The legacy of 1968 student protests on political preferences

The Legacy of 1968 Student Protests on Political Preferences

Guglielmo Barone [a], Guido de Blasio [b], Samuele Poy [c]

[a] Department of Economics, University of Bologna, Piazza Scaravilli 2, 40126 Bologna (Italy). Email: g.barone@unibo.it

[b] Structural Economic Analysis Directorate, Bank of Italy, Via Nazionale 91, 00184 Rome (Italy). Email: guido.deblasio@bancaditalia.it

[c] Corresponding author. Department of Economics and Business Studies, University of Piemonte Orientale, Via Perrone 18, 28100 Novara (Italy). Email: samuele.poy@uniupo.it

Abstract

This paper shows that people who lived their impressionable years (18-25) at the time of Student Movement matured, when adult, political preferences more in favor of right-wing instances, plausibly as a reversal reaction. No effect is detected on polarization.

Keywords: mass student protests, political orientations.

JEL classification: D72, Z10.

This research did not receive any specific grant from funding agencies in the public, commercial, or not-for-profit sectors.

Declarations of interest: none.

1. Introduction

The Student Movement (the Movement henceforth) is one of the most important cultural movements of the second part of the 1900s. It originated in 1964 on the Berkeley campus; then it spread out in many Western countries, showing its climax in 1968 and a long continuation up to the late seventies. It was featured by a strong leftist revolutionary strain, as well as riots and rebellion against authority and in favor of a pacifist, more inclusive and equal society. Over time, these ideals slowly gave way to less edifying aspects like the use of drugs and political violence.

In this paper, we explore the long-run effect of early-life exposure to the Movement on political preferences. At first glance, one might expect long-lasting leftist preferences. However, other mechanisms pushing in the opposite direction may be at work. For example, drugs and the political violence may have fostered a desire for law and order, typical right-wing identity issues; the egalitarian aspirations of the protesters may have accentuated fears of a too large role for the state; the lowering of meritocratic criteria for passing university exams (Maurin and McNally, 2008) may have triggered fears on the quality of the educational system. Moreover, despite the large media coverage, the Movement affected a minority share of young people and was concentrated primarily among those attending universities. The majority of those who did not attend universities, due to their lower social background, might have been mostly frightened by the claims of the Movement, particularly the more extreme ones. In the end, the persistence of leftist preferences vis a vis its reversal is an empirical issue. Identification is based on the "impressionable years" hypothesis, which is defined in social psychology as the ages of 18 to 25 during which experiences leave a lasting mark on an individual's opinions and attitudes (Giuliano and Spilimbergo, 2014).

Our investigation speaks to the large literature on long run determinants of cultural traits (Alesina and Fuchs-Schündeln, 2007, Grosfeld et al., 2013). Some notable works rely on the impressionable year assumption, analyzing, among others, the effect of recessions on attitudes toward redistribution and job preferences (Giuliano and Spilimbergo, 2014; Cotofan et al., 2021; Carreri and Teso, 2021), and the impact of income inequality on fairness views (Roth and Wohlfart, 2018). Our contribution is the first one that examines the long run effect of 1968 student protest in Western countries, so adding to our understanding of the consequences of ideologies (Bérnabou, 2008).

2. Data and empirical model

Our focus is on advanced-economy countries with democratic institutions that experienced mass student protests in the years around 1968. We use data from the World Bank database to preliminary identify the top 25% countries according to their per capita GDP in the 1965 while the selection of countries that are also deemed as democracies is based on the Polity project indicator.[1]

