Did the Murder of Theo van Gogh Change Europeans' Immigration Policy Preferences?

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I. INTRODUCTION

In the morning of 2 November 2004, the controversial Dutch film-maker Theo van Gogh was shot to death by Mohammed Bouyeri. The murder was extremely brutal, as Bouyeri shot van Gogh eight times before almost decapitating him with a knife. Bouyeri, a member of the radical Islamist group, the Hofstad Network, left a five-page note on van Gogh's body, threatening Western governments. Bouyeri was arrested shortly after the killing. The killing was met with anger in the Netherlands, causing a proliferation of violent attacks on mosques and Islamist schools, and even a number of attacks on Christian churches. Reactions were not restricted to the Netherlands, as the debate on the integration of (Muslim) immigrants hotted up across Europe: Danish prime minister Anders Fogh Rasmussen labelled the murder as an attack on Western society as such; the British newspaper, *The Independent*, wrote that the worsening of the relationship between immigrants and the native population in the Netherlands promises little hope for the rest of the continent; the *Frankfurter Allgemeine Zeitung* wrote that militant Islamism is dangerous because 'many societies on this continent have elevated their defenselessness into a virtue'; while *The Economist* warned that the van Gogh killing might make the tolerant intolerant.

  In this paper we analyze how the murder affected immigration policy preferences. Alternatively, how did the tensions and the negatively framed public debate in the aftermath of the murder impact on preferences toward immigration policies? Data for the second round of the European Social Survey (ESS) was collected in several countries at the time of the murder. This gives us an excellent opportunity to explore this question from a causal angle:

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a large number of individuals were interviewed right before the murder took place, and a large number were interviewed right after the murder took place. There is no reason to believe that those interviewed right before the murder should differ systematically from those interviewed right after the murder, which is nonetheless something we can explore empirically. This means that the murder can be viewed as a natural experiment of how an exogenous shock to the saliency of the immigration issue impacts on policy preferences.

Our study is important for several literatures in the social sciences. Most obviously, our results are of interest to the literature on the determinants of immigration policy preferences. Recent research argues that immigration policy preferences are strongly driven by cultural factors rather than egotropic economic considerations (Burns and Gimpel 2000, Citrin et al. 1997, Dustmann and Preston 2007; see Carlsson and Johansson-Stenman 2010 for a similar finding for vote choice). This literature typically relies on survey data where it is difficult to separate the two factors and/or to claim causality. Hainmueller and Hiscox (2010), however, conduct a survey experiment where respondents are randomly assigned to different survey items in a manner which allows them to separate different economic theories of immigration policy preferences, while holding cultural concerns constant. Their results support claims that economic self-interest is not important for immigration policy preferences. In some respects, our study is the flip side of theirs, as the exogenous variation in our design is the saliency of the cultural dimension. Thus, our results show how shocks to the saliency of cultural factors impact on preferences, holding egotropic economic considerations constant.

We are not aware of any studies of immigration policy preferences with a similar research design as ours. There are, however, some studies of how behavior and attitudes toward immigrants have been affected by terror attacks. Åslund and Rooth (2005) present aggregate trends in views on immigrants which show that negative views on immigrants, but not labour market discrimination, increased in Sweden after the September 11 attack, while Gautier et al. (2009) find that the Theo van Gogh murder had a negative effect on housing prices in areas of Amsterdam with a high percentage of Muslim inhabitants. Echebarria-Echabe and Fernández-Guede (2006) show that Spanish respondents interviewed right after the Madrid terror attack expressed more anti-Arab and conservative attitudes than respondents interviewed right before the attack. The latter study is most similar to ours, but we have a larger number of respondents and examine differences in policy preference on a specific area (immigration) rather than attitudes toward immigrants and general political orientation. Moreover, the van Gogh murder was not a blind attack on random citizens, which presumably makes it less likely that the attack has any effect on prejudice or preferences, but also implies that the mechanism driving any findings is more likely to be the public debate rather than personal fear of being a victim of a terrorist attack.

