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Communist socialization and post-communist economic and political attitudes☆

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ABSTRACT

We investigate the effect of individual exposure to communism on support for democracy and capitalism. We examine whether this effect varies across different types of communism, at different periods of people's lives, in different countries, and across different types of individuals. To do so, we propose a modified approach to solving the APC problem that relies on (a) survey data from multiple countries (b) historically defined cohorts and (c) variation in the time-periods related to these cohorts across countries. We provide a series of robustness tests for the method, and show that results are not very sensitive to panel structure. We conclude that generally communism had an indoctrinating effect, with more exposure to communism resulting in more opposition to democracy and capitalism.

1. Introduction: substantive motivation

Does exposure to communism affect the political attitudes and behaviour of citizens in post-communist countries? Although intuitively we would expect the answer to this question to be affirmative, it raises a number of more difficult follow-up questions: How do we conceive of more or less communist exposure? How do we differentiate exposure to Stalinism from exposure to *perestroika*? Is exposure likely to have a homogenous effect across individuals? Despite a few recent contributions (Neundorf, 2010; Pop-Eleches and Tucker, 2011, 2012), the topic remains largely underexplored. Nevertheless, as more and more studies of post-communist politics reject the *tabula rasa* approach to post-communism and point to the importance of taking account of what was left behind by communism (Jowitt, 1992; Kitschelt et al., 1999; Grymala-Busse, 2002; Ekiert and Hanson, 2003; Tucker, 2006; Wittenberg, 2006; Pop-Eleches, 2007), it becomes increasingly important that we be able to account for the role of communist legacies in affecting political attitudes and behaviour as well.

With this larger goal as motivation, here we investigate the more tractable question of the effect of individual exposure to communism on support for democracy and capitalism. We present two general ways of thinking about how exposure to communism might affect attitudes towards democracy and capitalism: *indoctrination*, whereby more exposure to communism would lead to more opposition to democracy and capitalism, and *resistance*, whereby more exposure to communism would lead to more support for democracy and capitalism.

To test these hypotheses, however, we need a way to measure "exposure" to communism. We begin by considering perhaps the bluntest measure of exposure: the number of years spent living under communist rule.[1] However,

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[1] In practice, we actually employ the number of years starting at age 6 that one lived under communist rule. From a pragmatic standpoint, our results would change little if we adjusted this starting point by a few years in either direction. See also Bartels and Jackman (2014) for another justification for beginning with age 6.

this measure relies on some strong assumptions: that one year of communism has the same impact regardless of the country in which one is living, the period of one's life in which that year occurs, or the type of communism (e.g., Stalinist vs. reformist) prevalent in one's country during that year, and that a year of communism has a homogenous effect on all individuals. Cognizant of the extent of these assumptions, we then adjust our analysis to relax each in turn.

Our contribution to a special volume on Age-Period-Cohort analysis stems from two features of our research question. First, while we clearly need to identify a cohort effect, *no data exist* that would allow us answer this question in a traditional APC approach, i.e., using a series of surveys that have been conducted a dozen times or more, drawing on the same population, and with the same question being asked year after year. Instead, as we explain in greater detail below, we use a survey that was conducted in fourteen countries, but with no more than two surveys in each country (see Appendix Table A1). However, our data also present us with an important resource for identifying our models: we have *a priori historically defined cohorts that exist in all of our countries*, but *not during identical time periods*. Thus we can leverage cross-country variation in exposure to communism, as well as within-country variation in exposure to communism. We therefore lay out a methodological approach that can be used by others who may want to study the effect of cross-national cohorts (e.g., exposure to authoritarian regimes in Latin America) in less data-rich environments.

In the following section, we lay out our methodological approach, including our identification strategy, as well as a series of robustness tests that one would want to conduct to ensure the method is working as expected. In Section 3, we elaborate on our theoretical argument, including both a more thorough justification of our exposure and resistance hypotheses, as well as more detail on the various methods by which we measure exposure to communism. In Section 4, we briefly describe the data and statistical models we employ to test our hypotheses before turning to our empirical findings in Section 5. In Section 6, we utilize data from Neundorf (2010) to provide an out-of-sample test of our methodological approach. In Section 7 we highlight the substantive and methodological conclusions of our analyses.

2. Studying cross-national cohorts with limited surveys

The challenge to assessing the effect of exposure to communism on any attitude in the post-communist era is disentangling these socialization effects from other variables, especially the age of the respondent but also the timing of the survey. This problem is known in the literature as the "Age-Period-Cohort" effect, whereby the challenge is to identify the "cohort" effect in a way which does not conflate this effect with simply being of a certain age ("age") at the time of the survey ("period") (Mason et al., 1973; Glenn, 2005; Neundorf, 2010).

To be clear, more survey data is always better than less survey data for estimating cohort effects. However, there are many questions that we might want to answer about cohorts in cases for which we do not have the ideal set of surveys for traditional forms of APC analysis. For example, what is the effect of living under a Latin American military regime on attitudes towards cooperation with the United States following democratization? Does living under a colonial regime lead to lower levels of trust in post-colonial institutions, and, if so, is the effect stronger for French or British colonialism? Or, as in our case, what is the effect of exposure to communism on attitudes towards democracy and the market in the post-communist era?[2]

To answer our question, we rely on: (a) having cohorts that can be defined *a priori* (e.g., in our case, exposure to communism); (b) the presence of comparable cohorts in different countries; and (c) at least some variation in the years of the cohort defining experience across countries. More specifically, we get identification of the cohort effect both from within-country temporal variation (e.g., if communism lasted for 45 years in country A, then both a 55-year old and a 75-year-old would have 45 years of exposure to communism in 1990) and from cross-country differences in when communism started and ended. All coefficients on cohorts, therefore, are estimated controlling for both age and the year of the survey. Moreover, these are not country-cohort estimates (e.g., what is the effect of living through 10 years of Polish communism) but rather general estimates of the effects of living through communism that draw upon the experiences of people from all 14 of the countries in our data set.

Of course, there are many other factors besides exposure to communism, the age of the respondent, and the year of the survey that might affect attitudes towards capitalism and democracy.[3] Thus the next step in applying the method is to control for appropriate individual *and* country level control variables.

Even beyond controlling for relevant country-level variables, we realize that to the extent that the intersection of age and exposure to communism is determined by one's country of residence (e.g., in Russia in 1995 all 20 year olds will be coded with the same number of years of exposure to communism), it is possible that results using our method can be driven by cross-country differences in the nature of either communist or post-communist experiences or institutions that are not sufficiently controlled for by the macro variables included in our regressions. To address these concerns, we take the following steps. First, we initially estimate all of our models with data pooled across countries and survey years simply controlling for age and a continuous indicator of survey year.[4] Second, to address concerns that the results produced by such an

[2] The Eurobarometer survey, which was been carried out almost annually between 1990 and 2003 and is utilized in Neundorf (2010), queried respondents concerning satisfaction with democracy, but not about attitudes towards democracy generally or about attitudes towards the market; we do, however, make use of this survey as part of our robustness tests in Section 6.

