Master's thesis

NTNU
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The Effect of Refugee Related Violence on Political Trust in the Global South and Europe

A quantitative study of the influence of violence by and against refugees on political trust in Afrobarometer and European Social Survey participating countries

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Abstract

This thesis examines and investigates the effect of refugee related violence on political trust as made possible by the use of a novel and not yet used dataset on refugee related violence: the *Political and Societal Violence by and Against Refugees* (POSVAR) data set. In combination with Afrobarometer and European Social Survey rounds I construct a political index scale and approximate a panel data set including both surveys for the period 2000 to 2016, which thus in combination with POSVAR takes aim at alleviating two concerns in the literature. First, the lack of emphasis on the region of the world where most refugees are indeed hosted: namely Africa. And secondly, the absence of a large cross-country analysis of the potential influence of hosting refugees on political trust through a concrete mechanism. Through utilizing POSVARs different measures which distinguish between both who the violence is perpetrated by and against, several effects and hypotheses may be explored, and I investigate possible findings inductively.

Ultimately however, the only specific measures producing convincing results is the influence of non-state actor violence against refugees on political trust, and the influence of terrorist attacks against refugees on political trust, and only within the Afrobarometer subset of data. The findings are robust to controls and impart a significant and strong effect on political trust, likely indicating that this form of violence is associated with the state no longer holding a monopoly on the legitimate use of force where such violence takes place, in conjuncture with the likely possibility of non-state actors and terrorists deliberately targeting refugees because they are vulnerable and so-called soft targets.

My findings are otherwise unable to disprove a scapegoating hypothesis and must eventually conclude that the data at hand is insufficient to produce significant and conclusive results on the basis of the remaining other refugee violence related measures – such as government and civilian violence, by and against refugees – and their effect on political trust.

Preface

I am grateful for the faith and trust people have placed in me, as well as for the help they provided me. They all deserve the respect of being mentioned explicitly. They will always have my gratitude

Integral to this project is my advisor Halvard Buhaug and the opportunity to participate as an associate student on PRIO's TRUST-project. My turn somewhat away from an emphasis on public policy to an emphasis on conflict through this project can, even at the best of times, be described as a bit slow as the project became the victim of my tendency to be chronically overcommitted to a number of other, albeit relevant, projects. For this I must apologize: for though he was generous in both, I should have burdened my advisor's cognition more, and much less his patience.

Yet at the same time I am infinitely grateful for the opportunity to participate in those very projects. Especially to Christoph Dworschak who trusted me to contribute to not just one, but two projects. Whether it involved reading several hundred papers in conflict journals or assessing security force response – I have learned immensely. Furthermore, to Anna Gora and Pieter de Wilde for involving me on the UnRep-project. Also, to Charles Butcher for including me in regular Violence, Instability and Peace seminars. Lastly, but not least, also to the institution itself: NTNU, for trusting me with the responsibility of being a teaching assistant for four of the five years, where I found socially rewarding work and much to learn also.

I am otherwise not sentimental. The conclusion of degree is to my enjoyment, but I do not consider this an end to a chapter or anything of the sort. I have no illusions: in the future I will have to apply myself to learning more and working harder, utilizing what I have learnt thus far towards that purpose. No pats on the back yet – *if ever*, for it is in this mode of thought that I find happiness. Therefore, I hope that on this final note *in this project*, more will begin than will end.

One individual deserves special praise though: Hermione, my cat. The *excellent kitty* herself, very *nearly* – due to an unfortunate tie-breaker – *officially* Trondheim's finest cat. It can be difficult to discern her precise role and relevance to this project; it is mysterious, … but it is important.

All errors are my own.

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1. Introduction

The amount of forcibly displaced people has been increasing steadily over the past few years and is according to the end-of-2020 UN Refugee Agency report, it is at its highest in three decades (UNHCR, 2020): 82.4 million people in total of which about 20 to 30 million – depending on the definition used – are refugees. Since, and in time of writing this remains somewhat unclear, the Russo-Ukrainian war has displaced several million people already (UNHCR, 2022). While this recent – and in our time unprecedented – crisis has shown a heartwarming capacity to welcome refugees in many European countries in particular, it should nonetheless serve as a stark reminder of the reality faced by the world's *other* refugees. Indeed, this total stock of refugees amounts to the equivalent of Ukraine's total population, and the majority of these refugees are not welcomed by wealthy European states with strong institutions and substantial state capacity: as of 2020, 85% are hosted in developing countries and 27% in the least developed countries (UNHCR, 2021).

Research on the effect of *receiving* refugees on a host countries has received some attention in relevance to security questions and international or transnational implications, as well as for its relevance for security and economic conditions. However, research on the *attitudinal effects* on host countries, and corresponding effect on political trust among individuals, has been primarily focused on western countries (Godefroidt, Langer & Meuleman, 2017). There are naturally ample reasons to suspect that findings in western countries may not translate well to the least developed, and as natural are the challenges to gathering data, both on refugees and their origins as well as attitudinal data in each host country. To contribute to this lack of research, I situate myself in relationship to two prior projects: Utmo (2021) who examined the direct impact of refugee presence on political trust via geocoded data from Kenya and Tanzania; and Grønset (2021) who examined the effect of refugees' ethnic makeup on institutional trust in a host country, using Lebanon as a specific case. The geocoded approach enables inference that comes closer to identifying refugees as the cause of changes in attitudinal effects, and the ethnic makeup is considered a likely component in what may or may not compound the effect of refugees. However, what is lacking is a cross country analysis of a mechanism which through the effect of hosting refugees works on attitudinal effects.

This thesis aims at alleviating this, by utilizing the newly produced *Political and Societal Violence by and Against Refugees* (POSVAR) dataset (Gineste & Savun, 2018) in combination

with the most recent Afrobarometer survey rounds to examine how refugee related violence affects political trust. The POSVAR-data has not seen much use[1] towards this purpose and the effects as they relate to refugees on political trust remains understudied. This novel data therefore enables this project to shed more light on the impact of refugees through a seemingly easily discernable mechanism that is likely to influence political trust: refugee related violence. The POSVAR-data does however include a number of measurements of refugee violence and examining all of them and their relevance will be a somewhat inductive project. As such, I do not formulate an explicit hypothesis, but will endeavor to explain and answer the following question: *How does refugee related violence influence political trust?*

As the POSVAR data spans from 1996 to 2015 on the country level, I am able to match this with four rounds – R6, R5, R4 and R3 – from the Afrobarometer survey. My emphasis and primary interest is in accordance with the broader goal of the TRUST-project which I relate my work to, where I am partly motivated by the lack of research on refugees where they are, namely Africa and the global south as stated. Fortunately, the Afrobarometer survey is from and including round 6, representative for 75% of Africans. Unfortunately, there is nonetheless a tendency for the less democratic countries to self-select away from participation.

However, as we shall see, we may have reason to doubt the precision of both the POSVAR data and Afrobarometer surveys as we aggregate from the individual respondents in the Afrobarometer data to match with POSVAR on the country level. Both in terms of timing, and overall observations, the data will prove insufficient to produce significant results. Therefore, I also included all rounds for the European Social Survey (ESS) as a best case scenario for attitudinal data. I spend less time visualizing the ESS data for this reason, and ultimately the ESS data proves as difficult to extrapolate convincing results from as the Afrobarometer data.

Regardless, due to the irregular interval of survey rounds – which are visualized in a convenient graph the data section – and POSVAR being on a country-year level, survey data must be aggregated up to the country level as well. I construct an index to capture political trust as a concept and primary dependent variable, and I assemble a data set to be reminiscent of panel data including yearly data on refugee population, population, GDP per capita, conflict and peace years; while the data which makes the basis for the dependent variable of political trust is still

[1] To my knowledge and in time of writing, I believe it has not yet seen use at all.

much too irregular to be treated as a true panel and I can only fit a relatively basic pooled-OLS model. Nonetheless, the methodological concerns in this project are extensive and intricate – and suffice to say, in social science, causal relationships between variables of interest interact and influence each other in such a way that while the statistical inference might seemingly be easy, the causal logic and concerns are not. I therefore spend a significant amount of time and space on discussing these aspects, as well as visualizing them in directed-acyclic-graphs. I hope that this will make it easier for the reader to follow my thought process, as causally, there will be a number of moving parts, and I will repeatedly stress the point that causal inference and statistical inference relate but are ultimately distinct.

Findings are quite limited. The data is simply not there, and we may have reasons to suspect that while the POSVAR dataset aims to avoid underreporting through the use of an ordinal scale, it nonetheless underreports somewhat, perhaps especially within the context of Africa. This is unfortunate, as it limits the project's ability to investigate the effects of refugee related violence where there are in fact the most refugees present in our world – a necessary condition, to say the least.

Regardless, findings show a negative and significant influence of both non-state actor violence against refugees, and terrorist attacks against on refugees, on political trust. The findings are robust to a number of considerations and specifications, and we can be relatively confident in this finding. As to why, I posit theory suggesting that non-state actors and terrorists select "soft targets" and that this, in turn, being detrimental to the notion of the state having a monopoly on the legitimate use of force results in a strong and adverse effect on political trust.

1.1. Structure and outline

I first define the term refugee and expand on what we typically mean by trust, before making a brief elaboration on why trust and political trust matters and touch briefly on political theory and political philosophy in this regard – but ultimately this project is not in the realm of political theory or political philosophy and as such I will be relatively brief and only do the elaboration justice as far as is necessary to explain how trust remains in the realm of the ideational.

I then move on the theory broadly speaking, introducing the concepts of, and distinction, between cultural theories and institutional theories, before gradually presenting literature as it relates more closely to the relationship between first refugees and political trust, then violence and trust, before finally expanding what is in the end an albeit somewhat uncertain framework of this understudied relationship on which I posit my findings.

Thereafter follows an introduction of the data, its structure and I begin with the dependent variable and index, then the explanatory variables of interest, control variables and summary statistics. Then I present the method and research design, discuss some limitations in this regard and then present OLS-regression and its assumptions and conditions necessary for valid statistical inference. Then lastly in this section, I extensively debate the potential for endogeneity effects, post-treatment bias and suppression effects as they relate to this project.

Then follows results for all variables of interest, first in a basic bivariate specification before moving on to multivariate with controls and establishing some inductive hypotheses and further questions to scrutinize. This produces another three causal relationships to examine statistically more closely, which takes a form similar to a robustness check although we are decidedly inductive and exploratory in the approach towards these questions. I debate considerations underway, but ultimately debate the findings themselves proper only in the discussion points. Here, I show how a simple database search and comparison gives us some reason to not be entirely pleased with the POSVAR data, before turning to the positive aspects and debating and presenting the key findings.

Then follows a brief conclusion.

The appendix is quite extensive, showing the necessary statistical tests for the political trust indexes in the form of correlation matrices, factor analysis and Cronbach's Alpha test reporting. Also included is a manual comparison of the Afrobarometer survey questions.

2. Theory

In this segment I will present first a brief description of definitions, but only so far as they are necessary. Thereafter, I present theory relating to first trust and its relevance to institutions and political institutions, starting broadly with the roots from political philosophy and moving on to the more concrete approaches to explaining trust: cultural theories and institutional performance theories. This concludes the trust-and-institutions side of the relevant framework, before moving on to the aspect of violence and situating the thesis in the more empirical and modern literature on violence by and against refugees.

2.1. Definitions

2.1.1. Forcibly and internally displaced people, refugees, migration

The term refugee is distinct from other terms that relate to migration as the term refugee, is a specific definition as defined by The United Nations High Commissioner for Refugees (UNHCR) which describes specific rights and considerations for "someone who is unable or unwilling to return to their country of origin owing to a well-founded fear of being persecuted for reasons of race, religion, nationality, membership of a particular social group, or political opinion" (CSR51). It is thus a specific legal definition. A refugee is therefore a specific subset of forcibly displaced people, and indeed it is explicitly intended to avoid confusion with the term *forced migration* (UNHCR, 2016); as forced migration may not include fear of politically motivated harm, and in the perspective of The United Nations, this does not warrant the same set of well-defined legal obligations (UNCHR, 2016). The term refugee also explicitly excludes *internally* displaced people (IDP) who are forcibly displaced within their own country. This definition is thus a furthermore specific subset and distinct from the term migration, as migration involves motivations for relocating which are much broader than for fear of politically motivated harm, thus constituting a general category of anyone relocating themselves across international boundaries for any reason (Saleyhan & Gleditsch, 2006). Regardless, I am following the example of Salehyan & Gleditsch (2006) more generally in the sense that while there is in this regard practically only one true definition, I agree with the intuition that we may in practice and daily conversation think of refugees as "anyone who flees a country of origin or residence for fear of politically motivated harm" which is by and large identical, but lacks some of the brevity in the explicit UNCHR definition which emphasizes the criterion of fear being well-founded and

persecution – a word which conveys a different connotation than just politically motivated harm alone. Regardless, the criterions of both the practical and formal definitions, while seemingly precise, do produce some ambiguity in the real world and complications exist for certain groups of refugees, such as UNRWA refugees which are constituted by Palestine refugees and descendants of fathers displaced in the 1946-1948 conflict (UNRWA, n. d.); and Venezuelans displaced abroad (as the reasons for being displaced are compounded).

2.1.2. Social trust, institutional trust & political trust

It is necessary to at first make some distinctions between trust as a term as well. Suffice to say for now, these terms are closely linked but we may distinguish between social and political trust, and in some contexts a distinction is also made in regard to institutional trust. Firstly, social trust is simply trust between people. Political trust on the other hand can be defined as "the trust individuals have in their state-wide legal-political institutions and actors" (Berg & Hjerm, 2010). Put more simply, it could be understood as the trust the individual has in the aspects of the state that prop up the "rules of the game" in a given society. As such, the terms institutional trust and political trust are sometimes used interchangeably.

For the most, measures of trust distinguish between these various (and predominantly political) institutions and actors, when surveying for attitudes towards them. Intuitively it makes sense that trust in – for example – courts (being a broader system) is conceptually different from trust in specific actors (such as an incumbent president or particular party) – a distinction formalized as diffuse support and specific support, respectively, by Easton (1965). Others (such as Newton, 2007, in *The Oxford handbook of political behavior*) also draw a distinction between *trust* in people and *confidence* in institutions. However, despite this conceptual difference, empirical studies using factor analysis do not support any prudence in making such distinctions (Godefroidt, Langer & Meuleman, 2017); and broadly speaking, attitudes tend to cluster in such a way that attitudes towards one institutional aspect seems to influence another. For instance, the Afrobarometer questioner makes no such distinction between trust and confidence, asking only questions concerning trust (towards individuals and institutions alike) whereas the World Values Survey asks for confidence. As a result, there is not a consensus on precisely which attitudes relating to which political institutions one ought to examine, but at its core trust is – and as we shall see this really echoes Rousseau – crucial to the functioning of society and in particular

democracies as they cannot rely on coercion to produce reliable outcomes (Godefroidt, Langer & Meuleman, 2017).

The difference between political trust and institutional trust is not necessarily intuitive. Political institutions (we may again look to the example of courts) are of course institutions, but that is not to say that *all* institutions are political institutions. Institutions in a society are not inherently and directly political in their nature, such as a handshake, marriage or academic tenure. They do however often become the subject of political debate and may end up directly relating to political life and in turn political institutions may shape these institutions just as much as these institutions shape them. While this may be something of a given and common knowledge, it is nonetheless important to stress as most empirical data indicates that what is commonly referred to as generalized trust (i.e.: trust in others, often captured in a questioner by asking questions as "do you have to be careful when dealing with others" or "people keep their word") correlates quite well with trust in political institutions. This is important because it presents an immediate causality issue as when we examine the effects of violence as it relates to refugees, as firstly, violence can be motivated both on entirely social grounds as well as on political grounds; and secondly, refugee being a specific legal definition immediately invokes a political component. I will discuss what creates trust in more detail in a later segment but note for now simply the difference – and yet also relationship between – political trust, institutional trust and social trust as terms and concepts.

