Bachelor Thesis:

Past Regime Type and Current Beliefs in Conspiracy Theories

A Comparative Analysis Across Europe

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Word Count:	11,982
Date:	29.06.2022

Abstract

Beliefs in conspiracy theories can have severe consequences beyond the individual on society. Recent findings show these beliefs are more volatile across time and context than to be purely explained by psychological predispositions. *Context effects* could influence their diffusion. The aim of this study is to understand the effect of one possible context effect, *regime type during socialization*. Distinct features of authoritarian regimes, like the absence of a free press, political communication, and lack of participation opportunities could affect individuals' beliefs in conspiracies. Using the *Reconnect 2019 EP Election Panel Survey*, I test the effect of exposure to autocracy in different operationalizations with linear regression. The analysis considers the *age-period-cohort problem* that demands consideration when analyzing socialization effects using survey data. I find no significant relationship of any of the analyzed variables when looking at the whole sample. An effect can be found when looking at Germany and its history before reunification. Concluding, the study could not show a context effect of regime type but highlights the importance of further research into these effects. It also showcases paths for future research for comparable research questions.

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List of Abbreviations

APC	Age-period-cohort
CM	Conspiracy mentality
CT	Conspiracy theory
EP	European Parliament
GDR	German Democratic Republic
RoW	Regimes of the World
RQ	Research question
V-Dem	Varieties of Democracy

1 Introduction

There is a plethora of conspiracy theories claiming the most abstract explanations for societal events. They range from claims that former US President Obama was not born in the US to assertions by the former British footballer David Icke "[...] who believes the royal family are in fact extraterrestrial 'archonic' reptilian beings [...]" (Drochon, 2018: 337). These *alternative explanations of reality* that refer to individuals or groups *acting in secret* are often easy to dismiss as "silly and without merit" (Mahl, Schäfer, & Zeng, 2022: 2). However, they can have severe consequences:

> "Conspiracy beliefs – especially those regarding science, medicine, and health-related topics – are widespread (Oliver & Wood, 2014) and capable of prompting people to eschew appropriate health-related behaviors (Jolley & Douglas, 2014)." (Uscinski et al., 2020: 1)

A survey from the first year of the Covid pandemic in the US found that more than 30% of respondents believe the Corona virus was intentionally created and spread – "[T]hese beliefs are dangerous even if only a fraction of Americans succumbing to them ignore best practices, such as social distancing." (Uscinski et al., 2020: 1) Apart from the pandemic, further evidence has stockpiled that "[...] conspiracy beliefs hinder pro-social behaviors while promoting anti-social ones [...]" (Smallpage, Enders, Drochon, & Uscinski, 2022: 2). For example, "[...] failing to vaccinate one's children can contribute to a resurgence in once eradicated diseases." (Uscinski et al., 2020: 1) Understanding *why* people turn to conspiracies has therefore "[...] increasingly become a priority for scholars across disciplines (e.g., Butter and Knight 2020)." (Smallpage et al., 2022: 2)

The relatively young area of research on conspiracy theories (CTs) has been dominated by scholars from the field of psychology. Their explanations for why people turn to CTs are predominantly based on individual attributes, for example psychopathological antecedents or personality traits (Goreis & Voracek, 2019: 2). While a lot of ground has been made in understanding conspiracies since the 1990s, a key issue remains underexplained: If conspiracy beliefs are purely a function of individual predispositions, how can the high volatility over time and context in these beliefs be explained? People are not fundamentally different in different countries, however, the prevalence of CTs varies considerably between them.

One possible path forward to understand these variations are *context effects* (Mahl et al., 2022; Schlipphak, Bollwerk, & Back, 2021; Walter & Drochon, 2020). Context effects often serve as explanatory factors in political science. For example, the decision to vote is not only dependent on individual attributes, such as interest in politics, but also dependent on the electoral system, e.g., proportional vs. majority representation. A similar effect could be at play when it comes to beliefs in conspiracies. In other words, some external factors might increase the likelihood for people to turn to conspiracies.

This study aims to combine research into context effects and CTs with the extensive literature on effects of regime type. Scholars have intensely studied the effects of democracy, or in reverse, the effects of autocracy, on a variety of variables, such as equality, education, or health (see for example Alizada et al., 2022). This thesis tries to understand how regime type influences beliefs in conspiracies by following the research question (RQ):

> *How does exposure to authoritarian regimes affect individual beliefs in conspiracy theories?*

To answer this question, chapter two will provide a literature review on beliefs in CTs as well as how regime types are commonly distinguished. The RQ is an explanatory RQ because it creates a link between a cause (authoritarian regimes) and an event (beliefs in CTs). Therefore, the goal of chapter three is to "establish a general causal relationship" (Toshkov, 2016: 35) and argue *why* the two concepts should influence each other based on existing literature. The focus will be on political communication, media, and participation which are distinctively different in democracies and autocracies. Based on this theory, hypotheses will be derived to be tested in chapters four and five. Chapter four will describe the research design, linear regression, by first introducing the main data set, the *Reconnect 2019 European Parliament Election Panel Survey* by Plescia, Wilhelm, Kritzinger, Schüberl, and Partheymüller (2020), and second provide an operationalization for the main variables. Furthermore, the problem of analyzing socialization effects using survey data will discussed, before finally presenting a regression equation for chapter five. This chapter will carry out the analysis by testing the hypotheses as well as performing additional analyses. Finally, the results will be discussed, an answer to the RQ will be formulated, and paths for future research will be laid out.

2 Literature Review

2.1 Beliefs in Conspiracy Theories

The study of CTs recently gained traction in political science (Butter & Knight, 2015; Mahl et al., 2022; Schlipphak et al., 2021). This increase in scholarly attention started in 2018 and has been backed up by the growing number of people believing in CTs related to the Covid pandemic (Mahl et al., 2022: 6-7). However, CTs are neither a new nor fringe phenomenon and have been adopted by people in the past and present (Butter & Knight, 2015: 20). To meaningfully study this phenomenon, this chapter will provide a clear conceptualization of CTs and a brief overview over known factors that influence individual beliefs in them.

Even though different definitions of CTs exist, they are broadly understood as "[...] unique epistemological accounts that refute official accounts and instead propose *alternative explanations of events or practices by referring to individuals or groups acting in secret.*" (Mahl et al., 2022: 17, emphasis in original) They often "cultivate[] a Manichean perspective on socio-political reality" and divide the world into "evil" outsiders who threaten the "good" in-group (Hameleers, 2021: 39). Findings from a systematic literature review by Mahl et al. (2022: 8-9) show that over 90% of analyzed articles utilize this perspective of a secret plot in the definition of CTs. Other definitions highlight the use of CTs to reduce complexity and make sense of the world while again others highlight the "[...] desire to understand critical events occurring in the society." (17.5% and 5.3% of the articles analyzed by Mahl et al., 2022: 8-9)

CTs are not a new phenomenon and have been prevalent throughout history. An example of this are the conspiracies against Jews which have been and continue to spread. "Back in the 1930s and 1940s, Jewish conspiracy theories were a major part of Hitler's speeches and a potent force in inspiring the Holocaust." (Van Prooijen & Douglas, 2017: 326)

Research suggests that people believing in one CT are also more likely to believe in other conspiracies. This can even go up to the extent of believing in mutually contradictory CTs: "[T]hose who believe that Princess Diana faked her own death are also more likely to believe

that she was murdered […]" (Bruder, Haffke, Neave, Nouripanah, & Imhoff, 2013: 1-2). A term coined *conspiracy mentality*[1] (CM) tries to explain this phenomenon:

> "As such, a *conspiracy mentality* then describes the general propensity to subscribe to theories blaming a conspiracy of ill-intending individuals or groups for important societal phenomena or, in more abstract terms, the tendency to subscribe to 'general conspiracist beliefs' (Swami et al., 2010)." (Bruder et al., 2013: 2, emphasis in original)

Compared to beliefs in single CTs, CM describes longer-term attributes. Beliefs in individual CTs are substantially subject to change dependent on time and location (Bruder et al., 2013: 2). For example, CTs about Barack Obama not being born in the US were more prevalent during his presidency and, obviously, more popular in the US. CM on the other hand suggests that people who believed in the so called *Birtherism* CT are also more likely to engage in other CTs now.

CTs are a highly interdisciplinary field of research with scholars from vastly different fields like philosophy, psychology or cultural studies conducting research. Butter and Knight (2015) argue a "great divide" has appeared between empirical and culturalist approaches who employ different methods and definitions. Following their suggestion on "bridging the great divide", I briefly recap which factors scholars have identified to affect beliefs in CTs/CM.

The influence of gender on CM is contested. Some studies find males, some females to be more likely to engage in CTs while others find no connection between gender and CM. Furthermore, younger, and unmarried people are expected to believe more in CTs. However, these links are "somewhat undertheorized" as Walter and Drochon (2020: 485) argue. The influence of low education on higher levels of CM seems to be more established and often explained by higher skills of analytical thinking by well educated people which negatively correlates with CM. Though, even here some findings suggest no clear relationship.

Psychological research has found some correlations between the big five personality traits and beliefs in CTs, especially high *openness to experience* and low levels of *agreeableness*

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[1] This term is otherwise also known as *conspiracist ideation* (e.g., Bruder et al., 2013), *conspiracy thinking* (e.g., Drochon, 2018), or *conspiratorial predisposition* (e.g., Uscinski and Parent, 2014). Even though the exact meanings slightly differ, they broadly describe the same phenomenon.

seem to be correlated with higher beliefs in CTs/ a higher CM. However, a systematic review by Goreis and Voracek (2019) failed to find any significant effects on the meta-level when combining effect sizes. Some psychological disorders, like paranoia or schizotypy, have also shown to be connected to higher beliefs in CTs.

Alienation from society as well as low social-political power are predictors that have shown to correlate with beliefs in CTs. Furthermore, political extremism in general and right-wing authoritarianism in particular are predictors for beliefs in CTs. The connection to right-wing authoritarianism is theoretically justified by a distrust against governmental structures and "[...] that persons with high amounts [of] right-wing authoritarianism endorse conspiracies that involve deviant, high-power groups (e.g., anti-Sematic conspiracies), that threaten the status quo." (Goreis & Voracek, 2019: 6) Being exposed to CTs also increases the susceptibility to belief in other CTs, as, for example, Swami, Chamorro-Premuzic, and Furnham (2010) were able to show in their study on 9/11 conspiracy beliefs.

Beliefs in CTs have shown to influence individual behavior meaningfully. For example, people who believe in more CTs are less likely to stick to public health measures, such as getting vaccinated and preventing the transmission of sexually transmitted diseases. Furthermore, beliefs in CTs can lead to disengagement in politics and society (Goreis & Voracek, 2019: 9).

2.2 Regime Type

The classification of regime types and more specifically which forms count as democracies is one of the oldest quests of political science (see examples given in Coppedge et al., 2011; Diamond, 2002; Lauth, 2010; Lührmann, Tannenberg, & Lindberg, 2018). It goes back to scholars as early as Aristotle who tried to differentiate between *who* ruled and for *whom* they ruled (Lauth, 2010: 95).

So, to meaningfully use the concept of (un)democratic regimes and avoid ambiguities, the conceptualization and operationalization needs to be precise. I will strictly separate conceptualization (here) and operationalization (chapter 4.2.2) to make clear that theoretical considerations do not interfere with the hypotheses-testing of the latter part (Lauth, 2010: 98). I will move from what are considered *thin* concepts of democracy to *thick* concepts. This order is consequential, as the *thicker* concepts usually incorporate elements of *thinner* ones.

A prominent *thin* definition of democracy has been put forward by the economist Joseph Schumpeter (also known as *minimal* or *Schumpetarian*). He argues the defining feature of democracy is that essential positions of power are assigned based on "[...] a competitive struggle for the people's vote." (Schumpeter, 1947: 269, quotation from Diamond, 2002: 21) Adding a layer of *thickness*, Robert Dahl famously introduced his concept of *polyarchy* which builds upon the idea of competition but explicitly adds measures that ensure effective participation by the electorate (Diamond, 2002: 21). He chooses the term polyarchy because in his argumentation, the ideal of true democracy strongly deviates from existing implementations of democracy. For this concept of democracy, other factors are needed to ensure the struggle for votes is really competitive, however, they are "[...] viewed as secondary to electoral institutions." (Coppedge et al., 2011: 253) This conception of democracy is also known as *electoral democracy* (Lührmann et al., 2018: 61).