[3] Although we seek to identify cohort effects independent of age, it is not *a priori* clear why, all else being equal, simply being older ought to make one more or less likely to support democracy or the market in the post-communist context.

[4] Note that since communism fell at roughly the same time in all the countries in our sample, survey year largely captures the length of post-communist exposure.

approach may be driven by unobservable differences between countries and/or survey years, we run a second set of models for each exposure measure in which we include both country and survey-year fixed-effects. This approach yields a much more conservative estimate of the effects of exposure, since it solely captures within-country attitudinal differences between respondents of different ages.

Even with country fixed effects, it is still possible that that results could be driven by individual countries, e.g. if the effect of reform communist exposure is particularly strong in Slovenia. To address this concern, we run two additional tests. First, we rerun all of our model specifications dropping one country at a time from the analysis.[5] Second, we constrain the effect of age to be constant across models, and then rerun the model interacting dummy variables for each country in turn with communist exposure.[6] Should neither test suggest that the results are driven by a few countries – as is the case with the results presented in this article –then we can go a long way towards ruling out this type of concern.

In addition to allowing us to estimate cohort effects with a relatively low number of surveys per country – in this article, we have two surveys for most countries – a nice additional feature of our method is that it leverages the additional data present when working with historically defined cohorts. More specifically, rather than exclusively assigning respondents to a single cohort and then modelling this membership as a dichotomous dummy-variable, our approach relies on coding the number of years of exposure that a respondent has to the cohort generating experience. This allows us to capture two nice features of the data-generating process. First, some people have *more* exposure to the experience that is supposed to generate the cohort-related behaviour than others. For example, if we simply assigned a dichotomous variable to each East European respondent who turned 6 between 1947 and 1988 and called this the communist cohort, then we would essentially be assuming that the effect of communist exposure would be the same for someone who turned six in 1988 as it was for someone who turned six in 1946, despite the fact that the latter respondent had been exposed to forty more years of communism. Our method, on the other hand, allows for the latter respondent to have had a much stronger dose of communist exposure than the former, which seems intuitively desirable.

Second, our model allows for a respondent to have been exposed to more than one cohort-generating experience. In most APC models, cohorts are defined in such a way that respondents can only belong to a single cohort, such as two-year birth cohorts (Bartels and Jackman, 2014) or cohorts defined in terms of the first election in which one is eligible to vote (Smets and Neundorf, 2014). In some cases, such as the ones featured here, it is possible to belong to multiple cohorts. For example, many individuals will have experienced both Stalinist and neo-Stalinist communism. Had we employed a dichotomous measurement strategy to cohorts, we would be forced to employ some arbitrary rule to classify individuals as a member of one of these cohorts but not another. Our method – simply measuring the number of years lived in each cohort generating period – avoids this concern.[7]

3. Indoctrination, resistance, and exposure to communism

Shifting gears from methodological considerations, our substantive goal is to understand how exposure to communism affects attitudes towards democracy and the market. More specifically, we are interested in exploring the variation in the extent to which post-communist citizens prefer democracy to other forms of government (Chu et al., 2008; Evans and Whitefield, 1995; Kitschelt, 1992)[8] and whether citizens prefer a market based economy ("capitalism") or not (Duch, 1993; Earle and Gehlbach, 2003; Hayo, 2004; Przeworski, 1991).

We expect exposure to communism to affect attitudes towards democracy and the market because communism arguably represented the most systematic and long-lived challenge to the economic and political model of Western liberalism. Politically, communist regimes were either de jure or at least de facto *one-party regimes*,[9] led by a Marxist-Leninist political party whose organization was closely intertwined – and often fused – with the state apparatus. Furthermore, there was much *greater penetration of all levels of society* by communist regimes compared to other authoritarian regimes.

Economically, communist countries were set apart from the non-communist world by *the central role of the state in the economy*. Communist countries stood out in their systematic suppression of private enterprise and in their heavy reliance on central planning, which produced a very different economic logic and a series of typically communist pathologies (Kornai, 1992). As late as 1989 the share of the private sector in overall economic output varied surprisingly little in most of communist Eastern Europe and Eurasia, largely ranging from about 5% in most Soviet Republics, Czechoslovakia and Albania to 15% in most of the Yugoslav Republics[10] (EBRD, 2008).

Therefore to the extent that any given individual was "indoctrinated" and adopted the political and economic perspective of the communist regimes, we would expect

[5] We thank Nathaniel Beck for this suggestion. See Appendix Tables A3–5.

[6] We thank Larry Bartels for this suggestion. See Fig. 1 and Appendix Table A6.

[7] Recognizing that this is somewhat untried ground for APC methods, in Section 6 we explicitly compare our results with a more traditional dichotomous approach to cohort membership.

[8] Note the difference between a preference for democracy (as opposed to other political systems) and *satisfaction* with the way democracy is functioning in one's country. We consider the former to be more of a fundamental preference, while the latter is more of an evaluation.

[9] A few countries, like East Germany and Poland, nominally allowed the existence of multiple parties but such parties were expected – and very consistently fulfilled the expectations – to toe the official party line.

[10] The only partial outlier was Poland, where the private sector in 1989 accounted for 30% of the economy, largely because of the partial failure of large-scale collectivization of agriculture.

Table 1
Communist experience by year and country.

Country	Transition to communism	Stalinist	Post-Stalinist Hardline	Post-totalitarian	Reformist
Bulgaria	1945	1946–53	1954–89		1990
Czechoslovakia	1945–47	1948–52	1953–67, 1969–89		1968
East Germany	1945–48	1949–62	1971–89		1963–1970
Hungary	1945–47	1948–53	1957–60	1961–1989	1954–1956
Poland	1945	1946–1956	1982–83	1963–1981, 1984–87	1957–62, 1988–89
Romania	1945–47	1948–1964	1971–89		1965–70
USSR[a]	1918–20	1928–1952	1953–55; 1965–69	1970–84	1921–27; 1956–64; 1985–1991
Yugoslavia	1945	1946–1948			1949–90

[a] The Baltic republics and Western Ukraine were coded as starting Communism in 1945 and exposure to regime subtypes was adjusted accordingly.

her to be opposed to democracy and the market.[11] Thus the *indoctrination hypothesis* would predict that – all else being equal – the more exposed an individual was to communism, the more likely she would be to oppose democracy or the market.