2.2. Briefly on why trust matters & trust in political thought

For many countries hosting refugees, it is more a case of institutional and political *dis*trust than it is a question of trust. Though for the countries in the very same region that finds itself hosting most of the refugees in the world, we find many developing economies and fledgling democracies, to which the role of trust is ultimately integral to the success on which the well-being and liberty of soon nearly one and a half billion people hinge. And while the debate concerning whether economic growth or democratization comes first is endless, given that institutional trust is considered vital for booth democracy and development, this is all the more important in the countries where distrust may erode the very institutions necessary to uphold further democratization. Indeed, trust can be understood as the essential element to democracy through being at the source of all legitimacy (Mishler & Rose, 1997); and in a society lacking

trust in political institutions, most use of power will be seen as illegitimate rather than rightful and with authority. Rousseau ([1762] 2008, pp. 16) phrases this elegantly when he states that "the strong is never so strong as to always be the master, unless he transform that strength into *right*" and similarly the elected incumbent in a society is not truly exerting leadership of that society unless the population has sufficient trust in the institutions upholding this arrangement – whether it be elections or a supreme court – to make the rule legitimate. Which is to say that in a country where trust is absent, might makes right, whereas in a country where trust enables democratic and inclusive institutions, right makes might.

The relevance of political theory and political philosophy may be questioned, but the philosophical roots associated with understanding of the role of trust in society and how it relates to political life is deserving of an albeit brief introductory mention. For irrespective of how empirically we study trust and its effects and causes, it remains somewhat in the realm of the ideational and while not necessarily directly applicable, political theory nonetheless shapes how we think about and approach concepts such as democracy, political institutions, rights, the legitimacy of violence, and so on. The same could be said for objections concerning whether or not it is apt or proper to apply a history of thought so decidedly western to a non-western context: these ideas shape how we think about these concepts. Suffice to say, nearly all prominent political thinkers identified trust as necessary for democracy and a well-functioning state: and to mention one, in *De Cive* Hobbes, in spite of his pessimistic anthropology, states: "Where's no trust, there can be no Contract." (Hobbes [1651] 1983, 8.3 [p. 118]); and yet more modern thinkers who advocate to do away with contract theory altogether, such as Fukuyama, identify trust as necessary for democracy and a well-functioning state (Fukuyama, 2014, pp. 97-100, 106-107; Fukuyama, 1995).

2.3. What creates (dis)trust – broad strokes

On the origins of trust, I draw primarily on the seminal work by Putnam, Leonardi & Nanetti (1993), *Making Democracy Work.* This work provides an excellent summary of the primary approaches to studying trust and argues we may first agree on two points: institutions can shape politics and institutions can be shaped by history. On this basis we may group theories around two broad primary categories:

First, *cultural theories* posit that shared cultural values and norms, especially those relating to a shared sense of nationality, forms the basis of interpersonal trust and forms the base on which institutional trust is placed and from there on influences the political sphere (Godefroidt, Langer & Meuleman, 2016). This is also the perspective to which we may associate liberal political philosophers such as Locke and Mill where Mill draws a direct similarity between trusting a fellow and political trust. In other words, trust between individuals due to a sense of shared identity or origin, determines institutional trust which is then determines the trust in political institutions – interpersonal trust is prior to institutional trust (Godefroidt, Langer & Meuleman, 2016). Worth noting for now is that while refugees intuitively may be assumed to not share a sense of nationality, states may have overlapping ethnic groups on which nonetheless shared cultural values persist across borders.

Institutional performance theories on the other hand adhere to the logic of rational choice where we assume that individuals respond to how successful and effective public institutions are at providing the goods, services or whatever it might be, and this in term decides trust (Mishler & Rose 1997). This is often the theoretical framework which a large number of concepts in political science are situated either within or against, where politics take place within a cycle where an impulse moves from societal demands (of voters); to political interaction, parties and parliament; to the executive or some form of political leadership or decision-maker; to administration, policy implementation and the role of the bureaucracy; before this impulse once again reaches society, societal demands and voters, and the process repeats. Depending on context, this basic model might include extra links in this chain of delegation, and it might be given different names and labels, but by and large they are all variations of this same basic model which describes the chain of principal-agent relations in political representation. This is especially relevant to the notion of a democratic process, though to be frank, political delegation and trust in the institutions relevant said political delegation, need not be democratic as such and

in certain contexts individuals have surprisingly high degrees of trust in seemingly non-democratic yet political institutions. For instance, even in the data we will eventually employ in this project, Russians trust the police more than they trust their politicians and parliament, and within well-established democracies there is on-going debate about how political trust is decreasing and studies on how individuals find new and alternative ways to feel represented (De Wilde, 2020). Regardless, this perspective can be quite demanding of constituents and the notion of a democratic process is for the most an ideal, Robert Dahl for instance stated: "no state has ever possessed a government that fully measured up to the criteria of a *democratic process* and none is likely to," (Dahl, 1998, p. 42). However, in this project we will be examining refugee related violence, and both relate to the utmost basic concerns of a functional state, as a state is expect to have a monopoly on violence – which unfortunately means potential for it to arbitrarily use it – yet also provide security and exert border control. The prior which refugees can be experienced as a threat to by individuals, and the latter by which refugees are legally speaking – and we could add morally – in the UNHCR definition diametrically opposite through having a right to freedom from involuntarily return to the country they face persecution.

2.4. Refugees and how they may impact trust

Whether we posit social trust as prior, posterior or compliment to political trust, is however is not the primary concern of this project and as such, I will simply begin by examining the influence of refugees on social trust. Many of these, truth be told, do not really require academic sources as they are relatively well known, but there is always a merit to proving something empirically. Though it is likely easy to imagine that social trust is shaped by how we relate to each other. It is often stated as a form of a given truism that Nordic societies are so highly trusting due to the high degree of ethnic and cultural homogeneity – which research by examples such Dinesten & Sønderskov (2015) find evidence to support. In social psychology we may utilize the terminology of in- and out-groups, where the in-group refers simply to a group the individual identifies with on some basis – often associated similarity – and places him or herself ahead of the out-group in some behavior (Sutton & Douglas, 2013, s. 351). Nonetheless this is not to presuppose that refugees always take the role of an outgroup and threat that does not share language and culture with the in-group, especially in the global south were perhaps arbitrarily drawn up to divide cultures, and indeed the Afrobarometer annual reports quite happily remark

that Africans are indeed accustomed to ethnic diversity and tolerant in this regard (ex.: Afrobarometer, 2016). These considerations relate primarily to the broader category of cultural theories, although some aspects relevant to institutional performance nonetheless reside in state capacity to effectively integrate refugees through language and the like.

While Putnam (1993) also distinguishes economic or material concerns as a possible third category, one might debate whether this is a unique category or a subset of cultural theories, given that many of the same aspects which motivate in- and outgroup-behavior is on the basis of relatively basic human instincts. Similarly, government performance might be gauged by an individual on the basis of how well it protects a seemingly valid interest for that individual – Brexit, perhaps a case in point. There is a great deal of research to whether or not refugee influxes in fact do bring positive or negative effects strictly economically, but while these discussions are important, this project concerns itself with data on individual attitudes aggregated to a country level, and as such, capturing the outcomes produced as they relate to the mechanisms to which we could trace how an individual feels or perceives his or her situation is difficult: any aggregate measure of economic conditions likely does not capture how an individual feels and as we shall see, it is only the recent-most rounds of the Afrobarometer data that surveys for such attitudes which might capture how an individual feels about and perceives his or her economic situation. Nonetheless, we should expect that with some economic security comes some trust, both social and political; and in turn, given that resources might always be perceived as limited even when they are not, refugees can be the target of immediate ire in host countries (Steinberg, 2008).

Political trust more specifically, and as we shall see, is more directly related to this project than the prior theoretical considerations as the index used, especially for the Afrobarometer data will be in its entirety based on institutions which directly relate to the quality of government and or its election. It is not immediately given that refugees can be or are perceived as a threat to these aspects, and precisely how and through what causal mechanisms remain debated, and to quote my forerunner on the TRUST-project, Utmo (2021) explicitly states that: “it is clear for this thesis, using newly updated refugee settlement data, just how unexplored institutional trust is”. Regardless, simply examining the effect of refugee presence on political trust leaves much to be desired as the mechanisms remain subject to debate. For instance, much hypothesized is the concept of scapegoating, which Fisk (2018) posits, and which

Choi & Salehyan (2013) also suggest, which is when refugees can take the role of a form of scapegoat or ostracized group for governments or certain political groups respectively. Fisk describes (2018) describes this precisely as follows: "attacks on self-settled refugee communities may be part of an overall attempt by governments fighting insurgencies to bolster support by punishing refugees – as an out group – for the host country's security problems". These intuitions have however not been examined more closely and the direction of violence (i.e.: refugee against government, non-state actor against refugees, and so on) have not been disaggregated from the broader measures of refugee violence – and as these intuitions suggest, we have ample reason to suspect that the intricacies of the violence perpetrated may place a significant role. It is after all, not a given that refugees are the victims of a government for the purpose of scapegoating, other groups may also systematically target refugees both because they can influence public opinion to their benefit, either directly or through someone to blame, or through simply being soft targets. While there may exist some literature on refugee recruitment into terrorist and paramilitary organizations, much less studied is how non-state actors and terrorists target refugees or why they would – and I believe it is not described as within the scope of the TRUST-project per se, but it would nonetheless be highly relevant. There is after all an extensive literature on the rational target selection of terrorists to increasingly include targets which are considered soft, which is to say easy to attack and not actively defended (Czinkotoa et. al 2010; Lambrechts & Blomquist, 2017; Wernick & Glinov, 2012); and terrorists and non-state actors might in fact be suspect to impart a double-effect on refugees: by in widespread influential control and attacks (such as in the case of ISIS) they are first the cause of refugees and yet also extensively target them thereafter precisely for having fled, or for simply being easy targets. Indeed, we may even extrapolate from the logic of Fisk (2018) as mentioned: if we can suspect governments to punish refugees as an out group on the basis of some motivation, so can non-state actors directly or inversely attack refugees precisely because it is the easiest way of producing a credible threat to security of the host country for the purpose of reducing perceptions of security in the population which is integral to political trust. It is not implausible that the host country might respond to such a threat by attacks on the refugees having fled terrorism and the influence of non-state actors due to them being possibly associated with terrorists on some basis such as country of origin or just due to terrorists frequently crossing borders.

As for the relationship between refugees and conflict, several studies examine this relationship (Salehyan & Gledtisch, 2006; Buhaug & Gleditsch 2008). Clearly, it should not be surprising that conflict and civil war can be detrimental to trust in all its forms, having the potential to cause social divides as well as to bring on the possibility of complete collapse of state capacity and again, refugees may be exceedingly vulnerable in such a context if already present. Furthermore, as we gradually approach theories which relate more to just violence, De Juan & Pierskalla (2014) find that exposure to violence reduces trust in the national government. Similarly, using rebel attacks in Mali, Gates & Justesen (2013) also find that violence significantly decreases trust in the executive, somewhat less so in the legislative and trust in local government is unaffected. In sum, these theoretical considerations will inform all choices of controls and variables to examine, but at its core we have every reason to assume that violence generally should influence political trust negatively, and as such I do not see fit to formulate a specific hypothesis towards that question as we will be examining *refugee related* violence.

2.5. On refugee related violence and its influence on political trust

It should be emphasized that the data – to be described in the next chapter – to be used to indicate refugee related violence in this project employs quite a few variables towards this purpose. Ten, to be precise. There are strengths and weaknesses to this, again as to be described, but suffice to say, given that we wish to examine the effect of refugee related violence on primarily political trust, we may be cautious in formulating explicit hypotheses and whether it is prudent to describe a hypothesis for every available measure. For instance, we could formulate that civilian violence against refugees influences political trust negatively and that refugee terrorist attacks influences political trust negatively, but in practice this is truly repeating minor alterations to the same broad research question: *how does refugee related violence influence political trust?* My approach towards answering that question will be slightly inductive and I will, after first fitting some basic model specifications using the many refugee-related forms of violence in the POSVAR data, present four hypotheses on an inductive basis which I then examine, discuss and explore in sequence. Nonetheless and as referred to, the potential for a scapegoating hypothesis is such a clearly debated concept that it can and will be examined directly and I will simply refer to it as the scapegoating hypothesis. Finally, as posited in the previous section, we have ample reasons to believe that refugees can in particular be the victims

of non-state actors and terrorists alike, and whether or not this is motivated by an ambition to create a fear and a threat perception, or to follow a motivation similar to that of government-scapegoating, it should have a detrimental effect on political trust. Even in the latter case, if a non-state actor somehow wins good will in a society by attacking refugees, it will likely do so at the expense of the government of the host country. As such, there is little reason to suspect that non-state actor attacks on refugees or that terrorist attacks on refugees can for any reason be particularly positive in its effect on political trust. Similarly, we should also expect to find that violence by refugees against civilians to invoke a strong feeling of an increasing threat and be detrimental to political trust, as border control and security are, as mentioned, the most basic functions of a government.

3. Data and method

In this chapter I begin by presenting the data which primarily motivates and forms the foundation of this project. Thereafter, I will elaborate precisely on what I use from these data sources and what alterations, transformations and processes I apply to this data and variables, beginning with the outcome, a political trust index, then the primary explanatory variables being various measures of refugee related violence and their direction between perpetrator and victim, and finally controls. Following this, figures showing time-distributions of the outcome and independent variable and descriptive statistics concludes the first section of this chapter. The second section then expands on the concerns as they relate to structuring the data and choice of method, before moving on to the motivation, benefit and challenges associated with the use of qualitative methods and OLS-regression with particular emphasis on what we encounter in a social science context such as this project. In short, this section describes choice of statistical methods and basic OLS-regression, but at the same time, and as I and this project will eventually make abundantly clear: when encountering a social phenomenon and the challenges to extrapolating from statistical inference towards causal inference, there is in a sense, truly no such thing as "basic". The hurdles to causality are still many in this project and quite complicated as they relate to each other, and ultimately, we cannot solve all these problems with the data at hand, but simply show an awareness towards them.

3.1. The data – broadly speaking

This project is enabled by the use of the Political and Societal Violence By And Against Refugees (POSVAR) data set produced and presented by Gineste & Savun (2018); and the Afrobarometer and European Social Survey rounds.

The POSVAR-dataset crosses certain hurdles to causality in relation to refugee related violence by providing specific data on who the violence is by and who the violence is against. In sum there are ten variables expressing directionality in refugee related violence: by government against refugees, by non-state actors against refugees, by civilians against refugees, the inverse of these, refugee against refugee violence, refugee riots and finally terrorist attacks by and against refugees. Thus, POSVAR enables us to examine the mechanisms and effects more closely through which refugee related violence might influence political trust. In order to identify

the level of violence for the variables in a given country, POSVAR authors searched a number of databases for the keyword "refugee": The US State Department Country Report on Human Rights Practices; Armed Conflict Location and Event Data Project (ACLED); Social Conflict in Analysis Database (SCAD); and the Sexual Violence in Armed Conflict (SVAC) dataset. After identifying events in the aforementioned databases and datasets, adjacent paragraphs were read, and variable specific keywords searched for in the following sources: USCRI World Refugee Survey; the United Nations High Commissioner for Refugees; LexisNexis; ReliefWeb and Proquest. (Gineste & Savun 2018). The relevant timeframe for POSVAR is relatively brief, ranging from the year 1996 and 2015, is sufficient to match with both Afrobarometer and ESS with the respect to all but the most-recent survey rounds.