Other authors stress that the idea of competitive elections as a sufficient condition for democracy is often in contrast to regimes that hold elections but fail to constitutionalize other elements of what they consider democracy (Lührmann et al., 2018; Schedler, 2002). This originated from the fact that democracy has become "the only broadly legitimate regime form, and regimes have felt unprecedented pressure (international and domestic) to adopt—or at least to mimic—the democratic form." (Schedler, 2002: 24) States will, on paper, hold multiparty elections but skew the democratic playing field heavily in their favor, making an opposition victory almost unattainable. In this conceptualization, democracy is not seen as a dichotomous variable, but rather as a matter of degree:

> "While liberal democracies go beyond the electoral minimum, electoral democracies do not. They manage to 'get elections right' but fail to institutionalize other vital dimensions of democratic constitutionalism, such as the rule of law, political accountability, bureaucratic integrity, and public deliberation." (Schedler, 2002: 37)

Free and fair elections are necessary but not sufficient. Going "beyond the electoral minimum" means ensuring horizontal accountability, minority rights and limits on the power of government. High value is given to individual rights, which should be institutionally protected from infringements by the majority. Electoral democracies fail to put into place effective limits of power on government, while still conducting fair elections (Coppedge et al., 2011: 253).

On the other hand, these conceptualizations also make a differentiation on the authoritarian side of the regime spectrum between closed and electoral autocracies. Both regimes *do not* hold *free and fair* elections, however, electoral autocracies do hold multiparty elections but under skewed circumstances (Lührmann et al., 2018: 62-63; Schedler, 2002: 37-39). Figure 1 gives an overview over this regime classification.
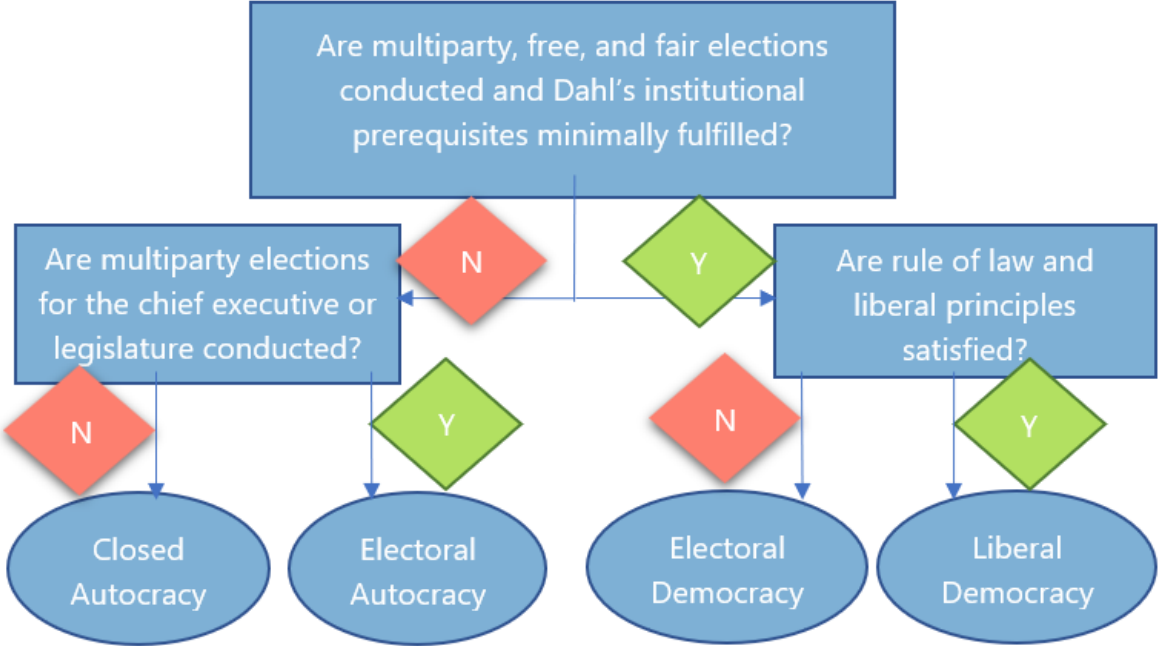
Figure 1. Regime classification scheme. Own graphics, content by Lührmann et al. (2018: 63).

More *thick* conceptualizations of democracy include *participatory, deliberative,* and *egalitarian* democracy. These conceptualizations are not necessarily exclusive from *liberal/electoral* democracy but highlight different elements of democracy (Coppedge et al., 2011: 253)

This section only provided a brief recap of research into the complex topic of regime types. Which regime type is most suitable for the analysis will become clear in the theory section, which follows next.

3 Theory

The *puzzle* this thesis is trying to solve is how geographical and temporal variations of CM can be explained. Scholars from psychological research have often focused on individual predispositions, personality traits, and individual attitudes to explain beliefs into CTs (for

example Goreis & Voracek, 2019; Walter & Drochon, 2020: 485). However, two questions arise when using individual factors to explain the phenomenon of CTs. First, assuming CM is just dependent on individual factors, why do some places show higher rates of CM than others? Second, why are the levels of CMs relatively volatile? Again, assuming CM can be explained by individual predispositions only, the beliefs in CTs should be relatively constant (Schlipphak et al., 2021: 1).

One solution to this puzzle could be found in *context effects*. For social scientists, context is often an import factor to explain individual behavior and could also be a basis to explain the variation in CM. Some external factors might increase the propensity to turn to conspiracies, while others decrease it. One example of context effects having an effect on CM is provided by Schlipphak et al. (2021). They were able to show "[...] mean levels of generic conspiracy beliefs being higher especially in countries in which actual conspiracies have happened in the past [...]" (Schlipphak et al., 2021: 11). One of the reasons context effects have received little scholarly attention this far is the focus on America when studying CTs. Hardly any cross-country research has been conducted and therefore research into context effects is still relatively underdeveloped (Mahl et al., 2022: 11-12; Walter & Drochon, 2020: 488-489).

Regime type could possibly be one context factor that influences levels of CM over time. Previous research has intensely studied the effects of regime type on a variety of dependent variables, such as social equality, economic growth, corruption, climate governance, or education (see for example Alizada et al., 2022). While some effects of democracy are believed to stem directly from free and fair elections, other features of democracy, such as freedom of association or judicial constraints, are usually the theoretical driving force behind positive outcomes of democracy. On the other hand, scholars have also looked at the effects of authoritarian governance. For example, Pop-Eleches and Tucker (2014) looked into the effect of exposure to communist regimes and its effect on support for capitalism and democracy.

Based on this, it is at least plausible to assume that regime type could affect beliefs in CTs. In this part of the thesis, I will present arguments based on previous research that justify a connection between *exposure to authoritarian regimes* and *beliefs in conspiracy theories*. I will present three arguments explaining the influence of regime type on CTs and argue why

it can act as a context factor: An effect of political communication, the influence of media, and the value of participation. Finally, I will present hypotheses that will be tested in the empirical part of the thesis.

The effect of *political communication* is based on the insight that authoritarian regimes use CTs more frequently in their communication and will thereby foster CM among citizens. The first part of this argument relies on the assumption that authoritarian governments use CTs more frequently while the second part assumes this will influence citizens.

Koehler-Derrick, Nielsen, and Romney (2021) present a convincing argument for why authoritarian regimes use CTs more frequently based on a calculation of *benefits* and *costs*. They see three basic *benefits* for why any (authoritarian or democratic) regime would communicate CTs. First, heads of government might *genuinely belief* in conspiracies against them. Second, conspiracies can be used to "[…] craft[] *national narratives* that enhance the state's symbolic power (Gray, 2010a, 133-134)." (Koehler-Derrick et al., 2021: 6, emphasis by author). As an example, they put forward the media outlet Russia Today which continuously portrays Russia in a good light and delegitimizes Western ideas. Finally, CTs can be used as a *diversionary tactic* to redirect attention from government failure and instead direct it towards CT. This strategy is often employed strategically, blaming bad news on outside actors while attributing good developments to national politicians (see also Rozenas & Stukal, 2019).

However, there are also *costs* of using CTs. While the *upfront costs* are relatively low, there are considerable *long-term costs* for governments. First, it *decreases human capital* by making citizens believe in false narrative. While communicating CTs might preserve power for governments in the short-term, it does "[…] not provide a recipe for long-term national success." (Koehler-Derrick et al., 2021: 9). Second, CTs may *backfire* by highlighting the problems a government had intended to conceal. Third, communicating CTs draws *international criticism* and backlash by the "alleged foreign perpetrators of conspiracies" (Koehler-Derrick et al., 2021: 9).

While these *benefits* and *costs* count equally for democracies and autocracies, there are good arguments to believe the result of this *cost-benefit calculation* is different. First, democratic institutions *increase the costs* of communicating CTs. "This is because political elites in democracies face incentives to satisfy relatively large portions of their populations

to stay in office, which encourages greater custodianship of public goods (Bueno de Mesquita et al., 2003) [...]." (Koehler-Derrick et al., 2021: 9) Second, strong political parties in democracies must look further than just the next election and therefore value *long-term reputational costs* higher than *short-term benefits* of communicating CTs. Third, autocracies face greater backlash by *independent media* when using CTs, as this is a crucial part of any liberal democracy.

This does not mean that no democratic regimes will opt for the use of CTs in their communication, nor that all autocratic regimes will use CTs frequently, but rather that democratic institutions and norms increase the costs and thereby, *on average,* reduce the salience of CTs in political communication.

Coming to the second part of the argument – the effects on citizens – there are two major reasons why this will affect beliefs in CTs. First, research has shown that being exposed to CTs is an important predictor for CM. Swami et al. (2010) were able to show that being *exposed* to conspiracies about the 9/11 terrorist attack considerably increased the likelihood of individuals believing in such theories. This could well be applicable for the communication of government officials. If they communicate more CTs, more citizens will start believing them. Second, the *social stigma* of believing in CTs will be considerably lower when they are prevalent in government communication (Schlipphak et al., 2021: 3). Figure 2 shows a summary of this argument.
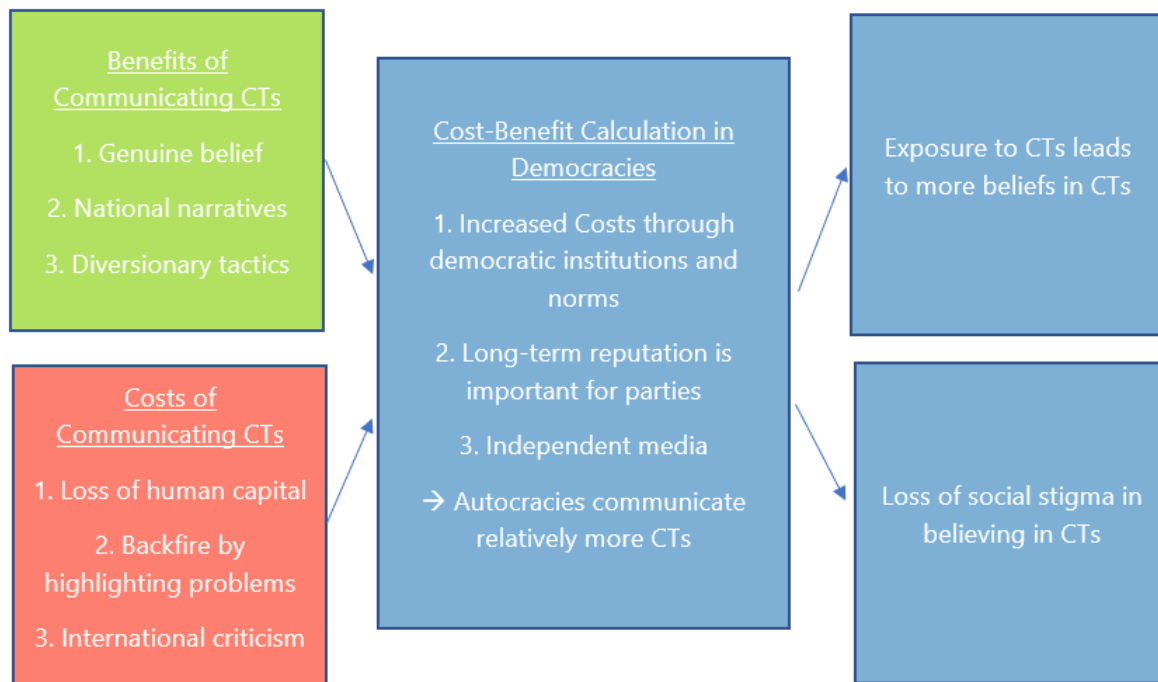
Figure 2. Cost-benefit calculation for use of CTs. Own graphics, content by Koehler-Derrick et al. (2021).