More generally, our study relates to the debate of the consequences of issue saliency, agenda setting and priming for political behavior. In political science, the importance of saliency and agenda setting is emphasized mainly in studies of voting behavior. Saliency is assumed to increase knowledge of the existence of a particular issue, affect the weight put on this issue when deciding whom to vote for, and improve knowledge of party positions on the particular issue. It has been shown, however, that the concept of issue saliency can fruitfully be expanded beyond voting, and seen to have an impact not only on the weight put on the particular issue when forming their general political orientation, but also on issue knowledge and the strength and direction of opinions (e.g. Weaver 1991, Weaver et al. 2004, DellaVigna and Kaplan 2007, Jakobsson and Kotsadam 2009, Löfgren and Nordblom 2010, Berger 2010). The literature suggests that media effects on public opinion ('priming') are more likely when issues are 'concrete' rather than 'abstract' and when issues involve dramatic events (see Soroko 2002). The media coverage of the van Gogh murder and the following events had a clear framing, defined by Entman (1993: 52) as 'to select some aspects of a perceived reality and make them more salient […] in such a way as to promote a particular problem definition […]'. Thus, it seems plausible that the murder and the following events had an effect on immigration policy preferences, in so far as non-economic concerns are an important part of individual's immigration policy preference formation.

The rest of the paper is organized as follows. The next section discusses data and research design, section three presents the empirical results, while section four concludes.

II. DATA AND RESEARCH DESIGN

Seventeen countries conducted data collection for the second round of the European Social Survey around the day van Gogh was murdered (2 November 2004), but we exclude Portugal since only 32 Portuguese respondents were interviewed in October. We exclude respondents interviewed on the day of the murder because we do not know whether these respondents knew of the murder at the time of the interview. We analyze a time window of 27 days before and 27 days after the murder (i.e. from 6 October to 30 November), which gives us a sample of 19 559 respondents, 10 376 interviewed before the murder and 9 183 after the murder. The distribution of respondents across months and countries is shown in Appendix Table A1.[1]

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1. In six countries a disproportionate share of respondents were interviewed in one of these months, which is potentially problematic because the 'control' and 'treatment' groups are dominated by respondents from one specific country. However, the results (available upon request) do not change much if we exclude these countries and the conclusions remain the same.

The dependent variable, *immigration policy preference*, consists of two survey items: 'Allow many/few immigrants of different race/ethnic group of majority' and 'Allow many/few immigrants of poorer countries outside Europe'. The answer categories are 'allow many to come and live here', 'allow some', 'allow a few', and 'allow none'. The two items are strongly correlated (Pearson's $R = .78$) and we combine the two items as an additive index constrained between zero and one, where a high score implies support for a restrictive immigration policy.

To assess the effect of the murder we construct a variable to represent the 'control' and the 'treatment' related to the van Gogh murder. A natural approach is to assign respondents to a control group and a treatment group based on whether they are interviewed closely before or closely after 2 November 2004. We thus construct a variable, *Treatment group*, which equals 1 if the respondent was interviewed up to roughly a month (27 days) after the murder and zero if the respondent was interviewed up to roughly a month (27 days) before the murder. Using this approach and comparing means on the immigration policy preference variable before and after the murder we get the average causal effect of the murder and the following debate. This approach has the advantage of being reliant on neither the constant effects assumption nor a correct model specification, and gives the average causal effect. However, this approach on its own does not allow us to distinguish between any potential mechanisms for the effects as it only answers the question: 'Was there an average effect in the month after the murder?'

In order to discriminate between mechanisms we also use a regression-discontinuity design, i.e. we include as an additional control a variable, *days*, referring to the number of days before or after the murder the respondent was interviewed, to explore whether there is a sudden break in the mean score on immigration policy preference around the time of the murder. This approach investigates the instant effect and answers the question: 'Was there an immediate effect of the murder?' By contrasting the two approaches, any differences in results can plausibly be attributed to the impact of the subsequent uproar and public debate.