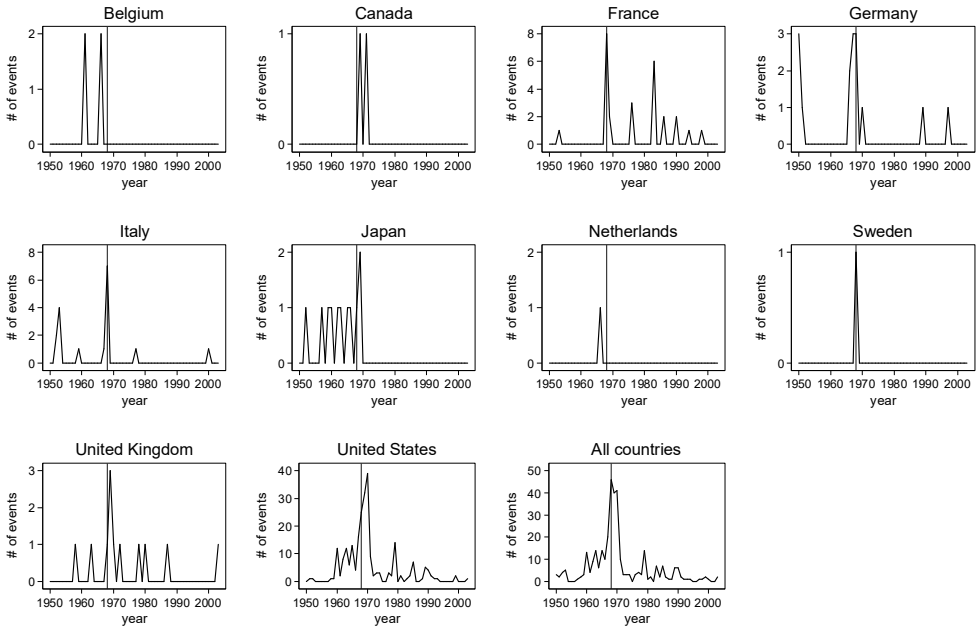
To identify the student protest episodes, we use data from The Social, Political and Economic Event Database Project (SPEED, https://clinecenter.illinois.edu/project/human-loop-event-data-projects/SPEED) that traces student protest episodes from 1950 on, based on a rich archive

[1] This indicator ranges from -10 to +10 (fully democratic). We keep countries that have an index greater than zero in all years since 1964.

of digitalized news taken from newspapers, and broadcasts. Historians agree that the first warnings were from Berkeley in 1964, while the duration of the Movement extended throughout the following decade. We include in the analysis only countries with at least one mass student demonstration in the 1964-1972 period (a 4-year window centered in 1968) to control for country-level unobserved heterogeneity leading to student protests. Data on individual-level on various dimensions of political preferences are obtained pooling the World Values Survey (WWS) and the European Values Survey (EVS).[2]

We end up with the following 10 countries: Belgium, Canada, France, Germany, Italy, Japan, Netherlands, Sweden, United Kingdom and United States.[3] Figure 1 depicts the total number of mass student protests recorded from 1950 onwards for selected countries. Overall, there is significant growth in the years preceding the 1968 peak; student protests remain high in the following years with a spike in 1979 (Figure 1, panel "All countries").

Figure 1. Mass student protest events



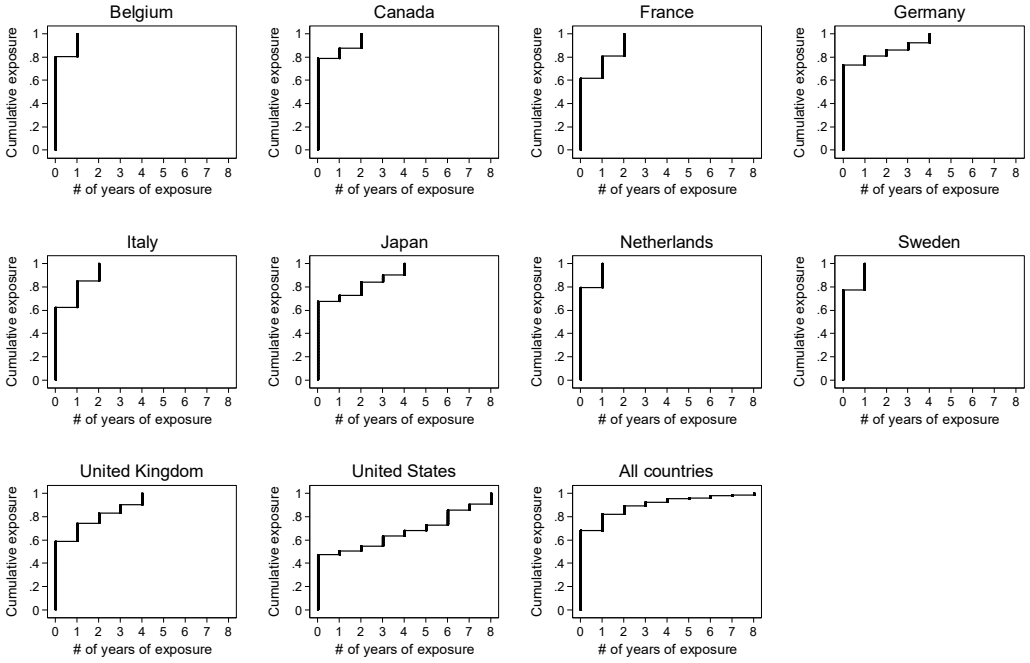
Note: The vertical line identifies 1968.