A second argument focuses on the lack of an *independent and trustworthy media* in authoritarian regimes. Coming back to the conceptualization of liberal democracy from chapter 2.2, it is important to remember that democracy is more than just the question of whether a leader came to power through elections. It also includes freedom of association, freedom of expression and other freedoms that make a contest for votes truly meaningful. "An authoritarian country, by contrast, lacks both (1) institutionalized democratic channels for interest aggregation and (2) independent and trustworthy news media for information transmission." (Huang, 2017: 288)

This lack of an independent media can lead people to turn to rumors which help them explain events happening in their society. Rumors are not absent in democratic countries, particularly among consumers of fringe information sources, however, are countered by an independent media which is trustworthy. Rumors are particularly problematic for authoritarian leaders, "[...] since rumors in a non-democracy are an alternative form of media that directly competes with official information and mainstream media, and therefore constitutes a counter-power against official power." (Huang, 2017: 283) Authoritarian regimes will feel obliged to rebuttal these rumors because, as mentioned before, they cannot allow rumors to damage the official government story and there is no free press to

hold them accountable. Huang (2017) finds that rumors reduce citizens trust in government and while rebuttals can sometimes help recover the lost trust, they are not always successful.

While rumors are not equivalent to CTs, a connection can be made. A lack of independent and trustworthy media will lead people to turn to alternative explanations of events. "Conspiracy theories provide people with simplified answers, specifically to questions of how a certain crisis situation emerged, and which societal actors can and cannot be trusted." (Van Prooijen & Douglas, 2017: 327) It is likely that a prevalence of rumors and rebuttals and general uncertainty about important events in society will fuel this desire and turn people to CTs for explanations. "More generally, a desire to make sense of the world is a core motive underlying belief in conspiracy theories." (Van Prooijen & Douglas, 2017: 327) This is also in line with the proposed definition of CTs, highlighting the explanatory feature of CTs.

A second effect of rumors and rebuttals on CM could be linked to *denialism. Denialism* is a psychological predisposition to not trust information by authority figures. It highly correlates with beliefs in CTs (Uscinski et al., 2020). Being constantly confronted with rumors and rebuttals could increase the disposition for *denialism* and thereby indirectly enhance believes in CTs.

The third argument focuses on *participation* as a key element of democracy and the feeling of *powerlessness* as an important predictor for beliefs in CTs. Participation in a liberal democracy also includes participation at the local level as well as a feeling of being able to influence decisions through various channels. These channels include, for example, protesting, engaging in interest groups, or petitioning. Authoritarian countries, on the other hand, often do not allow their citizens to participate in these ways and instead rely on centralized decision-making. How could this influence the mean level of CM? A variety of researchers were able to show that a feeling of *powerlessness* is a key predictor for beliefs in CTs. Not being able to influence decisions and not taking part in the decisions on who will lead a country could increase these feelings of powerlessness and thereby increase the beliefs in CTs. When people feel like their interests are not represented in the political system, they are more likely to believe in CTs (Walter & Drochon, 2020). This is a self-enforcing cycle, as people who feel powerless and turn to CTs, feel even more powerless. (Van Prooijen & Douglas, 2017: 328-329)

Coming back to the previous discussion about regime types, these arguments rely on a *thick* conceptualization of democracy. Only liberal democracies provide the accountability of leaders to citizens by emphasizing civil liberties and freedom of expression/press. Therefore, I will understand democracy as only liberal democracy and autocracy as all other regime types.

I have now presented three argument that could justify a *context effect* of *regime type* on *CM*: Higher salience of CTs in political communication by authoritarian leaders, a lack of an independent and trustworthy media, and a feeling of powerlessness caused by the lack of participation opportunities. Finally, there is an argument combining the three previously presented arguments: A "[...] *socialization effect of the context level* [which] expects citizens being shaped by their societal surrounding over a longer-term period." (Schlipphak et al., 2021: 3, emphasis in original) This argument builds on the idea that a society can be affected by long-term collective factors which provide a basis for CM. These long-term collective factors could for example be specific religious traditions or actual conspiracies becoming reality.

> "In the case of the US, for example, an argument in cultural studies is that the Puritan tradition of the founding generations, especially its perception of the devil being behind any negative event happening, prepared the way for a society being substantially fascinated by conspiracy theories (Butter 2014)." (Schlipphak et al., 2021: 3)

A similar effect could be seen for societies being exposed to authoritarian regimes for a long time. When individuals are constantly exposed to CTs by their leaders, have no independent and trustworthy media they can rely on, and feel a sense of powerlessness, it is much more likely that they will turn to CTs for explanations. This effect is expected to last beyond the time of autocracy, as for example Pop-Eleches and Tucker (2014: 77, emphasis in original) argue for the case of exposure to communism on individual attitudes towards capitalism and democracy: "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 [...]." This should be the same for the arguments at hand, as the tendency to look for explanations based on CTs is expected to not just hush after the regime type changed. This leads to the first hypothesis:

H₁: *Exposure to authoritarian regimes will lead to stronger beliefs in CTs.*

However, this does not consider the duration and timing during which one was exposed to authoritarian regimes and assumes that an exposure of one year will have the same effect as an exposure of, e.g., 30 years. A longer exposure to authoritarianism will also increase the strength of the effects of the arguments presented above. The more an individual is exposed to factors that increase his or her CM, the more he or she should be affected by it:

H₂: *Longer exposure to authoritarian regimes will lead to stronger beliefs in CTs.*

Different periods of life during which one is exposed to authoritarianism could also have an impact on the later beliefs in CTs. In particular, the *impressionable years hypothesis* "[...] suggests that citizens pick up many of their political values and attitudes at a relatively young age as they are entering adulthood." (Pop-Eleches & Tucker, 2014: 80) H_3 will test for the separate effect of exposure during this period of life:

H₃: *Exposure to authoritarian regimes during "impressionable years" will lead to stronger beliefs in CTs compared to people who were not exposed to authoritarian regimes during that period of life.*

Finally, recent exposure might also have an impact on individual beliefs in CTs. Current authoritarian regimes might affect citizens in the same way as described in the socialization argument but with an immediate impact. H_4 will therefore test for recent regime type:

H₄: *Recent exposure to authoritarian regimes will lead to stronger beliefs in CTs.*

4 Research Design

This chapter marks the starts of the empirical work of the thesis and will be concerned with testing the presented theory by statistically analyzing the derived hypotheses using linear regression. To meaningfully translate a complex theory into a language that can be mathematically analyzed, the concepts need to be operationalized into variables. This needs to be done with careful precision to make sure the variables represent the underlying concepts. Therefore, the remainder of this section will lay out how this translation was performed. In the first part, the data set will be briefly introduced, before going into the operationalization of the dependent, independent, and control variables. Finally,

particularities of analyzing socialization effects using regression will be discussed and the final regression equation presented.

4.1 Data

The main data source used for this thesis stems from the *Reconnect 2019 European Parliament Election Panel Survey* by Plescia et al. (2020) from the University of Vienna as part of the *Horizon 2020 Reconnect* project. The survey was conducted in two waves. One prior to and one after the European Parliament Elections in May 2019 and surveyed eligible voters in Denmark, France, Germany, Hungary, Italy, Poland, and Spain (Kritzinger, Plescia, & Wilhelm, 2019).

Participants were recruited online and had to be 18 years or older and eligible to vote in the 2019 EP elections. Quota sampling was employed to represent the populations of the surveyed EU member states based on age, gender, education, and religion compared to Eurostat 2017 data. Participants were awarded an equivalent of 1€ to participate in the first wave of the survey. After the first survey, participants were asked whether they want to take part in the second survey. Those who did not, were not invited again. "Harder-to-reach groups" were incentivized with an additional 0,50€ to participate in the second survey in Demark, Hungary and Poland (Plescia, Wilhelm, Kritzinger, Schüberl, & Partheymüller, 2019). Figure 3 shows the distribution of participants between the first and second survey. The survey questions most relevant for this research – on beliefs in CTs – are all found in the second wave of the survey. Participants who either did not take part in the second survey or took part but did not answer the questions on CTs, were therefore excluded from the analysis.
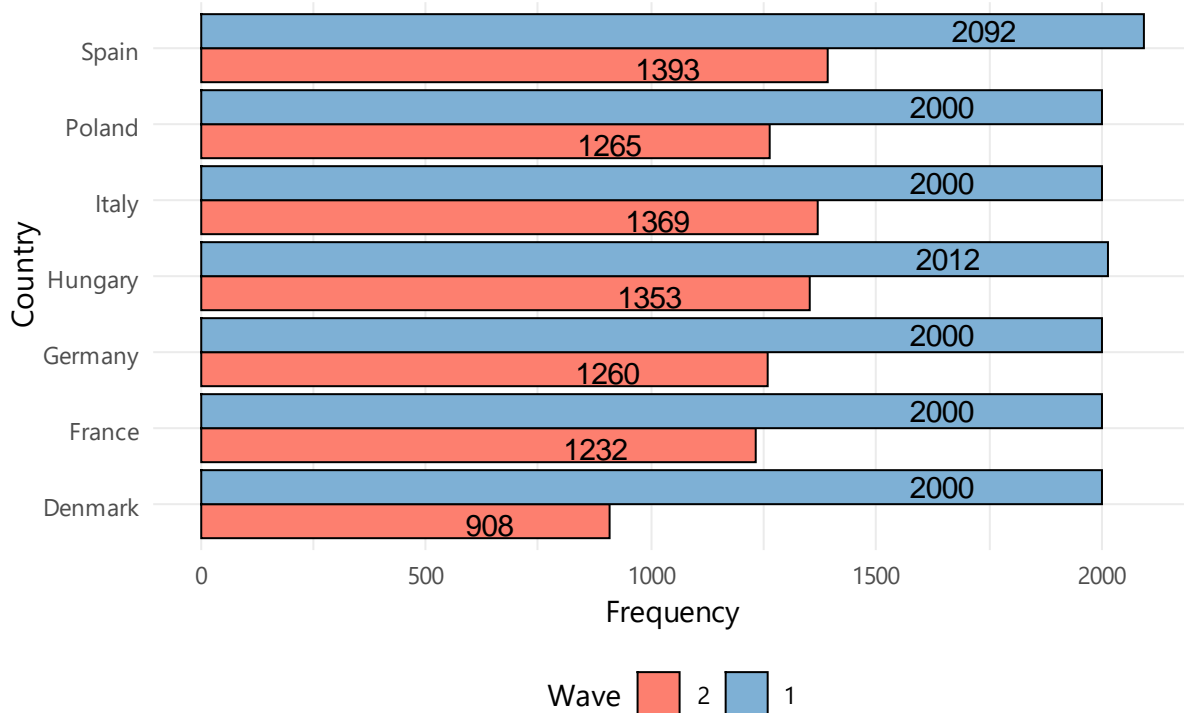
Figure 3. Respondents per wave.

4.2 Operationalization

In this section, the operationalization of the variables relevant to my hypotheses will be discussed. I will start with the dependent variable, beliefs in CTs, before operationalizing the main independent variable, exposure to authoritarian regimes. The independent variable will be operationalized in two steps. First, I will describe how to measure regime types before going into detail about the exact operationalization for the analysis at hand. Finally, I will showcase the control variables.

4.2.1 Conspiracy Mentality

Beliefs in CTs and CM have already been conceptualized in chapter 2.1. However, how to measure these beliefs (using surveys) is a further complicating topic and in this case further exacerbated by the fact the measurement must account for seven different countries and languages.

First, there is a general problem to measuring CM. CTs are prone to *social desirability bias* when measured using surveys, a bias not to reveal beliefs that are not acceptable because they would "[...] clash with social norms or otherwise cause others (e.g., interviewers) to judge [subjects] negatively." (Smallpage et al., 2022: 1) CTs are particularly susceptible to this because they are regularly described as fringe, portrayed as dangerous in the media,

and appear ridiculous to some. This can lead respondents of the survey not to give the answers they really believe in, but rather an answer that is socially desirable.

Second, measurement becomes more problematic when conducting surveys *across country contexts* for two reasons. First, popularity of CTs varies between countries. Second, what is and is not socially acceptable varies between cultural contexts – what people decide to conceal to an interviewer will inevitably also vary when confronted with specific CTs. This could lead to a general misestimation of beliefs in CTs (Smallpage et al., 2022: 1-5).

How can these two problems be accounted for in the measurement of CM in this thesis? Instead of using the support for specific CTs to measure CM, many questionnaires use generic questions.

> "An example item of such generic conspiracy would be 'Evidence of alien contact is being concealed from the public' (Brotherton et al., 2013, p. 4), as opposed to the specific 'Area 51 in Nevada, US, is a secretive military base that contains hidden alien spacecraft and/or alien bodies' (Swami et al., 2017, p. 14)." (Goreis & Voracek, 2019: 2)

The researchers behind one of the most used questionnaires to measure CM (the *Conspiracy Mentality Questionnaire* (*CMQ*) by Bruder et al. (2013)) argue that answers to more generally phrased questions are much less subject to change over time, culture, or current political events. This is particularly important for this study, as the answers to the survey questions could be influenced by country-specific cultural attributes and thereby overshadow the influence of exposure to authoritarian regimes. It could also reduce the effects of social desirability bias. This does not reduce validity, as it has also been shown that the replies to generic questions highly correlates with specific beliefs in CTs (see for example Goreis & Voracek, 2019: 2).