Alternatively, perhaps more exposure to communism actually leads to more *resistance* to the ideas of communism, precisely because life under communism could be so brutal and repressive. Thus the *resistance hypothesis* would predict that all else being equal – the more exposed an individual was to communism – the more likely she would be to support democracy or the market.

As noted in the introduction, our first attempt to measure exposure to communism will simply involve counting the number of years starting at age 6 that an individual lived under communist rule. Doing so, however, makes the sweeping assumption that an additional year of communism has the same effect in every circumstance on all individuals. Like many assumptions, this one allows us to simplify our analysis, but is intuitively implausible. Thus we also consider four different ways to relax this assumption.

3.1. Life-time effects

Socialization theory (Campbell et al., 1960; Greenstein, 1965; Jennings and Markus, 1984), and in particular the *impressionable years hypothesis* (Krosnick and Alwyn, 1989; Visser and Krosnick, 1998) suggests that citizens pick up many of their political values and attitudes at a relatively young age as they are entering adulthood. Thus it is possible that what we should instead focus on exposure during one's formative schooling years. It may also be the case, however, that communism is only truly experienced as an adult. Therefore, we examine the effect of childhood and adult socialization independent of one another.

[11] In our book manuscript in progress (Pop-Eleches and Tucker, 2013), we go into much greater detail concerning the theoretical underpinnings for any expectation that a dominant political view in society would be (both actively and passively) inculcated into its citizenry, how, when, and where we might expect this process to occur, and in what types of individuals we might expect this process to be more or less likely to occur. Given the methodological focus of the current article, we do not expand upon these points in more detail here. However the questions of which individuals are more likely to be "indoctrinated" and which are more likely to exhibit "resistance" are taken up in below in Section 3.3.

3.2. Varieties of communism

Communism was not a monolithic experience across countries and over time. To put this most starkly, we might expect that someone who came of political age in Russia under Stalinism in the early 1950s to have been socialized into somewhat different political preferences than someone who came of age under Gorbachev's *perestroika*. With this in mind, Table 1 breaks down the communist experience into five subcategories that represent different "types" of communist experiences.

Our first category is the initial years in which communism was being consolidated. The next category is the Stalinist period, essentially the high-water mark of communist orthodoxy and repression. The communist countries in our sample then all moved beyond Stalinism, and we break down these "post-Stalinist experiences" into three categories. "Neo-Stalinist Hardline" refers to regimes that moved beyond Stalinism, but essentially still pursued hardline policies (e.g., low dissent tolerance, an active repressive state apparatus but without widespread terror, active security services, etc.). "Post-Totalitarianism" builds on Linz and Stepan (1996), and refers to communist regimes where the communist monopoly on power was still in place, but true believers in the ideology were few are far between, with most party members now associating with the party for careerist rather than ideological reasons. Finally, "Reformist communism" refers to periods like the Prague Spring, Gorbachev's *perestroika*, Poland's various flirtations with greater political openness and independent trade unions, etc.

We thus expect to find stronger "doses" of indoctrination during the more orthodox periods because there may have been more exposure to regime propaganda, as well as more propaganda from "true believers" in communism who might have delivered these message with more conviction (especially under Stalinism) and because the greater reliance on repression in both Stalinist and neo-Stalinist Hardline regimes made ideological deviations costlier than in the more open periods. We especially suspect this to be the case in such fundamental matters as the regime's political and economic justification for existence; if there was anything the regime needed the citizenry to accept – especially in its earlier and more violent days – it was the superiority of its political and economic models. In contrast, we suspect that in the post-totalitarian and reformist periods, there may have been more of a growing – albeit by no means total – acceptance of the interpretation that indoctrination efforts

on the part of the regime were a sort of "joke" that could at the very least be safely ignored and, in some cases, even openly mocked. We can also predict differences within these two broad categories: our general expectation therefore is that the indoctrination "doses" should decrease in effectiveness as one moves from Stalinism to Neo-Stalinism to Post-Totalitarian to Reformist communism.[12]

3.3. Individual heterogeneity in resistance to communist indoctrination

Just as communism is not monolithic, neither are individuals. Thus we can also examine whether there are some types of individuals for whom exposure to communism might be more likely to have an indoctrinating effect and others for whom it might be more likely to generate a resistance effect.

There are of course a myriad of different directions in which one could develop a hypothesis of this sort, and exploring all of the potential avenues to empirically test this type of theoretical proposition is far beyond the scope of this article. Here we focus on one particular mechanism: the role of organized religion. Our survey respondents primarily identify with one of three religions (if they identify with a religion at all): Eastern Orthodox, Catholic, or Protestant. Janos (2000) argues that Orthodox churches have traditionally been more accommodating to political rulers than their Catholic and Protestant counterparts. In contrast, the Catholic Church has been credited with facilitating resistance to Communism in Poland (Ash, 1983). Moreover, Wittenberg (2006) demonstrates using finely grained settlement-level data that both Catholic and Protestant churches in Hungary mediated the impact of communist socialization efforts. Thus one way to relax our assumption of a homogenous effect for communist exposure at the individual level is to assess the extent to which communist socialization leads to more indoctrination among Orthodox respondents than Catholics or Protestants.[13]

4. Data and methods

To test these hypotheses we use data from the *Post-Communist Publics* (PCP) Study. The PCP study consists of two waves of surveys (1990–2 and 1998–2001) and was administered in twelve ex-communist countries for the first wave and in fourteen ex-communist countries plus West Germany for the second wave. All told, therefore, we have surveys that take place in seven different years across 14 countries (see Table A1 for full coverage details). In addition to the individual-level survey data, we collected data on economic performance and democracy scores for each of the 26 country-years for which we had survey data. We then merged these indicators with the individual-level survey data to construct a multi-level data set.[14]

4.1. Indicators

4.1.1. Dependent variables

To test the impact of communist exposure on support for democracy and capitalism, we use two indices. Our d*emocratic support index* is composed of five questions that asked about the desirability of elections for choosing authorities, the need for political parties and parliament, whether democracy would solve or worsen the country's problems and the relative desirability of one-party and multi-party systems for the country. Our *pro-capitalism* variable, in contrast, focuses on the extent to which post-communist citizens embraced or rejected the nascent capitalist systems that replaced the socialist command economies after 1989. The index captures the extent to which respondents continue to associate capitalism with inequality, selfishness, repression, and corruption (as it had been portrayed for decades by communist propaganda) or whether they embraced it as the best economic system for their country and as being capable of helping solve their country's problems. Both variables were defined in such a way that higher values indicate greater rejection of the communist system and its components. Therefore, while the magnitude of the regression coefficients for the socialization indicators is not comparable across models (because the DVs are not standardized) the sign of the coefficients is consistent across models: thus, for the communist socialization variables, positive effects indicate a rejection of the communist model and suggest that communist socialization primarily triggered *resistance*, whereas negative effects indicate a continued embrace of communist values and therefore indicate *indoctrination*. For details about the wording of the survey questions, see Table A2 in the electronic appendix.