We operationalize the impressionable years hypothesis as follows: for an individual located in a given country, we count the number of years she/he, in her/his 18-25, spent with at least one mass demonstration of students. By definition, exposure ranges from 0 to 8. Protests are those recorded between 1964 and 1979.[4] Only individuals aged 25 or older at the time of the interview

[2] The online Appendix shows a more detailed description of the variables (Table A1) and the descriptive statistics (Table A2).
[3] As to Germany, we use only data on individuals living in the West part to be consistent over time.
[4] The WVS/EVS has no information on the country where individuals lived when aged 18-25. We assume that it coincides with that identified at the time of the interview.

are included; people whose 18th year is before 1950 are dropped to avoid impressionable years in the aftermath of WWII. Eventually, the sample consists of all those born between 1932 and 1994. Figure 2 shows the cumulative distribution of exposure. On average, there is a large mass of probability on zero (68%), while an exposure equal to 1 or 2 years accounts for further 21% of the cases (the average is 0.8). Only few countries exhibit a distribution with non-zero weights for all possible values of exposed years.

Figure 2. cumulative distribution of exposure



The identification strategy hinges on cross-country variation in individual experiences during the impressionable years, while controlling for idiosyncratic shocks at the country, year of the interview, age, and cohort level. The main outcome of interest is the self-declared left-right positioning on the political spectrum on a range from 1 (left) to 10 (right). The estimating equation reads as:

$$Leftright_{ictabs} = \beta_0 + \beta_1 Exposure\ to\ 1968_i + \beta_2 X_i + \tau_c + \rho_t + \varsigma_a + \sigma_b +$$

$$+\omega_s + \varphi_c * age + \varepsilon_{ictabs}$$

where $Leftright_{ict}$ is the Left-Right political positioning of the individual *i*, living in country *c*, surveyed in wave *t*, whose age at the survey is *a*, born in year *b*, interviewed in survey of type *s* (s = WWS/EVS). β_1 is the coefficient of interest. X_i is a vector of individual level controls including in the baseline regression only gender (that can safely be assumed to be predetermined), whereas in a robustness check we also control for marital status, professional

status, religion, income. We also control for country fixed effects (τ_c), wave fixed effects (ρ_t), age of the individual at the survey fixed effects (ς_a), and year of birth of the individual – i.e. cohort – fixed effects (σ_b), a dummy (ω_s) for the type of the survey (WVS/EVS), and country-specific age trends ($\varphi_c * age$). ε_{ictabs} is the error term. Finally, we use Giuliano and Spilimbergo (2014)'s data to double check that in our sample there is no early exposure to deep recession that could have biased the estimates.

3. Results

Treated individuals show political preferences more in favor of right-wing instances (Table 1, Column 1 – the coefficient is statistically significant at 1%). Increasing by one standard deviation the number of exposed years during impressionable years (1.6 years) entails an increase in the dependent variable equal to 2.4% of its standard deviation. In Column 2 we discretize exposure because it has a strong mass of probability on zero (see Figure 2). The coefficient remains significant (at 10%) and the size of the estimated effect equals 3.4% of the standard deviation of the dependent variable, a value largely comparable to that Giuliano and Spilimbergo (2014) find for the effect of recessions on left-wing ideology (3.6%).