What does this mean for the construction of the variable *belief in conspiracy theories* in my bachelor thesis? The Reconnect 2019 survey by Plescia et al. (2020) contains two sets of questions asking about CTs. While one asks for direct CTs[2], the other set of questions

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[2] E.g., "Vaccines **cause autism**" or "George Soros **secretly finances immigration** to the European Union" (Plescia et al., 2020: question W2_Q14c, emphasis in original).

focuses on more general attitudes that hint at participants believing in a malevolent force behind significant events (see table 1). The second set of questions should be used as they are less prone to social desirability bias and *more* independent of the prevalence of specific CTs in the observed countries due to country context. Figure 4 shows the average answer separated by country.

Table 1. Survey items CM.

| Item	Question: How much do you **agree** or **disagree** with each of the statements below? Please indicate on the 0-10 scale, where '0' means "strongly disagree" and '10' means "strongly agree".
A	Legitimate questions about **refugees coming to Europe** are being suppressed by the government, the media, and academia
B	The real truth about **vaccines** is being kept from the public
C	An impartial, independent investigation of **Russian interference in the European Parliament election** would show that we've been lied to on a massive scale by European politicians

Note: Questions W2_Q86 1-3 from Plescia et al. (2019). Emphasis in original.
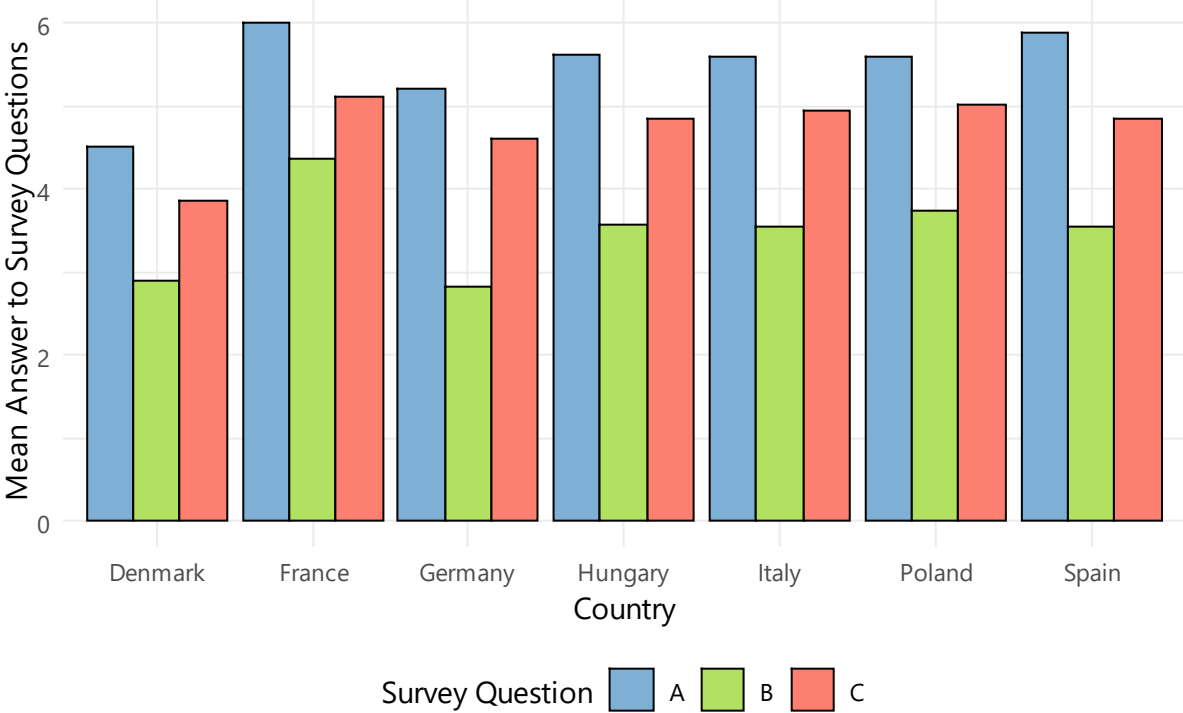
Figure 4. Average answers to survey questions separated by country.

The questions from table 1 resemble the style of questions asked in the *CMQ* by Bruder et al. (2013), while putting a focus on European issues in question A and C. Both the *CMQ* and the questions in the *EP Election Panel Survey* hint at government entities or other powerful actors hiding the truth about important issues. Both surveys also use "we"/"us" in their questions. It should be noted that especially question C is somewhat specific and does not necessarily fulfill the wished-for attributes as satisfactory as questions A and B. This is unfortunate and would have been designed differently for this research. It shows one of the drawbacks of using secondary data.

For the later regression, the three survey questions need to be combined into a single variable. As most indices measuring CM do, I will use the mean of the three survey questions available[3]. Figure 5 shows the distribution of the newly created index of CM. Figure 6 compares the mean CM value per country.
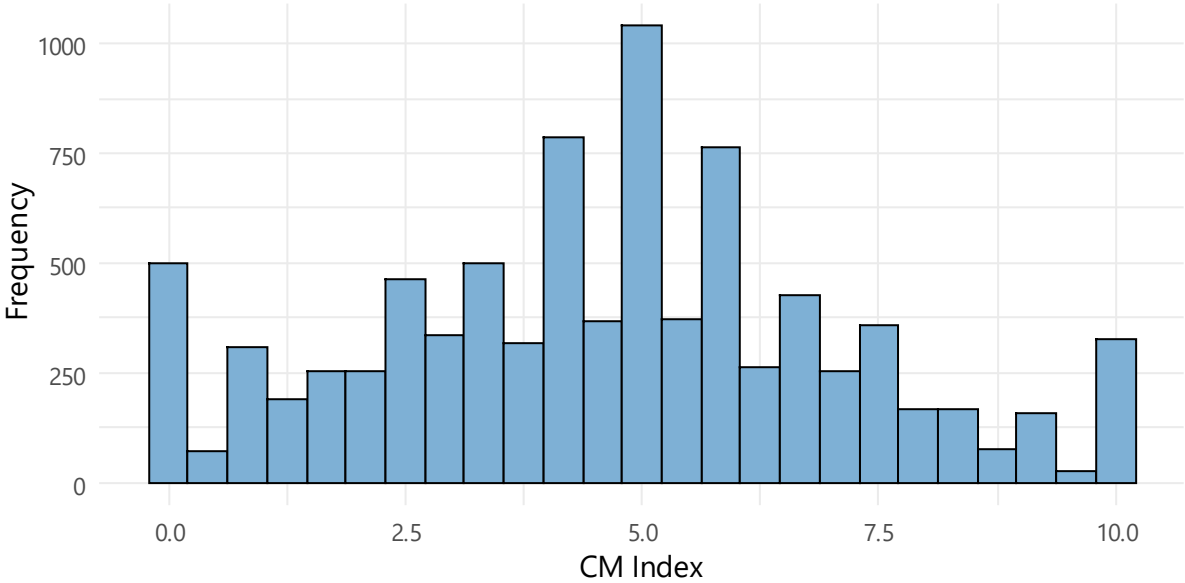
Figure 5. Distribution of CM index.

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[3] It should be noted that most questionnaires use more questions, e.g., the CMQ by Bruder et al. (2013) uses 5 questions, Walter and Drochon (2020) use 10 questions. However, confined by the available data, only the three questions could be used.
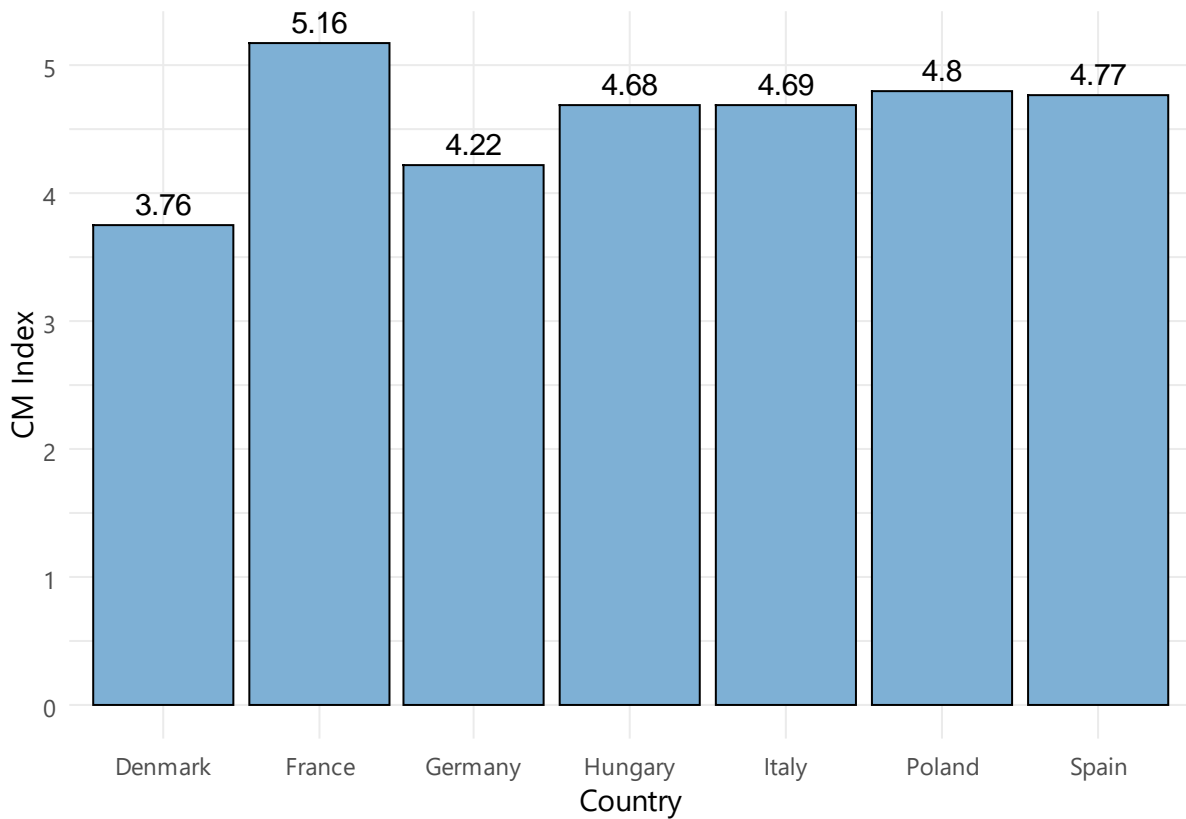
Figure 6. Mean CM index separated by country.

Looking at figure 5, a problem becomes obvious. The index variable is dominated by the values 0, 5, and 10. This is problematic for two reasons. First, this could indicate a problem of reliability. Especially the prominence of 5 could mean that respondents were indifferent about the question or did not reply with their actual beliefs and instead opted for the middle category. For the CM index to be 0 or 10, respondents had to indicate the same response on all three questions. This could mean they did not respond to all questions based on content. Second, the deviation from the normal distribution is problematic. The calculation of the p-value for significance is dependent on the assumption of a normally distributed dependent variable (van den Berg, 2020:35-40). This assumption is violated to at least some extent. These two problems highlight one drawback of working with survey data: The measurement of latent concepts is dependent on respondents answering genuine. At this point, differentiation between genuine and pseudo replies is impossible. Therefore, I will continue the analysis with the present construct. However, it should be kept in mind in the analysis and might affect the significance of effects.

To evaluate the internal validity of the index, Bruder et al. (2013) test, among other things, the internal consistency of the survey questions to confirm whether the items used measure the latent variable behind them. This is done by testing for internal consistency using Cronbach's Alpha. The three variables show an acceptable to good internal consistency overall as well as in each country separately (see table 2). While theoretically a factor analysis is possible, the number of items is low and there is reason to believe they all measure the same underlying concept based on Cronbach's alpha.

Table 2. Cronbach's alpha for CM index.

Country	Cronbach's Alpha
Poland	0.72
France	0.68
Germany	0.75
Italy	0.65
Spain	0.7
Denmark	0.74
Hungary	0.67
Overall	**0.71**

4.2.2 Democracy

Once settled on a concept of democracy, the operationalization should be easy: Either the defining features are present or absent (Lauth, 2010: 109). The abundant number of measures of democracy suggests something else. To name just a few Freedom House, Polity, Varieties of Democracy, and the Vanhanen Index all try to measure democracy (Coppedge et al., 2011; Lührmann et al., 2018).