4.1.2. Independent variables

Our primary independent variables are the various measures of exposure to communism that have been described in greater detail in the previous sections; we incorporate these measures into our analyses sequentially. Thus the first set of regressions in Table 2 simply captures the number of years a respondent has spent living under communism after the age of 6. In Table 3, we disaggregate the communism experience into our four different types of communism (Stalinist, Neo-

[12] In addition to experiencing different communist sub-regimes for different periods of time, post-communist countries also different in terms of their pre-communist experience (Darden and Grzymała-Busse, 2006). This too would be a fruitful subject for future research. In particular, we might suspect that strong pre-communist political identities that existed before communism could undermine indoctrination, and that citizens of more advanced pre-communist countries could be more critical of communism than citizens in countries where communism brought more obvious developmental benefits. We thank an anonymous reviewer for suggesting these two hypotheses.

[13] It would of course also be interesting to examine not just religious affiliation, but the intensity of one's religious affiliation. To the extent that Catholics might be more resistant to communist indoctrination, we would certainly expect more religious Catholics to be that much more resistant. Exploring all of the different interactions between denomination and religiosity is, however, beyond the scope of the current paper. Moreover, it is equally possible to think of religiosity itself as a measure of communist indoctrination, given at least the public emphasis on promoting atheism of most of the communist regimes.

[14] Upon publication, data will be available for replication purposes on the authors websites at https://files.nyu.edu/jat7/public/research.html.

Table 2
Cumulative communist socialization.

	(1) Democratic support	(2) Democratic support	(3) Pro-capitalist	(4) Pro-capitalist
Individual variables				
Communist exposure	−.011** (.003)	−.004** (.001)	−.006* (.002)	−.005** (.001)
Age	.009** (.003)	.003** (.001)	.001 (.002)	−.000 (.001)
Post-secondary education	.284** (.025)	.273** (.024)	.063# (.037)	.056 (.035)
Secondary education	.133** (.017)	.111** (.018)	.050* (.022)	.032# (.018)
Male	.085** (.014)	.084** (.013)	.052** (.013)	.050** (.014)
Survey-level variables				
Year	−.002 (.010)		−.053** (.016)	
Freedom House democracy	−.006 (.013)	−.021 (.015)	.007 (.016)	.082** (.013)
GDP change (2yr)	−.002 (.003)	−.034** (.005)	.012** (.003)	−.011# (.006)
Inflation (log)	−.007 (.016)	−.126 (.080)	.028 (.029)	−.413** (.083)
Unemployment	−.017* (.006)	.015* (.006)	.005 (.010)	.018** (.005)
GDP/capita (log)	.075* (.028)	2.465** (.217)	−.039 (.024)	.910** (.186)
Country dummies	No	Yes	No	Yes
Year dummies	No	Yes	No	Yes
Observations	26,929	26,929	24,455	24,455
R-squared	.057	.096	.114	.156

Robust standard errors in parentheses $^{**}p < .01$, $^{*}p < .05$, $\#p < .1$.
Note: Also included in regressions but not reported were indicators for locality size and religious affiliation.

Stalinist, Post-Totalitarian, and Reform), and our variables accordingly measure the number of years spent living under each type of communism. In Table 4, we disaggregate exposure to communism into early (# of years between age 6-17 living under communist rule) and adult (# of years aged 18 and up spent living under communism) exposure. In Table 5, we interact years of exposure to communism with self-reported religious affiliation to test whether communist indoctrination is more effective among certain individuals while triggering resistance among others.

In addition to the socialization indicators, our regressions include a number of basic demographic controls: education levels, religious denomination, population size bands of the respondent's town, and sex. Finally, in order to control for the potential effect of economic and political conditions on economic and political preferences, the regressions control for inflation, unemployment and Freedom House democracy levels in the year preceding the survey and for the average GDP change in the two years prior to the survey.

4.2. Statistical methods

Since the two dependent variables do not deviate significantly from a normal distribution, the statistical tests presented in this article rely on ordinary least squares (OLS)

Table 3
Cumulative communist socialization (by regime subtypes).

	(1) Democratic support	(2) Democratic support	(3) Pro-capitalist	(4) Pro-capitalist
Individual variables				
Stalinist total exposure	−.006* (.003)	−.003 (.002)	−.010* (.004)	−.004# (.002)
Neo-Stalinist exposure	−.007* (.003)	−.007** (.002)	.001 (.002)	−.006** (.002)
Post-totalitarian exposure	−.013** (.004)	−.007** (.002)	−.007** (.002)	−.008** (.002)
Reform communist exposure	−.011** (.003)	−.001 (.001)	−.003 (.003)	.000 (.002)
Age	.007** (.001)	.002** (.001)	.000 (.001)	−.000 (.001)
Post-secondary education	.288** (.024)	.278** (.023)	.058 (.037)	.060 (.035)
Secondary education	.139** (.016)	.116** (.018)	.048* (.022)	.036# (.018)
Male	.084** (.013)	.085** (.013)	.050** (.014)	.051** (.013)
Survey-level variables				
Year	−.009 (.012)		−.060** (.015)	
Freedom House democracy	.003 (.012)	−.018 (.015)	.020 (.015)	.087** (.013)
GDP change (2yr)	.002 (.005)	−.035** (.005)	.016** (.003)	−.012* (.006)
Inflation (log)	.010 (.018)	−.134 (.082)	.048 (.030)	−.425** (.085)
Unemployment	−.018** (.006)	.015* (.006)	.002 (.009)	.018** (.006)
GDP/capita (log)	.089* (.039)	2.513** (.228)	−.016 (.026)	.963** (.191)
Country dummies	No	Yes	No	Yes
Year dummies	No	Yes	No	Yes
Observations	26,929	26,929	24,455	24,455
R-squared	.060	.097	.121	.157

Robust standard errors in parentheses $^{**}p < .01$, $^{*}p < .05$, $\#p < .1$.
Note: Also included in regressions but not reported were indicators for locality size, religious affiliation, and dummy variables indicating missing values.

Table 4
Early vs. adult communist socialization.