III. EMPIRICAL RESULTS

Given balanced distributions on confounding variables, the difference in means between the two groups – those interviewed before the murder (control group) and those interviewed after the murder (treatment group) – would be the average causal effect of the murder. The raw difference in means is very small, .496 versus .504, a difference equal to approximately three percent of the standard deviation of the dependent variable, yet the difference is in the

Table 1

OLS Estimates of the Effect of the Theo van Gogh Murder on Immigration Policy Preferences.

	(1)	(2)	(3)	(4)	(5)
	Base	Strictly exogenous controls	Full set of controls	Country fixed effects	Regression discontinuity design
Treatment group	0.008*	0.011***	0.011***	0.011***	0.003
	(0.004)	(0.004)	(0.004)	(0.004)	(0.008)
Days					0.0003
					(0.0003)
Age/10		0.010*	0.041***	0.036***	0.011***
		(0.006)	(0.006)	(0.006)	(0.004)
Age/10 squared		0.002***	− 0.002***	− 0.002***	0.036***
		(0.001)	(0.001)	(0.001)	(0.006)
Male		0.005	0.009**	0.011***	− 0.001**
		(0.004)	(0.004)	(0.004)	(0.001)
Education			− 0.013***	− 0.015***	− 0.015***
			(0.001)	(0.001)	(0.001)
Paid work			− 0.016***	− 0.005	− 0.005
			(0.005)	(0.005)	(0.005)
Minority			− 0.009	− 0.044***	− 0.044***
			(0.011)	(0.011)	(0.011)
Constant	0.497***	0.397***	0.513***		
	(0.003)	(0.012)	(0.014)		
Country FE	No	No	No	Yes	Yes
Observations	18708	18568	18209	18209	18209
R-squared	≈ 0	0.04	0.07	0.15	0.15

Note: ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. Robust standard errors in parentheses. The dependent variable is support for restrictive immigration policy. The treatment group consists of those interviewed after the murder. The variable days refers to the number of days since the murder, centered at the day after of the murder, i.e. the variable is negative for those interviewed before the murder and positive for those interviewed after the murder.

expected direction as the support for a restrictive immigration policy is higher in the treatment group. The t-value is 1.87 (p = 0.06, two-way test) (Table 1, Column 1).

It turns out, however, that we cannot consider this difference an unbiased estimate of the average causal effect since the assumption of balanced distributions on confounding variables is not met. We examined mean differences between the treatment and control groups on several potentially important, strictly exogenous or reasonably exogenous variables, and we found that respondents in the treatment group have on average higher education and are younger, more likely to be in paid work, and less likely to report that they belong to an ethnic minority. We were surprised to find these differences, which suggest that more interviews took place in relatively disadvantaged areas in October than in November. Given that the young and those with high education are more supportive of liberal immigration

policies (e.g. Hainmueller and Hiscox 2007), it is reasonable to expect that the raw difference in means is a downward-biased estimate of the causal effect. In column 2 we control for age, age squared, and gender. Although the treatment and control groups do not differ in the share of male respondents we include it since it is clearly exogenous and as such may increase the precision of the estimates. The coefficients point in the expected direction, and the estimate of the difference between those interviewed before and after the murder increases (the difference is significant at the five percent level). In column 3 we also include indicator variables for having paid work, years of education, and an indicator variable for reporting minority status, as control variables. The first two of these variables are choice variables and as such they may be correlated with omitted confounding variables, and *reporting* minority status may be affected by the murder or the media debate. For that reason we introduce the variables in this stepwise fashion. The coefficients for these variables point in the expected direction and our treatment effect is clearly robust to their inclusion.

