Rally 'round which flag?
Terrorism's effect on (intra)national identity

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Abstract

Exploiting a unique panel of student respondents surveyed both shortly before and after the March 2016 bombings in Brussels, this paper analyzes the effects of terrorism on social identities and preferences over security policy. Social identification – including individuals' feelings of connection to social groups such as countries or regions – is often considered a possible determinant of political and economic preferences. In turn, it is widely supposed that social identities may be subject to influence by acts of terror; indeed, our respondents report stronger connections to Belgium after the attack than before. Problems with endogeneity and causality can confound analyses of those relations. Our novel pre- and post-attack responses help disentangle causality between terror and country connection: respondents feeling affected severely by the attacks show stronger connections ex-post at the country level, but close ex-ante connections do not predict stronger feelings of affectedness. Feelings of terrorism's influences also correlate significantly with preferences over security policy and police resourcing, unmediated by jurisdictional connections.

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JEL: D71; H77; Z13

1. Introduction

Many academic disciplines address terrorism and other political violence. Public choice is no exception, with a literature expanding rapidly in recent decades. That literature has included at least two broad strands. One of them addresses the possible determinants and origins of terrorism, including rent seeking (e.g. Kirk, 1983), economic and political freedoms and the nature of government (e.g. Kurrild-Klitgaard et al., 2006), ideology or religion (e.g. Bernholz, 2006), and background economic, social, and political circumstances (for a review see Krieger and Meierrieks, 2011). A second strand examines the consequences of terrorism relevant to public choice, such as electoral outcomes (Berrebi and Klor, 2008), trust and social capital (Geys and Qari, 2016; Arvanitidis et al., 2016), and commitment to civil liberties (Bozzoli and Müller, 2011; Mondak and Hurwitz, 2012).

Examining self-reported sentiments and policy preferences of members of a society targeted by terror, this paper contributes to the second strand. On 22 March 2016, Brussels experienced two bombings by attackers reportedly linked to the Islamic State, killing 32 people and injuring more than 300. The event delivered an exogenous shock to (among other things) social identity and jurisdictional connections. We exploit the timing of the attacks by conducting a follow-up survey of student subjects who had very recently been surveyed about their feelings of connection to five different jurisdictional tiers. This allows novel within-subject measures of these connections both before and after the attack. We follow a growing literature in suggesting that social identities – including feelings of connection to certain groups, e.g. to one's country

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– may influence preferences over public policy. As such, jurisdictional connections merit inclusion on the list of consequences of terror relevant to public choice.[1] At the same time, little doubt exists that terrorism influences targeted societies, with the effects being strongest shortly after the incident (e.g. Arvanitidis et al., 2016; Bozzoli and Müller, 2011; Geys and Qari, 2016). Within-subject measures of changes in identity, such as ours, are rare, as are assessments of connections to nested (e.g., federal) jurisdictions when confronted with terrorism.

Public choice also has brought a particular focus to the question of (potential) terrorists' rationality, in an economic sense. Caplan (2006), for instance, emphasizes as criteria for rationality rational expectations, responsiveness to incentives, and narrow selfishness.[2] Parallel questions pertain to the degree of rationality of citizens affected by terrorism: to what extent can their responses be captured by traditional conceptions of rationality? Behavioral public choice, in particular, addresses the bounds of citizens' and voters' rationality (for a survey, see Schnellenbach and Schubert, 2015).

One manner in which people may depart from traditional definitions of rationality is by adopting other-regarding preferences, implied by (among other theoretical frameworks) some operationalizations of social identity. Social identity theory posits individuals' marked tendencies to feel attachment to social groups, including jurisdictional groups such as their country, region, or community (e.g. Tajfel and Turner, 1986; Akerlof and Kranton, 2000). Individuals may demonstrate a willingness to incur some sacrifice in order to benefit (certain) others, in contrast to the Caplan (2006) rationality criterion of narrow selfishness.

Implications for (preferences over) public policy can be stark, with redistribution receiving particular attention in the literature (e.g. Shayo, 2009; Holm and Geys, 2018). Social identities may also influence provision of public goods, and security policy in particular. A purely rational citizen should weigh expected costs and benefits of prospective anti-terror policies only as they pertain to herself. Imperfectly rational individuals may take a wider view. Other-regarding individuals – including those who feel connected to jurisdictions or their fellow citizens – should weigh the costs and benefits of security, as well. Yet they may also account for costs and benefits accruing to others, and perhaps to communities as distinct from their individual constituents.

We investigate below whether (subjective) perceptions of a terror attack correlate with reported preferences over security policy, and whether jurisdictional connections mediate the relation – whether, that is, the possibility that social identities incorporate other-regarding preferences add explanatory power to a narrowly self-regarding account of security policy preferences more consistent with traditional rationality.

2. Literature and motivation

The literature on the impact of terrorism on society and politics has grown exponentially since 9/11 and other recent transnational terrorist attacks (Sandler, 2014, 2016; Silke, 2008). Attention has focused on both terrorist activities' determinants (e.g. Kirk, 1983; Kurrild-Klitgaard et al., 2006; Gaibulloev et al., 2017; Brockhoff et al., 2015; Gleditsch and Polo, 2016; Filote et al., 2016; Krieger and Meierrieks, 2011, for a review) and their consequences (e.g. Arvanitidis et al., 2016; Berrebi and Klor, 2008; Geys and Qari, 2016; Mondak and Hurwitz, 2012; Hirsch-Hoefler et al., 2016). Attacks may influence the ideologies and attitudes of targeted individuals and societies.[3] For instance, Echebarria-Echabe and Fernández-Guede (2006) find increasing prevalences of strong prejudices, authoritarianism, traditional conservative values, and a weakening of liberal values after the 2004 attack in Madrid. Concerning the influence of attacks on electoral and policy outcomes, Bali (2007) and Montalvo (2011), for instance, argue that the impact of

[1] Note that in their survey Krieger and Meierrieks (2011) consider (mixed) evidence regarding identities – particularly religion, ethnicity, and language – as potential *determinants* of terrorism. Kurrild-Klitgaard et al. (2006), for instance, finds positive associations between countries' probabilities of experiencing terrorism and their linguistic (but not ethnic) fractionalization.

[2] Suicide attacks may seem especially difficult to reconcile with selfishness, though Kirk (1983) and Caplan (2006) conclude that perpetrators of terror largely can be thought of as (at least partly) rational, and suggest that, as such, they can be countered or deterred.

[3] See Khan et al. (2017) for a categorization of theories regarding individual responses to terrorism, including tendencies to withdraw or turn inward in order to 'manage' the threat (e.g. Solomon et al., 1991), or to adopt more conservative or traditionalist attitudes (e.g. Jost et al., 2003). See also Moskalenko et al. (2006) and related discussion below.

the Madrid attack, just three days before national elections for the Spanish parliament, not only affected attitudes, but also voter turnout and voter behavior, most probably swinging the outcome to a victory for the opposition party.[4] Similar (short-term) effects on the importance of security-relevant policy and willingness to sacrifice civil liberties after terrorist events have been reported for the July 2005 attacks in London and December 2010 in Stockholm (Bozzoli and Müller, 2011; Geys and Qari, 2016).

Precisely what drives such attitudinal and behavioral changes is less clear, and shifts in identity may provide a potential mechanism. People have many potential avenues for social identification, including their homes, families, employers, sports associations and political parties (see e.g. Ashforth and Mael, 1989; Brewer and Caporael, 2006; Brown, 2000, 2001; Dutton et al., 1994; Hall et al., 1970; Kuehnhanss et al., 2017; Sen, 2006, for related research and discussion). One common association is *place*, which can be seen as any spatial setting, including the concentric levels of home, neighborhood, village or city, region, nation, country, or certain supranational constructs (Lewicka, 2011). Jurisdictional identification thereby may relate to varying intensities of affective or cognitive bonding (see Rollero and De Piccoli, 2010; Scannell and Gifford, 2010) with the physical location, its occupants, and existing social networks (Twigger-Ross, 1996).

Moskalenko et al. (2006) report students' group identifications with their country, families, ethnicities, religions, and university using 9/11 as a natural experiment. They observe stronger identification with the country and university shortly after the attacks, compared to both before and 18 months afterwards. Comparing the predictive accuracies of different identification theories, they conclude that group dynamic theory as originally developed by Festinger (1950, 1954) provides the most reliable predictions. Group dynamic theory sees group cohesion as driven by consensus-seeking in value-related questions, and stipulates that intergroup competition and external threats to the in-group strengthen cohesion (LeVine and Campbell, 1972; Stein, 1976). Social identity theory contains similar dynamics of greater cohesion in the face of threats (see Huddy, 2015), while national identity specifically has been identified as 'boost[ing] support for civic norms, driv[ing] democratic engagement, and increas[ing] support for a muscular response to national threat' (Huddy, 2013, p.1). While Moskalenko et al. (2006) compare the effect of 9/11 on different identities, these describe parallel aspects of social identity and are not necessarily nested.[5] Indeed, they find all five identities to be positively intercorrelated and suggest that one possible "implication would be that some individuals identify more with every group they participate in" (p.94). By focusing on the evidently more hierarchical subdivision of jurisdictional identities, we test that notion against a possible expectation of identity conflict and competing identities (see Hooghe and Marks, 2005), particularly when an exogenous shock is seen to target a specific nested identity (in our case the country). By shifting the balance of identification in favor of one identity over others, its associated norms and values may become more influential. Alternatively, reactions likewise may be driven by personal feelings of loss of control or increased salience of one's own mortality, as also suggested by terror management theory (Greenberg et al., 1986, 1990, 1994). Landau et al. (2004), for instance, find that mortality salience and reminders of 9/11 strengthened support for President George W. Bush and his counter-terrorism policies.