The following columns present other robustness checks. In Column 3, the treatment is based on exposure to the restricted 1964-1972 time span (the most salient years of the Movement). The estimated coefficient is significant (at 10%) and slightly smaller, probably because of the treatment is measured with some error. In Column 4, we follow the field literature and add a number of covariates that might capture part of the effect going from the treatment to the outcome: being in a relationship, professional condition (employed, retired, student, unemployed, other), religion (none, Roman Catholic, Protestant, Muslim, Orthodox, other), income level (low, medium, high). Nonetheless, the estimated effect is in the ballpark of previous ones.[5] Column 5 studies whether our key coefficient is capturing something that has to do with other types of demonstrations, potentially correlated to student protests. We redefine the dependent variable as the number of impressionable years exposed to other protests (workers, social groups, political groups, etc.) with the exclusion of student protests: nicely, we find no effect (the size is about 1/3 relative to Column 1 and there is no statistical significance).

Table 1. The impact of 1968 on Left-Right positioning

	(1)	(2)	(3)	(4)	(5)
	Baseline estimates	Alternative definition of exposure to 1968	Exposure to 1968 – short period	Including further controls	Exposure di other demonstrations
Exposure to 1968	0.0298***	0.0690*	0.0219*	0.0246***	0.0104
	(0.0095)	(0.0367)	(0.0117)	(0.0095)	(0.0081)

[5] Differently form the literature, this is not our preferred specification because some controls are likely to depend on the treatment (e.g. being in a relationship, income).

Observations	59,404	59,404	59,404	51,489	59,404

Notes: Robust standard errors clustered at the country-year of birth level in parenthesis; *** p<0.01, ** p<0.05, * p<0.1.

Table A3 shows further sensitivity checks, such different combinations of fixed effects, different clustering, weighted regressions, and the exclusion of cohorts 1932-1938 and 1962-1994, which we cannot include in the treatment group in any country since the Movement cover the 1964-1979 period (excluded cohorts are too old or too young to have their impressionable years during the Movement); this tests are run measuring the treatment either as a continuous or a discrete variable (see Columns 1-2 in Table 1). In almost all cases, results are confirmed.

In Table 2, we use the same regression framework to explore various aspects of the right-wing preference by exploiting other WVS/EVS questions (the answers are again in the 1-10 range). It turns out that the treatment is positively associated with the agreement on the role of income differences as incentives (Column 2), and on the importance of hard work for a better life (Column 3), while other issues are not affected: individual responsibility and the role for the government (Column 1), the extent of the public sector (Column 4), the desirability of competition (Column 5), and the primacy of economic growth (Column 6).

Table 2. The impact of 1968 on other politically relevant issues

	(1)	(2)	(3)	(4)	(5)	(6)
	People (not the government) should take responsibility	Large income differences serve as incentives	Hard work brings a better life	Private ownership of business should be increased	Competition is good	Wealth can grow so there's enough for everyone
Exposure to 1968	0.0071	0.0294**	0.0274**	-0.0187	-0.0112	0.0025
	(0.0123)	(0.0130)	(0.0122)	(0.0127)	(0.0108)	(0.0127)
Observations	62,151	61,079	35,857	55,030	61,456	26,687

Notes: Robust standard errors clustered at the country-year of birth level in parenthesis; *** p<0.01, ** p<0.05, * p<0.1.

We also investigated if the treatment produced polarization effects. To this end, we transform $Leftright$, as well as all the other outcomes in Table 2, into the distance from the country-wave average. We fail to find consistent and robust evidence that the Movement increases polarization (results are in Table A4).

4. Conclusions

In the long run the revolutionary ideology of 1968 has a small but significant positive effect of right-wing political preferences, differently from ex ante naïve expectations. No impact is detected on polarization.

References

Alesina, A., and Fuchs-Schündeln, N. (2007). "Good-bye Lenin (or not?): The effect of communism on people's preferences," *American Economic Review*, 97(4), 1507-1528.

Bénabou, R., (2008). "Ideology," *Journal of the European Economic Association*, 6(2-3), 321–352

Carreri, M., and Teso, E. (2021). "Economic recessions and congressional preferences for redistribution," *The Review of Economics and Statistics*, forthcoming.