Validity and reliability/objectivity are the biggest challenges in designing an index for democracy. First, there is a problem of validity. While competitive elections are an elementary part of almost all democracy conceptualizations, finding an authoritative indicator is much more contested (Lauth, 2010: 109-110). Can, for example, the share of seats in parliament for the ruling party be used? In *some* cases, this will successfully measure whether the executive is limited by a legislative branch, in other cases it will label existing democracies as autocracies. Furthermore, borderline cases pose a problem. Taking the

United States as an example, should it count as a democracy in the 19th and large parts of the 20th century even though women and people of color were excluded from the electorate for large parts of this time span? (Coppedge et al., 2011: 248)

Reliability can become a problem when using expert interviews/surveys. Unless criteria are carefully described, the same expert could come to a different result on different occasions. In this context, objectivity becomes a problem as well. Will an expert code every country with the same requirements? (Coppedge et al., 2011)

The different measurement attempts not only use different indicators, but also different levels of measurement. Should democracy be measured as a dichotomous, polychotomous, continuous or multidimensional concept? This question is further overshadowed by the need for "crisp regime measures" (Lührmann et al., 2018: 60) in many research questions, as apparent in this thesis. While an interval measure could convey more differentiation between cases, it becomes increasingly hard to handle in statistical analysis and makes results less interpretable.

Moving forward, I will use these criteria (validity, reliability/objectivity, and crisp regime measures) as metrics for which democracy index I will use in my analysis. I will briefly describe three indices – the Vanhanen Index, Freedom House, and the Regimes of the World index – before explaining my choice for the analysis.

Building on Dahl's model of polyarchy, the index developed by Tatu Vanhanen utilizes the two dimensions *public contestation* and *inclusiveness*. The persuading point of his measurement is its simplicity: *Contestation* is measured by 100 minus the share of votes for the strongest party. *Inclusiveness* is measured by the share of people from the general population who participated in an election. The advantages of his measurement lie in simplicity, transparency, and objectivity because both indicators are not influenced by personal preferences. However, the simplicity comes with drawbacks. The validity is vague as the system discriminates against two-party-systems compared to many-party-systems. There is no reason the US is less democratic than the Netherlands, *only because* there are less parties in parliament. Furthermore, the measure of inclusiveness discriminates against countries with a large young population which is not allowed to vote, and the thresholds used to qualify as a democracy are arbitrary. The index therefore cannot be used to measure

quality of democracy, but rather whether minimal criteria are fulfilled (Lauth, 2010: 110; Schäfer & Zürn, 2021: 29).

One of the most cited indices is the Freedom House Index. It does not measure democracy per se, but political and civil liberties instead. However, it is often used interchangeably with democracy indicators. For this indicator, country experts rate 25 indicators on a scale from 0-4. Countries are then labelled "free", "partly free", or "not free". The advantage of this indicator is its explicit focus on individual rights and a *thicker* conception of democracy. However, the transparency and objectivity are limited because of expert preferences. Comparisons of quality are also hard, as broad parts of the spectrum of available points is reserved for "not free" countries (Coppedge et al., 2011: 248-252; Lauth, 2010: 111-112).

Finally, the Regimes of the World (RoW) index is based on data from the Varieties of Democracy (V-Dem) project. V-Dem uses country experts who code indicators which are aggregated into different interval indices of democracy (electoral, liberal, participatory, deliberative, egalitarian conceptualizations) (Coppedge et al., 2019). The RoW index utilizes this dataset by combining necessary conditions with interval measures.

Based on these combinations, it categorizes countries into closed and electoral autocracies, and electoral and liberal democracies (Lührmann et al., 2018). One advantage of this indicator lies in the combination of necessary conditions with difference-in-degree indicators. This reduces the problem of ambiguous cut-off points because still necessary conditions need to be met to be characterized a democracy. Furthermore, the differentiation into four regime types reflects a more demanding conceptualization of democracy. One the other hand, one could still argue that cut-off points for the interval measures are arbitrary. Like the Freedom House index, problems of transparency apply for expert coded indicators. Table 3 presents a summary of this discussion.

Table 3. Ad- and disadvantages of select democracy indicators.

Democracy Measure	Validity	Reliability/Objectivity	Crisp Regime Measures
Vanhanen	-	+	-
	Indicators not suitable in many cases	Simple, transparent, broadly available indicators	Thin conception/ provides no differentiation
	Overestimates the number of democracies		
Freedom House	+	-	=
	25 indicators which cover different dimensions of liberal democracy	Expert surveys reduce reliability/objectivity	Three categories, but large part reserved for "not free" countries
RoW	+	=	+
	Interval measures and necessary conditions	Expert surveys *but* combined with more objective necessary conditions	Differentiation with demanding conditions for liberal democracies

Note: *"-" bad performance, "=" average performance, "+" high performance*

Concluding, each indicator has unique ad- and disadvantages. After all, "[…] we must appreciate that classificatory schemes […] impose an uneasy order on an untidy empirical world." (Diamond, 2002: 33) For this analysis, however, the RoW index is most fitting. Multiple, independent indicators capture the different dimensions of democracy. The combination of necessary conditions with interval measures reduces the problem of validity and reliability/objectivity by posing objective criteria the democratic group of countries must fulfill while still utilizing expert coding to consider country specific circumstances. Furthermore, it provides crisp regime measures and differentiation between four regime types, with the "liberal democracy" category following a *thick* conception of democracy. This is a further advantage compared to Freedom House. Figure 7 shows the development of regime type in the countries of analysis from 1920 up to 2019.
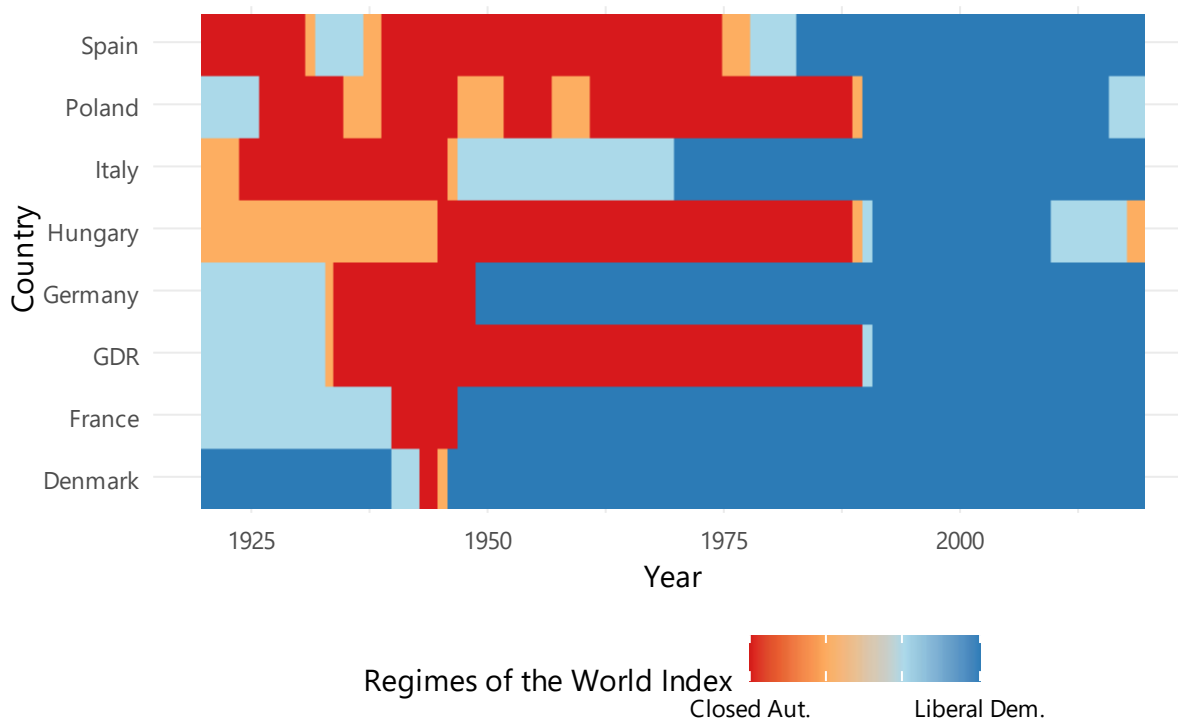
Figure 7. Time series analysis of the RoW index. Categories: Closed/electoral autocracies, electoral/liberal democracies.

The theoretical argument established in chapter 3 builds upon these *thick* features of democracy, especially civil liberties like freedom of expression and press. Therefore, only the conditions in the liberal category should be expected to make a meaningful difference on individual beliefs in CTs. Therefore, I combine the groups closed and electoral autocracies together with electoral democracies into the group *autocracies* as they do not meet the requirements of a *thick* conceptualization of democracy. Only liberal democracies will be counted as *non-autocracies*.

To test whether a different operationalization of autocracy would lead to different results, an additional regression will be performed where first electoral democracies will be considered *non-autocracies* as well and in a second step electoral democracies *and* electoral autocracies considered *non-autocracies*.

4.2.3 Exposure to Autocracies

The operationalization of the exposure variable is dependent on two factors: The age of the survey respondent and the country he or she lives in. Following Pop-Eleches and Tucker (2014), I will set an age limit of six years as a start of counting the exposure years. While exposure to autocracy during childhood is expected to already influence people – think of,

for example, schools – children under the age of 6 are not yet expected to be meaningfully influenced by the regime type around them.

The exact operationalization is different for the four hypotheses. H_1, which tests for any exposure to autocracy, will utilize a dummy variable for exposure to autocracy beyond the age of 6 up to 2019. Figure 8 shows the distribution of respondents exposed to autocracy in the different countries of analysis. As already seen in Figure 7, Denmark, France, and Germany have been liberal democracies since World War II and therefore few respondents have ever lived under autocratic rule.
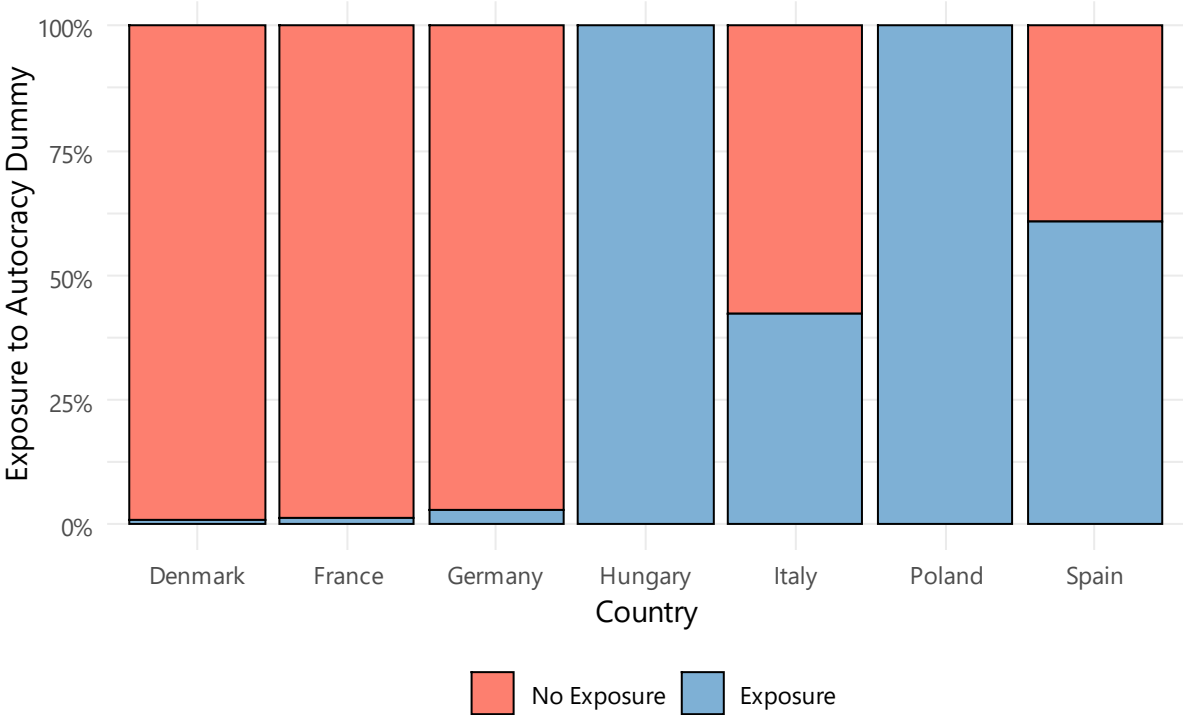
Figure 8. Distribution of dummy exposure variable separated by country.