The cross-national variation in support for a restrictive immigration policy is large, and since treatment and control groups are not equally large in each country, the cross-national variation may pollute the identification strategy. In column 4, we allow each country to have its own intercept, i.e. we include country fixed effects, to account for country-specific differences in level of support for a restrictive immigration policy. We also include a dummy for East Germany since there is a disproportional share of East Germans in the control group, and East Germans are found to differ significantly from West Germans in their political preferences and worldviews (e.g. Alesina and Fuchs-Schündeln 2007). The country-specific intercepts are as expected strongly significant, but the estimated difference between control and treatment group is robust to the inclusion of the country FE.[2] The size of the treatment effect is very small compared to the standard deviation on the dependent variable (four percent of the standard deviation). In substantive terms, the treatment effect is much smaller than the differences according to age, education, and minority status, however, it is of the same size as the gender gap in immigration policy preferences. As a sensitivity check, we ran this model seventeen times excluding one country at the time in rotating fashion to see if results are driven by a particular country. The smallest treatment effect is found when excluding Spain (beta = 0.009, t = 2.28), the largest when excluding Germany (beta = 0.013, t = 3.20). Thus, the results in Table 1 are not strongly driven by a single country.

2. We also did the same analysis correcting standard errors for within-country clustering (results available upon request). Doing this slightly increases the standard error of the treatment coefficient but it is still strongly significant at the one percent level. We decide to report unadjusted standard errors due to the unknown properties of clustered standard errors when the number of clusters is small (e.g. Angrist and Pischke 2009).

We also estimated the model using ordered probit instead of OLS, since the dependent variable might be considered as categorical and not continuous. The estimated treatment effect is positive and strongly significant. Finally, we relied on Blackwell et al.'s (2009) coarsened exact matching method to improve the estimate of the treatment effect. We matched on the covariates in Table 1 within each country, and then estimated the treatment effect in the pooled sample. After matching, the treatment effect increases slightly (beta = 0.013, standard error = 0.005, t = 2.84).

We conduct a number of falsification tests in order to strengthen the validity arguments of our design. These are presented in detail in Appendix B, and suggest that the findings reported in Table 1 are not driven by a pre-trend, or by seasonal patterns (for instance, seasonal variations in the labour market, which again might influence immigration policy preferences), nor do they reflect a general shift in political sentiments from October to November. We cannot rule out, however, that something else happened during the period that affected Europeans' attitudes toward immigration policy, but we deem that possibility as highly unlikely.

Figure 1 shows the respondents' mean score on the immigration policy variable *by days* in the period around the murder; thus, the figure visualises how the responses vary before and after the murder. The means are adjusted for country specific differences by centering the respondents' scores on the country mean. The figure includes two regression lines, one estimated for those interviewed before the murder (negative values on days) and one estimated for those interviewed after the murder (positive values on days). The figure illustrates the findings reported in Table 1: the mean is slightly higher for the post-murder period, i.e. support for restrictive immigration policies is higher after the murder than before.

Next we estimate the immediate causal effect of the van Gogh murder using a regression discontinuity (RD) design. RD is useful to estimate causal effects when the mechanism of assigning respondents to the treatment group is known and assignment to the treatment group varies discontinuously with an observable variable (see e.g. Angrist and Pischke 2009 for an introduction to RD). In our case, assignment to the treatment group varies discontinuously with the day of the interview. Using the number of days before/after the murder the respondent was interviewed as a control variable, we compare respondents interviewed just before and just after the murder, and identify the causal effect of the murder under the assumption that those interviewed just before the reform are virtually identical to those interviewed just after the reform. Column 5 in Table 1 presents the results. The coefficient is close to zero and the standard error is also much larger, resulting in a statistically insignificant effect of the murder. This implies that, although the mean score on the immigration variable is higher in the month after the murder compared to the month before