In examining security preferences after the Brussels attack, we propose that jurisdictional identities may help mediate a link between terrorist events and such outcomes as ideology, policy preferences, or behaviors. Verifying that link requires studying each of three relations among terror, identity and outcomes. A considerable literature examines patriotism as a response to external threats, (e.g Li and Brewer, 2004), as a determinant of outcomes (e.g Qari et al., 2012), or as a mediator between the two (e.g Skitka, 2005). Operationalizations of patriotism typically are situated explicitly or implicitly at the country level, and generally are distinct from concepts of jurisdictional identity (Schatz et al., 1999). Jurisdictional identities, particularly at levels other than the country, have received less attention as mediators or determinants of security-related policy preferences. Holm (2016) examines roles for national and regional identities in influencing (preferences for) redistributive policies when costs and benefits fall unevenly across heterogeneous

[4]Note, however, that in particular the handling of the aftermath of the attacks by the then-governing People's Party and a premature condemnation of ETA led to accusations of willful suppression of relevant information and large-scale protests.

[5]Moskalenko et al. (2006) base their results on three survey rounds with different student samples, which raises issues of selection and cohort effects. While we expect terrorism to increase identification, observations of the same individuals from before and after the Brussels attacks allow us a more robust within-subject test.

regions. When group identification entails selective altruism directed toward fellow group members, national (rather than regional) identification may be associated with stronger support for income redistribution in wealthier regions, all else equal, but with lesser support for such policies in poorer regions. Holm and Geys (2018) find evidence of those attitudes among Belgian and German survey respondents. Other-regarding preferences may play a similar role when a polity is seen to be under threat. For instance, if national identification entails internalizing security risks confronting fellow nationals, these may take on heavier weight relative to individual-level costs in convenience, privacy, or material wealth. If so, individuals' preferred cost-benefit trade-offs may in turn shift in the direction of greater security. Regarding identities' security policy effects, Barnes et al. (2014) find a mediating role for national identification between individual attributes and propensities to internalize national threats (including terrorism) and to support anti-terror policies. Swann et al. (2009) identify a 'fusion' of individual and country identities with expressed willingness to engage in extreme acts (such as military service or self-sacrifice) on behalf of the country. Meanwhile, Schoen (2008) finds that nested national and EU territorial identities help determine Eurobarometer survey respondents' support for common European foreign and military policies.

3. An exogenous shock and an ad hoc panel

In March 2016, terrorists reportedly linked to the Islamic State attacked Brussels at two locations, the airport and a metro station near the European institutions, killing 32 and injuring more than 300. Three weeks prior to the attack, the authors had administered an in-class pen-and-paper survey on a sample of undergraduate economics students ($N_1 = 152$) as a pilot study for another project. The pilot survey aimed to test the malleability of jurisdictional identification with a priming experiment, which contained a series of questions on participants' identification with different Belgian (plus European Union) jurisdictional levels. Specifically, one priming question was administered to each of two randomly assigned treatment groups: group Prime BE ($N_{1BE} = 49$) saw a question intended to increase (the salience of) connections to Belgium, while group Prime FL ($N_{1FL} = 52$) saw a question intended to increase (the salience of) Flemish connections. A control group ($N_{1C} = 51$) saw no priming question. A detailed description is available in supplementary information A. Given the unique circumstances and the clear interest in the literature in terrorism's effects (e.g. Bali, 2007; Friedland and Merari, 1985; Lerner et al., 2003; Montalvo, 2011), we administered a second survey with the same participant pool one week after the attacks to gauge their impacts on our subjects' jurisdictional connections.

The second survey examines several research questions. Firstly, does the attack cause changes in feelings of connection to different jurisdictional levels? Are any effects focused on the country level, as the literature typically suggests (e.g. Li and Brewer, 2004; Barnes et al., 2014), or do connections to other levels vary similarly? Participants' pre- and post-attack connection reports matched across surveys could suggest a clear causal relation. We also investigate the relation between jurisdictional connections and *perceptions* of the attack's severity, with the panel structure again disentangling the direction of effect: do people with particular ex ante connections feel more strongly affected by the attack, or do connections and feelings of affectedness move in tandem?

Additionally, we are able to explore mechanisms through which terror attacks may influence policy preferences. The political strategy of accentuating belonging and out-group defensiveness after attacks suggests that social identity plays a role in shifting preferences over security and civil liberties (Bozzoli and Müller, 2011; Geys and Qari, 2016). Questions on police force use and resourcing, along with reports of attack severity and pre- and post-attack connections, allow identifying the channels influencing security-related preferences.

We returned to the same classes in which students had taken part in our initial pilot study around one week after the attack and invited students to participate in a second pen-and-paper survey. At the time, news media already had reported casualty numbers and linked the attackers to the Islamic State. In total $N_2 = 127$ students completed the second survey. While both surveys were anonymous, the inclusion of repeated questions on each respondent's age, gender, academic program, and year of study provides an initial basis for matching participants across surveys. Because many of those characteristics are shared by multiple students, observations with non-unique backgrounds were inspected manually for handwriting matches by

all three authors. Including only those identified unanimously as matches, we obtain a panel of $N_{1\&2} = 89$ students across both surveys.[6] Because participants had been part of a priming experiment in the survey preceding the attacks, we take into account exposure to priming manipulations in the analysis that follows. Figure 1 outlines the paths respondents took through the two surveys.

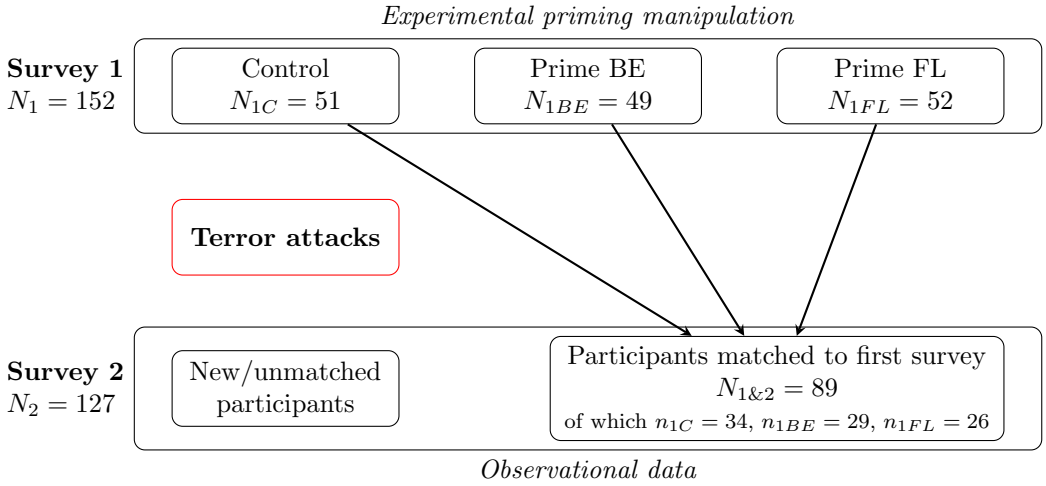

Figure 1: Participant distribution across groups and surveys

Connections to the different jurisdictions are identified by responses to the same (repeated) questions used in the first survey. As other control variables may have been affected by the exogenous shock, we also repeated some questions. They include questions asking participants to express on 11-point scales their views on *Flemish independence*, more or less *Flemish autonomy*, as well as self-placement on an ideological *left-right scale*. At population levels, those controls remained stable for participants across both surveys (see Table SI3 in the supplementary information). Such stability alleviates concerns of biased inferences owing to attrition between surveys or systematic shifts in the controls. The three variables, along with a gender dummy, comprise our standard controls.

Made possible by the unforeseen occurrence of the attacks, our panel survey 'design' is improvisational by necessity. As such, it is subject to notable limitations. In addition to selection and generalizability concerns common to student subject pools, our sample is distinctly small, being based on what was originally intended to be a pilot survey only. The sample is not representative of broader populations. Still, we argue that the results below offer unique perspectives best understood bearing these caveats in mind. Please see section 5 and the supplementary information for further discussion of sample and design limitations.

4. Results

Our hypothesis test is operationalized in two different ways in the following analyses of jurisdictional connections. First, we test for within-subject changes in jurisdictional connections occurring between pre- and post-attack surveys. Second, we turn to relations between jurisdictional connections and respondents' reports of how strongly they felt affected by the attack personally. Subsequently, we explore security preferences and their possible dependence on (changes in) jurisdictional connections. For brevity, we omit most controls from tables in the main text; full results for all of the empirical analyses that follow are available in supplementary information E.

[6] Supplementary information D contains details on the matching procedure. No demographic variables differ statistically across surveys. See Table SI4 in the supplementary information.

4.1. Within-subject connection changes

To estimate the attack's within-subject effects on jurisdictional connections, our previous experimental manipulation of the dependent variables must be taken into account. Of the 89 participants ($N_{1\&2}$) who are matched across both surveys, 34 (n_{1C}) were in our original control condition, 26 (n_{1FL}) in the Flanders prime treatment group, and 29 (n_{1BE}) received the Belgium prime in the first survey.[7] Only the changes for participants previously in the control group (n_{1C}) are independently informative. Generalizing the results from the control group to the entire sample requires somewhat more involved interpretation. As a precautionary check, we first address any possible direct influence of the priming manipulation prior to the attacks on the connection levels reported after the attacks. To assess the possibility of lasting divergences between treatment and control groups, we begin by testing for differences in connections reported (only) *after* the attacks according to the treatment group assigned in the first survey. As Table 1 shows, no differences in the primed connections to Flanders and Belgium persist significantly into the second survey; see supplementary information F for further discussion. Still, to account for any possible persistence of the priming effects across surveys, we control for first-survey treatment in the following analysis.

Table 1: Do survey 1 priming effects persist in survey 2?