	(1)	(2)	(3)	(4)
	Democratic support	Democratic support	Pro-capitalist	Pro-capitalist
Individual variables				
Early communist exposure	−.013** (.002)	−.007** (.001)	−.008** (.002)	−.005** (.002)
Adult communist exposure	−.009# (.005)	.000 (.002)	−.003 (.004)	−.003# (.002)
Age	.006 (.005)	−.002 (.002)	−.002 (.003)	−.001 (.002)
Post-secondary education	.286** (.025)	.278** (.024)	.065# (.037)	.057 (.035)
Secondary education	.135** (.017)	.115** (.018)	.052* (.022)	.033# (.018)
Male	.085** (.014)	.084** (.013)	.052** (.013)	.050** (.014)
Survey-level variables				
Year	.000 (.011)		−.050** (.016)	
Freedom House democracy	−.006 (.013)	−.021 (.015)	.007 (.016)	.082** (.013)
GDP change (2yr)	−.002 (.003)	−.034** (.005)	.011** (.003)	−.011# (.006)
Inflation (log)	−.007 (.016)	−.127 (.081)	.028 (.029)	−.413** (.083)
Unemployment	−.017* (.006)	.015* (.006)	.005 (.010)	.018** (.005)
GDP/capita (log)	.075* (.029)	2.458** (.218)	−.040 (.024)	.908** (.187)
Country dummies	No	Yes	No	Yes
Year dummies	No	Yes	No	Yes
Observations	26,929	26,929	24,455	24,455
R-squared	.057	.097	.115	.156

Robust standard errors in parentheses $^{**}p < .01$, $^{*}p < .05$, $\#p < .1$.
Note: Also included in regressions but not reported were indicators for locality size, religious affiliation, and dummy variables indicating missing values.

Table 5
Communist exposure, religion and economic and political attitudes.

	(1)	(2)	(3)	(4)
	Democratic support	Democratic support	Pro-capitalist	Pro-capitalist
Individual variables				
Communist total exposure	−.0116** (.0030)	−.0043** (.0013)	−.0063* (.0024)	−.0048** (.0017)
Communist exposure* Catholic	.0012 (.0015)	.0010 (.0012)	.0025 (.0017)	.0024 (.0016)
Communist exposure* Protestant	.0021 (.0014)	.0017 (.0011)	.0046# (.0022)	.0041# (.0020)
Communist exposure* Orthodox	.0001 (.0015)	−.0006 (.0015)	−.0010 (.0013)	−.0006 (.0012)
Age	.008** (.003)	.002* (.001)	.001 (.002)	−.001 (.001)
Post-secondary education	.283** (.024)	.272** (.023)	.061 (.036)	.055 (.035)
Secondary education	.133** (.017)	.110** (.018)	.048* (.022)	.031 (.018)
Male	.085** (.014)	.084** (.013)	.052** (.013)	.050** (.014)
Catholic	.003 (.064)	.030 (.045)	−.008 (.064)	.054 (.057)
Protestant	−.041 (.054)	.014 (.048)	.007 (.060)	−.027 (.053)
Orthodox	.111 (.065)	.057 (.040)	.041 (.062)	.038 (.059)
Survey-level variables				
Year	−.002 (.010)		−.053** (.016)	
Freedom House democracy	−.006 (.013)	−.021 (.015)	.007 (.016)	.083** (.014)
GDP change (2year)	−.002 (.003)	−.035** (.005)	.011** (.003)	−.012* (.006)
Inflation (log)	−.008 (.016)	−.135 (.081)	.026 (.029)	−.428** (.087)
Unemployment	−.017* (.006)	.015* (.006)	.005 (.010)	.020** (.006)
GDP/capita (log)	.075* (.028)	2.496** (.224)	−.039 (.025)	.970** (.192)
Country dummies	No	Yes	No	Yes
Year dummies	No	Yes	No	Yes
Observations	26,929	26,929	24,455	24,455
R-squared	.060	.097	.121	.157

Robust standard errors in parentheses $^{**}p < .01$, $^{*}p < .05$, $\#p < .1$.
Note: Also included in regressions but not reported were indicators for locality size and dummy variables indicating missing values.

regressions with robust standard errors clustered at the country-year level with the various combinations and random and fixed effects described previously.[15] All the regressions use equilibrated survey weights, which combine any within-country survey weights with a cross-country component that adjusts for sample size differences across countries.

5. Empirical results

The first set of regressions in Table 2 simply captures the number of years past the age of six that a respondent has spent under communism. The baseline specification in model 1 suggests that even controlling for the individual

[15] We get similar results running hierarchical models in Stata 11 (results available from the authors).

and country-level controls described in the previous section, including age and survey year, individuals with a longer exposure to communism were less supportive of democratic ideals. This effect was not only highly statistically significant but fairly large in substantive terms: thus, an additional 30 years lived under communism were associated with an expected reduction of half a standard deviation in the democratic support index. However, model 2 indicates that once we include country and year dummies, the magnitude of the effect is reduced by about 60%, though it is still highly statistically significant. Given that these results only capture attitude differences between individuals within a given country and survey year, the exposure effect in model 2 arguably represents a conservative estimate of the socialization impact of communism.

Models 3–4 repeat the same model specifications for attitudes towards capitalism. Once again the negative and highly significant coefficients suggest that on average communist regimes were effective in inculcating anti-capitalist values into their subjects. While the substantive of the effect in model 3 was more modest than in model 1 – a 30-year increase in communist exposure was only associated with a quarter-standard deviation reduction in support for capitalism – the magnitude of the effect was much less affected by the inclusion of country and year fixed effects in Model 4. In other words, in the case of economic attitudes most of the differences seem to originate from within-country differences between individuals, whereas for democratic attitudes communist exposure also seemed to capture significant cross-country differences.

While a more detailed discussion of the other results in Table 2 is omitted due to space constraints, it is worth noting that the effects of age and year differ significantly across the two sets of attitudes. Thus, the positive and statistically significant effects of age in models 1–2 suggest that once we control for communist socialization, older respondents were actually more democratic; they were not, however, more pro-market (see models 3–4).[16] The comparison of temporal trends also reveals important differences. While survey year had a limited impact on democratic support in model 1, the large and statistically significant negative effect of survey year in model 3 is consistent with the widely held belief that negative experiences associated with post-communist economic liberalization depressed support for markets and capitalism.

As discussed in Section 2, in order to address the possibility that these results may have been driven unduly influenced by a single country, we next run a series of models in which we repeat the fixed effects model specifications from Table 1 but drop one country at a time from the sample (see electronic Appendix Table A3). Since the magnitude and the statistical significance of the exposure coefficients were not significantly affected by this procedure, this test suggests that our findings about overall communist exposure are not driven by any particular country.[17]

In Fig. 1 we present the results of a more demanding robustness test: we run a series of 14 models with the same specification as model 2 in Table 3 but for each model we include an interaction term between the communist exposure measure and the country-dummy for each of the 14 countries in the PCP sample.[18] As the country-specific conditional effects suggest, the effects of communist exposure were negative and statistically significant for 13 of the 14 countries in our sample, though the magnitude of the effect ranged from −.002 to −.008 and the differences between some of the countries were statistically significant (e.g. between Poland and Romania). However, the only country where communist exposure did not undermine democratic support was East Germany, which exhibited – albeit statistically insignificant – a positive effect.[19] Overall, these findings suggest that while the effects of communist socialization on democratic support vary somewhat across the former Soviet bloc, our overall result regarding the indoctrinating effect of exposure to communism on attitudes towards democracy is remarkably consistent and is not being driven by any one particular country.[20]

As a next step, in Table 3 we allow for the possibility that individual attitudes could be affected differently by different sub-types of communist regimes. We do so by accounting for the number of years past age 6 that a given respondent spent in any of the four subtypes of communist regimes (see Table 1 for subtype classifications).