Figure 1

Mean scores on support for restrictive immigration policy by days. Note: The X-axis refers to the date of the interview where 0 refers to the day after the murder, 27 refers to 28 days after the murder, and −27 refers to 27 days before the murder. The Y-axis is the average of the country-specific mean scores on support for restrictive immigration policy. The mean scores are adjusted for country-specific differences by subtracting the country mean. The dots in the figure are the average of the country-specific mean scores by days, i.e. the figure displays how policy preference changes over the period we study. The lines are OLS regression lines where the line to the left is estimated on the data before the murder, while the line to the right is estimated on the data after the murder.
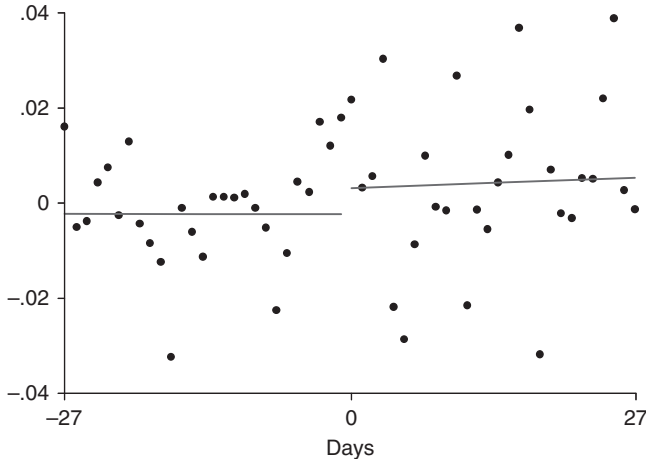
the murder, we cannot claim that the difference is due to an immediate change in preferences due to the assassination.

As discussed in the previous section, and as the RD results suggest, it might be misleading to only consider the immediate effect of the murder since the murder started a chain of events and a heated public debate that perhaps should be considered as the 'treatment'. If so, the 'amount of treatment' varies within the treatment group. Figure 1 suggests that this might be the case, as the highest daily averages (i.e. those wanting the most restrictive immigration policies) are found in the treatment group, and the post-murder slope is slightly positive – though the slope is far from steep enough to reach statistical significance. Nonetheless, the difference between the two ways of estimating the causal effect of the murder clearly point in the direction of an effect of the public and media debate following the murder.

If we consider the public debate as the treatment, the amount of treatment is not likely to vary only within the treatment group. The relative attention given to the murder and the media framing of the event might also vary across countries. Thus, restricting the treatment effect to be the same across countries might be unreasonable. In Table 2 we present the results from an analysis where

Table 2

OLS Estimates of the Effect of the Theo van Gogh Murder on Immigration Policy Preferences.

Spain	0.043**
	(0.018)
Slovakia	0.036**
	(0.018)
Norway	0.031**
	(0.016)
Luxembourg	0.025
	(0.019)
Great Britain	0.022
	(0.018)
Czech Republic	0.019
	(0.013)
Finland	0.013
	(0.012)
Belgium	0.013
	(0.017)
Poland	0.011
	(0.013)
Sweden	0.009
	(0.013)
Slovenia	0.009
	(0.015)
Switzerland	0.002
	(0.014)
Germany	− 0.011
	(0.018)
The Netherlands	− 0.011
	(0.019)
Estonia	− 0.027
	(0.019)
Denmark	− 0.030
	(0.019)
Controls	Yes
Country FE	Yes
Observations	18208
R-squared	0.15

Note: ***$p<0.01$, **$p<0.05$, *$p<0.1$. Robust standard errors in parentheses. The dependent variable is support for restrictive immigration policy. The country-specific coefficients are estimates of the country-specific treatment effect and are the sum of the coefficient for the treatment group and the interaction between the treatment group and the respective country dummy. The same control variables as in Table 1, column 4, are included.