	(1) Municipality	(2) Province	(3) Flanders	(4) Belgium	(5) EU
Prime BE	−0.882	0.219	−0.156	0.348	−0.773
	(0.645)	(0.661)	(0.575)	(0.538)	(0.582)
Prime FL	−1.508**	−0.175	−0.056	−0.469	0.088
	(0.670)	(0.687)	(0.598)	(0.559)	(0.605)
Constant	7.158***	5.321***	2.221**	7.939***	7.437***
	(1.164)	(1.192)	(1.038)	(0.970)	(1.050)
Controls			full		
Observations	85	85	85	85	85
R^2	0.115	0.028	0.161	0.190	0.129

Ordinary least squares regression estimates for jurisdictional connections reported in the second survey, only, for all participants matched between both surveys; $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

We turn next to our main analysis of changes in jurisdictional connections before and after the attacks. In the odd-numbered columns of Table 2, we report the results of individual-level fixed effect models for the control group only.[8] We enter the dummy *Post-attack*, with values 0 and 1 corresponding to surveys 1 and 2, respectively. Coefficient estimates for that dummy thus isolate changes in the dependent variable between surveys. Column (7) shows the size of the effect for the connection to Belgium to be positive and relatively large (0.691 points on our 11-point scale).[9] Despite the small sample size, the increase is significant at the 10% level.[10] No significant changes in connections to any other jurisdictional levels are evident. As other jurisdictions' (non-significant) coefficient estimates are uniformly positive, they *might* be seen as consistent with the idea that the attack tended to strengthen jurisdictional connections generally (c.f. Moskalenko et al., 2006). Statistical significance and the magnitude of the coefficient estimate clearly set the country level apart, however.

[7] Three matched, control group respondents gave incomplete responses to control variables in (only) one survey, resulting in the unbalanced panels reported here. One respondent gave incomplete responses to both surveys and drops from this analysis entirely. The results change negligibly upon restricting the sample to the 30 control group participants with complete data for all controls in both surveys to obtain a fully balanced panel.

[8] Omitting (sets of) the controls does not alter the qualitative findings throughout. We observe some significant effect estimates for control variables, chiefly for attitudes toward Flemish independence and Flemish autonomy, see supplementary information E.

[9] Supplementary information Table SI5 presents corresponding mean connection levels by treatment group and survey for the matched sample.

[10] Omitting from the regression four subjects who reported very low ex-post assessments of the attack's severity (see below) strengthens this result considerably. The estimated post-attack effect on Belgian connections remains the strongest, at 0.978, with $p = 0.021$.

To assess the overall pre- to post-attack changes in connections across our entire sample, we apply generalized least squares models with random effect estimators. Unlike the fixed effects approach, that technique allows analysis of all three groups at once, along with estimation of the effects of invariant individual-level controls (here, gender and priming group assignment). To facilitate inferences for the differently primed participants, we enter interaction terms between the *Post-attack* dummy and the different treatment groups. By excluding the main, uninteracted effect for *Post-attack*, the model provides distinct coefficient estimates for each group's change between surveys, taking the form:

$$\begin{aligned} Y_i = \alpha \\ &+ \beta_1 \text{Post-attack} \times \text{Control group} \\ &+ \beta_2 \text{Post-attack} \times \text{Prime BE} \\ &+ \beta_3 \text{Post-attack} \times \text{Prime FL} \\ &+ \beta_4 \text{Prime BE} \\ &+ \beta_5 \text{Prime FL} \\ &+ \textit{Control variables} \\ &+ \varepsilon_i \end{aligned} \tag{1}$$

Table 2's even-numbered columns report the results. Because unmanipulated observations from before the attacks are not available for participants in the treatment groups, the control group remains the most informative subsample. Accordingly, the regressor of primary interest is the interaction of the post-attack dummy with the dummy for control group assignment – corresponding directly to the post-attack estimates reported in the odd-numbered columns. Interaction terms for the treatment groups capture their between-survey changes directly, as well, but they begin from starting levels potentially manipulated by our primes.

The between-surveys increase in Belgian connections for the unmanipulated control group is nearly identical in magnitude to the estimate reported in column (7), and now is significant at the 5% level. This country-level effect remains strongest among all tiers in estimated magnitude. Connection to the EU now is marginally significant and other tiers remain consistently positive, but fall short of significance.[11] The consistency of findings for both the control group subsample and the full matched sample corroborates the main finding: the attack increased reported connections to Belgium significantly among those not subject to priming interventions. Other jurisdictions may be suggestive of broadly strengthened connections, though they are smaller in magnitude and (with the exception of the EU) fail to approach statistical significance.[12]

4.2. Attack severity and jurisdictional connections

We turn next to a different operationalization of (perceptions of) the attack in order to capture some aspect of its effect, a second-survey question in which participants rated on an 11 point scale how severely

[11] Note also that the total differences in connection levels for the treatment groups (relative to the control group in survey 1), after both priming and the attack, are obtained by summing the prime coefficients with their respective post-attack interaction coefficients. In, e.g., the case of Belgian connection, Wald tests indicate that the total difference for the Belgian prime group is significantly positive and that neither prime group's total difference diverges significantly from the control group's pre- to post-attack difference (although the Belgian prime group's is somewhat larger and the Flemish prime group's is somewhat smaller). Thus, while the evidence could be taken to suggest that the primes *may* have some persistent effect across surveys, final Belgian connections do not differ significantly across treatment groups. See supplementary information F for further discussion.

[12] Our analysis emphasizes country-level connections. That approach follows both from our own prior beliefs regarding jurisdictional identities affected by terrorism, and from a large literature linking terror and (stronger) national identities, often in the form of patriotism or a rally 'round the flag effect. Still, we acknowledge that concerns regarding potential multiple testing issues may arise in light of our study of five different jurisdictional levels. In particular, when applying Bonferroni corrections for the five tiers, the results for connection to Belgium in columns (7) and (8) of Table 2 fall below conventional levels of statistical significance, narrowly so in the case of column (8). We argue that our findings remain informative in light of our limited sample size, established priors about the country level's importance, and the consistency of the findings across the different variables of interest.

Table 2: Jurisdictional connections pre- and post-attack – matched panel

	Municipality		Province		Flanders		Belgium		EU	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	FE	RE	FE	RE	FE	RE	FE	RE	FE	RE
Post-attack	0.238		0.327		0.120		0.691*		0.441	
	(0.391)		(0.496)		(0.351)		(0.380)		(0.332)	
Post-attack × Control		0.407		0.405		0.247		0.694**		0.609*
		(0.347)		(0.438)		(0.326)		(0.314)		(0.320)
Post-attack × Prime BE		0.146		0.715		0.300		−0.298		0.022
		(0.360)		(0.456)		(0.338)		(0.325)		(0.332)
Post-attack × Prime FL		−0.294		0.303		−0.225		0.170		0.530
		(0.380)		(0.480)		(0.357)		(0.343)		(0.350)
					Control is reference					
Prime BE		−0.569		−0.042		−0.153		1.484***		−0.054
		(0.635)		(0.611)		(0.586)		(0.554)		(0.564)
Prime FL		−0.750		0.013		0.449		0.276		0.287
		(0.666)		(0.641)		(0.615)		(0.581)		(0.592)
Constant	7.181**	6.080***	9.390**	4.881***	6.608**	2.198**	2.398	6.555***	5.727**	6.860***
	(3.280)	(0.983)	(4.162)	(0.960)	(2.950)	(0.909)	(3.186)	(0.862)	(2.784)	(0.878)
Controls	excl. gender	full	excl. gender	full	excl. gender	full	excl. gender	full	excl. gender	full
Observations	63	172	63	172	63	172	63	172	63	172
R^2	0.173	0.080	0.089	0.031	0.052	0.185	0.144	0.184	0.197	0.100

FE: Fixed effect generalized linear regression estimates for the pre- to post-attack changes in jurisdictional connections reported by control group respondents with matched observations across both surveys;

RE: Random effects ordinary least squares regression estimates for jurisdictional connections reported by all participants matched across both surveys. The regressor of primary interest is the interaction of the post-attack dummy with the dummy for control group assignment, thus capturing directly between-survey connection changes for the subset of respondents whose connections were not manipulated by priming in Survey 1. Uninteracted prime dummies correspond to priming effects in Survey 1.

$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

affected they personally felt. Figure 2 presents the response distribution. The considerable variation evident helps allay potential concerns that demand bias in reporting (e.g., responses driven by social expectations of conformity regarding expressions of personal affectedness) might cause clustering at extremes.

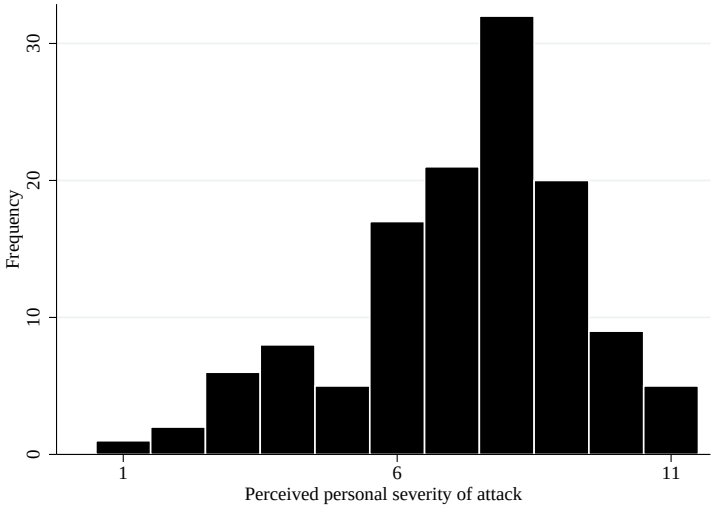

Figure 2: Histogram of perceived personal severity of the attack

In the following, we emphasise a dummy variable indicating perceived attack severity. It takes a value of 1 when a participant reports the median score of seven or above (value 0 otherwise), producing two groups roughly equal in size (66 and 60 respondents, respectively). All of our results are robust to the use of the severity scale as a continuous variable; we report the dummy results for ease of interpretability.

We estimate the correlation of perceived attack severity to jurisdictional connections using ordinary least squares regressions with a full set of control variables and the *perception of attack* dummy as the main independent variable.[13] This analysis begins with the full survey 2 sample (N_2), i.e., *not* only those respondents matched across surveys. Columns (1) through (5) in Table 3 present results for each jurisdictional level. Respondents who perceived the attack as severe report closer connections to all five levels, significantly so on all but the EU level. The largest and most significant difference occurs for Belgium: participants who felt severely affected score around 1.5 points higher on our 11-point scale (see also Geys and Qari, 2016). For a sense of estimated differences relative to jurisdiction-specific baselines, we inspect the ratios of the severity coefficients in Table 3 to the respective control-group means from Table SI1 in the supplementary information. Again, the relative difference is strongest for Belgium and weakest for the EU.[14] The positive coefficients for perceptions of attack severity across levels again suggest that nested jurisdictional identifications are not necessarily substitutes for one another, but may shift in tandem. Still, the country-level result stands out in size and significance.