Cotofan, M., Dur, R. A., and Meier, S. (2021). "Does Growing Up in a Recession Increase Compassion?: The Case of Attitudes Towards Immigration," *The Review of Economics and Statistics*, forthcoming.

Grosfeld, I., Rodnyansky, A., and Zhuravskaya, E. (2013). "Persistent antimarket culture: a legacy of the pale of settlement after the holocaust," *American Economic Journal: Economic Policy*, 5(3), 189-226.

Giuliano, P., and Spilimbergo, A. (2014). "Growing up in a Recession," *Review of Economic Studies*, 81(2), 787-817.

Maurin, E., and McNally, S. (2008). "Vive la revolution! Long-term educational returns of 1968 to the angry students," *Journal of Labor Economics*, 26(1), 1-33.

Roth, C., Wohlfart, J., (2018), Experienced inequality and preferences for redistribution, *Journal of Public Economics*, 167, 251-262.

Online Appendix

Table A1. Variable description

Variable	Description
Outcomes:	Respondents rated their agree on a scale from 1 to 10 with regards to the following statements:
Left-Right positioning	1=Left, …, 10=Right
People (not the government) should take responsibility	1=The government should take more responsibility, …, 10=People should take more responsibility
Large income differences serve as incentives	1=Incomes should be made more equal, …, 10=We need larger income differences as incentives
Hard work brings a better life	1=Hard work doesn´t generally bring success - it´s more a matter of luck, …, 10=In the long run, hard work usually brings a better life
Private ownership of business should be increased	1=Government ownership of business should be increased, …, 10=Private ownership of business should be increased
Competition is good	1=Competition is harmful, …, 10=Competition is good
Wealth can grow so there's enough for everyone	1=People can only get rich at the expense of others, …, 10=Wealth can grow so there´s enough for everyone
Controls:	
Marital status	A dummy variable for being in a relationship (married or living together as married) or not
Professional status	Employed, retired, student, unemployed, other status
Religion	Do not belong to a denomination (i.e. none), Roman Catholic, Protestant, Muslim, Orthodox, other religion
Income	Self-declaration of family income: Low, medium, high
Treatments:	
Exposure to 1968	Number of years in which individuals, in their 18-25 (during the 1964-1979 period), lived in a country with at least 1 mass demonstration of students.
Exposure to 1968 – short period	Number of years in which individuals, in their 18-25 (during the 1964-1972 period), lived in a country with at least 1 mass demonstration of students.
Alternative definition of exposure to 1968	A dummy being 1 for those individuals who lived at least 1 year (in their 18-25) in the years 1964-1979 in a country with at least 1 mass demonstration of students.
Exposure to other demonstrations	Number of years in which individuals, in their 18-25 (during the 1964-1979 period), lived in a country country with at least 1 mass demonstration (excluding those concerning students).

Table A2. Summary statistics

	Mean	Std. dev.	Min	Max	Obs.
Outcomes:					
Left-Right positioning	5.3652	2.0367	1	10	59,404
People (not the government) should take responsibility	5.8948	2.6727	1	10	62,151
Large income differences serve as incentives	5.6188	2.6124	1	10	61,079
Hard work brings a better life	6.4444	2.5030	1	10	35,857
Private ownership of business should be increased	6.5469	2.2296	1	10	55,030
Competition is good	6.9606	2.3050	1	10	61,456
Wealth can grow so there's enough for everyone	6.3150	2.2818	1	10	26,687
Controls:					
Marital status	0.6876	0.4635	0	1	51,489
Professional status					
Employed	0.6498	0.4771	0	1	51,489
Retired	0.1747	0.3797	0	1	51,489
Student	0.0109	0.1036	0	1	51,489
Unemployed	0.0476	0.2129	0	1	51,489
Other	0.1171	0.3215	0	1	
Religion					
None	0.3686	0.4824	0	1	51,489
Roman Catholic	0.2991	0.4579	0	1	51,489
Protestant	0.1780	0.3825	0	1	51,489
Muslim	0.0107	0.1029	0	1	51,489
Orthodox	0.0049	0.0699	0	1	51,489
Other	0.1387	0.3456	0	1	51,489
Income					
Low	0.2469	0.4312	0	1	51,489
Medium	0.4723	0.4992	0	1	51,489
High	0.2808	0.4494	0	1	51,489