The Afrobarometer aims at being an independent cross-national survey for informing and shaping African policy and decision making (Afrobarometer, n.d.). It includes an expanding list of African countries, growing from just 12 countries in the first round two decades ago to 34 in the most recent two rounds, and surveys for attitudinal data through a wide range of questions relating societal, cultural and political aspects. The European Social Survey is similar to Afrobarometer, aiming to chart "the social, political and moral fabric" of Europe (ESS, n.d.). It spans the same two-decade timeframe, albeit with more countries participating at both more regular intervals as well as more regular participation.

Note however that Afrobarometer and ESS are distinct where both survey methods, question phrasing and scale differ. As such, these are at best an apples-to-oranges comparison, and therefore I constructed a data set for each of the two geographical regions.

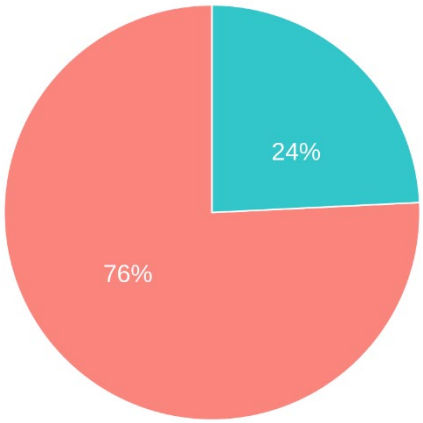


Fig. 1.: A pie chart showing the % of Africans represented through the latest Afrobarometer survey round

3.1.1. Afrobarometer & ESS – *Political Trust*

The dependent variable for this project is an additive index constructed with the intention of capture the broad concept of political trust through survey data. There are however

challenges to this, and in order to compare across years, survey methods must be similar, and question phrasing as well as answer options need to be identical between rounds in order to be eligible for comparison.

The ESS is quite easy to use in this regard, as respondents can place their answer on a very broad scale. This possibly enables greater variety to capture change – not necessarily a benefit in and of itself, but in contrast, the Afrobarometer respondents can in some cases "hit the bottom" which is to say respondents reply so negatively that adverse events in a country would not influence the survey results due to respondents being unable to select any lower answer options. This excludes the possibility of using the first difference – between-round change – rather than the absolute level, which would have had the benefit of automatically removing some potential structural confounders and sources of bias which examining *change* as opposed to an absolute measure with alleviate. However, the survey respondents "bottom out" on the individual level, not in the aggregate country level, and as such there is no immediate concern to the potential of the political trust variable being skewed to make an OLS-based approach inappropriate in the sense that outliers would inflate the mean. Still, we cannot justify using the first difference on a country level because the aggregate potentially *would show* change *if* it could on the disaggregate individual level. In other words, to think that a first difference measure is valid simply because it seems so in aggregate, could be very misleading in capturing change, nonetheless and thus the absolute measure must be used.

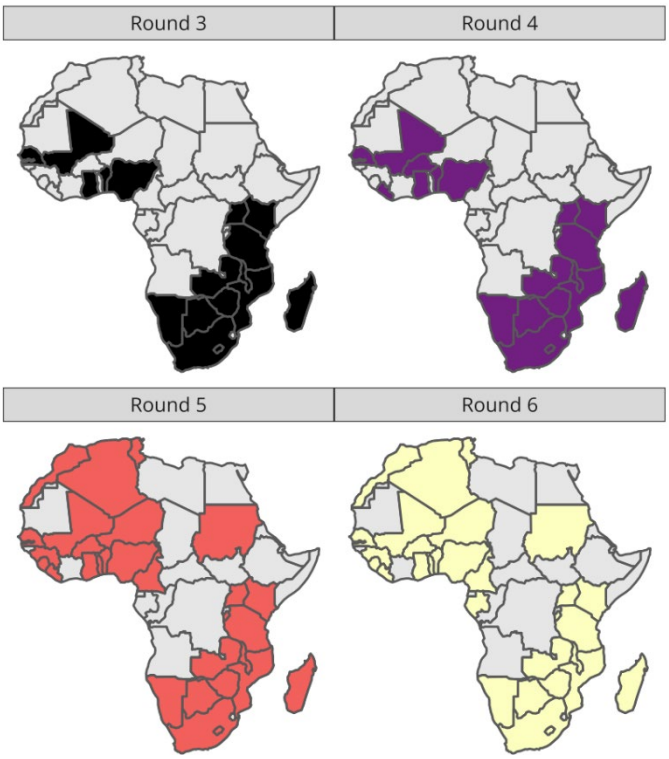


Fig. 2.: Maps of Africa showing the participating countries per round

Furthermore, survey questions should be identical between rounds if the intention is to combine data from multiple rounds – again, to avoid apples-to-oranges comparisons. Therefore, I manually compared every question which could be

used for the basis of a *direct* political trust index component, and I compared between all Afrobarometer rounds. Direct, as in questions which survey for trust in the political system and its core institutions: the executive, the legislative and the judiciary; and also, local assembly as it may play a similar role on a local level, and finally also the electoral commission. I am in no way the first to construct such an index and I thus follow the examples of other TRUST-project contributions such as Grønset (2021) and Utmo (2021), but also published articles such as Hutchison & Johnson (2011) or Godefroidt & Meuleman (2016) – and countless others.

There is however no real consensus on precisely which trust measures to include when a survey includes questions relating to wide variety of options. Therefore, my index, while primarily posited on the basics of political theory, is first and foremost intuitive and prudent, as we may relatively easily theorize on the spot reasons to *not* include say, trust in opposition parties (very diverse, could be many across a political spectrum) or trust in the army (in countries where historically a military has seized control).

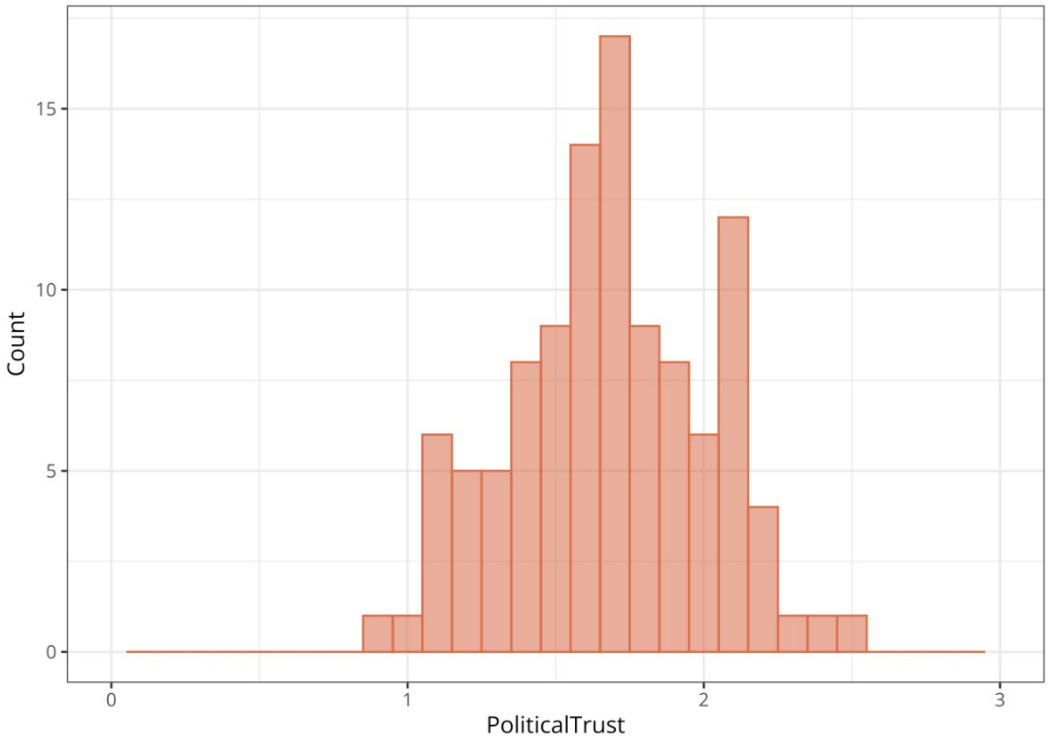


Fig. 3.: Simple histogram showing the distribution of countries on the constructed political trust scale for the Afrobarometer data countries.

Nonetheless, all trust-related Afrobarometer variables were examined in a correlation matrix, subjected to a factor analysis and a reliability test which showed that an index comprised of trust in the president, trust in the parliament, trust in the electoral commission, trust in local government and trust in courts was valid, apt and prudent – as including more measures began to increase the Eigenvalue of a second factor to greater than one. Nonetheless, I have included both my extensive manual survey question comparison and the mentioned index-related tests in the

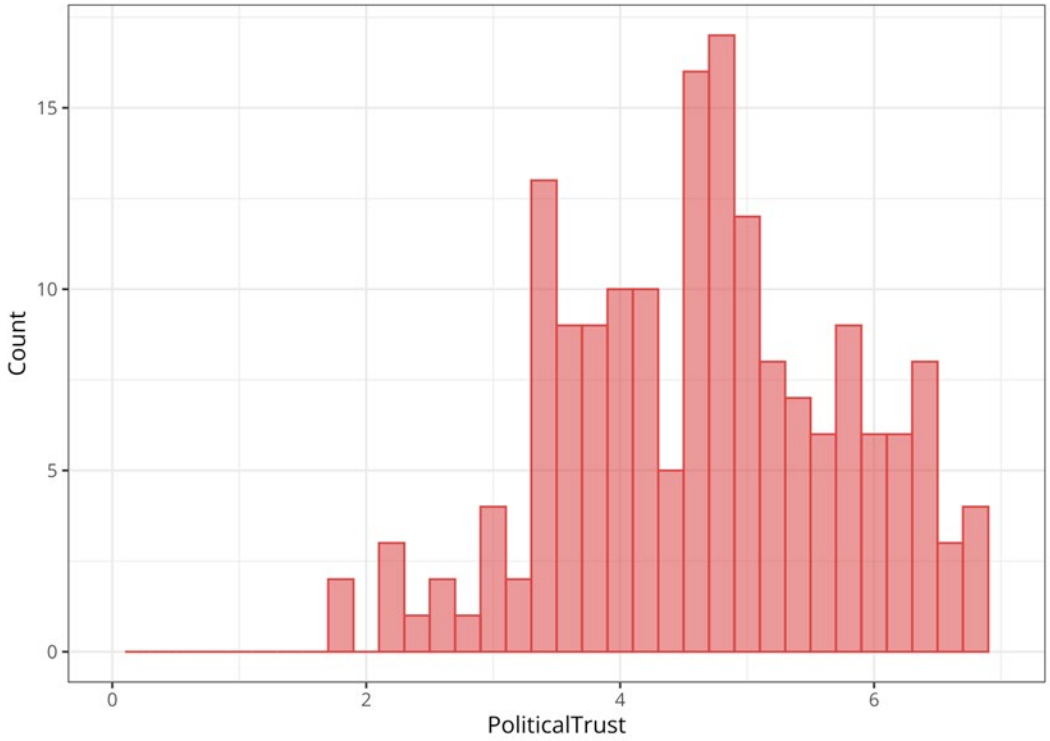


Fig. 4.: Simple histogram showing the distribution of the constructed political scale for European Social Survey countries.

appendix. As for the ESS side of the data, this was much easier as the questions are identical between rounds and I utilized the trust in country's parliament, trust in the police, trust in the legal system and trust in politicians – and here the problem is inverse: the trust measures correlate *nearly* so well that they border into being statistically speaking the same measure. Regardless, tests are included in the appendix for this data also. In sum this inquiry into the data, shows that the Afrobarometer changes the options for respondents from round 3 and on, making only three out of seven rounds viable to drawn on for this project, compared to about eight out of ten for ESS (depending on if the survey concluded prior to 2017 or not in each country).

For large surveys such as these there are always deviations and some problems associated with each round. For example, Estonia lacks interview dates in one round, but mistakes such as these tend to precisely that: mistakes. Thus, data missingness is not systematic. Nonetheless I did construct a variable from survey response date did test its inclusions as a control in the full models and also regressed this against the other variables I included – to be elaborated, but such as civil conflict and peace years – and found no significant results. This absence of evidence is not evidence of absence though, and this is a fairly rudimentary check as events within a country can still influence survey *planning* just as much as *execution*, and we have no real data on that prior aspect, and as such we cannot exclude the possibility convincingly here, but I have approached this with some checks and awareness towards its possibility.

However, while there may not be immediate concerns about systematic data missingness, there might still be a great deal of noise to the data, or problems associated with first aggregating the data and then to the process of assigning said country data to a specific year. Surveys such as these, often prefer to tip-toe around election dates, which in the case of the ESS results in a tendency for surveys to be conducted around Christmas or the new year, meaning that whether the year set is very shortly after the conclusion of the survey round or very nearly a year after the survey round, can be a bit arbitrary. Again though, this should not vary systematically, but information and thus precision is lost when aggregating and setting the year to the year after the survey completion.

As for the process of aggregating the data to the country level to match the POSVAR data, this is a relatively simple process of collapsing the data to a country level and using the mean. Mathematically, whether we construct an index and then collapse the data, or collapse the data and then construct the index, should make no difference. *However,* this is not entirely the case here as indexes can be used to "fill out" for missing data on both first the individual level and then also on the country level. What is meant by this is that an individual which does not respond to an index component survey question, need not be omitted entirely, as the remainder of the respondent's answers can still be used to construct an index, just using a denominator that accounts for the reduction in index components. On the individual level, this is not a problem as each country usually has respondents enough to be considered representative, and as such I simply removed them – a likely good practice anyway. However, on the *country level* we have relatively few observations, and I employed this strategy to ensure as many observations as

possible. This is of course, a little bit sketchy, as this missing data tends to not be non-random. For instance, in some countries, respondents are not surveyed for trust in the electoral commission simply because it has been *decommissioned.* In other words, the data missingness can be quite systematic in this sense but given the solid correlation between index components and likely interaction between them, should nonetheless make this viable and we should nonetheless expect that the absence of one index component should have an adverse effect on the remaining components to realistically capture the political reality which finds it source in that component being missing (examples include whether this being trust in the president not being polled for in Madagascar election controversies and interregnum period, or the electoral commission not being present in Morocco).

In summary, there are a number of concerns and considerations to the dependent variable, ranging from aggregating, placing it in time, distinctions between datasets and, it, taking the form of an index. Therefore, on the following two pages, I have presented an overview of the Afrobarometer data and ESS data respectively. The different colours represent the different survey rounds, where the coloured bars above the x-axis for each country represents the average value of the political trust scale for the respondents on that day, and the black bars below the x-axis represent the total amount of respondents.