For H_2, which tests the effect of length of exposure, the exposure variable is continuous and needs to be a sum of the years an individual spent under autocratic rule. Figure 9 represents the distribution of exposure across the four countries which meaningfully deviate from 0. Put as an equation, this variable can be formulated as follows:

$$f(country, age) = \sum_{i=2019-age+6}^{2019} authoritarian\ regime(Y/N)_{country,\ year_i}$$
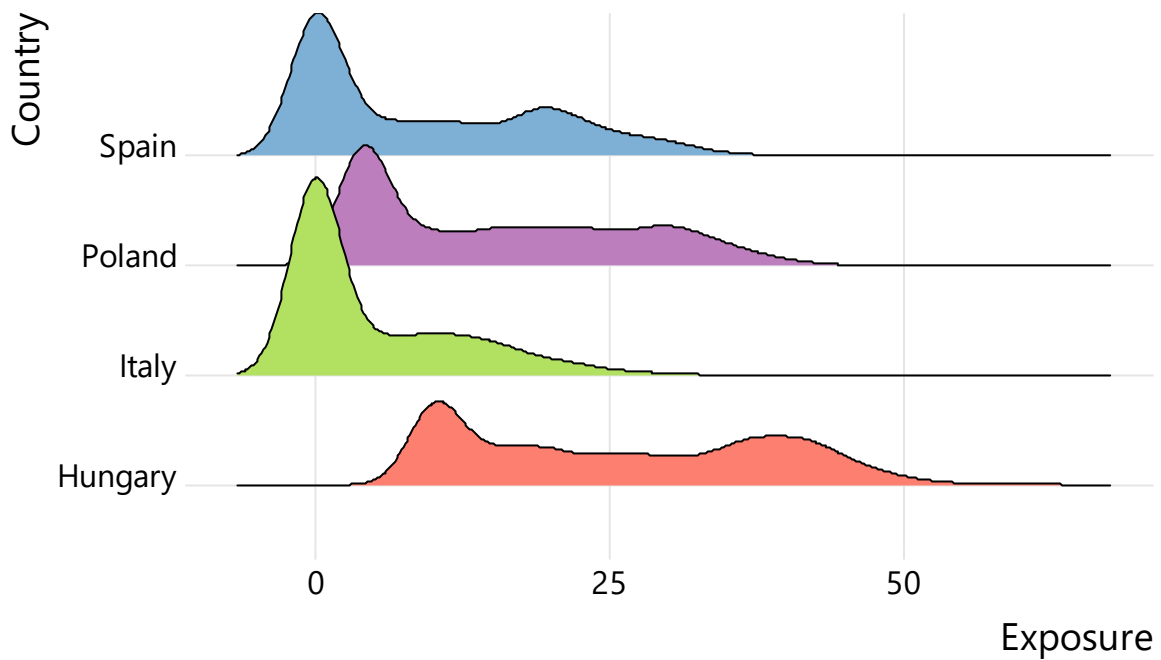
Figure 9. Distribution of continuous exposure variable for selected countries.

H₃ tests for exposure during impressionable years, that is between the age of 6 and 18. This hypothesis will be performed both with a continuous variable counting years of exposure as well as a dummy variable if any year during socialization was spent living under autocratic rule. The previous equation needs only minimal adjustment by limiting the upper end of the sum to the time respondents turned 18:

$$f(country, age) = \sum_{i=2019-age+6}^{2019-age+18} authoritarian\ regime(Y/N)_{country,\ year_i}$$

Finally, H₄ is concerned with recent exposure to autocracy. Looking back at Figure 7, only Poland and Hungary have been anything apart from liberal democracies since the 1990s. Therefore, only respondents from Poland and Hungary will be coded as recently being exposed to autocracy in a dummy variable.

This concludes the exposure to autocracy operationalization for the different hypotheses. Table 4 shows a summary of the different exposure variables. However, there is a special case in the selected countries: Germany. Up to 1990, + Germany today was divided into a democratic West and an autocratic East with the German Democratic Republic (GDR). For the previously presented argument, it would only make sense to differentiate between people who grew up in the GDR from people who grew up in West Germany. Problematically, the *EP Election Panel* does not include any question on place of birth within

Germany. To still account for this problem, I will conduct an extra analysis where I code people currently living in the states that used to be part of the GDR as having grown up in the GDR. This is problematic, because since the reunification people have moved between the two former countries. However, this heuristic is the closest guess possible with the available data and should at least be controlled for in a separate regression. Using this approach, 257 respondents were coded as having grown up in the GDR, while 977 were grew up in West Germany[4].

Table 4. Coding/range of exposure variables.

Exposure Variable	Coding/Range	Mean	Standard Deviation
Dummy Exp.	0-1	0.47	.50
Continuous Exp.	0-61	8.52	12.4
Exp. Impressionable Years	0-12	3.85	5.22
Exp. Impressionable Years Dummy	0-1	.41	.49
Recent Exp.	0-1	.3	.46
Note:			*Total N = 8,734*

4.2.4 Control Variables

Four additional variables are used to control the regression. These are gender, education, and religion[5] and have shown to influence beliefs in CTs in previous literature (see section 2.1). Furthermore, I include age as a control, the reason for that will be discussed in section 4.3.

In the *Reconnect* survey, the question on gender is purely binary. The distribution between male and female respondents is almost perfectly 50%. The *Reconnect* team asked

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[4] Berlin, which was divided between the two countries, was coded as being part of West Germany. Respondents from Berlin were least likely to have grown up in the GDR, as half of all the respondents grew up in the democratic part of the city. Furthermore, Berlin probably experienced the highest amount of migration from the former West.

[5] For exact phrasing of the questions see Plescia et al. (2019) questions W2_Q1b, W2_Q2, W2_Q_edu, and W1_Q74.

respondents about their education degree based on national education systems. They recoded the responses into generalized categories low, medium, and high. Notably, in the distribution, countries vary considerably in the percentage of people having obtained higher education. This could partly be due to national education systems, but the exact coding is not provided in the documentation. Finally, religion is operationalized by a survey question asking respondents about their self-assessment on how religious they think they are. Respondents answer on a 10-step scale from "Not at all religious" to "Very religious" (Plescia et al., 2019). Figures 10-13 show the distributions and Table 5 shows the descriptive statistics of all included control variables.

Table 5. Descriptive statistics of control variables.

Variable	N	Mean	St. Dev.	Min	Median	Max
Age	8,734	49.39	15.36	18	50	98
Gender	8,734	1.51	.5	1	2	2
Education	8,734	2.35	.64	1	2	3
Religion	8,734	3.62	3.25	0	3	10

Note: *Gender coded as 1 = "male", 2 = "female"*
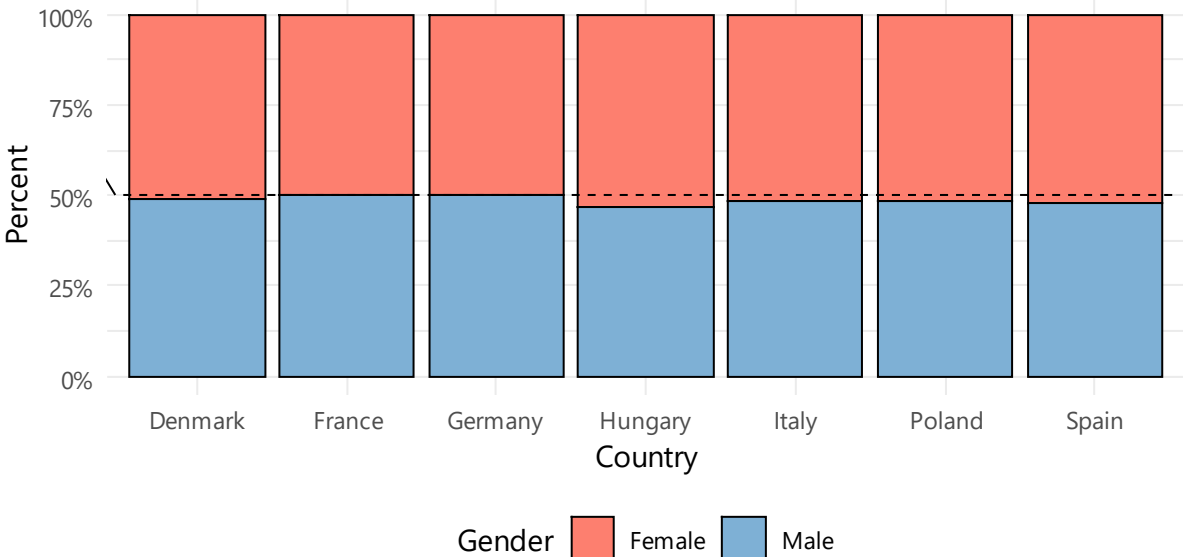
Figure 10. Distribution of men and women separated by country.
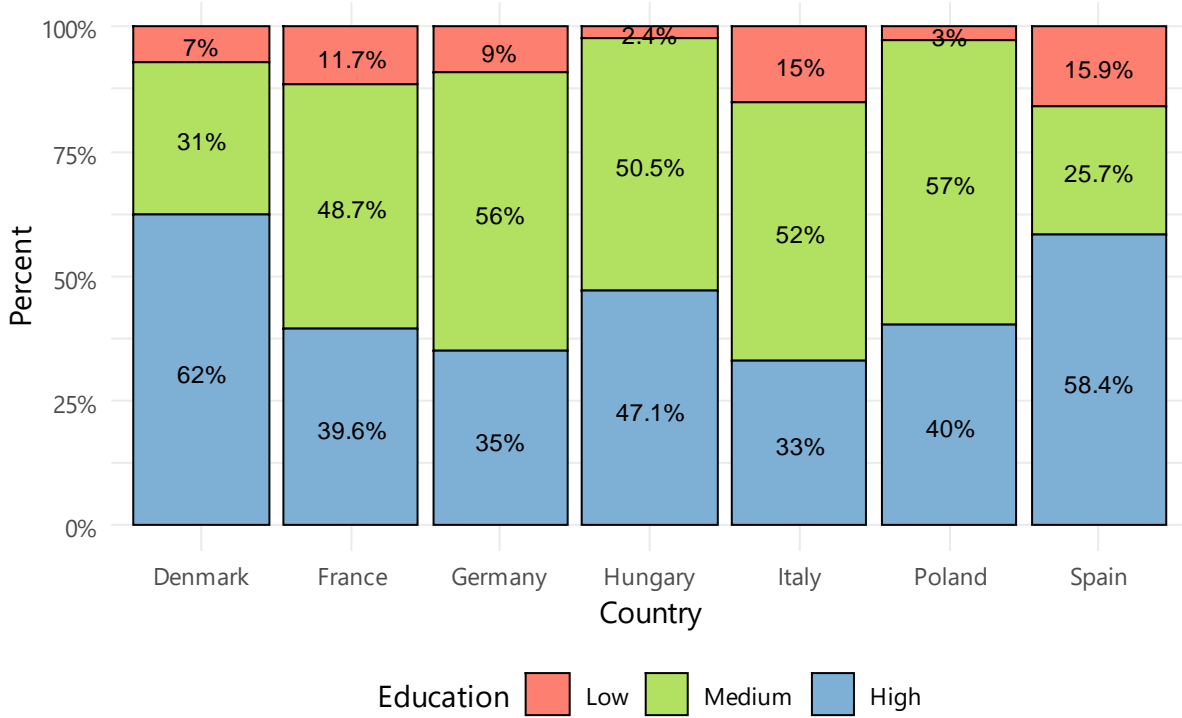
Figure 11. Distribution of low, medium, and high education separated by country.
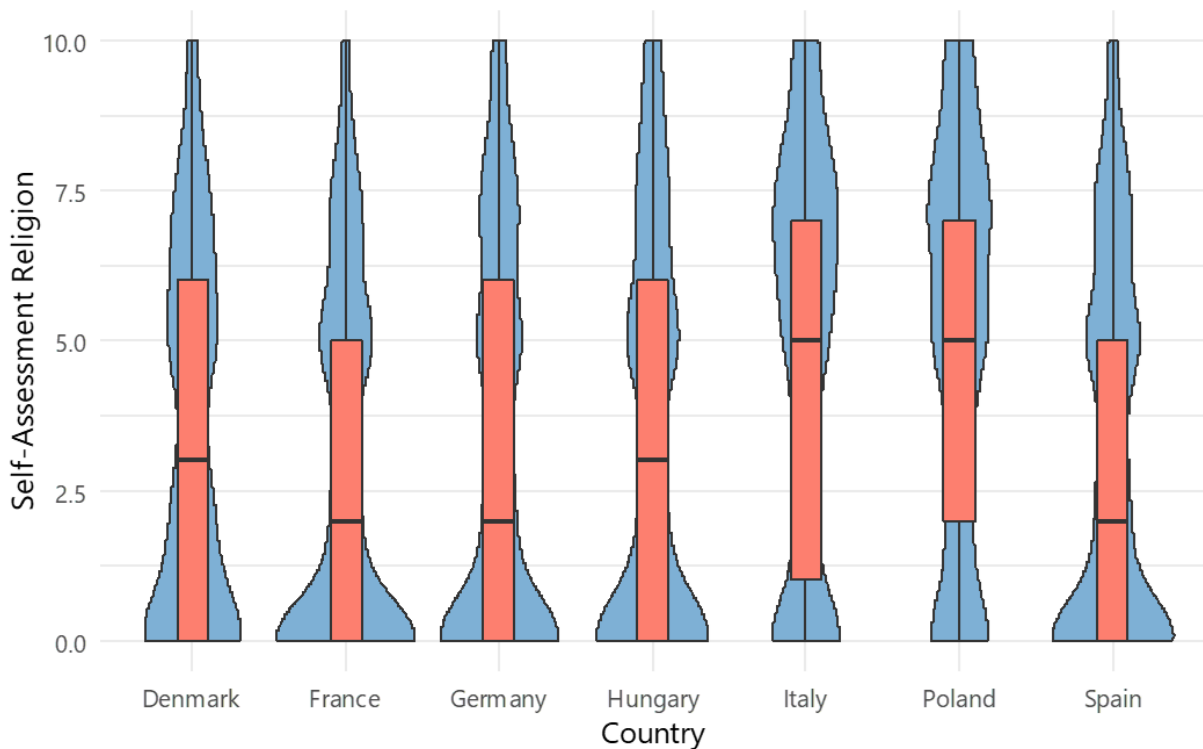
Figure 12. Distribution of replies to self-assessment question on religiosity from 0-10 separated by country.
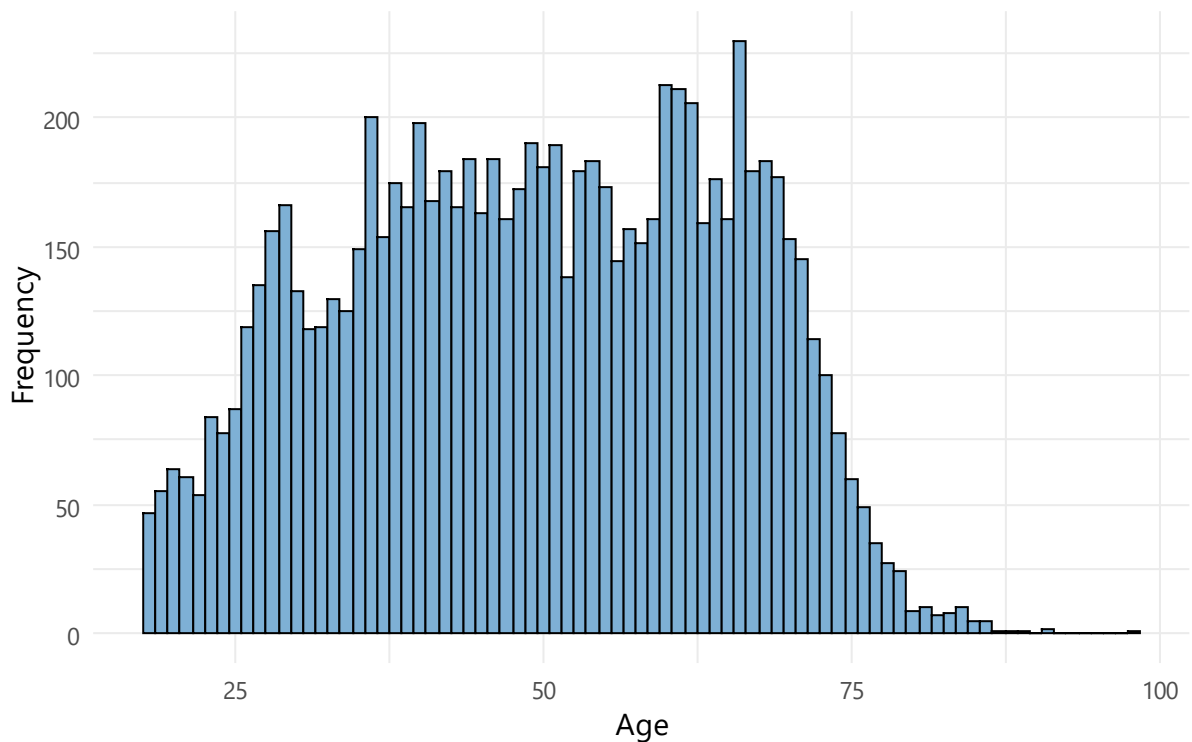
Figure 13. Distribution of age.