[13] Participants were also asked if they were in Brussels on the day of the attacks. Controlling for their whereabouts does not influence our results.

[14] An alternative specification interacts the severity measure with the post-attack dummy in a random effects panel model, testing directly whether pre- to post-attack connection changes are most evident among subjects who report being affected strongly by the attack. That is found to be the case – again, most powerfully for Belgium – with results corresponding closely to those presented in Table 3. Our thanks to an anonymous referee for the suggestion.

Table 3: Perceived attack severity and jurisdictional connections

	(1) Municipality	(2) Province	(3) Flanders	(4) Belgium	(5) EU
Perception of attack	1.125**	0.922**	0.895**	1.508***	0.396
1=severe	(0.492)	(0.454)	(0.411)	(0.373)	(0.408)
Constant	4.988***	4.406***	1.627*	6.559***	6.368***
	(1.024)	(0.947)	(0.856)	(0.778)	(0.849)
Controls			full		
N	120	120	120	120	120
R^2	0.101	0.060	0.190	0.239	0.115

Ordinary least squares regression estimates of above/below median perceptions of attack severity and reported connections to five jurisdictional tiers; standard errors in parentheses; $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

4.3. Direction of effect between jurisdictional connections and attack severity

An immediate concern in the estimations reported in Table 3 above is the direction of effect. Participants may feel more connected to a jurisdiction after the attack irrespective of their initial levels of identification, or those who already felt strongly connected may have felt the attacks more severely. To distinguish between those two hypotheses, we first examine the influence of *first-survey* connections on the perceived personal severity of the attacks in Table 4.[15] At all jurisdictional levels, and across first-survey treatment (sub)samples, prior connections are small in effect size and not statistically significant in determining our participants' perceptions of the attack.[16]

Much stronger findings result from direct tests of perceived attack severity's relation to the *changes* in connections with each jurisdictional entity. We regress those pre- to post-attack connection changes on the severity measure, treatment group dummies where appropriate, and a full set of controls. Table 5 presents the results; again, because of their unmanipulated survey 1 baseline, the control group subsample is the most informative. The more affected participants indeed more sharply increase their reported connections to most jurisdictions in the second survey. As seen in previous sections, Belgium itself is strongest among jurisdictions in magnitude and significance.

We thus find no evidence that respondents with strong ex-ante jurisdictional connections felt the attack more severely, but do find significant correlations between reports of greater attack severity and increases in jurisdictional connections, particularly at the country level.

4.4. Terrorism and policy preferences

Previous studies of terrorist attacks find increases in the importance assigned to security policy issues by affected populations, greater concern over terrorism, and more willingness to trade civil liberties for increased security (Bozzoli and Müller, 2011; Geys and Qari, 2016). An important question regarding those reactions is whether they follow immediately from an attack's severity, or are instead driven (or mediated) by shifts in identity or in-group versus out-group balance. By asking two questions on the use and resourcing of local police forces in our second survey, we address possible relations among security policy preferences, perceptions of attack severity, and (shifts in) jurisdictional connections. Respondents indicated, on 7-point scales ranging from 'much less' to 'much more', whether in the near future (1) local police should allocate their current resources to combat terrorism, and (2) police should receive additional resources financed, for example, by additional taxation.

Table 6 presents the results. Columns (1) through (4) address preferences over the use of existing police resources and columns (5) through (8) preferences over the amount of resources. The regressor of primary interest is the *perception of attack* dummy; we retain our standard set of control variables throughout. For

[15] Note that, to keep the results comparable to previous sections, we use the 11-point scale scores for perceived attack severity in this subsection. The results change little when estimating ordered logit models, or when the continuous measure of attack severity is replaced by a below/above-median dummy.

[16] The same holds when all five jurisdictional connections are included in a single regression, both for the control group and the full sample.

Table 4: Parting out the direction of effect: prior connection levels

	Perceived personal severity of attack									
	(1) Control	(2) Full sample	(3) Control	(4) Full sample	(5) Control	(6) Full sample	(7) Control	(8) Full sample	(9) Control	(10) Full sample
Connection in survey 1										
Municipality	0.140	0.012								
	(0.201)	(0.090)								
Province			−0.185	0.014						
			(0.218)	(0.099)						
Flanders					0.257	0.003				
					(0.191)	(0.095)				
Belgium							0.082	0.018		
							(0.184)	(0.100)		
EU									0.175	0.086
									(0.176)	(0.102)
Prime BE		0.839		0.831		0.804		0.831		0.836
		(0.524)		(0.520)		(0.541)		(0.520)		(0.517)
Prime FL		−0.333		−0.344		−0.348		−0.346		−0.362
		(0.560)		(0.553)		(0.554)		(0.557)		(0.551)
Constant	6.359***	7.263***	7.992***	7.264***	6.430**	7.221***	6.763***	7.342***	5.544**	6.733***
	(2.256)	(1.195)	(2.218)	(1.168)	(2.518)	(1.243)	(1.947)	(1.030)	(2.522)	(1.255)
Full controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Observations	33	87	33	87	33	87	33	87	33	87
R^2	0.170	0.122	0.177	0.122	0.161	0.123	0.208	0.122	0.184	0.130

Ordinary least squares regressions of perceived attack severity (11 point scale) on *first-survey* jurisdictional connections; standard errors in parentheses; $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 5: Parting out the direction of effect: perceived attack severity

	Δ Connection to Municipality		Δ Connection to Province		Δ Connection to Flanders		Δ Connection to Belgium		Δ Connection to EU	
	(1) Control	(2) Full sample	(3) Control	(4) Full sample	(5) Control	(6) Full sample	(7) Control	(8) Full sample	(9) Control	(10) Full sample
Perceived personal severity of attack	0.427**	0.274***	0.420	0.207	0.370*	0.243**	0.640***	0.247***	0.397**	0.090
	(0.199)	(0.102)	(0.267)	(0.134)	(0.179)	(0.097)	(0.136)	(0.091)	(0.164)	(0.098)
Prime BE		−0.280		0.128		−0.104		−0.981**		−0.519
		(0.475)		(0.624)		(0.450)		(0.423)		(0.457)
Prime FL		−0.476		−0.199		−0.348		−0.262		−0.013
		(0.494)		(0.650)		(0.469)		(0.440)		(0.476)
Constant	−3.217	−2.508**	−2.617	−2.157	−1.980	−1.502	−5.793***	−0.976	−1.556	0.526
	(2.492)	(1.163)	(3.334)	(1.530)	(2.243)	(1.104)	(1.702)	(1.037)	(2.047)	(1.120)
Full controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Observations	30	85	30	85	30	85	30	85	30	85
R^2	0.298	0.202	0.147	0.121	0.212	0.138	0.620	0.185	0.319	0.095

Ordinary least squares regressions of between-surveys *changes* in jurisdictional connections on reported severity of the attack (11 point scale); standard errors in parentheses; $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

each dependent variable, we estimate four different specifications. We begin by focusing on the full set of respondents in the second survey (N_2) and those variables recorded exclusively after the attacks. The base model in columns (1) and (5) of Table 6 retains (only) measures of attack severity and the controls. We expect that feeling more severely affected correlates positively with a preference for shifting the use of the police towards combating terrorism and with a preference for more police resources. While the severity estimate for the use of existing resources does not fall into the conventional margins of statistical significance, it reaches the 5% level for the amount of resources. Note that the question was asked during a period of already dramatically heightened police activity across Belgium, particularly in Brussels. The results should be interpreted relative to that reference point, which may explain their limited significance. It is also conceivable that respondents may have been reluctant to express support for reallocating existing resources (i.e., columns (1) to (4)) if they understood this as unduly critical of security policy or personnel.

As participants' connections to various jurisdictions may influence their attitudes towards (police) institutions – particularly those in Brussels – we control for their reported levels of connection in our second specification (columns (2) and (6), respectively). The connection levels used here are those reported post-attack. The negative coefficient for connection to Flanders suggests that those respondents expressing stronger identifications with the region are less supportive of allocating more resources to counter terrorism. Taking the jurisdictional connection levels into account, the coefficient for *perception of attack* now reaches (marginal) significance for that use of police resources. The *perception of attack* coefficient for the amount of resources remains significant at the 5% level; connections to Flanders and Belgium seem not to have any influence on that preference.

As connection to Flanders shows the only significant effect, we emphasize it in the following analysis. However, all estimations below have been repeated for the other jurisdictional levels, yielding no significant effects.[17] Connection to Flanders thus serves as an upper bound for the influence of jurisdictional identification on the policy issues analyzed.

Because the results from the first two specifications in Table 6 suggest a positive correlation between perceptions of the attack's severity and security preferences, we investigate the within-subject changes in reported connection levels further to disentangle the potential influence(s) of post-attack severity perceptions and of potential shifts in jurisdictional connections. Columns (3) and (4) and columns (7) and (8) therefore report the results for our (full) matched sample $N_{1\&2}$, taking into account the changes in participants' reported connections to Flanders. In themselves, shifts in the reported levels of connection from pre- to post-terror attack seem not to have any discernible relation to the support for either of our two security policy questions (see columns (3) and (7)). To test whether changes in jurisdictional connections possibly mediate the relation between affectedness and policy preferences, we include an interaction term between the *perception of attack* dummy and the *difference in connection to Flanders* between the two surveys. The interaction term coefficient estimates remain small and not significant.[18] Note also that, if (changes in) jurisdictional connections were a significant mediator or channel through which perceptions of the attack's severity influenced preferences over police policy, we would expect the magnitude of coefficient estimates for the severity variable to *decline* after adding controls for (changes in) jurisdictional connections to the base model. That is, we would expect smaller coefficient estimates in columns (2) through (4) than in (1), as well as in columns (6) through (8) than in (5). Instead, the estimates in models with jurisdictional connection measures are somewhat *larger* than those in the respective models without them.[19] More formally, structural equation modeling confirms that no combination of the jurisdictional connections measured, nor differences therein, is a significant mediating or moderating channel between respondents' experience of the attack and their preferences over police policy; details available upon request.