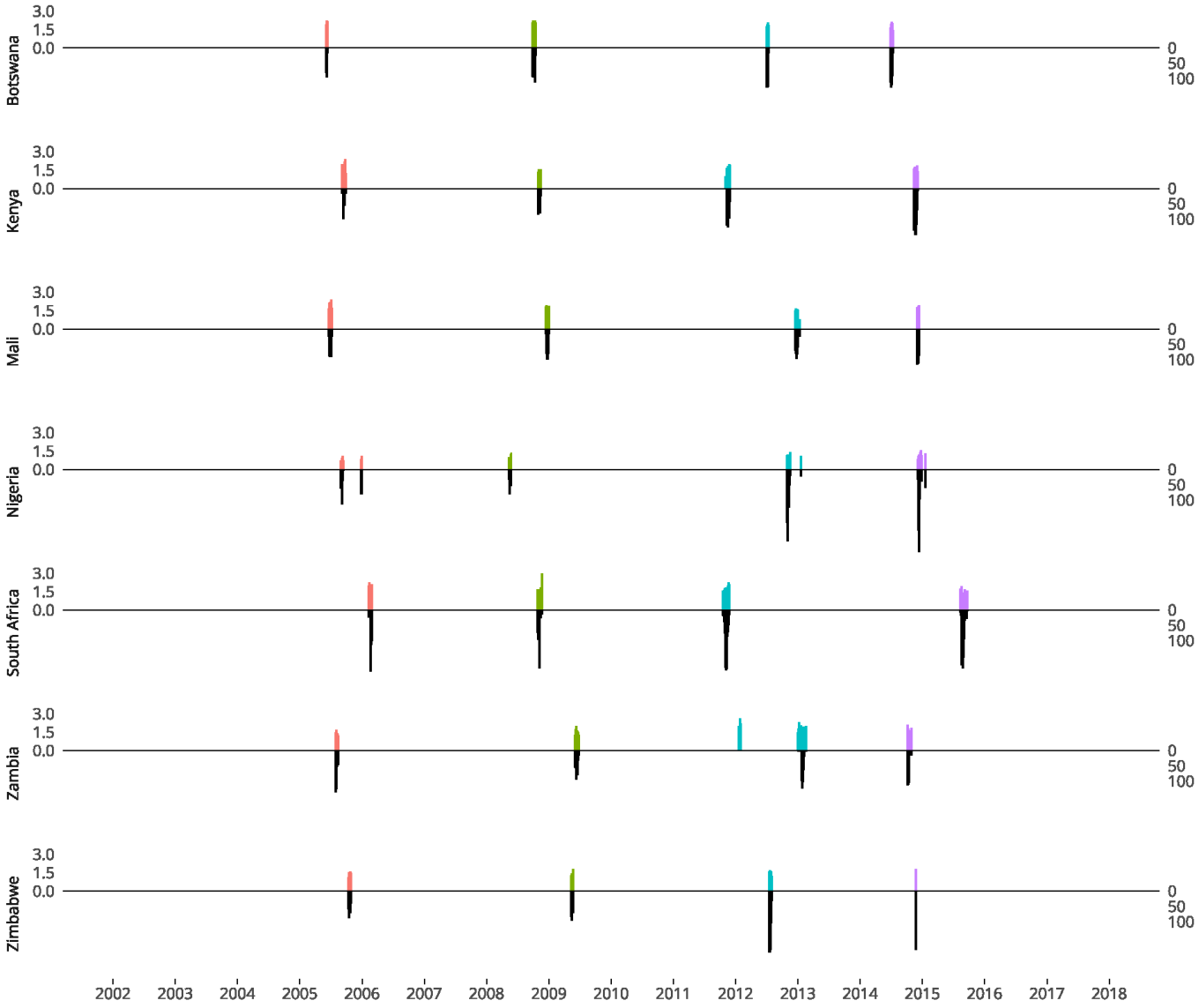


Fig. 5.: Distribution of Afrobarometer respondents for eight selected countries per day.

Bars above the x-axis for each country represents the average Political Trust score corresponding to the group of respondents on a given day, with colours indicating the survey round. The bar below the x-axis for each country indicates the number of respondents on a given day. The Y-axis on the left indicates the Political Trust index score, and the Y-axis on the right indicates the number of respondents. Countries selected on no particular basis other than to give a broad impression of countries participating.

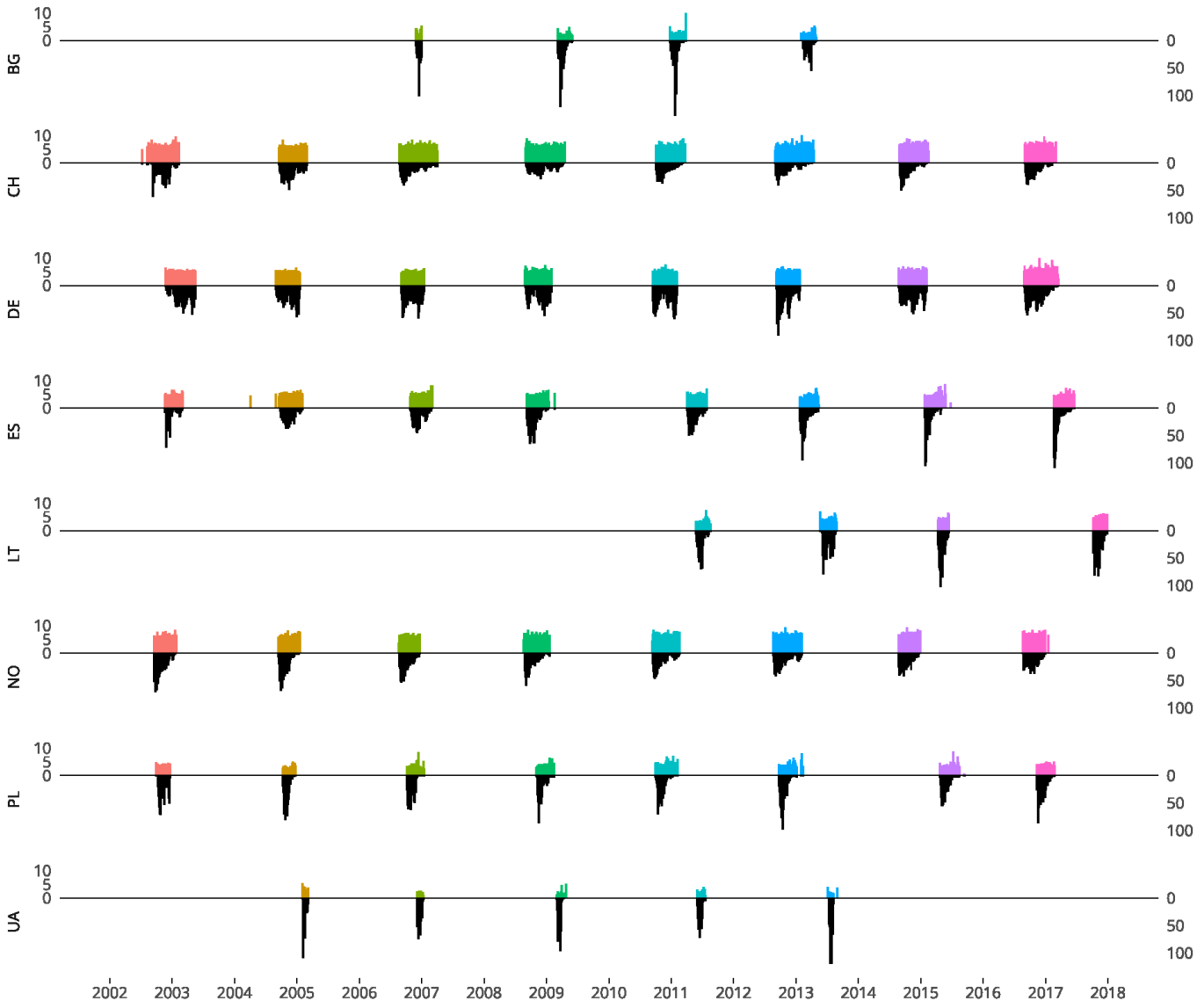


Fig. 6.: Distribution of ESS respondents for eight selected countries per day.

Bars above the x-axis for each country represents the average Political Trust score corresponding to the group of respondents on a given day, with colours indicating the survey round. The bar below the x-axis for each country indicates the number of respondents on a given day. The Y-axis on the left indicates the Political Trust index score, and the Y-axis on the right indicates the number of respondents. Countries selected on no particular basis other than to give a broad impression of countries participating.

Interpretation and comment on both figures:

Emphasis is on survey timing, duration and time between rounds: I do not expect the reader to extrapolate much information from the respondent amount and the corresponding political trust response average – it is primarily visualized because it could be and what I wish to convey is primarily that deciding what year to assign a country result to is not necessarily an easy task when survey rounds cross years, and results can be spread out over time. Note also that in general, *Political Trust* in countries, does not vary much between rounds, although variation between countries can still be considerable. Also, it is interesting that the pattern of Ukraine and Bulgaria is similar to the quick survey execution time in the Afrobarometer countries.

3.1.2. POSVAR and the basis for the primary independent variables

In POSVAR, each unit of analysis is a country-year, and all variables are placed on a zero-to-three point ordinal scale, where: 0 denotes *no presence* of the category of violence; 1 indicate *isolated violence* in the form of reports violence and or 1-25 victims per year; 2 denotes prevalent violence, described as common place, pattern, widespread, numerous and or 26-99 victims in a year; and finally, 3 indicates systematic violence, described as massive or systematic, with 100 or more victims per year. The exceptions are the terror and riot variables, where terror are simply count-variables and riots are dichotomous.

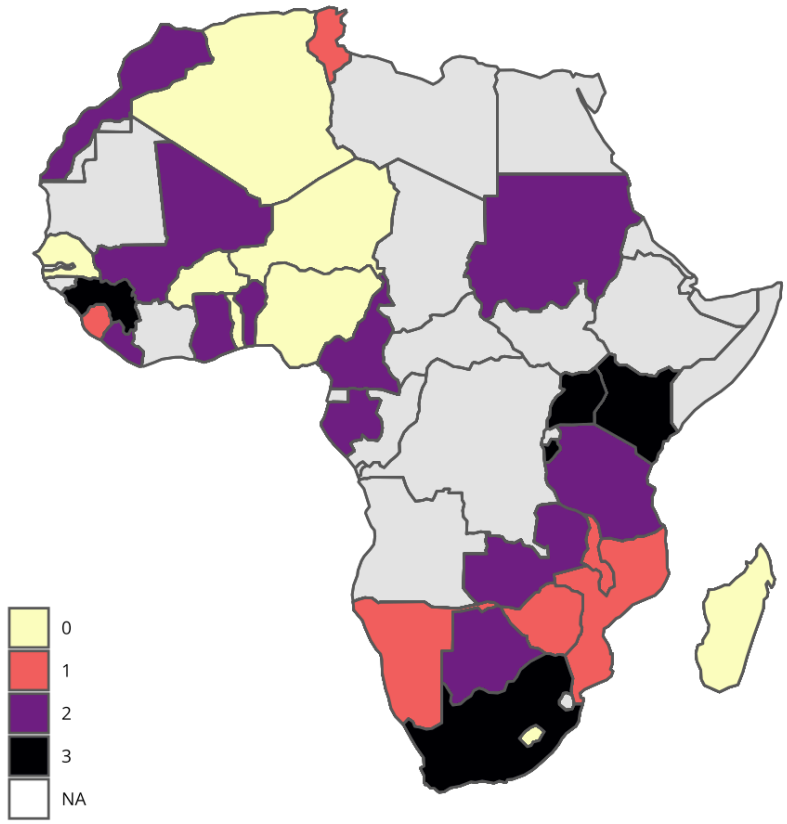


Fig. 7.: POSVAR data on Africa, selecting the highest value of any of the refugee-related violence sub-categories and displaying the maximum value thereof for a country, 2000-2015.

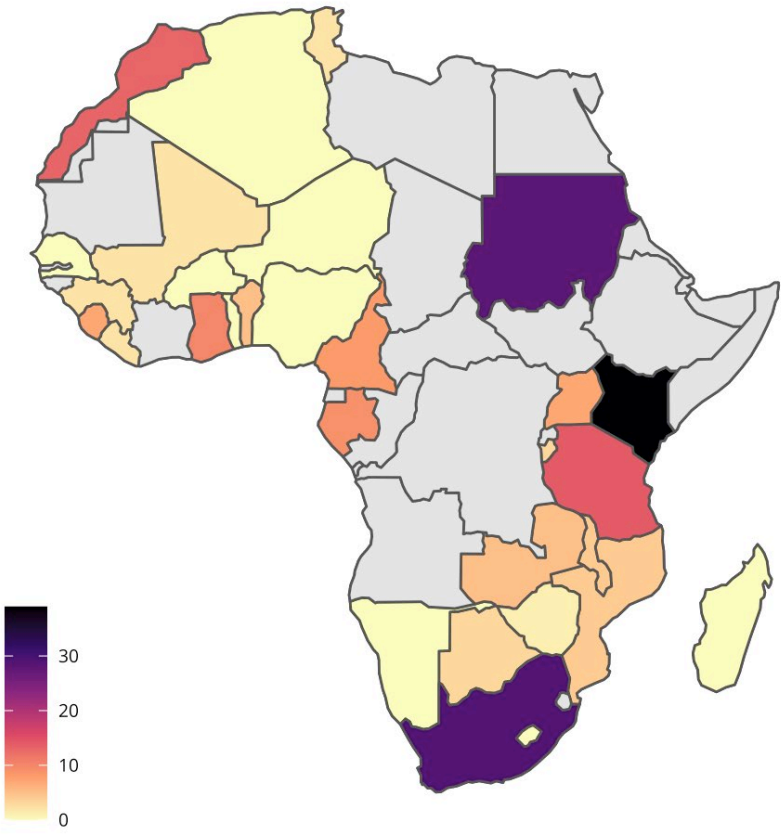


Fig. 8.: POSVAR data on Africa, selecting the highest value of any of the refugee-related violence sub-categories and displaying the total sum for a country in the period 2000-2015.
Note that the POSVAR variables are ordinal and as such a sum does paint an honest picture of the total sum of violence but does when viewed in conjuncture with the previous figure give a somewhat idea of what countries that are and are not associated with refugee related violence.

POSVAR also makes the distinction between acts of violence perpetrated by groups and individuals but given the challenging nature of data from reports of violence, I chose to omit this distinction and the basis of the data is already quite thin in this regard and making more distinctions would only worsen this. Initially I constructed a variable for each of the types and directions of violence on the basis of a 2- and 3-year prior maximum in the given type and direction.[2] This is firstly on intuitive grounds and the assumption that particularly violent years may impart an effect on attitudes which stretches for well over a year, as well as smoothing out some of the concerns in respect to both missing data and the lack of granularity associated with

[2] i.e.: the violence measure in year n takes the value of the maximum measured violence in year n, n-1 and n-2 (or for instance, the year n-1 takes the maximum value from the years n-1, n-2 and n-3 when lagging the variable).

violence (being events on days in a year) possibly being somewhat arbitrarily assigned to a year, especially when matched to survey data which might similarly be also somewhat arbitrarily assigned to a year when survey rounds span across multiple years. Unfortunately though, this approach did not result in any significant results, and as such did not perform better than a simple model using only the prior year measurement of violence. For the ESS-dataset, this is much less a concern, as surveys coincide and are run regularly, and therefore I do not produce or use such a variable for the ESS data.

3.1.3. Control variables

For the full models I employ a number of control variables, some of which could be considered standard controls, some which are intuitive, and some being motivated on theoretical grounds.

I use a number of measures from the World Bank: on GDP per capita in current US$, relative GDP growth and population. In combination with these measures I also imported GINI-coefficients from the *Standardized World Income Inequality Database* (SWIID) as constructed by Solt (2020), but for reasons to be discussed in the discussion section, I ultimately decided to exclude the GINI-measure. Regardless, these are catch-all measures intended to say something about the economic situation in a country, and thus also to a limited degree also captures an element of state capacity.