4.3 Age-Period-Cohorts Effects

When doing research on the effects of political socialization, one is faced with the methodological challenge of accounting for the three confounded effects of age, period, and cohort (APC) which are hard to disentangle. First, differences in attitudes can be attributed to the effect of age. As people grow older or as they accumulate more experience, their political viewpoints or general attitudes change. Second, the current period an individual is living through also affects their political beliefs. One can think of attitudes towards Russia changing due to the war in Ukraine. The effect of generation or cohort remains and describes the effect of having lived through a certain time that left an impact for the rest of a person's life. This is the effect that can be attributed to socialization (Neundorf & Niemi, 2014: 1-2).

The problem in studying the effects of cohort is that one needs to account for all the APC effects simultaneously. Otherwise, it is not possible to ascribe an effect to one of the factors age, (A_{it}, where i indicates individual and t time), the current period (P_t) or cohort (C_j, where j signals a specific cohort). This is because the three effects can explain each other perfectly (Neundorf & Niemi, 2014: 4):

$$C_j = P_t - A_{it}$$

To clarify with an example, the birth cohort 1998 can be described by the current period, 2022, minus the current age of an individual, here 24.

To solve this issue, restrictions must be set on the APC factors. One strategy is to categorize birth years into cohorts themselves, e.g., in 5-year groups. Another strategy is to remove one of the factors completely, arguing that it does not affect the outcome. A transformation of age, period or cohort is also possible, for example by squaring age. A final approach can be to use a "'proxy' variable [...] that assumes the cohort or period effects are proportional to certain measured variables." (Neundorf & Niemi, 2014: 4)

How does this apply to the puzzle of measuring exposure to authoritarianism and its effect on conspiracy mentalities? The assumed effect could theoretically be caused by either three of the APC components. One could assume that age is the determining factor for beliefs in CTs and that, for example, older people tend to believe more in CTs. The current period and its political events could also shape a person's believes in CTs. Take the Covid pandemic, undoubtedly did it affect the proportion of people believing in CTs. However, this research is only interested in the cohort effect. How is it possible to disentangle the effect of cohort from the other two factors?

Faced with a similar puzzle, Pop-Eleches and Tucker (2014) analyze the effect of exposure to communism on support for democracy and capitalism. They are not able to answer their question in a "traditional APC approach" (p. 78) using different iterations of the same survey asking the same questions because no data exists. Instead, they solve the APC-problem using "[...] *a priori historically defined cohorts that exist in all of [their analyzed] countries*, but *not during identical time periods*." (Pop-Eleches & Tucker, 2014: 78, emphasis in original) The historically defined cohorts, in this case, are determined by the time and duration of communism in their countries of analysis. First, this creates "cross-country variation in exposure to communism" because some countries will have more and some less exposure to communism. Second, the "within-country variation" will also be higher. For example, "[...] 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 [...]" (Pop-Eleches & Tucker, 2014: 78). This allows for the age effect to be disentangled from the cohort effect.

For the research question at hand, the situation is similar. As seen before (figure 7), the periods during which countries are autocratic vary and therefore people of the same age have experienced vastly different amounts of time under autocratic rule. Also, some countries experienced no authoritarian regimes since WW2 (e.g., Denmark) and can work as a control group. This weakens the conflict between age and cohort.

The period during which the Reconnect 2019 survey was taken could still influence the results meaningfully. Why could this be a problem? In a most unfortunate situation, political events in country A, which has a history of uninterrupted democracy, stimulate conspiracy theories, while country B, which has a history of autocratic regimes, lives through a time of relatively few conspiracies. The regression analysis might not show a significant effect of exposure to authoritarianism because of the *period* the survey was taken in and not because the effect does not exist. In the present case, this is unlikely for two reasons: First, all countries are in the EU and experience, at least to some degree, the same political events. Second, conspiracy mentality is assumed to be less volatile than the attitudes examined by Pop-Eleches and Tucker (2014). By its nature, a conspiracy mentality is less influenced by current events, other than for example, specific CTs. Furthermore, variables that can reasonably be expected to influence the dependent variable are included as controls and further reduce the APC problem.

Still, the APC-problem should be kept in mind. And, nevertheless, there is the possibility to conduct future research, using different iterations of the same survey to further test the relationship.

4.4 Regression Equation
Concluding the research design section, the final regression equation looks as follows:

$$CM = \beta_0 + \beta_1 * f(country, age) + \beta_2 * age + \beta_j * control\ variables_j$$

The function f changes depending on the hypothesis as described in section 4.2.3. As one way to account for the APC effect, age is included in the analysis for all hypotheses.

A further limitation must be met to account for the clustered nature of the data. Regression models assume that model errors are independent of another. This assumption, however, is not met with the present data because country-context is expected to influence the answers to the survey meaningfully. If not accounted for in the analysis, the standard errors will be

lower than they should be, and effect significance will be overestimated. The regression models will take this into account by using *cluster-adjusted standard errors*.

5 Analysis

5.1 Bivariate Correlations

The correlation matrix (see figure 14) enables a first overview of the bivariate correlations between the main variables. Starting from the bottom left, the different variables on beliefs in CTs expectedly correlate with another, as already seen in Cronbach's Alpha earlier. Looking at the control variables, a slight negative correlation between higher education and the conspiracy variables becomes clear. Higher education is also less common with higher age. Religion especially correlates with the second survey question on beliefs in CTs[6]. The five different exposure variables seem to not correlate at all with any of the conspiracy items. They show an unsurprising relationship with age, as their construction includes the age variable. The recent exposure variable correlates negatively with age. A slight correlation is seen between religion and the exposure variables, hinting that the formerly authoritarian countries from this sample are more religious today. All exposure variables also correlate strongly among another.

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[6] "The real truth about **vaccines** is being kept from the public"
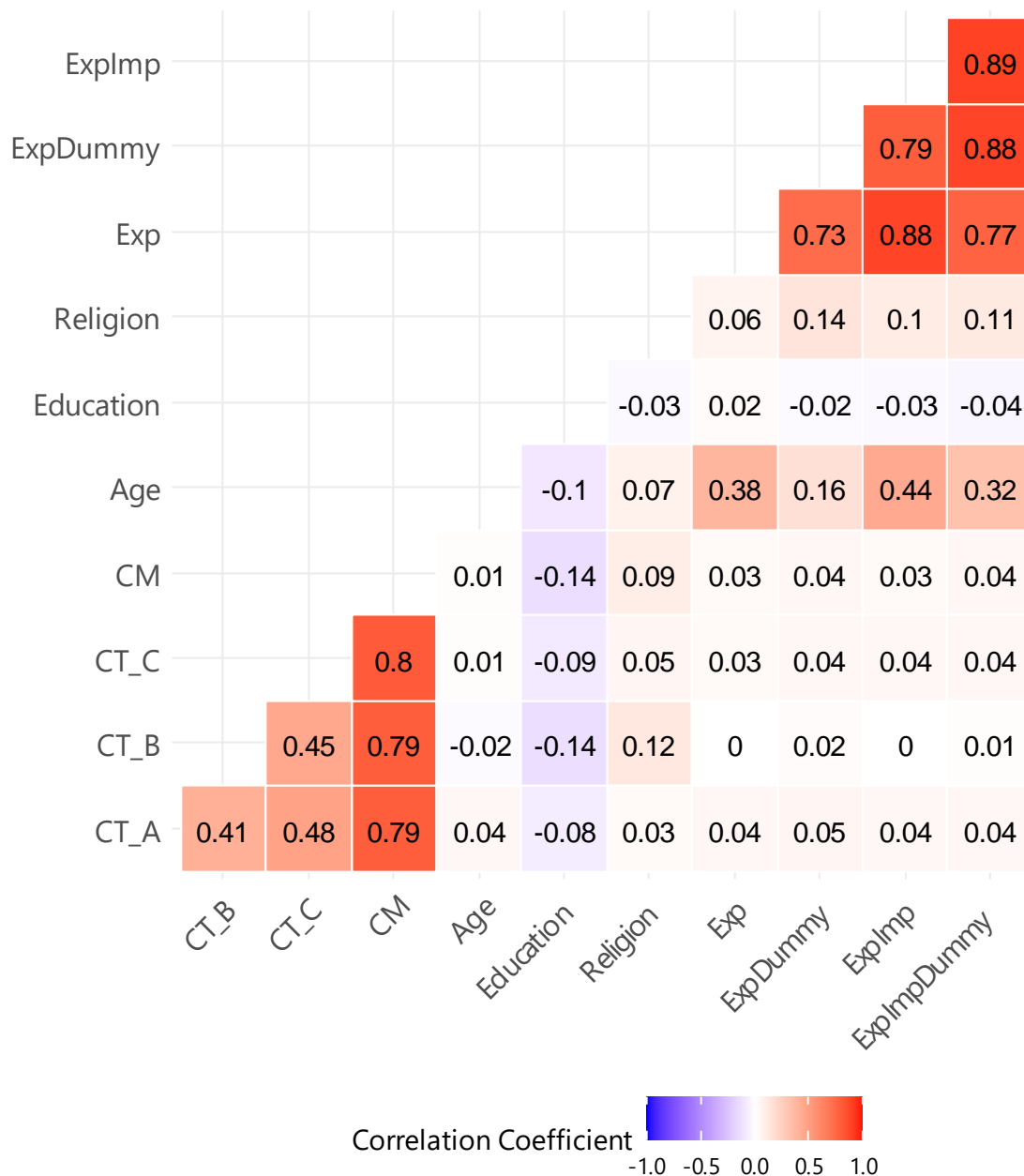
Figure 14. Correlation matrix of all included variables.