The results in Table 3 confirm the analytical utility of differentiating between sub-types of communist regimes. Even though in model 1 exposure to all subtypes of communism was associated with weaker democratic support, the size of the effect was greater for post-totalitarian regimes than for Stalinist regimes, but the difference was at best marginally significant (at .16 two-tailed). Given that the exposure to different sub-regimes varied much more across countries than total communist exposure, it is not surprising that the fixed effects specification in model 2 affects the differences in coefficients to varying degrees. Thus, the effects of neo-Stalinism are unchanged, whereas the effects of post-totalitarianism and Stalinism are reduced by roughly 50% (though they remain highly significant for the former). Most striking, however, is the virtual disappearance of the reform communism exposure effect, which suggests that the relatively large effect in model 1 largely captures differences in democratic support between countries with short vs. long periods of reform

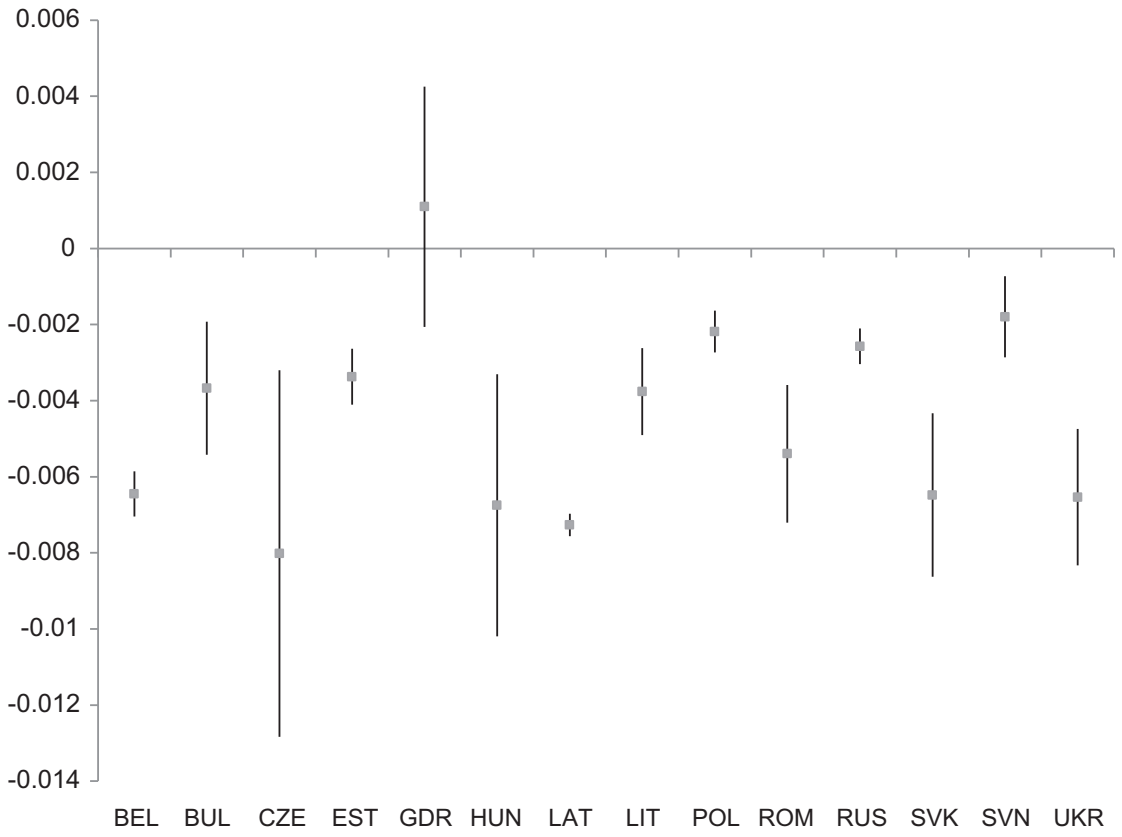
[16] As an interesting aside, the age coefficient in model 2 is very similar to the result we obtained when estimating the effect of age on democratic values in the only non-communist country in the PCP sample, West Germany in 1998 (.026 vs. .025).

[17] Results were very similar in the case of capitalist attitudes and are omitted for space reasons.

[18] The effect of age was constrained to be the same as in model 2 of Table 3 across all the models. We thank Larry Bartels for suggesting this approach to us.

[19] While the reasons for this East German exceptionalism are beyond the scope of this paper, they may include the role of West German media availability during the Cold War and the peculiar mode of post-communist transition-via-absorption experienced by East German citizens.

[20] When we reran these tests using capitalist support as a dependent variable, we found a significant negative effect for all countries except Slovenia.



Note: Country-specific conditional effects and 95% confidence interval of total exposure to communism.

Fig. 1. Communist exposure effects – by country.

communist governments. As a result, it appears that when using the conservative within-country estimates in model 2, only neo-Stalinist and post-totalitarian exposure had a lasting (and comparably sized) impact on democratic values, whereas the effects of Stalinism were significantly weaker (and at best marginally significant) and reform communism was largely ineffective.

Models 3–4 reveal broadly similar patterns with respect to economic preferences. Once again the effects of Stalinist exposure are reduced significantly by the inclusion of fixed effects (though they stay significant at .06 even in model 4), while the already weak effects of reform communism in model 3 entirely disappear in model 4. By contrast, the effect of neo-Stalinist exposure was actually significantly strengthened in the fixed effects specification, while for post-totalitarianism the effect increase was smaller and statistically insignificant.

Overall, the picture that emerges from Table 3 is remarkably consistent across both democratic and economic preferences: the two intermediate regime subtypes – post-totalitarianism and neo-Stalinism – appear to have been more effective in indoctrinating individuals who lived through them for longer. Meanwhile, Stalinism had only a marginal effect, whereas reformist communist regimes seem to have had very little impact on aggregate within-country attitude differences.[21] However, the stronger effects of both regimes in the random-effects models suggest that these regimes may have nevertheless affected subsequent attitudes through other channels, possibly by leaving behind institutions that influenced citizens irrespective of their actual exposure to the regimes.

In Table 4 we depart from the baseline models in Table 2 along a second dimension: the distinction between early and adult socialization.