Intrastate conflict and peace years is controlled for using the Uppsala/PRIO Conflict Data Project (Gleditsch et al., 2002; Pettersson 2021); where peace years is calculated as the number of years since the last war.

In addition to this the POSVAR conveniently includes data from the UNHCR's Refugee Population Statistics Database on refugee populations and refugee populations relative to host country population in a given year.

As for controlling for levels of democracy, this is also debatable. A fact-of-the-matter is that Afrobarometer participating countries – Eswatini/Swaziland being a notable outlier – have quite high democracy ratings and we should not be surprised to see that primarily somewhat more democratic countries self-select into the survey. Meanwhile in the ESS countries simply are quite democratic – though Turkey and Russia being the notable outliers there. And herein lies a problem on reason based on the theory section: political trust is considered integral to a well-functioning democracy, yet we have ample reasons to think satisfaction with democracy would influence political trust. As such, the causal direction is very mixed, and when considered

in relation to the primary independent variable of interest, the potential for democracy having a mediating effect in relation to refugee related violence (such as through a refugee scapegoating hypotheses) warrants caution to its inclusion as a control. I therefore omit such a variable.

Lastly, a preliminary debate is necessary in respect to how we treat the various measures, especially GDP, population, and refugee population. The distribution of GDP will in nearly all samples using such a measure – and it is the case for both Afrobarometer and ESS countries indeed – be highly skewed towards a lower end with outliers in the higher end. This does not ruin a model per se, and as we will discuss in the next segment on OLS-regression assumptions, the assumption of a *normal distribution* relates not to distribution in general but the distribution of *residuals*. As such, a skewed variable is not a concern towards whether OLS-regression is valid or not, but it may nonetheless be somewhat inappropriate for the purpose as the OLS-regression will model the mean, and outliers in the data from a skewed distribution may adversely impact the mean in such a way that the predicted value is negatively or positively inflated. This is primarily a concern for the dependent variable, and what form of GDP measure I chose made very little difference on model performance, so while I included them all in the dataset, I ultimately only used the log GDP per capita, for no other reason than this being the most common practise when a GDP measure is used as a broad control.

With the population and refugee population, their relative relationship or relatively change or absolute change, these are motivated due to different theoretical concerns. First, controlling for absolute values of population and refugee population is intuitively prudent when the variable of interest indicates an amount of violence: put simply, a large populous country with a high refugee population can in sum total show more refugee related incidents of violence simply because such a country is larger, and similarly we can expect a large country also to host more refugees because it is a large country. This is important as the interest here is the effect of refugee related violence on political trust, not the effect of Nigeria being Nigeria, or the effect of Egypt being Egypt, on political trust. Nonetheless we may also control for the relative relationship between population and refugee population, as well as change in refugee population. These are more causally related to violence, and controlling for them can obfuscate certain causal relationships we are interested in. For instance, it may simply be the case that it is on the immediate influx of refugees that violence is most likely to take place, and it may be the case that both – relatively speaking – high and low refugee populations relate to violence. But again,

the interest in this project is who commits the violence, not so much the context. These relative population measures can however be quite useful when probing the data for reasons as to why we see certain effects which we might find difficult to explain. For instance, a sudden large refugee influx could have the effect of being the root cause of a GDP per capita shock, leading to adverse economic conditions which produce more violence and less government control, amplifying the effect of a refugee related independent variable. If we have reason to suspect a GDP control variable is introducing suppression effects and biasing results – more on those in the next subchapter – we can investigate this using such a variable. I therefore I include them in the datasets and the descriptive statistics.

3.1.4. Summary statistics

Table 1.: Descriptive statistics, Afrobarometer 2000-2016

Descriptive statistics for Afrobarometer country sample

	N	min	max	Mean	Std. Dev.
Political Trust	108	.912	2.528	1.68	.327
Gov. V. against ref.	576	0	3.000	.267	.588
Civ. V against ref.	576	0	3.000	.134	.451
Non-state V against ref.	576	0	3.000	.083	.368
Ref. on ref. V.	576	0	3.000	.085	.37
Ref. V against the gov.	576	0	1.000	.014	.117
Ref. V against civ.	576	0	2.000	.036	.205
Ref. recr. by non-state act.	576	0	1.000	.045	.208
Terrorist attack by ref.	576	0	1.000	.003	.059
Terrorist attack against ref.	576	0	2.000	.01	.117
Refugee riots	576	0	1.000	.069	.254
Refugee population	549	0	689371.813	59872.731	114574.65
Refugee pop.(log)	549	0	13.444	8.667	3.284
% of pop. = ref.	549	0	5.800	.335	.581
GDPpc (cur. US$)	611	113.567	10809.685	1891.718	2124.334
Population	612	142264	185960244.000	21147138	28056632
Civil conflict	612	0	1.000	.175	.38
Time since last civil conflict	612	0	70.000	27.243	24.51

Table 2.: Descriptive statistics, ESS 2000-2016

Descriptive statistics for ESS country sample

	N	min	max	Mean	Std. Dev.
Political Trust	173	1.751	6.825	4.654	1.121
Gov. V. against ref.	560	0	3.000	.105	.403
Civ. V against ref.	560	0	2.000	.089	.309
Non-state V against ref.	560	0	1.000	.005	.073
Ref. on ref. V.	560	0	0.000	0	0
Ref. V against the gov.	560	0	2.000	.016	.139
Ref. V against civ.	560	0	3.000	.023	.21
Ref. recr. by non-state act.	560	0	1.000	.005	.073
Terrorist attack by ref.	560	0	17.000	.055	.772
Terrorist attack against ref.	560	0	26.000	.08	1.173
Refugee riots	560	0	1.000	.018	.133
Refugee population	560	0	1620635.000	51317.655	146078.41
Refugee pop.(log)	560	0	14.298	8.433	2.592
% of pop. = ref.	560	0	2.091	.233	.348
GDPpc (cur. US$)	595	658.345	123678.703	29155.494	23343.377
Population	595	281205	146596869.000	22406057	31054199
Civil conflict	595	0	1.000	.084	.278
Time since last civil conflict	595	0	70.000	42.834	24.609

3.2. Methodology and research design

In this section I will first explain and elaborate on challenges associated with the data at hand and how to structure it as cross-sectional and or as panel data. This section will admittedly jump somewhat ahead of itself as these aspects can hardly be discussed outside the basis of discussing the basics of quantitative methods and OLS-regression first, but the concerns relating to structuring the data does precede the methods, even if they relate. Therefore, I will thereafter return to the basics of how quantitative methods relate to causality and in a sense touching on how statistical inference relates to, yet is also distinct from, causal inference; and then discuss the conditions necessary for a valid OLS-model. After this, I turn to concerns relating precisely to problems which arise from statistical inference being distinct from causal: which is to say endogenity problems, mediating effects, omitted variable bias, suppression effects and concerns relating to model specification.

3.2.1. Limitations and considerations, and resulting data and data structure

While the POSVAR data and both Afrobarometer and ESS have their merits and strong suits as described, good quality longitudinal data on the individual level is expensive and rare, even in western countries, and is all the more difficult to produce in developing countries. As the Afrobarometer is run on a three or four-year interval and takes significant amounts of time to conclude, this does unfortunately exclude the possibility of using time-series longitudinal data in a fixed or random effects model, and while ESS is regular enough to interpolate across years where the survey is not conducted, this would not really provide much benefit as the trust indicators are relatively stable anyway. Regardless, the data is not particularly fine grained and attempting to work around the survey data on individuals as it relates to yearly POSVAR-data would produce a hodgepodge patchwork of data as some countries conclude their survey rounds in different years from other countries and the sample would likely not be representative when split. Put simply, it would be a bit of a mess. Furthermore, as the POSVAR data is on a country level, this also makes it redundant to approach the attitudinal data from the Afrobarometer survey rounds as hierarchical data in a multilevel model. Had the POSVAR data been geocoded or the Afrobarometer data been more longitudinally fine grained, this would of course have been preferable and the resulting available modeling techniques would enable a more convincing level of statistical inference.

However, no matter how much more convincing or appealing such methods might be, the methods used are ultimately limited by the data at hand and thus the project choses the method, and not the other way around. As a result, and as is often the case in social sciences, the ideal analytical approach is one that is not available, and the remaining possible approach will have to be a relatively standard OLS-regression model using a pooled-OLS approach which is simply treating the would-be time series panel data as cross-sectional, regressing for all country years. Indeed, whether we approach this as regressing standard cross sectional data or a pooled-OLS model in a panel configuration is for all intents and purposes the same. Nonetheless I assembled a dataset for each of the two surveys, taking the form of panel data, spanning from 2000 to 2016 and with the included variables as described this provides a yearly measure for all relevant variables with the exception of the regular, albeit slightly scattered and spread, survey rounds of the ESS and Afrobarometer surveys. To put together panel data is a bit more labour intensive but nonetheless has its merits when using a simple pooled-OLS approach. It can be beneficial for other researchers using one's own data and makes replicability easier, but it also enables us to use certain modeling techniques which would otherwise not be available, and to exert control more readily of causal relationships as lagging variables becomes relatively easy. Of particular interest is techniques that alleviate the effects of cross-sectional dependence. Cross-sectional dependence typically occurs when all units in the same cross-section correlate. In social data, the very character of the data being the result of social processes relating to people, groups, culture, nations – and so on – means that observations are rarely if ever independent (Stephan, 1934). Thus, when using panel data a – likely in this case – random effects model with a control for year-fixed effects would have been ideal, as controlling for year-fixed effects captures a number of possible cross-sectional sources of dependence (such as economic shocks). Again, this is unfortunately not available to us due to the lack of fine-grained panel data, but we may use modeling techniques which enable some alleviation of cross-sectional dependence, such as using Driscoll-Kraay standard errors. Driscoll-Kraay standard errors have the benefit of being accurate even when there is some cross-sectional dependency present (Hoeschle, 2007); and may be employed even in a pooled-OLS specification. Therefore, I will first run a basic pooled-OLS regression and thereby afterwards use pooled-OLS with Driscoll-Kraay standard errors on findings which I consider to be reliable. Nonetheless, not being able to structure and use the data as time series in full will limit confidence in findings and will have to be discussed in later

sections, and suffice to say, it is an argument for future research in and of itself: if these questions are to be answered with precision, more precise data on refugee related violence will be necessary.

Thus, to conclude and drawing on a paraphrasing overheard at a small seminar: you go to war with the data that you have, not the data you might want or wish to have at a later time.

3.2.2. On quantitative methods and OLS-regression explained

Scientific work involves many aspects, but one of the most central concerns – if not the most central concern – is causality. To answer any causal question, involves inferring a counterfactual outcome to compare with what actually happens and the fundamental problem of causal inference arises from not being able to observe the counterfactual outcome: which is to say, we may examine when a treatment (X) seems to impart an effect on an outcome (Y), but to identify a true causal effect would involve a counterfactual case of an identical nature where a not-treatment decidedly showed a not-effect. This fundamental problem can be circumvented through clever research design, although as argued design possibilities are limited, and the counterfactual comparison must be estimated using observed data. A regression analysis based on Ordinary Least Squares (OLS) selects the parameters (typically as an intercept, alpha; and a slope, beta) in a linear function on the principle of minimizing the sum of squares of the difference between the observed dependent variable and the predicted function based on the independent variable. This is most simply understood and visualized in a plot showing all observations where a line drawn (or is "fitted") based on these selected parameters (again, typically the alpha describes the intercept and the beta the slope) to indicate the general relationship between X and Y on the basis of these observations. Thus, by examining to what extent a treatment variable (X) is associated with an effect on an outcome variable (Y), the counterfactual outcome is approximated by including observations which are, are not, or are to some degree associated with, what we identify as the treatment variable. As such, in an OLS-regression, the treatment variable is a treatment variable in name only, and it is only on some argument, logical grounds or perhaps indeed even in name only we distinguish the treatment variable from other independent variables. In other words, in OLS-regression, the equivalence between it as statistical inference, and it as causal inference, is wholly absent. While variables can and will be ordered (lagging) in such a way that the causal order is logical, the true

counterfactual remains unobserved and as an experimental design is not possible, internal validity is reduced, but simultaneously if the effect is shown to be both significant and strong one may have evidence to suggest that findings are having external validity – i.e.: less is known about whether the treatment is the true cause of the outcome, but more is known about the applicability of the treatment causing the outcome, to a wide variety of cases.

3.2.3. Assumptions and conditions necessary for OLS-estimation

In order to be valid as statistical inference, linear regression models make a number of assumptions about the variables, both in response and treatment, as well as their relationship to each other and residuals. However, models are nonetheless quite robust to relaxing all of the assumptions with some caveats and conditions relating to interpretation.

3.2.3.1. Linearity

The most immediate being the assumption of linearity, which is daunting given that there is, *a priori*, no reason to assume a linear relationship in a social phenomenon. This assumption is less challenging than it might seem as OLS-regression is still relatively robust as long as the relationship is approximately linear within the scope of the data at hand (Skog, 2017, p. 238); and one might still fit a nonlinear (polynomial) model to the data, as the *combination* of parameters can still be linear. Furthermore, to what extent a model can or should include transformed predictor values (such as in the case of polynomial regression) can be a theoretically informed decision, which is to say we may have reasons to assume a curve-linear relationship and we should thus consider fitting such a model; and we may also approach the process of fitting a model as a more iterative, inductive or exploratory process – which is to say, simply seeing what seems to fit best within reason.

3.2.3.2. Absence of multicollinearity

This also relates to the prior section, but two explanatory variables cannot be perfectly correlated in the same model (Mehmetoglu & Jakobsen, 2017, p. 146). The word *perfectly* should be stressed though, as regression models are typically relatively robust to relaxing some assumptions in some circumstances, and in this case the concern relating to two explanatory variables correlating to a high degree, but not perfectly, is that the coefficient estimates are overly sensitive to variations in whatever the two correlating explanatory variables must be assumed to capture. If explanatory variables correlate with each other to an extreme degree, they

will – statistically speaking for all intents and purposes – simply capture the same phenomenon and may be omitted without any great loss of information (Mehmetoglu & Jakobsen, 2017, p. 146). However, with a high, but not perfect or extreme degree of correlation, this may be beneficial in collapsing multiple variables into an index (Mehmetoglu & Jakobsen, 2017, p. 146). Which is of particular relevance to this thesis as attitudinal responses relating to trust to specific political institutions tend to show a degree of overlap in such a manner that one may – and we will – employ a variance-covariance matrix and factor analysis.

3.2.3.3. Residuals: homoscedasticity & errors

Residuals must be assumed to follow a gaussian – normal – distribution with constant variance and a mean of zero; and the product of this assumption resulting in an error term with constant variance is referred to as homoscedasticity and its inverse is heteroscedasticity (Mehmetoglu & Jakobsen, 2017, p. 149). This is often not the case when a model better predicts values (which is to say, exerts less variance) for parts of the range of the outcome variable, and in other words: the variance in the outcome variable should not systematically increase or decrease in its trajectory (increasing or decreasing as it relates to the predictor). This is rarely presented with an intuitive example in textbooks but consider a correlation between how much alcohol a student consumes and its effect on how much money the student spends: irrespective of the extent of how positive the association is, it is also probable that *variance* in spending increases with alcohol consumption, thereby creating heteroscedasticity.