5.2 Regression Analysis

5.2.1 Hypotheses

This part will showcase and interpret the results of the different linear regressions performed. All the hypotheses will be tested in two steps: First, *only* including a version of the exposure variable as an independent variable and second, adding the control variables. Starting from H_1, the first two models will show the effect of a dummy exposure variable and then move on to models 3-4 for H_2 and a continuous variable. Exposure during impressionable years in H_3 will be tested in multiple versions in models 5-8 with a dummy

and a continuous exposure variable. The final hypothesis H_4 will be tested in models 9-10, with a dummy variable for recent exposure. Finally, additional analyses on West and East Germany, as well as country-specific effects will be discussed.

The first two models can be seen in table 6. In model 1, no significant effect of the exposure variable can be observed. The adjusted R^2 is very low, with an explained variance of under 1%. Model 2 includes the control effects which were left out in model 1. No significant effect for age can be observed, while gender, education, and religion show to influence CM. From this sample, males tend to have a lower CM than females, though with a p-value of only under 10%. Education has a significant as well as strong negative effect on CM. A step from low to high education decreases the conspiracy variable by a more than a whole point from the scale of 1-10. Religion is significantly positively related with CM. That means people who self-assessed as more religious also have higher values on CM.

What do these two models mean for H_1? The exposure dummy has no significant effect in any of the two models. A non-significant relationship means that based on the sampling error, it cannot be determined if the observed effect is only due to sampling error. Furthermore, the effect observed within the data is not very high. A person exposed to autocracy in their lifetime only has a 0.17 higher predicted CM value when including necessary controls. This is, for example, about a third of the effect of one step on the education variable. This means H_1 needs to be rejected.

Table 6. Effects of dummy exposure variable on CM.

Variable	Model	
	(1)	(2)
Constant	4.52***	5.73***
	(.26)	(.17)
Exposure dummy	0.22	0.17
	(.25)	(.26)
Age		0
		(0)
Gender: Male		-0.23.
		(.1)
Education		-0.52**
		(.12)
Religion		.06**
		(.01)
Observations	8,734	8,734
Adj. R^2	0	.03
Note:		*Cluster-adjusted standard errors in parentheses*
		.p < .1, *p < .05, **p < .01, ***p < .001

Testing for H_2 in table 7 shows a similar picture. Model 3 shows no significant effect of the continuous exposure variable on CM. Adding the control variables in model 4 reveals the previously observed effects. Gender, education, and religion have a significant effect on CM with almost identical values compared model 2. The continuous exposure variable again does not go beyond the threshold for significance. The effect size is lower compared to models 1 and 2, which makes sense as the range goes from 0-61 instead of 0-1 (see table 5). Again, ignoring the significance, this would not be a very impressive result either. 10

years of exposure to autocracy only increase CM by 0.06/0.07, an almost ignorable effect. H_2 needs to be as clearly rejected as H_1.

Table 7. Effects of continuous exposure variable on CM.

Variable	Model	
	(3)	(4)
Constant	4.57***	5.82***
	(.21)	(0.3)
Exposure continuous	.01	.01
	(.01)	(.01)
Age		-0.01
		(.01)
Gender: Male		-0.22.
		(.11)
Education		-0.52**
		(.12)
Religion		0.06**
		(.01)
Observations	8,734	8,734
Adj. R^2	0	.03

Note:	*Cluster-adjusted standard errors in parentheses*
	.p < .1, *p < .05, **p < .01, ***p < 0.001

H_3 is tested in two steps with two models each (see table 8). First with a dummy (models 5-6) and second with a continuous exposure during impressionable years variable (models 7-8). Neither of the four models show any significant or strong effect for the exposure variables. The effects of the control variables are very similar to the previous models. H_3 needs to be rejected as well.

Table 8. Effects of dummy exposure during impressionable years variable on CM.

Variable	Model			
	(5)	(6)	(7)	(8)
Constant	4.55***	5.77***	4.56***	5.82***
	(.23)	(.32)	(.22)	(.30)
Exposure during impressionable years dummy	.18	.14		
	(.23)	(.26)		
Exposure during impressionable years continuous			.02	.02
			(.02)	(.03)
Age		0		0
		(0)		(0)
Gender: Male		-0.23.		-0.22.
		(.1)		(.1)
Education		-0.51**		-0.51**
		(.12)		(.12)
Religion		.06**		.06**
		(.01)		(.01)
Observations	8,734	8,734	8,734	8,734
Adj. R^2	0	.03	0	.03

Note:	Cluster-adjusted standard errors in parentheses
	.p < .1, *p < .05, **p < .01, ***p < 0.001

Finally, the results of the regressions for H_4 are displayed in table 9. Recent exposure to authoritarian regimes also seems to not have any meaningful influence on CM. Again, a similar conclusion as for the previous models is appropriate and the hypothesis needs to be rejected once again.

Table 9. Effects of dummy recent exposure variable on CM.

Variable	Model	
	(9)	(10)
Constant	4.57***	5.70***
	(.21)	(.36)
Recent exposure dummy	.17	.17
	(.21)	(.2)
Age		0
		(0)
Gender: Male		-0.23.
		(.10)
Education		-0.52**
		(.12)
Religion		0.06**
		(.01)
Observations	8,734	8,734
Adj. R^2	0	.03

Note:	*Cluster-adjusted standard errors in parentheses*
	*.p < .1, *p < .05, **p < .01, ***p < 0.001*

5.2.2 Additional Analyses

As an additional control, the model of H_2 was repeated with a different operationalization of the democracy variable. Instead of looking at only liberal democracies as non-democracies, the operationalization was relaxed, and other regime types included as non-autocracies. This analysis can be found in appendix A and showed almost identical results to the operationalization used above.

A special analysis was conducted for Germany, which was divided into a democratic West and authoritarian East until 1990. Two analyses were made: First, the previous models were repeated to check if the results change, when coding respondents from the East as exposed to autocracy before 1990. The results are very similar to the results presented above and show no significant effect for any of the exposure variables (see appendix B).

Second, an analysis was conducted looking only at a difference within-Germany. Table 10 shows the result of this analysis. Model 1 tests the effect of a dummy exposure variable, model 2 of a continuous variable, and model 3 of a dummy exposure during impressionable years variable. Models 1 and 3 do not reach significance, however, show stronger effect sizes than models 2 and 6 (0.166/0.137), respectively. Interestingly, model 2 shows a significant effect for the exposure variable. This means that respondents from the East of Germany who experienced many years of autocracy will score higher values on the CM index. In this model, age is for the first time significant, though at a very low level and with a weak effect size. The control variables mostly resemble values of the whole data set from previous models. Males continue to believe less in CTs, and religion still correlates with higher beliefs in CTs. Education is still a negative predictor of CM with an even larger effect size, however, loses its significance in this model.

What could this mean for the analysis? After all, the coding that was used to determine country of birth is vague at best. Migration within Germany has been high since the reunification and respondents who now live in the East did not necessarily grow up there. Furthermore, the sample size is rather low, with just 257 respondents from the East. Also, there are a lot of other factors that might explain the differences between East and West Germany that are not part of this analysis.

Table 10. Regression with Germany coded into East and West on CM. Only within-country analysis.

Variable	Model		
	(1)	(2)	(3)
Constant	5.36.	5.49.	5.36.
	(.77)	(.48)	(.77)
Exposure Dummy	.39		
	(.18)		
Exposure Continuous		.03**	
		(0)	
Exposure During Impressionable Years Dummy			.39
			(.18)
Age	.01	0	.01
	(0)	(0)	(0)
Gender: Male	-0.41.	-0.40*	-0.41.
	(.05)	(.03)	(.05)
Education	-0.65	-0.66	-0.65
	(.26)	(.26)	(.26)
Religion	.05.	.05.	.05.
	(0)	(0.01)	(0)
Observations	1,234	1,234	1,234
Adj. R^2	.03	.03	.03

Note:	Cluster-adjusted standard errors in parentheses
	.p < .1, *p < .05, **p < .01, ***p < 0.001

Still, this could mean that there is an actual relationship between exposure to autocracy and CM when looking at within-country differences. In an additional model, the regression that

reached significance was repeated, this time only looking at within-country differences between people who were and who were not exposed to autocracy in appendix C. However, these results were not reproducible in other countries. The exposure variable only had a significant effect in Italy, this time, however, in a negative direction, suggesting that individuals exposed to autocracy show a lower CM than individuals not exposed to autocracy. This could be evidence of an intervening variable at play, as different countries show opposite effects. After all, the study was designed for cross-country comparison.

6 Discussion and Conclusion

This thesis started with a puzzle: What explains the variance and volatility in beliefs in CTs across countries? The theory section offered a possible explanation for the context effect of past regime type on current degrees of CM. Features of non-democratic countries were expected to leave a mark on their citizens and make them more vulnerable to beliefs in conspiracies today. To test these expectations, regression analyses were performed using the *Reconnect 2019 EP Election Panel Survey* and the RoW index. In multiple regression models, the influence of exposure to autocracy was tested. Various operationalizations of the exposure variable were used to conduct this analysis, including exposure as a dummy and continuous variable, exposure during impressionable years, and recent exposure.

The results of the analysis were conclusive. None of the hypotheses could be accepted across any operationalization or model. Leaving significance aside, the observed effect sizes were modest and adjusted R^2 values low. Only when looking at a within-Germany comparison, a significant effect could be detected for one model. This insight, however, is limited by a vague operationalization and many other possible explanations for the difference between the former East and West of Germany.

Concluding, the presented hypotheses need to be rejected in their current form. It is important to transparently communicate this, null findings also contribute to the research process by highlighting where there are *no* relationships. Still, an analysis of possible improvements for future research can be helpful. In the following, I will discuss conceptual and statistical considerations for a related analysis.

The operationalization of the dependent variable, CM, could be improved. The questions from the Reconnect survey were not ideal to measure general tendencies to turn to

conspiracies for explanations. As outlined in the operationalization already (section 4.2.1), question C is formulated with a focus on the EU and could be affected by anti-EU resentments that vary between countries. It is also very specific and might fail to live up to the metrics set up for a solid question to measure CM. Another problem with these questions is apparent in the deviation from the normal distribution, which violates regression assumptions to some extent. Maybe a different scale could reduce this problem, leaving out a middle category. Furthermore, more questions could be included. In future research, a dedicated questionnaire, like the CMQ, could be used. Still, this operationalization did not completely fail the objective of measuring CM, as the control variables mostly showed the expected relations, reflected in the consistent effects of education and religion.

Second, the operationalization of autocracy could be specified. The approach of this thesis to use democracy indicators and use the reverse to categorize autocracies could be replaced by an approach genuinely measuring features of autocracy, instead of the absence of democracy. These indicators, however, are not as common and available as the broad data available on democracy indicators.

Other control variables could also be admitted, e.g., measures of political orientation or extremism. Also, the broad research on individual psychological factors could be incorporated by analyzing for character traits. As brought forward by Schlipphak et al. (2021: 12), the current research design cannot exclude that country differences are based on differences in individual attributes rather than country context. Including character traits to the analysis could shed light on this potential issue and be a step towards "bridging the great divide" as pledged for by Butter and Knight (2015).

Apart from operationalization, the research design itself could be adapted. The case selection was imperfect, including only few countries with a relatively recent history of autocratic regimes. This reduced variation in the exposure variables. An analysis beyond the EU would also be interesting, especially when it comes to current autocratic regimes.

Additionally, the APC problem could be more in focus of further research. The most obvious approach to this would be to repeat the analysis after the next EP election, when a similar

survey will be conducted, and the effect of cohort could be singled out more sharply by including period into the analysis.

Research into CTs has never been more important than now. The Covid pandemic came with a sharp rise in the spread of CTs that can result in harmful effects for society. As figure 6 shows, the problem of country variation remains largely unexplained and there are significant differences in the proportion of people who believe in CTs. Context effects can be a potential tool to explain these variations and provide an important path forward for research into conspiracies. However, more scholarly attention towards context effects is needed to gain a better understanding of how they work.

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