we include a full set of interactions between the treatment group and the country indicators. Thus, we allow the treatment effect to vary across countries, but restrict the coefficients of the control variables to be the same across countries. We find this approach, which is similar to that of multilevel modelling, to be a reasonable compromise between restricting all coefficients to be the same across countries (as we do in Table 1) and allowing all coefficients to differ between countries (as we do if we estimate separate regressions for each

country) since we allow full flexibility regarding the treatment effect, while we rely on the large sample size to get efficient estimates of the control variables. In Table 2 we present the treatment effect for each country and the respective significance test of whether the effect is different from zero.[3] The coefficients for the control variables are similar to those in Table 1 and are left out for ease of presentation. This exercise reveals a statistically significant treatment effect in only three countries: Spain, Slovakia and Norway. If we compare the country-specific estimates with the treatment coefficient from the pooled analysis, one might argue that the country-specific coefficients can be considered as random draws from a normal distribution around the pooled estimate; however, the country ranking based on the size of the treatment coefficient makes us less inclined to believe in this interpretation. In particular, we find no treatment effect in the Netherlands;[4] this interesting finding suggests that mechanisms affecting policy preferences are not necessarily similar in different policy or political contexts, and that effects of media issue framing and agenda-setting on policy preferences might be overstated in the literature. The absence of a significant effect in the Netherlands might also reflect that public opinion on immigration and integration issues were already strongly mobilized before the murder (e.g. Penninx 2006), perhaps more so in the Netherlands than in other European countries, thus leaving less room for change in preferences. It could also be the case that Dutchmen to a lesser extent tied the murder to issues of immigration since they had more information on the prior controversy surrounding Theo van Gogh.

IV. CONCLUSION

In this paper we use data from the European Social Survey (ESS) to investigate if the murder of Theo van Gogh and the subsequent debate on the integration of (Muslim) immigrants affected immigration policy preferences. A large number of individuals were interviewed right before the murder took place, and a large number were interviewed right after the murder took place. We view the murder as a natural experiment of how an exogenous shock to the saliency of the immigration issue impacts on immigration policy preferences.

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3. Remember, we estimate a model with an indicator for whether the respondent belongs to the treatment group, a full set of country indicators, and a full set of interactions between the treatment indicator and the country indicators. The country-specific treatment effect is the sum of the coefficient for the treatment indicator and the respective interaction term. In Table 2 we present the sum of these coefficients and a significance test of whether the sum of the coefficients is different from zero.
4. We examined whether the Dutch sample is particularly imbalanced on the relevant covariates. This is not the case, but relatively few Dutch respondents were interviewed after the murder (see Table A1). We also examined whether polarization in preferences changed as a consequence of the murder, but we find no evidence of increased polarization after the murder.

We find that, on average across countries, respondents interviewed after the murder were more positive toward restrictive immigration policies than respondents interviewed before the murder. The treatment effect is very small compared to the standard deviation on the dependent variable, and clearly smaller than differences according to age, education, and minority status, however, the estimated effect is of the same size as the gender gap in immigration policy preferences. This result is not driven by a seasonal pattern and attitudes toward other issues did not change during this period. We find, however, that there was no immediate change in Europeans' immigration policy preferences, and therefore suggest that the treatment effect is driven by the public debate after the murder. However, although the treatment effect is robust and significant on average across countries, the average affect appears to mask a substantive cross-national heterogeneity in the treatment effect. When we allow the treatment effect to vary across countries, we find that the treatment effect is significant in only three countries (Spain, Slovakia, and Norway), however, the country-specific treatment effects appear to be centered around the treatment effect found in the pooled analysis. The interesting absence of a statistically significant effect in the Netherlands seriously questions whether the treatment effect is driven by the public debate after the murder. It could be that the mechanisms affecting policy preferences are not necessarily the same in different policy or political contexts. It might also be the case that the Dutchmen to a stronger degree than other Europeans were already mobilized on the immigration issue at the time of the murder, thus leaving less room for change, or it could be that more information on the prior controversy of Theo van Gogh among Dutchmen somehow muted the response. We urge future studies to examine similar events with a similar research design in order to reach a better understanding of the mechanisms behind attitudinal change, in relation to both immigration policies and other policy areas.