This does not ruin the model per se, but introduces biased standard errors (Mehmetoglu & Jakobsen, 2017, p. 149). It must be stressed that the principal problem is not the concern of whether the resulting biased standard deviation is biased towards being too small or too large, but rather that we cannot know. As the standard deviation determines confidence interval, the resulting confidence interval may thus be more likely to span across zero, or it might be more likely to not span across zero when it should. In the prior case, we are likely to erroneously produce a p-value that is greater than the significance level, which would cause us to erroneously fail to reject a null hypothesis and constitute a type-II error – a false negative. In the latter case, we are likely to erroneously produce a p-value that is less than the significance level, causing us to erroneously reject a null hypothesis leading to a type-I error – a false positive. A way to understand heteroscedasticity is thus to think of it as a more easily discerned concern relating to the distribution of residuals – a subset, if you like. But any systematic variance, which is to say

any pattern, in residuals ought to be considered a problem, and it need not take the form of heteroscedasticity and I will have more to say about suppression effects in a later segment, which broadly speaking inflates significance levels through controls, but this may also take place through having an effect on the distribution of residuals. As such, heteroscedasticity is not necessarily the greatest concern of a regression model, but systematic variance in residuals likely is as it, directly relates to how we consider our hypotheses.

The error term should also not be constituted of errors which correlate, or which do not show a normal distribution of errors. The prior, which would result in autocorrelation, is seldom a problem in cross-sectional data although it may also be present in data in which observations are geographically related to each other (Mehmetoglu & Jakobsen, 2017, p. 150); and is thus a somewhat concern for this thesis. The latter can also be relevant as data will be collapsed to the country level, thereby producing a smaller number of observations, which increases the likelihood of autocorrelation being a problem.

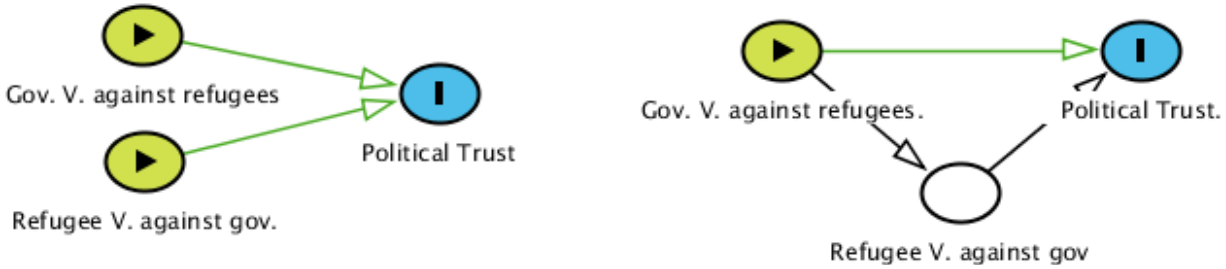
All the concerns mentioned in this subsection relate to distributions and can therefore be examined visually, through histograms and quantile plots, as ordinary OLS-regression is relatively robust, and these generally convey the most intuitive understanding of the problems at hand. Furthermore, one may run various tests, such as a Breusch-Pegan Cook-Weisberg test for hetereoskedasticity, and examining skewness and kurtosis values as they relate to the normal distribution of residuals, and also a Shapiro-Wilk normality test.

3.2.4. Omitted variable bias & over-control: posttreatment bias

What explanatory variables are relevant to include in a model is primarily a theoretical question and a good model ought to be theoretically informed (Mehmetoglu & Jakobsen, 2017, p. 135). Although to *precisely* say what constitutes such a relevant predictor is a bit fickle. It is perhaps one of the more unrealistic assumptions when applied to social science, and many fields employ a wide variety of standard controls, even when there is perhaps no explicit theoretical reason to include them. A simple way of thinking about this concern is to consider an omitted variable as an enlarging of the error term, and similarly introducing predictors and controlling for every imaginable aspect arbitrarily runs the risk of producing results which are artificial and inflated in their levels of significance.

Including a large number of variables may also obfuscate results more generally. As stated, there is no equivalence between statistical inference and causal inference: the model may be perfectly valid strictly statistically speaking, but logically speaking variables may confound in such a manner that cause does not come before an effect. Thus, this is not actually a concern relevant to the assumptions relevant to linear regression as such, *but* it is most apt to mention in this context as it applies to the concern of correct model specification in relevance to the inclusion (or exclusion) of variables and when to infer from the model is valid. Researchers are typically hyper-aware of the potential for reverse causality between the primary variable of interest and outcome and take great steps to overcome this most basic "hurdle to causality" (Møller, 2015, pp. 86-89; Kellstedt & Whitten, 2018, pp. 60-73). They are, however, much less aware of how other explanatory variables might interact with each other. For instance, and of particular relevance to this project, consider two examples: first, a model containing all explanatory variables relating to refugee violence, government violence against refugees, refugee violence against government, and so on; and their effect on political trust. Second, a model with some explanatory variable relating to refugee violence affecting political trust, which then controls for GDP. In the first example, we may happily examine seemingly valid results, again the model is statistically speaking perfectly valid, but *causally* the story is different: the explanatory variables interact with each other, thereby making it impossible to discern whether the effect on political trust is explained by government violence against refugees or refugee violence against the government. This is of utmost importance to this project and modelling violence in general as – as the biblical phrase states – violence begets violence. Therefore, I have illustrated this below in the form of a directed-acyclic-graph, or DAG:

Fig. 9.: Example of potential mediating effects between forms of violence in a DAG.



The model on the left indicates how we might think, casually. However, the model on the right indicates a more likely causal pathways if we simply ran all explanatory variables of interest in the same model: the explanatory variables are themselves both causes and effects when relating

to each other, they *mediate* the effect: one form of violence might explain another – violence begets violence, and the inverse is equally true for a model inversing the emphasis on the opposite explanatory variable. This is a non-concern if we are interested in the effect of violence broadly, but given that this project is primarily motivated by data that distinguishes between different forms of violence and the direction of violence, this is extremely important. The second example using GDP as a control could be visualized as follows:

Fig. 10.: Example of post-treatment bias using GDP as a control, illustrated in a DAG.

Again, the example on the left illustrates how we might like to think: we have good reason to think GDP might also play a role in shaping political trust as informed by institutional performance theories and our concern is omitted variable bias, so we control for GDP. Again, the model is statistically speaking valid, but causally, the reality is more likely akin to the model on the right. In practice, violence imparts and effect on GDP and vice versa, and political trust can also influence GDP, thereby introducing *bias* – indicated with pink arrows – into our results: in this model we cannot discern to what extent the effect is explained by what. Simply put, if we remain in the world of statistics only, there is no practical distinction between an explanatory variable as an independent variable or control: the model does not care what call a variable.

At its core, including variables resulting in this form of overcontrol is a basic violation of cause-and-effect. However, the concern in social science tends to be the possibility of including too few variables – omitted variable bias – and emphasized to such an extent that this inverse concern and problem resulting in a form of overcontrol referred to as post-treatment bias, is commonplace. I stress this because even as much as half of recent articles in journals relevant to conflict – to which this thesis most aptly fits – shows this tendency in some form and often directly as illustrated in the two examples above, even though conflict studies tend to be quite vigorous in modelling efforts (Dworschak, 2021).

These concerns are of particular relevance to this project as fine-grained panel data is not available, which could alleviate some of these concerns by introducing a time component which enables some approximation or modelling of causal orders and or pathways, but with the

infrequent survey rounds the outcome variable is potentially not capturing attitudinal *change* as well as one would like. It is still possible to lag explanatory variables and controls, and I will be doing so, but this brings us to a somewhat philosophical discussion concerning whether necessarily spatial lag is equivalent to causal lag. With variables capturing broad concepts like political trust, or GDP intended to indicate the overall state of an economy, these variables are quite long run and tend not to necessarily be as causally susceptible to bias when examined against more quickly moving variables such as those capturing violence. This limits the concern relating to potential for significant amounts of bias – we should not expect the introduction of a per capita GDP measure to causally turn a model on its head. But the introduction of controls to a model may remove more explanatory power and significance from the primary variables of interest then they would if the variables were more independent from each other. Gradually introducing controls to an expanding model with some awareness towards this potential is warranted, although it does not truly solve the issue at hand: variables still causally impart effects on each other even if we de facto treat them as unobserved by omitting them from our models.

3.2.5. Suppression effects

Suppression effects are control-variable-induced increases in the estimated effect sizes (Lenz & Sahn, 2021). This relates to post-treatment bias in a sense: although it could be simply a mathematical coincidence, likely this takes place through some casual logic. It can however be very difficult to discern precisely why. Nonetheless this is important because when the introduction of a control variable increases the effect size it will also increase the likelihood of significant results. As we shall see this is quite important for this project as we will be presented with a few near-significant results that in fact become significant with the inclusion of control variables. This is to put it simply, a fickle matter. Researchers often simply employ standard controls, as do I in this project, and in some cases, these may conveniently inflate the significance levels and we must be vigilant when this takes place. Furthermore, researchers tend to prioritize including variables on theoretical grounds, though there might be a variety of theoretical arguments, considerations or even just intuition for both what control variables are used and in what manner. There may often be a trade-off between concerns and no consensus on a correct practice. For instance, as in the above example where GDP potentially introduces posttreatment bias, we face a tradeoff. We could simply lag the variable to precede the

independent variable of interest, but when dealing with a slow-moving catch-all measure such as GDP per capita, we run the risk of pushing the variable so far back in time in regard to how it relates to the dependent variable, that it functions less and less as a control at all.

Researchers naturally wish to produce significant results, and just like with posttreatment bias there is an incentive to ignore the issue. This need not be through direct attempts at malice per se, but we are often just content when a model produces the results which are convenient. The prevalence can, as with posttreatment bias, not be understated. Examining all articles published in the American Journal of Political Science in the years 2013-2015, Lenz & Sahn (2021) find that about 30% of all articles depend on suppression effects to achieve statistical significance in findings.

3.2.6. Additional tests

Linktest and Breusch-Pagan tests are reported in the appendix for all models. The Afrobarometer data gives no cause for concern although some ESS model specifications show potential for omitted variable bias and heteroscedasticity. This however primarily relates to the second set of variables and count variables, where the number of observations containing real event information in a year are so few that this is likely not relevant.

4. Results

Due to the number of independent variables of interest relating both to the direction of violence and the source, as well as using two distinct datasets, there will be a large number of models and tables. Furthermore, and as stated, the approach of this project is somewhat inductive, and as such the results section will begin with a presentation of a bivariate regression table, which is then discussed, before moving on to the full models and presenting these. I divide the variables of interest into two groups as they respect to the Afrobarometer and ESS subsets: first examining the concrete and ordinal violence related variables as they relate to violence by and against refugees, before examining the refugee recruitment, terrorism and riot variables. The results inform four relationships between violence and political trust that may be examined more closely in a subchapter following the multivariate regression models. As stated, this project is somewhat inductive, and as such they are not robustness checks per se but will ultimately revolve somewhat around the robustness of findings, and thus these relationships can be considered the basis of inductive hypotheses to be examined, which are:

(a) the effect of government violence against refugees in political trust in the ESS subset – what underlying causes might the bivariate regression have conveyed and could controls have the adverse effect of blocking some causal pathways?

(b) the effect of civilian violence against refugees on political trust in the Afrobarometer subset – the findings were near-significant in the bivariate regression, what controls might both diminish and strengthen the effect by working through the independent variable?

(c) the effect of non-state actor violence against refugees on political trust was near-significant in the bivariate models and is significant in the full model – can we trust these results, or could the inclusion of some controls be inflating the significance levels?

(d) the effect of terrorists' attacks against refugees on political trust show a similar pattern to the prior concern, again, are these findings reliable?

4.1. Bivariate regression

	Political Trust Afrobarometer	Political Trust ESS
Gov. V. against ref. $_{(t-1)}$	0.0052	-0.401
	(0.924)	(0.039)
Civ. V against ref. $_{(t-1)}$	-0.0712	0.318
	(0.213)	(0.204)
Non-state V against ref. $_{(t-1)}$	-0.288	0.673
	(0.133)	(0.551)
Ref. on ref. V.	0.0506	(-)
	(0.493)	(-)
Ref. V against the gov. $_{(t-1)}$	-0.0467	0.0158
	(0.809)	(0.981)
Ref. V against civ. $_{(t-1)}$	-0.0774	-0.0534
	(0.815)	(0.887)
Constant	1.685	4.672
	(0.000)	(0.000)
Observations	108	173

Table 3.: Bivariate regression, Political Trust and each of the independent variables.

Note: though they are presented in the same table, these are conducted as individual sets of bivariate regressions, and are only presented in the same table due to space concerns.

Bivariate regression showing the influence of violence by refugees against refugees is omitted and displayed as (-) from the ESS set due to there being no reports of any refugee violence against refugees for the given subset. This does not imply complete absence of refugee against refugee violence in Europe, but simply that no refugee against refugee violence took place in a country the year prior to said country completing its survey round.

Significance levels are presented in parenthesis. The number of observations is identical for each set, and the constant is simply the average of all constants – not precisely good practice, but good enough as the constant varied only about 0.1 between the bivariate models.

The immediate results of the simple regressions are not promising. Only the relationship between government violence against refugees and political trust show clearly significant results, and only within the context of the ESS data. Though within the Afrobarometer subset we do observe that the effect of civilian and non-state actor violence against refugees are near-significant enough to potentially produce results when controls are included. In both the case of government violence against refugees in the ESS subset as well as the near-significant non-state actor violence against refugees in the Afrobarometer subset, the effect is negative and quite strong –

especially in the latter case as we must remember that the Afrobarometer political trust index ranges from 0-3 although having a p-value of 0.133, is not quite within the confines of the standard appreciated 0.05 p-value, and we ought to be hesitant to compare any non-significant result. We may however immediately consider a reason for why one result is significant in the ESS data, and one result is significant within the Afrobarometer data, *but not vice versa.* Firstly, who are the non-state actors? The immediate actor-group that comes to mind is of course terrorist groups or paramilitary groups. These might be less frequent in Europe, so even if the data is more generous in the ESS, there might still be certain forms of violence which are more present in one set than the other. Furthermore, the finding of government violence having an effect in ESS, but not in Afrobarometer, might also be the product of reporting and data on violence by the government against refugees being more accurate in Europe. I will examine both these aspects in the exploratory segment.

Regardless, what we may note, is that we find no evidence supporting a scapegoating hypothesis thus far – and in fact, within the ESS, we find evidence to *the contrary.*

	Political Trust Afrobarometer	Political Trust ESS
Ref. recr. by non-state act.	-0.0373	0.673
	(0.846)	(0.551)
Terrorist attack by ref.	-0.0233	-0.0800
	(0.944)	(0.887)
Terrorist attack against ref.	-0.276	0.0487
	(0.239)	(0.234)
Refugee riots	0.00665	-0.0611
	(0.959)	(0.915)
Constant	1.682	4.651
	(0.000)	(0.000)
Observations	108	173

Table 4.: Bivariate regression, Political Trust and each of the independent variables.
Note: though they are presented in the same table, these are conducted as individual sets of bivariate regressions, and are only presented in the same table due to space concerns.

Significance levels are presented in parenthesis. The number of observations is identical for each set, and the constant is simply the average of all constants – not precisely good practice, but good enough as the constant varied only about 0.001 between the bivariate models.

Moving on to the count variables in POSVAR relating to recruitment of refugees by non-state actors, terrorist attacks by and against refugees and refugee riots, the results are similar, albeit even less significant. Only terrorist attacks against refugees come close to being significant.