In line with previous findings in the literature on issue importance and more abstract preferences for trade-offs between different policy areas (e.g., civil liberties and security), the results suggest that terrorist

[17] Omitting connection to Belgium from models (2) and (6) also leaves them effectively unchanged.

[18] Again, we repeat the same estimations with (changes in) connections to Belgium and the other tiers, but still find no significant effects.

[19] Adding the various measures for (changes in) other connections causes even less of a change in the coefficient estimates for perceived attack severity.

Table 6: Effect of perceived severity of the attack on security policy relevant preferences

	Use of police resources				Amount of police resources			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Perception of attack	0.336	0.388*	0.546***	0.531**	0.408**	0.471**	0.595***	0.604***
1=severe	(0.204)	(0.221)	(0.207)	(0.207)	(0.189)	(0.206)	(0.217)	(0.219)
Connection to EU		0.001				0.056		
		(0.054)				(0.051)		
Connection to Belgium		0.028				−0.019		
		(0.056)				(0.053)		
Connection to Flanders		−0.118*	−0.058	−0.056		−0.080	−0.026	−0.027
		(0.060)	(0.047)	(0.047)		(0.056)	(0.049)	(0.050)
Connection to Province		0.048				0.012		
		(0.054)				(0.050)		
Connection to Municipality		−0.030				0.004		
		(0.051)				(0.047)		
Difference in FL connection			0.024	0.093			−0.062	−0.102
			(0.061)	(0.094)			(0.064)	(0.099)
Perception × Difference FL				−0.114				0.066
				(0.117)				(0.124)
Constant	4.631***	4.572***	4.587***	4.621***	3.743***	3.568***	3.816***	3.796***
	(0.425)	(0.641)	(0.410)	(0.412)	(0.393)	(0.599)	(0.430)	(0.434)
Controls	full	full	full	full	full	full	full	full
Observations	120	120	85	85	120	120	85	85
R^2	0.032	0.082	0.138	0.148	0.116	0.141	0.131	0.134

Ordinary least squares regression estimates; standard errors in parentheses; $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

attacks likewise may relate to preferences over specific security policy issues. What is most important, such differences appear to be linked directly to our subjects' immediate perceptions of their own affectedness, rather than to changes in their reported jurisdictional connections.[20]

5. Limitations and discussion

While providing unique and useful insights on the composition of jurisdictional identities, this study is best understood as an unforeseen ad hoc analysis of an extreme exogenous shock, conducted on a small sample of Dutch-speaking undergraduate economics students. Standard caveats regarding statistical power and generalizability apply. Moreover, it is likely that the specific selection of our sample is relevant as well. Belgian and Flemish identities relate in complex, and sometimes antagonistic, ways. By their presence in Brussels alone, our students may be distinct from other Flemings – see the supplementary information for discussion. Such subject pool distinctions must be borne in mind when interpreting our findings.

Our data neither allow separating strictly individual responses from other potential external (for instance, media) influences on perceptions, nor reliable inferences on how the observed effects would develop over a longer time frame. However, similar effects on, for instance, institutional and social trust appear to be short-lived (Arvanitidis et al., 2016; Geys and Qari, 2016). Nonetheless, they may have drastic impacts on political outcomes in the short term (Bali, 2007; Montalvo, 2011)

Given those caveats, the magnitudes and significance levels of the effects observed still speak to the 2016 terror attacks' impact on jurisdictional connections. Our results offer a rare glimpse into the effects of extreme external events on nested jurisdictional identifications and may open avenues for further study. While replication in terms of such extreme exogenous shocks is (to be hoped) unlikely, more targeted approaches, e.g., inducing controlled experimental manipulations, may provide a better understanding of the identified effects.

6. Conclusion

Nationalistic themes increasingly are prevalent in many countries' political discourses. Partly in response, social identity has gained attention across scholarly disciplines. Forceful exogenous shocks to identity, such as the terror attacks discussed herein, may arrive unexpectedly and consequentially. It thus may be important for politicians, voters and society at large to understand the composition and determinants of social identities, as well as the possible causes and implications of *changes* in their types and levels of salience.

We exploit the coincidental timing of an in-class pilot survey for a separate project on jurisdictional identification conducted shortly before the 2016 terror attacks in Brussels. To test the inter-relatedness of that extreme exogenous shock and jurisdictional identification, we study within-subject changes in reported connections before and after the attacks.[21]

In the control group that had not been exposed to any priming in the pilot survey, pre- and post-attack observations *might* be consistent with a broad strengthening of various jurisdictional connections (i.e, connections to municipality, province, region, country and the European Union), although most differences are suggestive at best and generally not statistically significant. The notable exception is a significant increase in reported country-level connections. The attacks may have had a unifying effect if our participants perceived the target to be the country as a whole, more than any specific part of it, making Belgian identities more salient (see also Geys and Qari, 2016). Those findings are supplemented by an additional investigation of reported subjective assessments of the *severity* of the attacks: perceptions of the attacks as severe correlate with stronger reported jurisdictional connections across the board – significantly so, in most cases – but with Belgium in particular. Again, the pre- and post-attack panel structure helps disentangle causality,

[20] Consistent with prior work, we also find evidence that perceptions of attack severity correlate with a small rightward shift in participants' positioning on the left-right ideological scale, significant at the 5% level. As was the case with jurisdictional connections, we find *no* evidence that (changes in) left-right positioning mediates the effect of the attack on security preferences.

[21] The limitations of the sample available for the current paper should, however, be kept in mind, and future research should verify the observed effects.

suggesting that severe feelings of affectedness by the attacks are either determined jointly with strengthened jurisdictional identity or precede it entirely; in contrast, we find no evidence for any tendency of strong ex-ante identifiers to feel affected more severely by the terrorist event. The rallying around the flag effect of the attack thus is supported strongly and quite unambiguously in its direction. Further, while terror's effect on connections appears at least weakly positive across jurisdictional tiers (and typically significantly so when employing our measure of severity), it again is concentrated distinctly at the country level.

Finally, in analyzing questions on police force use and resourcing, we take some steps towards clarifying the mechanism through which terror attacks influence policy preferences. The pre-to-post attack changes in jurisdictional connections and our respondents' reports of the attack's severity allow testing whether their security preferences are influenced by jurisdictional identities (e.g., through other-regarding preferences) alongside their own individual experiences of the event. We find no evidence that (either levels or shifts in) jurisdictional connections predict support for anti-terror police policies or financing. Rather, stronger support for those policies among our respondents appears to correlate directly to personal perceptions of the attacks' severity, consistent with traditional concepts of self-regarding rationality.

It is perhaps not surprising that, so shortly after the shock of the March 2016 Brussels attacks (and the attendant, dramatic rise in the salience of terrorism), the immediate experiences of terror's severity would be a significant determinant of security preferences. Future research should elucidate possible longer-term linkages between jurisdictional connections and perceptions of threat, as well as security policy. In the short run, however, our findings indicate that individual experience or perceived personal threats are more immediately decisive for our subjects' security preferences than are their senses of jurisdictional connectedness.

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Supplementary information A. Pilot survey

This paper arose from an attempt to test the malleability of jurisdictional (social) identities, beginning as a pilot study with Flemish student subjects conducted in preparation for a larger (between-subject) priming experiment. The pilot coincidentally took place around three weeks before the terror attacks in Brussels in March 2016. Bearing in mind all caveats relating to our small student sample, and (partly) in order to provide context to the terror results in the main paper, we report the priming results below. These confirm the efficacy of survey priming interventions in targeting and altering (some) jurisdictional connections. They also provide background detail for our precautionary inclusion of priming controls in the terror analysis in the main paper, and for our emphasis there on the unprimed control group.

Literature and Motivation for the pilot survey

Political, social, and economic preferences and behaviors may be influenced by people's social identities, including feelings of connection to jurisdictional levels such as nations or regions. Social identities and their determinants and malleability have received extensive attention (see e.g. Brewer, 1991; Brewer and Caporael, 2006; Brewer, 2007; Gioia and Thomas, 1996; Pickett et al., 2002). Multiple attributes may help define social identities. Considerable research investigates identification with different groups, organizations, and places (see e.g. Lewicka, 2011, for a review), and the determinants shaping the resulting group identities – including the self-evaluation and self-construal of identities by individuals, tendencies to sort the self and others into groups, and perceptions of external threats (Brewer, 1991, 2007). One form of identification frequently relevant to political, social, and economic preferences and behaviors is people's connection to different regional and political entities (see e.g. Cramer Walsh, 2012; Lindqvist and Östling, 2013; Shayo, 2009), and in particular their *jurisdictional identification* (see Holm, 2016; Holm and Geys, 2018). Importantly, different levels of territorial identification may be either competing or mutually reinforcing (Hooghe and Marks, 2005). At the same time, "multiple identities are the norm rather than the exception and [...] different settings make different types of identities salient" (Lewicka, 2011, p.213). However, existing studies typically examine social identities' impacts on public policy or political preferences only at the country level (e.g. Li and Brewer (2004) and Barnes et al. (2014); see Cramer Walsh (2012) for a notable exception). As discussed by Sen (2006), single-level analysis of a multi-faceted construct such as social identity may yield oversimplified or biased inferences. Distinguishing between different overlapping or competing identities may reveal important nuance. Given nested identities targeted for external influence, for instance, questions arise not only whether a specific (e.g. jurisdictional) connection can be elicited, but also how connections to other (jurisdictional) levels react.