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APPENDIX A

Table A1

Number of Respondents Per Country and Month of Interview.

Country	November	October
Belgium BE	683	406
Switzerland CH	540	555
The Czech Rep. CZ	766	1,354
Germany DE	453	583
Denmark DK	865	222
Estonia EE	547	370
Spain ES	561	457
Finland FI	576	860
Great Britain GB	320	931
Luxembourg LU	399	540
The Netherlands NL	252	620
Norway NO	364	641
Poland PL	728	927
Sweden SE	637	784
Slovenia SI	810	488
Slovakia SK	682	638
Total	9,183	10,376

APPENDIX B: FALSIFICATION ANALYSIS

Table A2 shows three falsification regressions. In columns 1 and 2 we pretend that the murder happened one and two years before it actually did happen (again using the one-month window in creating the control and treatment group). If our results are driven by a pre-trend or by seasonal patterns, we would expect these regressions to yield statistically significant 'treatment effects' as well. While the coefficients for the control variables remain roughly the same, the treatment effects are not different from zero and their point estimates even alternate in signs. Unfortunately, the survey was not distributed in October or November in 2005 or 2006 so the same exercise cannot be done to investigate possible post trends in seasonal effects. Finally, in order to see if something else happened at the same time as the murder and to further strengthen the causal interpretation of our results we ran a number of regressions with attitudes toward education, trust in political parties and the importance of the environment for our treatment effects dummy. The results for the importance of the environment are presented in column 3 of Table A2 and we see that the treatment effect is clearly insignificant. The results for the other variables (available upon request) are equally insignificant.

Table A2

Falsification Tests with Placebo Regressions Pretending the Murder Happened One or Two Years Before and a Placebo Variable that Should not be Affected by the Murder (Environmental Attitudes).

	(1)	(2)	(3)
	2002	2003	Environment
Treatment group	0.003	− 0.019	0.014
	(0.004)	(0.015)	(0.015)
Age	0.04***	0.084***	− 0.224***
	(0.006)	(0.024)	(0.022)
Age squared	− 0.0003***	− 0.001**	0.013***
	(0.0001)	(0.0002)	(0.002)
Male	0.018***	− 0.024*	0.060***
	(0.004)	(0.015)	(0.014)
Education	− 0.015***	− 0.016***	− 0.019***
	(0.001)	(0.002)	(0.002)
Paid work	− 0.005	0.008	0.043**
	(0.005)	(0.018)	(0.018)
Minority	− 0.065***	− 0.056	0.033
	(0.011)	(0.045)	(0.039)
Country FE	Yes	Yes	Yes
Observations	16250	1018	19457
R-squared	0.20	0.16	0.06

Note: ***p<0.01, **p<0.05, *p<0.1. Robust standard errors in parentheses. The dependent variable in column 1 and 2 is support for restrictive immigration policy. The dependent variable in column 3 is whether the respondent disagrees that it is important to care for nature and environment. The treatment group consists of those interviewed in November.

SUMMARY

To what degree are preferences determined by fundamental and stable value orientations, or are they vulnerable to exogenous shocks to issue saliency? We exploit that the second round of the European Social Survey was conducted around the time when Mohammed Bouyeri murdered Theo van Gogh on 2 November 2004. The murder was covered extensively across Europe and led to a debate about the impact of mass immigration. We consider the murder as a natural experiment which allows us to explore how a shock to issue saliency affects immigration policy preferences. We compare preferences of those interviewed right before the murder (control group) with those interviewed right after the murder (treatment group). We find robust evidence of a significant treatment effect in a pooled analysis with country fixed effects. However, when we allow the treatment effect to vary across countries, we find evidence of more support for restrictive policy in only three countries (Norway, Spain, and Slovakia).