4.2. Multivariate regression with controls

Presenting a table for all relevant measures of refugee related violence in an expanding sequence as controls are added would likely consume over tens of pages. Therefore, I have decided to simply condense the relevant controls in the initial full-model specification, for both Afrobarometer and ESS full-model results before examining significant findings more closely. In these configurations I control for the presence of conflict in a given country, the years since said conflict in a given country, natural log of GDP per capita, population and refugee population in absolute; and my reason for selecting these particular controls in their respective forms is extensively explained in section 3.1.3 on control variables. I begin with a simple lag of all independent variables by one year:

	Political Trust Afrobarometer	Political Trust ESS
Gov. V. against ref.	0.01547 (0.77) [0.05262] -0.089 – 0.120	-0.05192 (0.67) [0.12195] -0.293 – 0.189
Civ. V against ref.	0.00729 (0.90) [0.05855] -0.109 – 0.124	0.12147 (0.47) [0.16890] -0.212 – 0.455
Non-state V against ref.	-0.43023 (0.03) [0.19210] -0.812 – -0.049	0.38724 (0.58) [0.70044] -0.996 – 1.770
Ref. on ref. V.	-0.00629 (0.94) [0.07872] -0.163-0.150	(-)
Ref. V against the gov.	0.11547 (0.54) [0.18741] -0.257 – 0.487	0.38724 (0.58) [0.70044] -0.996 – 1.770
Ref. V against civ.	-0.21775 (0.48) [0.30486] -0.823 – 0.387	-0.10684 (0.65) [0.23353] -0.568 – 0.354
Controls:	✓	✓
Observations	103	173
Adjusted R^2	0.178 – 0.218	0.630 – 0.631

Table 5.: Multivariate regression, Political trust and each of the independent variables

Note: though they are presented in the same table, these are conducted as individual sets of multivariate regressions controlling for civil conflict, years since last civil conflict, the natural log GDP per capita, total refugee population and total population. Presented in the same table with controlling only being indicated due to space concerns.

The influence of violence by refugees against refugees is omitted and displayed as (-) from the ESS set due to there being no reports of any refugee violence against refugees for the given subset.

Significance levels are presented in parenthesis, standard errors in square brackets followed by 95% conf. interval beneath. The number of observations is identical for each set. Reporting the range of adjusted R^2 is imprecise but likely sufficient prior to scrutinizing relevant results further.

	Political Trust Afrobarometer	Political Trust ESS
Ref. recr. by non-state act.	-0.11572 (0.53) [0.18186] -0.477 – 0.245	0.38724 (0.58) [0.70044] -0.996 – 1.770
Terrorist attack by ref.	-0.18885 (0.57) [0.33555] -0.855 – 0.477	-0.16026 (0.65) [0.35030] -0.852 – 0.531
Terrorist attack against ref.	-0.42772 (0.06) [0.22903] -0.882 – 0.027	0.02547 (0.32) [0.02530] -0.024 – 0.075
Refugee riots	0.16595 (0.19) [0.12573] -0.084 – 0.416	-0.04994 (0.89) [0.34833] -0.738 – 0.638
Controls:	✓	✓
Observations	103	173
Adjusted R^2	0.180 – 0.206	0.630 – 0.632

Table 6.: Multivariate regression, Political trust and each of the independent variables

Note: though they are presented in the same table, these are conducted as individual sets of multivariate regressions controlling for civil conflict, years since last civil conflict, the natural log GDP per capita, total refugee population and total population. Presented in the same table with controlling only being indicated due to space concerns.

The influence of violence by refugees against refugees is omitted and displayed as (-) from the ESS set due to there being no reports of any refugee violence against refugees for the given subset.

Significance levels are presented in parenthesis, standard errors in square brackets followed by 95% conf. interval beneath. The number of observations is identical for each set. Reporting the range of adjusted R^2 is imprecise but likely sufficient prior to scrutinizing relevant results further.

Only two results are significant with the inclusion of controls: the effect of non-state actor violence against refugees and terrorist attacks on refugees, both in the Afrobarometer subset and the latter being a change from insignificant to significant from the bivariate models. Both show a strong effect on political trust. Changes from the bivariate models include a complete reduction of the effect of government violence against refugees on political trust in the ESS subset, and the near-significant effect of civilian violence against refugees is diminished completely.

4.2.1. Examining the effect of government violence against refugees

The introduction of controls to the bivariate regression showing the effect of government violence against refugees on political trust reduced significance levels and findings were no longer significant. Usually, one would settle for simply concluding that our findings were not robust to controls, but notably the effect was negative, which immediately gave reason to discredit scapegoating hypothesis – at least within the ESS subset. As such, while we can no longer claim to prove or disprove this hypothesis, we might still find some indications of on what grounds we do not find evidence of such a hypothesis: can this distinction provide any information as to within what context we can and cannot disprove the hypothesis?

Intuitively we should not expect government violence against refugees to be particularly sensitive to population. The government remains a somewhat cohesive single actor in this regard, and thus the effect of being populous in this regard remains mild. The same likely applies to refugee population, and for nesting a regression model we can thus place the population measures in the same block to illustrate the fact that this is indeed the case.

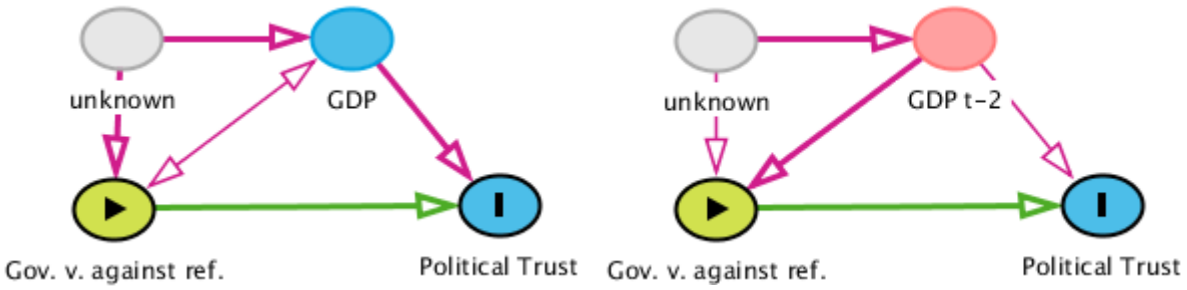
Furthermore, we may consider the conflict and years since conflict variables. Europe is generally considered peaceful, but these measures capture conflict by which there are some examples in the ESS subset: Turkey, Ukraine, Russia and Israel. Then lastly, we have GDP. We may introduce controls in the following sequence:

	Political Trust	Political Trust	Political Trust	Political Trust	Political Trust
Gov. V. against ref.	-0.40075	-0.32310	-0.22722	-0.05192	-0.07757
	(0.04)	(0.08)	(0.20)	(0.67)	(0.54)
	[0.19273]	[0.18368]	[0.17833]	[0.12195]	[0.12781]
	-0.781--0.020	-0.686-0.039	-0.579-0.125	-0.293-0.189	-0.330-0.175
Population		-0.00000	-0.00000	-0.00000	-0.00000
		(0.00)	(0.00)	(0.06)	(0.04)
		[0.00000]	[0.00000]	[0.00000]	[0.00000]
		-0.000--0.00	-0.000--0.00	-0.000-0.000	-0.000--0.00
Refugee population		0.00000	0.00000	0.00000	0.00000
		(0.00)	(0.00)	(0.42)	(0.27)
		[0.00000]	[0.00000]	[0.00000]	[0.00000]
		0.000-0.000	0.000-0.000	-0.000-0.000	-0.000-0.000
Civil conflict			1.39421	0.87315	0.89096
			(0.00)	(0.01)	(0.01)
			[0.47788]	[0.32720]	[0.34334]
			0.451-2.338	0.227-1.519	0.213-1.569
Time since-last civil conflict			0.01534	0.00508	0.00538
			(0.00)	(0.07)	(0.06)
			[0.00388]	[0.00274]	[0.00288]
			0.008-0.023	-0.000-0.010	-0.000-0.011
log of GDPpc				1.07767	
				(0.00)	
				[0.07719]	
				0.925-1.230	
log of GDPpc					1.00423
					(0.00)
					[0.07893]
					0.848-1.160
Constant	4.70536	4.83255	4.05510	-6.49600	-5.71564
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
	[0.08792]	[0.10356]	[0.22765]	[0.77145]	[0.78494]
	4.532-4.879	4.628-5.037	3.606-4.505	-8.019--4.97	-7.265--4.16
Observations	173	173	173	173	173
Adjusted R^2	0.019	0.117	0.201	0.630	0.593

Table 7.: Multivariate regression, the effect of government violence against refugees on political trust based on ESS, models nested and gradually introducing relevant controls.
Significance levels are presented in parenthesis, standard errors in square brackets followed by 95% conf. interval beneath. The number of observations is identical for each set. Reporting the range of adjusted R^2 is imprecise but likely sufficient prior to scrutinizing relevant results further.

As intuited the population variables impart the least amount of change. It should be noted however that the table says nothing about the effect of having a refugee population on government violence against refugees, which is *not* to imply, causally, that having a refugee population *makes* a government violent towards refugees; but hosting refugees *is* a necessary condition for a government to be violent against refugees. After all, the TRUST-project is partly motivated by the fact that most refugees are *not* hosted in Europe, and some European countries (the Baltics especially) hosted – using the past tense very deliberately – extremely few refugees, and governments would have limited refugees to be violent towards even if it so unfortunately inclined. The conflict measures do reduce confidence in findings somewhat, but it is the introduction of the GDP per capita measure which reduces it completely, and we should not that GDP per capita can be influenced considerably by conflict, and as such I present a specification for both a GDP per capita measure in t-1 and t-2. I do this because we may suspect that government violence against refugees (or an underlying cause of such violence) may impact GDP per capita and mediate the effect on political trust in such a manner findings *could* in fact have been valid but obfuscated by the introduction of this control through post treatment bias. I have included a simple causal diagram to show the difference between GDP per capita in the two model specifications:

Fig 11.: Avoiding post-treatment bias by lagging the GDP variable to precede the primary explanatory variable of interest – i.e.: the treatment.



My intention here is to show how lagging the GDP per capita variable to precede government violence against refugees ensures that the effect of government violence on political trust is not mediated by the introduction of GDP per capita as a control. It is a seemingly valid control. However, a problem persists. There remains potential for an underlying causal pathway to be shared by government violence against refugees and GDP: even if we lag GDP, that potential

unknown may precede both. Thus, we have dealt with a known unknown, but there remains potential for unknown unknowns – what we do not know that we do not know. Thus, there is still some potential for the finding to be valid, and our conclusion to be a type II error rejecting a false negative.

4.2.2. Examining the effects of civilian violence against refugees

In the bivariate regression, civilian violence against refugees had a near-significant impact on political trust, but the introduction of controls reduced the significance of findings to next to nothing. Here though, we should be much less surprised as the logic at work is intuitively and likely the inverse of the previous example: the population controls likely make a significant difference in the model, and we can assume that there might be more violence by civilians against refugees with more civilians and refugees. It nonetheless ought to be shown:

	Political Trust	Political Trust	Political Trust	Political Trust	Political Trust
Civ. V against ref.	-0.07117	-0.03913	-0.03860	0.00729	0.01023
	(0.21)	(0.48)	(0.50)	(0.90)	(0.86)
	[0.05683]	[0.05527]	[0.05656]	[0.05855]	[0.05858]
	-0.184-0.042	-0.149-0.071	-0.151-0.074	-0.109-0.124	-0.106-0.127
Population		-0.00000	-0.00000	-0.00000	-0.00000
		(0.00)	(0.00)	(0.00)	(0.00)
		[0.00000]	[0.00000]	[0.00000]	[0.00000]
		-0.000--0.00	-0.000--0.00	-0.000--0.00	-0.000--0.00
Refugee population		0.00000	0.00000	0.00000	0.00000
		(0.02)	(0.03)	(0.13)	(0.14)
		[0.00000]	[0.00000]	[0.00000]	[0.00000]
		0.000-0.000	0.000-0.000	-0.000-0.000	-0.000-0.000
Civil conflict			0.02406	0.05559	0.06309
			(0.81)	(0.57)	(0.52)
			[0.10014]	[0.09873]	[0.09888]
			-0.175-0.223	-0.140-0.252	-0.133-0.259
Time since- last civil conflict			0.00118	0.00235	0.00241
			(0.41)	(0.12)	(0.11)
			[0.00142]	[0.00148]	[0.00148]
			-0.002-0.004	-0.001-0.005	-0.001-0.005
log of GDPpc				-0.08857	
				(0.02)	
				[0.03734]	
				-0.163--0.014	
log of GDPpc					-0.08830
					(0.02)
					[0.03577]
					-0.159--0.017
Constant	1.69414	1.75380	1.71232	2.29372	2.28634
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
	[0.03326]	[0.04031]	[0.06504]	[0.25319]	[0.24099]
	1.628-1.760	1.674-1.834	1.583-1.841	1.791-2.796	1.808-2.765
Observations	108	108	108	108	108
Adjusted R^2	0.005	0.150	0.138	0.178	0.181

Table 8.: Multivariate regression, the effect of civilian violence against refugees on political trust based on Afrobarometer, models nested and gradually introducing relevant controls.
Significance levels are presented in parenthesis, standard errors in square brackets followed by 95% conf. interval beneath. The number of observations is identical for each set. Reporting the range of adjusted R^2 is imprecise but likely sufficient prior to scrutinizing relevant results further.

In this case we have no theoretical or intuitive basis to doubt the results, and while GDP per capita could bias results, neither does lagging the GDP per capita measure to precede the other variables influence how we ought to interpret results. We can say nothing about the effect of civilian violence against refugees and its effect on political trust based on these results.

4.2.3. Examining the effects of non-state actor violence against refugees and examining the effects of terrorist attacks against refugees.

The effect of non-state actor violence against refugees on political trust was near-significant in the bivariate models and is significant in the full model. As such, it is the most promising basis of any kind of findings, although it is also perhaps the most obvious. Now, not all non-state actors are terrorists, but certainly some are and perhaps indeed the more violent are. As such, the fact that both of these explanatory variables prove significant should bring some confidence to the findings, and I present the model showing the effect of non-state actor violence against refugees-on political trust first, then the model showing terrorist attacks.

	Political Trust	Political Trust	Political Trust	Political Trust	Political Trust
Non-state V. against ref.	-0.28776 (0.13) [0.19028] -0.665-0.089	-0.38801 (0.03) [0.18115] -0.747--0.029	-0.46050 (0.02) [0.19623] -0.850--0.071	-0.43023 (0.03) [0.19210] -0.812--0.049	-0.42905 (0.03) [0.19168] -0.810--0.049
Population		-0.00000 (0.00) [0.00000] -0.000--0.000	-0.00000 (0.00) [0.00000] -0.000--0.000	-0.00000 (0.00) [0.00000] -0.000--0.000	-0.00000 (0.00) [0.00000] -0.000--0.000
Refugee population		0.00000 (0.01) [0.00000] 0.000-0.000	0.00000 (0.01) [0.00000] 0.000-0.000	0.00000 (0.04) [0.00000] 0.000-0.000	0.00000 (0.05) [0.00000] 0.000-0.000
Civil conflict			0.12331 (0.23) [0.10308] -0.081-0.328	0.13359 (0.19) [0.10078] -0.066-0.334	0.13936 (0.17) [0.10068] -0.060-0.339
Time since last civil conflict			0.00129 (0.36) [0.00139] -0.001-0.004	0.00233 (0.11) [0.00143] -0.001-0.005	0.00238 (0.10) [0.00142] -0.000-0.005
log of GDPpc				-0.08193 (0.02) [0.03443] -0.150--0.014	
log of GDPpc					-0.08130 (0.02) [0.03290] -0.147--0.016
Constant	1.68829 (0.00) [0.03171] 1.625-1.751	1.75381 (0.00) [0.03932] 1.676-1.832	1.70575 (0.00) [0.06312] 1.580-1.831	2.24849 (0.00) [0.23629] 1.779-2.718	2.23952 (0.00) [0.22457] 1.794-2.685
Observations	108	108	108	108	108
Adjusted R^2	0.012	0.183	0.181	0.218	0.222

Table 9.: Multivariate regression, the effect of non-state actor violence against refugees on political trust based on Afrobarometer, models nested and gradually introducing controls.
Significance levels are presented in parenthesis, standard errors in square brackets followed by 95% conf. interval beneath. The number of observations is identical for each set. Reporting the range of adjusted R^2 is imprecise but likely sufficient prior to scrutinizing relevant results further.