Connections people feel to their social environments, and the social groups they see themselves as belonging to, influence perceptions of themselves and others. Along with other factors (among them public resources, institutions, and inequality among individuals or regions), these social identities may help shape political views, voting behavior, and ultimately public policy (see e.g. Alesina et al., 1999; Shayo, 2009; Freier et al., 2016; Holm, 2016; Holm and Geys, 2018). Because social identities are potentially decisive drivers, and can themselves be fluid, they are often in the political focus. For political actors, campaigners, or other interested groups seeking to exert influence on public opinion, it is a common strategy to appeal to people's identities or to certain social groups.

We make use of a survey prime experiment to test the susceptibility of connections to nested jurisdictions to salience manipulation. *Priming* manipulation can be seen as influencing the response to a stimulus by *a priori* increasing the salience of another, separate stimulus. People rely on established anchors, like memory traces or activation tags, in memory linking to known concepts or issues. When new information is processed, it intersects with and activates previous tags (Scheufele, 2000). In turn, in memory-based models of information processing, people judge issues based on that information which they find most accessible when the judgment is made. The more easily information can be accessed, the more likely it is to directly impact judgment and attitude formation (Iyengar, 1990; Tversky and Kahneman, 1973; Scheufele, 2000).

A large literature has addressed priming and its power to (temporarily) alter people's attitudes and behavior. For instance, priming religious concepts may lead to an increase in social prejudice (Johnson et al., 2010) or increase pro-social behavior (Shariff and Norenzayan, 2007), whereas priming ethical considerations

may alter people's honesty (Mazar et al., 2008; Welsh and Ordóñez, 2014). More closely related to this paper, Benjamin et al. (2010) prime social identity to alter economic preferences, and Hertel and Kerr (2001) use primes of group behavior norms to manipulate in-group identification.[22] Given its federal structure with clearly distinguished jurisdictional tiers, Belgium offers a particularly interesting laboratory for testing the effect of salience priming on identification. A typical Belgian may identify, in some combination, with Europe, Belgium, a region (Flanders, Wallonia, or Brussels), one of the ten provinces, or a local municipality. The split between French-speaking Wallonia and Dutch-speaking Flanders gives particular importance to the difference between identification with one's region and the federal construct of the Belgian state.[23]

A priming survey experiment

To test the susceptibility to salience cues of respondents' identification with different jurisdictional entities, we assess jurisdictional *connections* following the German ALLBUS household surveys.[24] Identification is operationalized through participants' responses regarding five jurisdictions to the question: "How strongly connected do you feel to the following regions and their inhabitants?" We use separate 0-10 point scales for each jurisdiction, corresponding to administrative tiers pertinent to our Flemish[25] participants: village or city, province, the region of Flanders, Belgium, and the European Union (see Deschouwer, 2012; Freier et al., 2016; Holm and Geys, 2018). Consequently, jurisdictional identity is a multi-level construct. Participants were invited to participate in an in-class survey on identity, political views, and policy preferences. They were randomly assigned to either one of two treatment groups or a control group. All participants received a range of questions on their personal and study background, their views on Flemish autonomy and independence, general political orientation, and their involvement with political organizations. In the two treatment groups, an additional block of questions at the start of the survey served as the prime. Treatment participants read a scenario describing the ongoing development of a campaign promoting either Flanders or Belgium, depending on the group.[26] In each prime, participants were asked to assess the suitability of five different themes (food, architecture, sport, history, and arts and culture) for inclusion in the proposed campaign. A list of examples corresponding to the respective jurisdictional level was presented for each theme (see Supplementary information B for question texts). The cover story and themes provided were chosen to evoke associations which were broadly positive, yet chiefly apolitical and nonpartisan. Participants scored on a 5-point scale ('definitely not' to 'definitely yes') whether they would include each theme. They could also suggest additional themes and items. Highlighting (stereo-)typical, favorable associations with a jurisdictional level is intended to increase its salience and to strengthen respondents' current identification with that level. The promotion campaign thereby serves as a background scenario significantly distinct from standard political or policy preferences.[27] We hypothesize a targeted increase in the salience of a jurisdictional entity in our survey-experiment to increase the reported level of connection to it, but not (necessarily) to any other jurisdictional levels.

Participants

The survey experiment (between-subject) was conducted using pen and paper among undergraduate economics students in classroom settings at the Vrije Universiteit Brussel, the Dutch-language Free University of Brussels. All participants were above 18 years of age, and participation was voluntary and not

[22]Priming studies have been among the research affected by the recent "replicability crisis" in the social sciences. At the suggestion of an anonymous referee, we confirm that the studies we cite have been corroborated by closely complementary work, and in some cases directly replicated. We are unaware of any unsuccessful direct replication attempts.

[23]See Deschouwer (2012) for an overview of the Belgian polity and the distinction between language communities and geographical regions.

[24]`http://www.gesis.org/en/allbus/allbus-home/`

[25]The surveyed students all speak Dutch and attend a university associated with the Flemish community. However, we note that around one quarter of our sample are from the Brussels region, and three quarters from Flanders. As controlling for this attribute does not alter any of our results, we treat subjects as one group of 'Flemish' students.

[26]We emphasize the levels of Belgium and Flanders in this section because they provide two distinct concepts which remain uniform enough to operationalize through one single prime each. Lower jurisdictional levels would require more localized testing. We focus on the domestic influence of internally competing identities; see, for instance, Hooghe and Marks (2005) for interrelations between (sub-)national and European identities.

[27]Indeed, none of the background political attitudes we assess are significantly different across groups; see below.

incentivized. According to their responses, our total of (N_1 =152) participants were 21 years old on average (sd = 2.49, $median$ = 20.5), 48.7% were female, and their political orientation was predominantly center-right ($median$ = 7 on a 1-11 point scale, sd = 2.03).

Results

Participants were assigned either to one of the two treatment groups or to the control group by random sequencing during the survey distribution in each class, yielding roughly equally-sized groups. Testing for differences in demographic characteristics among the three groups reveals no significant divergences (see table SI.2).

Table SI.1 presents simple group means of reported connections to each jurisdictional level, split by treatment group. In the control group in column (1), connections are closest with the municipality and with Belgium, followed by Europe and Flanders, and weakest for the Province. Columns (2) and (3) present connections within the Belgium and Flanders prime groups, respectively. Between-group differences are tested with pairwise comparisons of means t-tests, with significant differences indicated in the table and discussed below. We estimate the inter-item correlations among the reported connections. For all five connections, Cronbach's alpha is 0.633, suggesting that the various tiers measure distinctly different concepts. This interpretation is even clearer when considering only the two scales for Flanders and Belgium, for which $\alpha = 0.324$. The low Cronbach's alpha values support the view that identification with different jurisdictional tiers are best seen as meaningfully separate.

Table SI.1: Connections to jurisdictional entities for control and treatment groups

	(1)	(2)	(3)
	Control	Belgium Prime	Flanders Prime
Municipality	5.882	6.063	5.885
	(2.479)	(2.453)	(2.662)
Province	4.333	4.813	4.942
	(1.751)	(2.557)	(2.623)
Flanders	4.745	4.938	5.231
	(2.180)	(2.693)	(2.587)
Belgium	5.529	7.188^{a***}	6.115^{b***}
	(2.023)	(2.130)	(2.463)
Europe	4.824	5.583	5.423
	(1.997)	(2.508)	(2.243)
N	51	48	52

Mean connections to five different jurisdictional entities (0-10 point scales), reported by Flemish students, for Control, Belgium prime, and Flanders prime groups; standard deviations in parentheses. Differences tested with pairwise comparisons of means t-tests, yielding (only) two significant differences: [a] Belgium Prime versus control groups; [b] Belgium versus Flanders Prime groups. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Across nearly all tiers, jurisdictional connections appear somewhat higher in the treatment groups (the sole exception is the municipality level in the Flanders prime). The Belgium prime yields the only significant difference between the three groups within one jurisdictional level: the mean connection to Belgium increases by nearly one standard deviation compared to the control group. The difference to the Flanders prime group is somewhat smaller, but still substantial (more than half a standard deviation).

Thus the Belgium prime produces a significant, and relatively targeted, increase in Belgian connection reported by our subjects. Still, other connections also increase rather consistently – if not significantly – suggesting that these jurisdictional identities are not strictly substitutable. These findings are closely corroborated by regression analyses controlling for background characteristics; these are robust to inclusion or exclusion of various sets of the control variables we have available; details available upon request.[28]

[28] Neither left-right position nor support for Flemish independence or autonomy differ significantly among treated and control groups in supplementary regressions, suggesting that our primes affect jurisdictional identities at least partly independently of background political attitudes. We thank an anonymous referee for prompting these tests.

Supplementary information B. The prime treatments

Belgium
Imagine that a new campaign is being designed to advertise Belgium. The campaign will be used to promote tourism and business in Belgium abroad. It should demonstrate the uniqueness of Belgium. Which of the following characteristics specific of Belgium would you highlight?

1. Belgian cuisine (e.g. Belgian beer, chocolate, fries, waffles . . .)
   Definitely not O O O O O Definitely yes
2. Architecture (e.g. the Atomium, Art Nouveau . . .)
   Definitely not O O O O O Definitely yes
3. Sports (e.g. the Red Devils . . .)
   Definitely not O O O O O Definitely yes
4. Heritage and history (Waterloo, World War II sites . . .)
   Definitely not O O O O O Definitely yes
5. Art and culture (e.g. Comics, Belgian artists like Magritte . . .)
   Definitely not O O O O O Definitely yes
6. Other: . . .

Flanders
Imagine that a new campaign is being designed to advertise Flanders. The campaign will be used to promote tourism and business in Flanders abroad. It should demonstrate the uniqueness of Flanders. Which of the following characteristics specific of Flanders would you highlight?

1. Flemish cuisine (e.g. vol-au-vent, chicory with ham, beef stew . . .)
   Definitely not O O O O O Definitely yes
2. Architecture (e.g. cloth halls, belfries . . .)
   Definitely not O O O O O Definitely yes
3. Sports (e.g. track and field, the Tour of Flanders . . .)
   Definitely not O O O O O Definitely yes
4. Heritage and history (Guldensporenslag, World War I sites . . .)
   Definitely not O O O O O Definitely yes
5. Art and culture (e.g. Flemish Masters, Rubens . . .)
   Definitely not O O O O O Definitely yes
6. Other: . . .

