitutional or supreme courts in safeguarding human rights after 9/11.

29. We also reran the specifications with separate dummies for countries with independence and full review and countries with independence and limited review. In most specifications, Wald tests on the coefficients of the interactions of these dummies with the variable U.S. Allies × (2001–2006) did not reject the null of equal coefficients. This suggests that we can analyze the two groups as a common aggregate.

30. Judicial reform was defined as a change in at least one of the 18 provisions related to judicial review that we identified in the constitutional texts of countries (Blaustein and Flanz 1973–2006).

not change the results, which suggests that our findings are not explained by post-2000 judicial reforms.

Independent judicial review could also be correlated with omitted institutional characteristics that affect human rights. To address this concern, we collected 13 institutional indicators, capturing general institutional quality, democracy and autocracy, law and order, checks and balances, voice and accountability, political stability, government effectiveness, regulatory quality, rule of law, and control of corruption.[31] Since many of these indicators have ordinal scales, we constructed 13 dummy variables that take a value of one for high institutional quality and zero for low institutional quality. As thresholds, we use the medians for the common sample of treatment observations in Tables 2 and 3. Independent judicial review is not strongly correlated with other institutional characteristics. The correlations range from −.09 for law and order to .13 for checks and balances, while for most of the dummies the correlation is close to zero. To investigate whether our results are explained by institutions other than independent judicial review, we first replaced independent judicial review in Table 5 by the institutional dummy variables and reestimated the specifications for each of the 13 dummies separately. We do not find any evidence that institutions (other than independent judicial review) mitigate the adverse effect of 9/11 on human rights. We then repeated this exercise but now including independent judicial review alongside each of the institutional dummy variables. Controlling for other institutional characteristics does not substantially change our results for independent judicial review, which further indicates that it is independent judicial review that is important and not any other institutional characteristic.

### 4.2. Majoritarian Checks

We now turn to majoritarian checks. As an indicator, we use the political constraints indicator polconiii from Henisz (2002), as it corresponds closely to our notion of majoritarian checks. In particular, this indicator captures whether a country has constitutionally effective upper and lower houses of parliament that are controlled by a party different from

31. We use the International Country Risk Guide "composite risk" and "law and order" ratings from the PRS Group, executive constraints (exconst), democracy (democ), autocracy (autoc), and a combined measure of democracy and autocracy (polity2) from PolityIV, checks and balances (checks) from the Database of Political Institutions (Beck et al. 2001), and the six World Bank governance indicators from Kaufmann, Kraay, and Mastruzzi (2008).

the other branches of government. It also takes into account whether the preferences within an opposition branch are homogeneous, as a more homogeneous branch constitutes a stronger veto power. Just as in the case of countermajoritarian checks, we use a dummy variable, which takes a value of one for high levels of political constraints and zero for low levels. As a threshold, we use the median of the variable polconiii for the common sample of treatment observations in Tables 2 and 3. For the 353 treatment observations for which we have data on political constraints, the political constraints dummy is one in 176 cases and zero in 177 cases. Using this dummy variable, we estimate the same specifications as for countermajoritarian checks in Table 5. The results are reported in Table 6. As can be seen, we do not find any systematic difference between the human rights effects in countries with low levels of political constraints and the human rights effects in countries with high levels of political constraints. Hence, after 9/11, majoritarian checks did not constrain governments in violating rights. This suggests that a majority of citizens were willing to trade liberty for more (perceived) security.

## 5. CONCLUSIONS

This paper is the first to provide cross-country evidence of the effects of 9/11 on human rights as well as how this plays out under different institutional conditions. As one may have suspected from recent newspaper headlines, human rights have significantly deteriorated in the West after 9/11. These violations concerned physical integrity rights and in particular torture and political imprisonment. Moreover, we find that this 9/11 effect is not confined to human rights incidents abroad, such as Guantánamo Bay and Abu Ghraib, or to violations against foreign nationals. It also pertains to violations on a country's home territory and against its own citizens, suggesting that the estimated 9/11 effect at least partly works through domestic counterterrorism measures. But we also find that in countries with independent judicial review, courts prevented such rights violations. By contrast, veto players in the legislative branch were unimportant.

**Table 6.** The Effect of the Post-9/11 Terror Threat Conditional on Majoritarian Checks: Political Constraints

| | pters | | | ptera | | | physint_tp | | |
|---|---|---|---|---|---|---|---|---|---|
| | pc = 0\|1 | pc = 0 | pc = 1 | pc = 0\|1 | pc = 0 | pc = 1 | pc = 0\|1 | pc = 0 | pc = 1 |
| Troops Afghanistan or Iraq: | | | | | | | | | |
| U.S. Allies × (2001–2006) | .21+ | .20+ | .28+ | .20 | .19 | .24 | .22+ | .23+ | .36* |
| U.S. Allies × (2001–2006) × pc | .01 | | | −.01 | | | .14 | | |
| NATO Plus: | | | | | | | | | |
| U.S. Allies × (2001–2006) | .22+ | .21+ | .16 | .35** | .34** | .00 | .19 | .21+ | .26* |
| U.S. Allies × (2001–2006) × pc | −.06 | | | −.33 | | | .08 | | |

**Note.** Results are from estimating three variants of the specifications of Tables 2 and 3, columns 4–6.

+Denotes significance at the 10% level.