Treatments:					
Exposure to 1968	0.8024	1.6123	0	8	59,404
Exposure to 1968 – short period	0.5985	1.3526	0	8	59,404
Alternative definition of exposure to 1968	0.3198	0.4664	0	1	59,404
Exposure to other demonstrations	2.0490	2.7450	0	8	59,404

Table A3. Further sensitivity checks

Panel A: treatment measured as continuous exposure to 1968					
	(1)	(2)	(3)	(4)	(5)
	Different FE #1	Different FE #2	Different clustering	Weighted regression	Excluding non-impressionable cohorts
Exposure to 1968	0.0322***	0.0350***	0.0298***	0.0218**	0.0081
	(0.0094)	(0.0098)	(0.0095)	(0.0101)	(0.0141)
Observations	59,404	59,404	59,404	59,404	31,397
Panel B: treatment measured as alternative (discrete) exposure to 1968					
	(1)	(2)	(3)	(4)	(5)
	Different FE #1	Different FE #2	Different clustering	Weighted regression	Excluding non-impressionable cohorts
Exposure to 1968	0.0728**	0.0722**	0.0690*	0.0713*	0.0717**
	(0.0335)	(0.0366)	(0.0376)	(0.0387)	(0.0354)
Observations	59,404	59,404	59,404	59,404	31,397

Notes: The dependent variable is the self-declared left-right positioning on the political spectrum. In Panel A the treatment variable is the number of years in which individuals, in their 18-25 (during the 1964-1979 period), lived in a country with at least 1 mass demonstration of students. In Panel B the treatment is a dummy being 1 for those individuals who lived at least 1 year (in their 18-25) in the years 1964-1979 in a country with at least 1 mass demonstration of students. Each column shows some variation of the baseline specification. In Column 1, cohort fixed effects are based on periods of 5 years each starting from years whose last digit is "1" while year of the survey fixed effects substitute for wave of the survey fixed effects. In Column 2, age fixed effects are based on periods of 5 years each starting from years whose last digit is "1" while year of the survey fixed effects substitute for wave of the survey fixed effects. In Column 3, standard errors are clustered at the age-cohort level. In Column 4, regression is weighted by survey weights. In Column 5, the sample does not include cohorts 1932-1938 and 1962-1994 that can not be treated. Robust standard errors clustered at the country-year of birth level in parenthesis; *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A4. The impact of 1968 on polarization.

Panel A: treatment measured as continuous exposure to 1968							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Left-right political position	People (not the government) should take responsibility	Large income differences serve as incentives	Hard work brings a better life	Private ownership of business should be increased	Competition is good	Wealth can grow so there's enough for everyone
Exp. to 1968	0.0002	-0.0219***	-0.0017	-0.0009	-0.0183***	0.0039	-0.0016
	(0.0051)	(0.0071)	(0.0077)	(0.0067)	(0.0060)	(0.0061)	(0.0075)
Observations	59,404	62,151	61,079	35,857	55,030	61,456	26,687
Panel B: treatment measured as alternative (discrete) exposure to 1968							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Left-right political position	People (not the government) should take responsibility	Large income differences serve as incentives	Hard work brings a better life	Private ownership of business should be increased	Competition is good	Wealth can grow so there's enough for everyone
Exp. to 1968	0.0077	-0.0321	-0.0062	-0.0064	-0.0120	0.0278	-0.0080
	(0.0196)	(0.0228)	(0.0222)	(0.0273)	(0.0225)	(0.0197)	(0.0307)
Observations	59,404	62,151	61,079	35,857	55,030	61,456	26,687

Notes: In Panel A the treatment variable is the number of years in which individuals, in their 18-25 (during the 1964-1979 period), lived in a country with at least 1 mass demonstration of students. In Panel B the treatment is a dummy being 1 for those individuals who lived at least 1 year (in their 18-25) in the years 1964-1979 in a country with at least 1 mass demonstration of students. Robust standard errors clustered at the country-year of birth level in parenthesis; *** p<0.01, ** p<0.05, * p<0.1.