The results confirm that the distinction between early and adult socialization is analytically useful: whereas the effects of early communist exposure are highly statistically significant (at .001) and substantively fairly large in all four models, the effects of adult communist exposure are more

[21] An interesting follow-up question, which cannot be pursued here due to space constraints, is whether the underlying mechanisms for these superficially similar outcomes differ for the two regimes: from our indoctrination/resistance perspective, we might expect Stalinism to produce both strong indoctrination among some citizens and strong resistance from others (which may largely cancel each other out), whereas reformist communist regimes would trigger both weaker indoctrination and weaker resistance.

uneven: while in model 1 the negative effect of adult communist exposure was at least marginally significant and the substantive effect of a one-standard-deviation change in adult exposure was twice as large as a one-standard-deviation change in early exposure,[22] the effect disappears in the fixed-effects specification in model2. With respect to economic attitudes, adult socialization was of a similar magnitude in both models 3 and 4, but in the fixed-effects specification in model 4 the statistical significance of adult socialization was greater (.1 one-tailed) and the difference between early and adult socialization was statistically insignificant. Taken together, these results suggest that at least in the earliest years of the transition, early communist socialization efforts had a more consistent effect on shaping (anti)capitalist and (anti)democratic values than subsequent adult socialization.[23]

As a final step, we address the possibility that the same regime could have very different effects on political attitudes depending on the specific social context and personal background of the individuals exposed to communist persuasion efforts. In line with our earlier discussion we focus here on the mediating role of organized religion by creating interaction terms between these religion indicators (Catholic, Protestant, and Eastern Orthodox) and the communist exposure variable. As discussed, we expect that communist socialization will lead to more indoctrination among Orthodox respondents than their Catholic and Protestant counterparts.

The signs of the interaction effects across the four models in Table 5 confirm that the Orthodox were more receptive to communist indoctrination than their Protestant and Catholic counterparts. While the statistical significance of the interaction terms generally fell short of statistical significance (with the partial exception of Protestant respondents in models 3 and 4), the important metric is the difference between the exposure effects of Protestant/Catholics vs. Orthodox respondents.[24] In this respect, the differences are quite clear: thus, for the democratic attitudes in model 2, the conditional indoctrination effect for Orthodox respondents was twice as large as for their Protestant counterparts (−.050 vs. −.026 per year of exposure) and this difference was statistically significant (at .05 one-tailed).[25] For economic attitudes the differences in model 4 are even larger: on average, Protestants appear to have been largely immune to anti-capitalist indoctrination, while Orthodox respondents were twice as susceptible as Catholics (−.053 vs. −.024 per year of exposure), with the effect for the latter being only weakly significant (at .16).

6. Robustness tests

From the perspective of this special issue, a very important question is how robust our findings are to changes in the sample composition, particularly with respect to the number and the timing of surveys for different countries. As mentioned earlier, the PCP surveys span a period of 11 years in 14 countries but each country has at most two surveys (with two of them only having one survey). While we have argued that by pooling these unevenly spaced surveys across countries we can get around identification problems that often plague APC models with few temporally distinct surveys per country/geographic unit, in this section we will briefly test this assumption by changing a number of key parameters in the temporal distribution of surveys.

As this could obviously not be done using the PCP surveys, we will use a dataset assembled by Anja Neundorf, and described in greater detail in Neundorf (2010). The dataset assembled surveys for 10 East European countries over the period from 1990 to 2003, and while surveys were not available for all countries in all years, it nevertheless provides much greater temporal coverage than the PCP dataset, with each country having between 9 and 11 surveys in the dataset. Since the surveys did not include questions about the democratic and economic values we analyze in this article, we will use the democratic satisfaction question Neundorf used in her original article.[26] Since the dependent variable is a dummy variable (those satisfied or very satisfied with how democracy developed/functioned in their country), we report logistic regression coefficients. In line with her original analysis, we also included age, country and year fixed effects, and a number of demographic controls and two measures of communist exposure: the first, used in models 1–4, is calculated using the same approach as in our analysis above, whereas the second uses Neundorf's original dichotomous Cold War cohort indicator, which includes respondents born between 1930 and 1975 (Table 6).

While each set of models uses the same model specification, the models differ with respect to the panel structure of the data. Models 1&5 use the full sample from Neundorf's article, models 2&6 attempt to recreate the data structure from the PCP dataset we used in the earlier sections.[27] The remaining models use two evenly spaced

[22] The effect of one year of early exposure is of course larger than for a year of adult exposure (though the difference is not statistically significant) but there is much wider variation in adult than early exposure (since the latter is capped at 12 years.).

[23] It is worth noting that the difference between early and adult communist exposure was greater for democratic attitudes in the first PCP survey wave (1990–92) but stronger for capitalist attitudes in the second PCP survey wave (1998–2001). These differences raise interesting questions for future research but are beyond the scope of the current article.

[24] Given that the reference category is made up largely of respondents without a religious affiliation, the interaction terms capture both the effects of religiosity and of religious affiliation, which is difficult to interpret given that religiosity itself could be a product of communist socialization. However, if we compare the attitudes of respondents who subscribe to different religions, then we should largely control for religiosity differences and thereby capture the inter-denominational differences.

[25] The differences were similar but slightly weaker for Catholic respondents.

[26] To be perfectly clear, since we are utilizing a different dependent variable from the previous section, this analysis should be considered a test of the robustness of the method, not of the specific findings we reported previously.

[27] Our analysis mirrors the structure of the PCP surveys in that for all countries (except Latvia, which had a single survey in both datasets) we included one survey from the 1990–92 period and a second one from the 1998–2001 period. The main deviation from the original PCP data is that Neundorf's dataset does not include surveys from East Germany, Russia, Belarus and Ukraine, but this arguably means that our "PCP replication" dataset should be – if anything – more vulnerable to unstable results since it has a smaller number of countries and surveys.