Supplementary information C. Overview of control and independent variables

Table SI.2: Sample demographics in survey 1 by prime

	Prime	N	Mean	Median	Sd	Min	Max
Gender	Control	51	0.451	0	0.503	0	1
1=male	Belgium	49	0.551	1	0.503	0	1
	Flanders	52	0.539	1	0.503	0	1
Left-Right scale	Control	50	7.020	7	1.824	3	11
11-point scale	Belgium	47	6.702	7	2.176	2	10
	Flanders	50	6.380	6	2.070	2	10
Flemish independence	Control	51	3.529	4	2.386	1	9
11-point scale	Belgium	49	3.061	2	2.585	1	9
	Flanders	52	3.173	2	2.463	1	11
Flemish autonomy	Control	51	6.137	6	1.625	1	9
11-point scale	Belgium	48	5.917	6	2.268	1	11
	Flanders	52	5.962	6	1.836	1	11

Table SI.3: Sample demographics by survey

	Survey	N	Mean	Median	Sd	Min	Max
Gender	1	152	0.513	1	0.502	0	1
1=male	2	127	0.504	1	0.502	0	1
	both	89	0.461	0	0.501	0	1
Left-Right scale	1	147	6.701	7	2.029	2	11
11-point scale	2	123	6.496	7	1.993	3	11
	both	86	6.628	7	2.000	3	10
Flemish independence	1	152	3.257	2	2.470	1	11
11-point scale	2	125	3.000	2	2.293	1	10
	both	88	3.011	2	2.342	1	10
Flemish autonomy	1	151	6.007	6	1.910	1	11
11-point scale	2	125	5.888	6	1.927	1	10
	both	88	5.955	6	1.952	1	10

Columns with survey denominator 'both' present variable summaries for participants whose responses could be matched between the two surveys.

Table SI.4: Sample demographics of matched participants before and after the attacks

	Survey	N	Mean	Median	Sd	Min	Max
Left-Right scale	1	87	6.667	7	2.038	2	10
11-point scale	2	86	6.628	7	2.000	3	10
Flemish independence	1	89	3.214	2	2.447	1	9
11-point scale	2	88	3.011	2	2.342	1	10
Flemish autonomy	1	89	6.090	6	1.762	1	11
11-point scale	2	88	5.955	6	1.953	1	10

Table SI.5: Mean jurisdictional connections for control and treatment groups by survey, matched sample

	(1) Control	(2) Belgium Prime	(3) Flanders Prime
Survey 1			
Municipality	6.265	5.931	5.846
	(2.327)	(2.685)	(2.664)
Province	4.353	4.345	4.500
	(1.631)	(2.539)	(2.454)
Flanders	5.029	4.724	5.154
	(2.110)	(2.914)	(2.664)
Belgium	5.588	7.103	5.962
	(1.971)	(2.410)	(2.537)
Europe	5.088	5.034	5.577
	(2.065)	(2.322)	(2.194)
Observations	34	29	26
Survey 2			
Municipality	6.618	6.034	5.462
	(2.310)	(2.612)	(2.731)
Province	4.676	5.034	4.731
	(2.358)	(2.485)	(2.585)
Flanders	5.088	4.931	4.923
	(2.109)	(2.520)	(2.331)
Belgium	6.088	6.862	6.231
	(2.261)	(1.959)	(2.338)
Europe	5.618	5.172	6.115
	(2.310)	(2.106)	(2.338)
Observations	34	29	26

Mean connections to five different jurisdictional entities (on 0-10 point scales), reported for Control, Belgium prime, and Flanders prime subjects who are matched across surveys; standard deviations in parentheses. The top panel reports subjects in the priming experiment. The bottom panel reports repeated observations of those subjects matched after the terror attacks.

Supplementary information D. Panel matching methodology

While ad hoc by necessity, the procedure for matching respondents across surveys was designed to be as robust and reliable as possible. All respondents in the second survey were asked whether they had taken part in the initial survey. Among those who responded in both surveys, general background characteristics such as age, gender, and course of study were sufficient to narrow sets of potential matches to at most five respondents, and in most cases considerably fewer. These were examined separately by each of the three authors for handwriting matches. Only those matches independently identified by all three authors are included. As an additional check of robustness, we also used the full set of responses (i.e. including second-survey responses which did not report participating in the first survey) and performed matching only on the basis of handwriting. The two methods returned the same matches, increasing our confidence in their validity.

Supplementary information E. Full regression results

Table SI.6: Do survey 1 priming effects persist in survey 2?

	(1) Municipality	(2) Province	(3) Flanders	(4) Belgium	(5) EU
	Control is reference				
Prime BE	−0.882	0.219	−0.156	0.348	−0.773
	(0.645)	(0.661)	(0.575)	(0.538)	(0.582)
Prime FL	−1.508**	−0.175	−0.056	−0.469	0.088
	(0.670)	(0.687)	(0.598)	(0.559)	(0.605)
Gender	0.190	0.101	0.032	0.518	0.048
1=male	(0.559)	(0.573)	(0.499)	(0.466)	(0.505)
Left-Right scale	−0.228	−0.191	0.090	−0.090	0.014
	(0.142)	(0.146)	(0.127)	(0.119)	(0.129)
FL independence	0.138	−0.010	0.167	−0.355***	−0.225*
	(0.130)	(0.133)	(0.116)	(0.108)	(0.117)
FL autonomy	0.133	0.130	0.294**	0.001	−0.157
	(0.160)	(0.164)	(0.142)	(0.133)	(0.144)
Constant	7.158***	5.321***	2.221**	7.939***	7.437***
	(1.164)	(1.192)	(1.038)	(0.970)	(1.050)
Observations	85	85	85	85	85
R^2	0.115	0.028	0.161	0.190	0.129

Ordinary least squares regression estimates for the connections to Flanders and Belgium reported in the second survey, only, for all participants matched between both surveys; $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table SI.7: Changes in connection to jurisdictional entities post-attack - fixed effects

	(1) Municipality	(2) Province	(3) Flanders	(4) Belgium	(5) EU
Post-attack	0.238	0.327	0.120	0.691*	0.441
	(0.391)	(0.496)	(0.351)	(0.380)	(0.332)
Left-Right scale	−0.360	−0.596	−0.266	0.449	−0.050
	(0.380)	(0.482)	(0.341)	(0.369)	(0.322)
FL independence	0.417	−0.093	0.106	0.002	0.395
	(0.334)	(0.424)	(0.300)	(0.324)	(0.284)
FL autonomy	0.079	−0.089	−0.020	0.018	−0.233
	(0.225)	(0.286)	(0.203)	(0.219)	(0.191)
Constant	7.181**	9.390**	6.608**	2.398	5.727**
	(3.280)	(4.162)	(2.950)	(3.186)	(2.784)
Observations	63	63	63	63	63
R^2	0.173	0.089	0.052	0.144	0.197

Fixed effect generalized linear regression estimates for the pre- to post-attack changes in reported connections to Flanders and Belgium for participants with matched observations between both surveys in the control group; $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table SI.8: Changes in connection to jurisdictional entities post-attack - random effects

	(1) Municipality	(2) Province	(3) Flanders	(4) Belgium	(5) EU
Post-attack × Control	0.407	0.405	0.247	0.694**	0.609*
	(0.347)	(0.438)	(0.326)	(0.314)	(0.320)
Post-attack × Prime BE	0.146	0.715	0.300	−0.298	0.022
	(0.360)	(0.456)	(0.338)	(0.325)	(0.332)
Post-attack × Prime FL	−0.294	0.303	−0.225	0.170	0.530
	(0.380)	(0.480)	(0.357)	(0.343)	(0.350)
	Control is reference				
Prime BE	−0.569	−0.042	−0.153	1.484***	−0.054
	(0.635)	(0.611)	(0.586)	(0.554)	(0.564)
Prime FL	−0.750	0.013	0.449	0.276	0.287
	(0.666)	(0.641)	(0.615)	(0.581)	(0.592)
Gender	0.519	−0.143	0.008	0.521	0.171
1=male	(0.509)	(0.461)	(0.469)	(0.442)	(0.450)
Left-Right scale	−0.167	−0.168	0.077	−0.036	−0.106
	(0.113)	(0.110)	(0.104)	(0.099)	(0.101)
FL independence	0.096	0.046	0.122	−0.216***	−0.108
	(0.090)	(0.093)	(0.084)	(0.080)	(0.081)
FL autonomy	0.160	0.093	0.289***	−0.041	−0.119
	(0.105)	(0.116)	(0.098)	(0.093)	(0.095)
Constant	6.080***	4.881***	2.198**	6.555***	6.860***
	(0.983)	(0.960)	(0.909)	(0.862)	(0.878)
Observations	172	172	172	172	172
R^2	0.080	0.031	0.185	0.184	0.100

Ordinary least square regression estimates for the connections to Flanders and Belgium reported in the second survey, only, for all participants matched between both surveys; $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table SI.9: Effect of perceived severity of the attack on connection to jurisdictional entities

	(1) Municipality	(2) Province	(3) Flanders	(4) Belgium	(5) EU
Perception of attack	1.125**	0.922**	0.895**	1.508***	0.396
1=severe	(0.492)	(0.454)	(0.411)	(0.373)	(0.408)
Gender	−0.085	0.022	0.266	0.446	0.039
1=male	(0.505)	(0.467)	(0.422)	(0.384)	(0.419)
Left-Right scale	−0.214*	−0.152	0.005	−0.100	0.067
	(0.127)	(0.117)	(0.106)	(0.096)	(0.105)
FL independence	0.067	−0.041	0.085	−0.363***	−0.303***
	(0.115)	(0.106)	(0.096)	(0.087)	(0.095)
FL autonomy	0.264*	0.179	0.416***	0.089	−0.064
	(0.139)	(0.128)	(0.116)	(0.106)	(0.115)
Constant	4.988***	4.406***	1.627*	6.559***	6.368***
	(1.024)	(0.947)	(0.856)	(0.778)	(0.849)
Observations	120	120	120	120	120
R^2	0.101	0.060	0.190	0.239	0.115