*Denotes significance at the 5% level.

**Denotes significance at the 1% level.

## APPENDIX: COUNTERTERRORISM AND HUMAN RIGHTS IN AUSTRALIA AND THE UNITED KINGDOM AFTER 9/11

**Table A1.** Australia

| Year | pters | ptera | physint |
|---|---|---|---|
| 1997 | 1 | 1 | 1 |
| 1998 | 1 | 1 | 1 |
| 1999 | 1 | 1 | 1 |
| 2000 | 1 | 1 | 1 |
| 2001 | 1 | 1 | 1 |
| 2002 | 2 | 1 | 1 |
| 2003 | 2 | 2 | 1 |
| 2004 | 1 | 2 | 1 |
| 2005 | 2 | 1 | 1 |
| 2006 | 1 | 1 | 1 |

*Pre-9/11:* No explicit counterterrorism legislation.

*Response to 9/11:* Forty pieces of counterterrorism legislation (by 2008), expanding executive power, allowing questioning and detention of nonsuspects for 7 days, and introducing control orders. U.S. State Department (2002) states that "there were occasional reports that police beat or otherwise abused persons. Several inquiries during the year, including one prepared by the U.N. Human Rights Commission, expressed concern over the impact of prolonged mandatory detention on the health and psychological wellbeing of asylum seekers." U.S. State Department (2003, 2005) documents similar concerns. Amnesty International (2003) states that "national security was invoked to justify the erosion of human rights safeguards in draft laws on 'antiterrorism' measures and refugee rights." And "the U.N. Human Rights Committee urged the release from immigration detention of Roqia Bakhtiyari and found that she and her children . . . had been arbitrarily detained." Amnesty International (2004) repeats concerns over counterterrorism legislation. It adds that "the government dismissed allegations of torture and ill-treatment of Mamdouh Habib, detained without charge." Also "Indian national Peter Qasim . . . entered his seventh year in indefinite detention."

**Table A2.** United Kingdom

| Year | pters | ptera | physint |
|---|---|---|---|
| 1997 | 1 | 1 | 2 |
| 1998 | 2 | 2 | 1 |
| 1999 | 1 | 1 | 1 |
| 2000 | 1 | 2 | 2 |
| 2001 | 2 | 2 | 1 |
| 2002 | 2 | 2 | 1 |
| 2003 | 2 | 2 | 1 |
| 2004 | 2 | 2 | 1 |
| 2005 | 3 | 2 | 3 |
| 2006 | 2 | 2 | 1 |

*Pre-9/11:* Long-standing experience with counterterrorism. But the Terrorism Act 2000 had in fact removed the strongest displays of executive powers and enhanced judicial oversight.

*Repsonse to 9/11:* Declaration of state of emergency and derogation from European Convention on Human Rights. New counterterrorism legislation (2001, 2005, and 2006), allowing for infinite detention of foreign citizens (2001) and control orders (2005). U.S. State Department (2001) states that "members of the police and military occasionally abused detainees and some other persons," which is "a matter of serious concern." It adds, "The law gives administrative detention power to immigration officers. There is no time limit to such detention, but detainees have the right to request a judicial review . . . . As of September 30, approximately 1,330 asylum seekers were in detention, either in immigration detention centers or in regular prisons." Subsequent reports document similar incidents. U.S. State Department (2005) report adds, "[M]embers of the Metropolitan Police Service killed Jean Charles de Menezes on July 22, the day after failed bombing attempts in London . . . ," although he "was not a suspect in the terrorist attacks." Amnesty International (2001) states that "new security legislation, in the wake of the 11 September attacks in the USA, opened the door to human rights violations." It reports that "AI had documented cases of discriminatory practices in relation to deaths in police custody, detention, ill-treatment, investigations into racist killings and attacks, and other aspects of the criminal justice system." Subsequent reports add further concerns over counterterrorism measures. Amnesty International (2002) notes that "by the end of the year, 11 foreign nationals were interned under the Anti-terrorism, Crime and Security Act 2001 (ATCSA) which allows for indefinite detention without charge or trial on the basis of secret evidence of foreign nationals who cannot be deported."

In addition, "David Shayler, a former intelligence agent who had alleged that the security and intelligence agencies were guilty of misconduct, was imprisoned for breaching the Official Secrets Act. The Act does not afford a public interest defense" (Amnesty International 2003). Amnesty International (2004–7) mentions Iraq: "There were allegations of unlawful killings, torture, ill-treatment and other violations of international human rights and humanitarian law by U.K. forces at the time when the U.K. was recognized as an occupying power in Iraq."

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