Table 6
Robustness tests to different survey panel structures.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Communist exposure	−.034** (.003)	−.028** (.006)	−.032** (.005)	−.030** (.005)				
Cold War cohort					−.346** (.027)	−.286** (.066)	−.342** (.046)	−.350** (.052)
Age	.017** (.002)	.017** (.005)	.015** (.004)	.016** (.004)	−.008** (.001)	−.004* (.002)	−.009** (.002)	−.007** (.002)
Years of education	.016** (.005)	.022* (.009)	.021* (.010)	.020* (.010)	.017** (.005)	.021* (.009)	.021* (.009)	.022* (.010)
Female	−.154** (.020)	−.226** (.044)	−.195** (.052)	−.186** (.063)	−.155** (.020)	−.224** (.043)	−.199** (.052)	−.193** (.063)
Unemployed	−.276** (.044)	−.126 (.125)	−.284** (.092)	−.109# (.056)	−.267** (.046)	−.129 (.134)	−.270** (.094)	−.085 (.056)
Rural	−.029 (.032)	−.150** (.054)	−.065 (.046)	−.106* (.045)	−.027 (.032)	−.149** (.055)	−.064 (.046)	−.106* (.045)
City	−.073* (.031)	−.106 (.079)	−.093 (.058)	−.144* (.066)	−.075* (.031)	−.109 (.079)	−.095# (.058)	−.147* (.066)
Country fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
# countries	10	10	10	10	10	10	10	10
# years/country	9–11	1–2	2	2	9–11	1–2	2	2
# survey years	14	7	2	2	14	7	2	2
# surveys	102	19	20	20	102	19	20	20
Year range	1990–2003	1990–2001	1992–2002	1992–1997	1990–2003	1990–2001	1992–2002	1992–1997
% Correctly predicted	67.0%	71.3%	66.3%	64.7%	67.0%	71.4%	66.6%	64.6%
# observations	75,192	15,362	18,866	14,369	75,192	15,362	18,866	14,369

Robust standard errors in parentheses $^{**}p < .01$, $^{*}p < .05$, $\#p < .1$.

surveys from each country but vary the time span between the two surveys: in models 3&7 we use data from 1992 to 2002 (thus mimicking a study with two surveys ten years apart), whereas in models 4&8 we reduce the time window to only five years by just using surveys from 1992 to 1997.

Comparing the coefficients for the exposure and age coefficients in models 1–4, we find that the estimates are affected surprisingly little by the rather drastic reduction in the number of surveys per country used in the estimation. Compared to the full sample in model 1, the size of the communist exposure coefficient is slightly lower in model 2 but post-estimation tests indicate that this difference is not statistically significant,[28] while the estimates for the age effects are virtually identical (though the standard errors were predictably larger in model 2 due to the significantly smaller samples size.) Perhaps even more surprisingly, the coefficients in models 3&4 are even closer to the full sample estimates, which suggests that the presence of multiple survey years in the PCP sample is not essential for ensuring identification. Furthermore, the minimal difference between models 3&4 suggests that the results are not particularly sensitive to the length of the time interval between surveys from the same country.[29] Overall, these comparisons suggest that the estimation approach we used in our earlier analysis is quite robust to even fairly restrictive limitations on the number and temporal distribution of surveys across different countries.

Turning briefly to the dichotomous cohort measure used in models 5–8, the results are remarkably similar. Once again, there is a somewhat larger – but statistically insignificant – drop in the cohort coefficient between models 5&6 but the two "balanced panels" in models 7&8 yield virtually indistinguishable results to the full-data estimation in model 5. The one area where the results for the two exposure indicators differed was with respect to the effects of age: not only were the effects of age in models 5–8 only roughly half the size of the corresponding models using the continuous exposure indicator, but the difference between the age coefficients in models 5&6 was fairly large and statistically significant, which suggests that these cohort-indicator models may be somewhat less stable for samples with fewer time observations.

Finally, the model fit statistics for the two sets of models do not reveal large differences: the dichotomous cohort measure performed slightly better in models 2&3, while the cumulative exposure measure had a higher percentage of correctly predicted cases in model 4. While these differences do not offer a definitive answer about the "right" communist exposure indicator, they suggest that the continuous exposure measure we propose represents a potentially valuable alternative to the traditional dichotomous indicators typically used in cohort studies.[30] Undoubtedly, the answer will depend greatly on the particular context, but the continuous indicator is likely to offer a more flexible approach when dealing with multiple countries whose historical experience of certain regimes do not overlap as neatly as in the case of East European communism.

7. Conclusions

In this article we analyze how the experience of living through communism affected post-communist attitudes towards democracy and the market. Since the survey data available to answer these questions was not available for as many survey waves as in typical APC studies, we have

[28] The difference was reduced by about 40% if we exclude data for 2003 from the full set of surveys, as the communist exposure coefficient was −.0316 for this model.

[29] This seems to hold true even if we further reduce the duration: we found comparable results when only using surveys from two consecutive years (1996 & 1997).

[30] When we included both the cumulative exposure and the dichotomous cohort indicator in the same model, both variables remained highly significant but the size of their effects declined by about 35% and 50% respectively. This suggests that the two measures could be used as complements to capture the complicated functional form of the relationship.

proposed an alternative identification strategy that relies on historically defined cohorts that vary cross-nationally and thereby avoids the multicollinearity problems inherent in APC analyses with limited time periods. Since the basic approach relies on the rather strong assumption that the effects of communist exposure are similar across different countries and individuals, we show that our findings are robust to the inclusion of country and year fixed effects, and to changes in sample composition. Furthermore, using Neundorf's (2010) data on democratic satisfaction, we find that our approach yields very similar estimates about the impact of communist exposure irrespective of whether we rely on a small (1–2) or larger (7–9) number of surveys per country, which is encouraging for its broader applicability to political contexts where data availability falls short of the standards that are typically used in advanced democracies.

While our overall finding that exposure to communism contributed to weaker support for democracy and markets was surprisingly robust across post-communist countries, our article identified and tested a number of theoretically motivated ways to relax the uniformity assumption by differentiating between different subtypes of communist regimes, between early and adult exposure and between individuals whose religious background might make them more or less receptive to communist indoctrination. This approach yielded three main findings.

First, with minor exceptions, communism appears to have had more of an indoctrinating than resistance effect, though this effect was weaker among Catholic and Protestant respondents. Otherwise, though, greater communist exposure – even after controlling for age – generally meant more opposition to democracy and the market.

Second, there were a number of significant differences in the effects of living through particular types of communism, which confirm the analytical utility of distinguishing between particular regime subtypes. In particular, it appears that the indoctrination efforts of neo-Stalinist and post-totalitarian communist regimes were more effective vis-à-vis both democratic and economic attitudes than those of Stalinist and particularly reformist communist regimes. However, we still need clearer theoretical explanations for these differential patterns, which for now raise more questions than they answer.

Third, in general it appears that early socialization (exposure during ages 6–17) consistently diminished support for both democracy and capitalism, whereas the effects of adult exposure were also negative but their magnitude and statistical significance were more uneven and more sensitive to model specification. This finding is in line with other work suggesting the importance of political socialization during one's early years (Campbell et al., 1960; Bartels and Jackman, 2014), but also suggests a potential continued role for communist political socialization as an individual entered adulthood. Given the relationship to the extant literature on political socialization, this question of childhood vs. adult socialization among post-communist citizens seems ripe for additional research.

Appendix A. Supplementary data

Supplementary data related to this article can be found at http://dx.doi.org/10.1016/j.electstud.2013.06.008.

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