Ordinary least square estimates for the effects of median or above levels of severity of attack perceptions on the reported connections to the five different jurisdictional entities; standard errors in parentheses; $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table SI.10: Parting out the direction of effect: prior level of connection

	Perceived personal severity of attack									
	(1) Control	(2) Full sample	(3) Control	(4) Full sample	(5) Control	(6) Full sample	(7) Control	(8) Full sample	(9) Control	(10) Full sample
Connection in survey 1										
Municipality	0.140	0.012								
	(0.201)	(0.090)								
Province			-0.185	0.014						
			(0.218)	(0.099)						
Flanders					0.257	0.003				
					(0.191)	(0.095)				
Belgium							0.082	0.018		
							(0.184)	(0.100)		
EU									0.175	0.086
									(0.176)	(0.102)
Prime BE		0.839		0.831		0.804		0.831		0.836
		(0.524)		(0.520)		(0.541)		(0.520)		(0.517)
Prime FL		-0.333		-0.344		-0.348		-0.346		-0.362
		(0.560)		(0.553)		(0.554)		(0.557)		(0.551)
Gender	-1.620*	-0.580	-1.163	-0.563	-1.275	-0.577	-1.615+	-0.566	-1.271	-0.593
1=male	(0.948)	(0.477)	(0.812)	(0.468)	(0.809)	(0.471)	(0.827)	(0.468)	(0.798)	(0.467)
Left-Right scale	-0.257	-0.058	-0.214	-0.057	-0.213	-0.059	-0.299	-0.060	-0.167	-0.045
	(0.214)	(0.118)	(0.210)	(0.118)	(0.214)	(0.118)	(0.212)	(0.118)	(0.217)	(0.118)
FL independence	-0.268	-0.227**	-0.183	-0.226**	-0.213	-0.220**	-0.270	-0.224**	-0.211	-0.216**
	(0.211)	(0.108)	(0.190)	(0.107)	(0.192)	(0.109)	(0.192)	(0.107)	(0.188)	(0.106)
FL autonomy	0.529*	0.215	0.425	0.216	0.493*	0.216	0.455	0.213	0.512*	0.224
	(0.293)	(0.155)	(0.281)	(0.155)	(0.286)	(0.155)	(0.272)	(0.163)	(0.280)	(0.155)
Constant	6.359***	7.263***	7.992***	7.264***	6.430**	7.221***	6.763***	7.342***	5.544**	6.733***
	(2.256)	(1.195)	(2.218)	(1.168)	(2.518)	(1.243)	(1.947)	(1.030)	(2.522)	(1.255)
Observations	33	87	33	87	33	87	33	87	33	87
R^2	0.170	0.122	0.177	0.122	0.161	0.123	0.208	0.122	0.184	0.130

Ordinary least square estimates for the effect of reported connections to Flanders and Belgium in the first survey on the perceived severity of the attack (11-point scale); standard errors in parentheses; $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table SI.11: Parting out the direction of effect: personal severity of attack

	Δ Connection to Municipality		Δ Connection to Province		Δ Connection to Flanders		Δ Connection to Belgium		Δ Connection to EU	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Control	Full sample	Control	Full sample	Control	Full sample	Control	Full sample	Control	Full sample
Perceived personal	0.427**	0.274***	0.420	0.207	0.370*	0.243**	0.640***	0.247***	0.397**	0.090
severity of attack	(0.199)	(0.102)	(0.267)	(0.134)	(0.179)	(0.097)	(0.136)	(0.091)	(0.164)	(0.098)
Prime BE		−0.280		0.128		−0.104		−0.981**		−0.519
		(0.475)		(0.624)		(0.450)		(0.423)		(0.457)
Prime FL		−0.476		−0.199		−0.348		−0.262		−0.013
		(0.494)		(0.650)		(0.469)		(0.440)		(0.476)
Gender	−0.793	−0.656	0.524	0.536	−0.217	0.074	0.884	−0.074	0.195	−0.165
1=male	(0.784)	(0.411)	(1.049)	(0.540)	(0.706)	(0.390)	(0.536)	(0.366)	(0.644)	(0.395)
Left-Right scale	−0.164	−0.103	−0.144	−0.167	−0.132	−0.170*	0.190	−0.071	0.178	0.087
	(0.208)	(0.104)	(0.278)	(0.137)	(0.187)	(0.099)	(0.142)	(0.093)	(0.171)	(0.100)
FL independence	0.007	−0.042	−0.155	−0.106	−0.072	−0.075	−0.405***	−0.174**	−0.184	−0.118
	(0.165)	(0.095)	(0.221)	(0.125)	(0.149)	(0.090)	(0.113)	(0.085)	(0.136)	(0.091)
FL autonomy	0.290	0.314***	0.196	0.380**	0.099	0.204*	0.218	0.133	−0.249	−0.125
	(0.221)	(0.117)	(0.296)	(0.153)	(0.199)	(0.111)	(0.151)	(0.104)	(0.182)	(0.112)
Constant	−3.217	−2.508**	−2.617	−2.157	−1.980	−1.502	−5.793***	−0.976	−1.556	0.526
	(2.492)	(1.163)	(3.334)	(1.530)	(2.243)	(1.104)	(1.702)	(1.037)	(2.047)	(1.120)
Observations	30	85	30	85	30	85	30	85	30	85
R^2	0.298	0.202	0.147	0.121	0.212	0.138	0.620	0.185	0.319	0.095

Ordinary least square estimates for the effect of the severity of attack perceptions (11-point scale) on the *change* in reported connections to Flanders and Belgium between surveys one and two; standard errors in parentheses; $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table SI.12: Effect of perceived severity of the attack on security policy relevant preferences

	Use of police resources				Amount of police resources			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Perception of attack	0.336	0.388*	0.546***	0.531**	0.408**	0.471**	0.595***	0.604***
1=severe	(0.204)	(0.221)	(0.207)	(0.207)	(0.189)	(0.206)	(0.217)	(0.219)
Connection to EU		0.001				0.056		
		(0.054)				(0.051)		
Connection to Belgium		0.028				−0.019		
		(0.056)				(0.053)		
Connection to Flanders		−0.118*	−0.058	−0.056		−0.080	−0.026	−0.027
		(0.060)	(0.047)	(0.047)		(0.056)	(0.049)	(0.050)
Connection to Province		0.048				0.012		
		(0.054)				(0.050)		
Connection to Municipality		−0.030				0.004		
		(0.051)				(0.047)		
Difference in FL connection			0.024	0.093			−0.062	−0.102
			(0.061)	(0.094)			(0.064)	(0.099)
Perception × Difference FL				−0.114				0.066
				(0.117)				(0.124)
Gender	−0.088	−0.072	0.146	0.146	−0.198	−0.171	0.099	0.099
1=male	(0.210)	(0.211)	(0.200)	(0.200)	(0.194)	(0.197)	(0.209)	(0.210)
Left-Right scale	0.011	0.015	0.004	0.008	0.035	0.033	0.030	0.028
	(0.053)	(0.054)	(0.052)	(0.053)	(0.049)	(0.050)	(0.055)	(0.055)
FL independence	0.020	0.044	0.070	0.069	−0.090**	−0.073	−0.023	−0.023
	(0.048)	(0.053)	(0.048)	(0.048)	(0.044)	(0.050)	(0.050)	(0.050)
FL autonomy	0.006	0.052	0.014	0.010	0.143***	0.178***	0.093	0.096
	(0.058)	(0.061)	(0.059)	(0.060)	(0.053)	(0.058)	(0.062)	(0.063)
Constant	4.631***	4.572***	4.587***	4.621***	3.743***	3.568***	3.816***	3.796***
	(0.425)	(0.641)	(0.410)	(0.412)	(0.393)	(0.599)	(0.430)	(0.434)
hline Observations	120	120	85	85	120	120	85	85
R^2	0.032	0.082	0.138	0.148	0.116	0.141	0.131	0.134

Ordinary least square estimates; standard errors in parentheses; $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Supplementary information F. Estimating total connection changes for the primed groups

To interpret jurisdictional connections for the treatment groups across surveys, including possible effects of both priming and the attacks, we must account for the initial treatment assignment in the first survey. First survey control group participants reported a 'base' level of connection to the five jurisdictions. Participants in the primed groups, otherwise assumed not to differ significantly, were manipulated to report (sometimes) stronger connections. Connection differences between treated and control groups following the priming intervention but before the attack are given by coefficient estimates for the *uninteracted* prime terms in table 2 in the main text; these correspond to the differences among groups in table SI1. It is plausible that, between survey 1 and the attack, connection levels would have remained roughly constant for the control group, and dropped back to(ward) base levels for the treatment groups.

Note, however, that some connections (e.g. to Belgium) remain somewhat higher among the Belgian prime group than among control subjects even post-attack, though not statistically significantly. We remain agnostic as to whether any such persistence of priming effects exists, and if so whether it is a lasting artefact of our primes or if participants were instead simply reminded of their first-survey prime when they saw the same experimenters conducting the second survey. In the model presented in the even numbered columns of table 2 in the main text, between-survey connection differences for the control group are simply the interaction terms between the post-attack dummy and the indicator for assignment to the control group. (This is equivalent to the control-only results in the odd numbered columns.) For the primed groups the interactions also capture between-survey changes in connections, but these begin at (potentially) altered levels due to first-survey priming. 'Combined' effects for the primed groups would include any lingering influence of the primes plus the effect of the attack; these are obtained by summing the prime estimates in table 2 with their respective post-attack interactions.

Wald tests for divergences among the combined effects for the prime groups and the control yield no significant differences, suggesting that subjects from all groups reported similar connection levels in survey 2 and that prime effects were not significantly persistent. Still, combined effects in table 2 in the main text indicate that e.g. survey 2 Belgian connection was somewhat (though not significantly) stronger among subjects who saw the Belgian prime than in the control group. We cannot categorically exclude any persistent influence of the primes. Survey 2 results for treated groups should thus be interpreted with due caution, and we opt to control for survey 1 priming even in analyzing survey 2.

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