	Political Trust	Political Trust	Political Trust	Political Trust	Political Trust
Terrorist attacks against against ref.	-0.27584 (0.24) [0.23290] -0.738-0.186	-0.40932 (0.07) [0.22216] -0.850-0.032	-0.46267 (0.05) [0.23405] -0.927-0.002	-0.42772 (0.06) [0.22903] -0.882-0.027	-0.42119 (0.07) [0.22879] -0.875-0.033
Population		-0.00000 (0.00) [0.00000] -0.000--0.000	-0.00000 (0.00) [0.00000] -0.000--0.000	-0.00000 (0.00) [0.00000] -0.000--0.000	-0.00000 (0.00) [0.00000] -0.000--0.000
Refugee population		0.00000 (0.01) [0.00000] 0.000-0.000	0.00000 (0.02) [0.00000] 0.000-0.000	0.00000 (0.05) [0.00000] -0.000-0.000	0.00000 (0.05) [0.00000] -0.000-0.000
Civil conflict			0.09504 (0.35) [0.10139] -0.106-0.296	0.10679 (0.28) [0.09914] -0.090-0.304	0.11186 (0.26) [0.09908] -0.085-0.309
Time since last civil conflict			0.00134 (0.34) [0.00140] -0.001-0.004	0.00239 (0.10) [0.00144] -0.000-0.005	0.00243 (0.09) [0.00144] -0.000-0.005
log of GDPpc				-0.08290 (0.02) [0.03469] -0.152--0.014	
log of GDPpc					-0.08169 (0.02) [0.03318] -0.148--0.016
Constant	1.68541 (0.00) [0.03169] 1.623-1.748	1.75222 (0.00) [0.03953] 1.674-1.831	1.70302 (0.00) [0.06366] 1.577-1.829	2.25241 (0.00) [0.23817] 1.780-2.725	2.23964 (0.00) [0.22663] 1.790-2.689
Observations	108	108	108	108	108
Adjusted R^2	0.004	0.174	0.168	0.206	0.209

Table 10.: Multivariate regression, the effect of terrorist attacks against refugees on political trust based on Afrobarometer, models nested and gradually introducing controls.

Significance levels are presented in parenthesis, standard errors in square brackets followed by 95% conf. interval beneath. The number of observations is identical for each set. Reporting the range of adjusted R^2 is imprecise but likely sufficient prior to scrutinizing relevant results further. Note that the terrorist attacks against refugees variable is an event count in a given year and not ordinal as the other variables.

Remember that the terrorist attacks against refugees-variable is an event count in a given year and not ordinal as the other variables, and as noted in the descriptive statistics, its maximum is in fact just two and the mean is 0.01 for all Afrobarometer countries in the period 2000-2015. This implies that the effect of just a single terrorist attack against refugees is in and of itself an outlier and has a strong effect on political trust – at least, statistically, although causally we may have reasons to reflect on this. Regardless, in conjuncture, these two tables indicate that we can in fact infer that it has a convincing and detrimental effect on political trust in the Afrobarometer subset of countries. I would like to present one final table though for the last two measures though now using robust Driscoll-Kraay standard errors as discussed in section 3.2.1, which only supports this finding further:

	Political Trust	Political Trust
Non-state V against ref.	-0.43023 (0.00) [0.07200] -0.596--0.264	
Terrorist attack against ref.		-0.42772 (0.00) [0.06976] -0.589--0.267
Population	-0.00000 (0.00) [0.00000] -0.000--0.000	-0.00000 (0.00) [0.00000] -0.000--0.000
Refugee population	0.00000 (0.02) [0.00000] 0.000-0.000	0.00000 (0.02) [0.00000] 0.000-0.000
Civil conflict	0.13359 (0.22) [0.09938] -0.096-0.363	0.10679 (0.31) [0.09873] -0.121-0.334
Time since last civil conflict	0.00233 (0.11) [0.00130] -0.001-0.005	0.00239 (0.11) [0.00131] -0.001-0.005
log of GDPpc	-0.08193 (0.03) [0.03098] -0.153--0.010	-0.08290 (0.03) [0.03074] -0.154--0.012
Constant	2.24849 (0.00) [0.15298] 1.896-2.601	2.25241 (0.00) [0.15069] 1.905-2.600
Observations	108	108

Table 11.: Multivariate regression using robust Driscoll-Kraay standard, showing, the effect of terrorist attacks against refugees on political trust based on Afrobarometer

Significance levels are presented in parenthesis, standard errors in square brackets followed by 95% conf. interval beneath. The number of observations is identical for each set. Reporting the range of adjusted R^2 is imprecise but likely sufficient prior to scrutinizing relevant results further.

5. Discussion points

5.1. Findings, or lack thereof – is it the data?

Unfortunately, while the research question which motivated this project was to investigate how various forms of refugee related violence influences political trust, we have little basis on which to do so on. As stated in the introduction, the realities of attitudinal data to capture relatively abstract and wide concept such as political trust is known to be difficult, and this motivated a priority towards including the regular and tidy execution of the ESS in an effort to give the POSVAR data the best possible chance at showing an effect. Unfortunately, this proved fruitless – although as I have argued to a somewhat extent, it remains perfectly plausible that the refugee related violence on political trust remains subtle in Europe due to the fact that within the timeframe of the POSVAR data, European countries do not – with the exception of Turkey – truly host that many refugees.

Nonetheless, on purely intuitive grounds, we ought to expect more violence against refugees in the POSVAR data and we may suspect there being some degree of underreporting, even though an ordinal scale was intended by the authors to alleviate this. I speak from anecdotal and personal experience but given that the authors use the (among other sources) the Social Conflict Analysis Database (SCAD) it is surprising that no violence and so few terrorist attacks on refugees are picked up. I have somewhat, albeit very limited and anecdotal experience using SCAD as a reference point for querying for news articles in a related project. Though despite of this being somewhat informal, I believe the reader will agree with me on that it is most peculiar that Nigeria, Niger, and Algeria, countries which firstly are included in the Afrobarometer rounds, shows no refugee related violence of any sort whatsoever. This is a region in which there have been quite a lot of refugees and violence between these three countries and their neighbors (the Chadian civil war for instance, and now Cameroon although this is at the tail-end of our data). And, given that the one effect we do find to be significant in the data, is the influence of non-state actor violence against refugees on political trust and terrorist attacks against refugees, I am all the more skeptical. The influence of groups in Nigeria and nearby such as Boko Haram and other Salafi militant groups are quite well reported and while it is of course entirely possible that these groups primarily target *internally displaced people*, it seems rather implausible if we look to Europe for a comparison. Because if we are to

accept the data at face value, we must accept that refugees in Finland, Estonia and Latvia are *much more* at risk of being the victims of violence than refugees in Nigeria, Niger and Algeria, even in absolute terms and remember, these Baltic countries have very small refugee populations.

Thus, as a final inquiry into the data quality I accessed the Dow Jones Factiva database. Factiva is in its inception a business tool owned by Dow Jones & Company which aggregates news content from a large number of newspapers. I then selected a country which had no refugee related violence of any form and which I believed was less likely to produce erroneous results due to a prevalence of internally displaced people (even though a search string can avoid this, we nonetheless run the risk of IDPs being erroneously *reported* as refugees). Now the search string I employed was quite crude and so brief I can repeat it here:

hlp=(Algeri) AND hlp=("refugees") AND hlp=("killed") NOT(internal OR IDP*) AND SN=("BBC")*

This did in fact produce news reports from a reliable news source (the BBC) that could have been presented in the data (see Appendix): firstly, Malian refugees killing a number of people; and secondly a report describing attacks on refugees returning to Morocco. One can doubt the credibility of these news sources, and perhaps the POSVAR authors have good reason for doing so in a number of events but unfortunately, the replication material for the POSVAR introduction lacks the codebook as promised by the authors, and therefore we cannot know precisely how they went ahead to query for information or what criterion they used for violence – for instance, often reports mention deliberate neglect forcing refugees to starve, which we do not know how the authors would code.

5.2. But the positives! Regarding non-state actors and terrorists

Regardless, examining the influence of non-state actor violence against refugees and terrorist attacks against refugees, produced convincing and reliable results in line with the theory I posited for such an effect. Of course, we do not precisely know the motivation of non-state actors and terrorists, but we may speculate that refugees make “soft targets” in accordance with the literature on terrorist attacks. As explained, the fact that both these measures prove significant indicates that terrorism possibly makes up part of the non-state actor groups. Nonetheless we

should be somewhat concerned with endogeneity problems. If a militant group is active in an area, they may be the cause of refugees in the first place, which is not to say that the finding is any less, but we may be somewhat skeptical as militant terrorist groups tend to be violent and show little regard for conventional borders. As such, the effect on political trust, can be just as much a byproduct of such groups being present as it is a product of said groups specifically attacking refugees. There may thus be an element to observation bias in this finding, but this is likely to be a problem with all data on militant groups and terrorists.

Furthermore, and as promised in section 3.1.3 on control variables, the use of GDP per capita must be discussed, and also what we can extrapolate from such a measure. It is especially important as its inclusion as a control reduced to insignificant the would-be key findings to disprove a scapegoating hypothesis. Firstly, GDP is in practice a rather nasty confounder which is temporally fickle in the sense that temporal lags do not necessarily mean causal lags or causal order. Many economic aspects which are integral to GDP as a measure as they influence the economy generally, such as just very basic spending or inflation. Put simply, people have expectations to an economy and changes to an economic or geopolitical situation influence those expectations well ahead of the arrival of refugees, and even more so prior to any violence that might relate to them. Other measures could therefore perhaps be more prudent such as food prices and Gini-coefficients, but in truth, so long as we remain on the country level, we are quite detached from what might motivate an individual or even group to be violent against refugees. Furthermore, such individuals or groups may choose to be violent for reasons which concern much less food prices or real economic conditions as refugees may be *perceived* as a threat to economic security nonetheless, even if they are not in fact adverse to economic conditions in a country – perhaps they are considered threats even when they are beneficial to an economy. Thus, ideally, we would prefer to get closer to the causal mechanisms as they relate to the individual, perhaps through survey data on thoughts concerning perceptions as they relate to economic security or security more generally. In this context it is worth mentioning that the Afrobarometer rounds concluding *after* the POSVAR-timeframe are increasing the number of questions and the latest round does contain some questions which could be used to capture perceptions of threat to economic safety. Though for matching with the current POSVAR-timeframe: no such luck.

We can think causally about the inclusion of GDP and what it *would take* for the inclusion of GDP as a control to impart an adverse suppression effect to significance levels. I believe there is only one causal order in which this can happen and results in a type II error. First, we assume that GDP influences political trust positively – not particularly controversial. Now, in order for a spurious and adverse suppression effect to take place, GDP must impart a positive influence on government violence against refugees while government violence against refugees must impart a negative influence on political trust, or vice versa. Only in these two configurations can GDP simultaneously influence government violence and political trust for the effect to be a suppression effect which is reductive to significance levels through being moved in both directions. If we presume then that the finding *was* correct and that government violence against refugees influences political trust negatively, then the only way this finding would be valid but simultaneously made erroneously insignificant by a suppression effect introduced by a GDP control would be *if* GDP has a positive influence on government violence against refugees.[3] Now, this is not impossible, but improbable. There are intuitively not many scenarios in which there are theoretical reasons to think an increase in GDP can increase violence against refugees with the presumption that violence against refugees negatively influences political trust, yet while an increase in GDP also increases political trust, nor the inverse. Put simply: in what environment is it, at the same time true that: (1) a better economy leads to more political trust; (2) government violence against refugees reduces political trust; and (3) a better economy leads to more government violence against refugees? Which is not to say it cannot happen, we could perhaps find situations in the data where this is indeed the case, but it could be the byproduct of the arbitrary year-assignment, maybe the sample is not homogenous enough, or just chance.

Regardless, although it would be satisfying to conclude and argue that I find limited evidence contrary to a scapegoating hypothesis within the ESS subset, I argue there is nothing to warrant such a claim: it unfortunately seems that we cannot discern anything about the effect of government violence against refugees in either dataset; and as for whether a scapegoating hypothesis is true, it remains to be shown.

[3] In fact, the "vice versa" configuration is in fact aligned with the scapegoating hypothesis where government violence would positively influence political trust and GDP negatively influences government violence against refugees – as we could presume beneficial economic conditions to lessen the need for a regime to employ scapegoating. However the "vice versa" configuration was not what the bivariate model indicated.

Thus, we are left with only the two fairly related effects: the data associated with and modelling of non-state actor violence against refugees and terrorist attacks against refugees show significant and highly impactful effects on political trust in the Afrobarometer data. In relevance to the theoretical grounding, we may suppose that ultimate, the effect of non-state actors and terrorists are probable to be more relevant to institutional performance theories rather than cultural theories or immediate economic concerns, as such groups directly challenge a state's monopoly on the legitimate use of force.

5.3. On the use of Driscoll-Kraay standard errors

A reader might inquire, if robust Driscoll-Kraay standard errors are better and safeguard against cross-sectional dependence, why not employ them across all models and specifications? After all, researchers in the field of statistics routinely run complex simulations and debate the ideal methods so we may use the solutions they provide to navigate our way to more convincing results – we of course listen to the advice of experts in their respective fields on which the basis of our methods reside. However, originally, Driscoll and Kraay's contribution (1998) only considered balanced panels. Make no mistake: my panel matching up a patchwork of survey rounds with yearly data for the purpose of approaching it as cross-sectional data through using pooled-OLS in a panel configuration is decidedly not a balanced panel. Now, Hoechle (2007), using unbalanced panels and Monte Carlo simulations argues that the adaptation of xtscc function in STATA which uses Driscroll-Kraay standard errors sufficiently adjusts for unbalanced panels even if a pooled-OLS configuration, and at the very least outperforms alternative estimators, even in small sample sizes such as what would apply to this project. However, just *how* unbalanced remains uncertain to me, and thus I remain hesitant to use the technique when my panel data is not really panel data *at all* but rather just a convenient way of structuring the data at hand in relation to survey rounds.

Furthermore, actual attempts to use Driscoll-Kraay standard errors made for the most no difference with the exception of a few surprisingly impactful examples. For instance, in identical full-model specifications, using a Driscoll-Kraay function as opposed to regular pooled-OLS altered the P-value of the influence of civilian violence against refugees on political trust from 0.9 to less than 0.000 – this radical difference in significance warrants suspicion. And I will (happily) admit, that firstly: Hoechle (2007) is but one source running one simulation; and

secondly: I am but one student whose knowledge of the even literature in political science is limited – let alone statistics. Therefore, I employed the additional technique not as a solution to cross-sectional dependence, but rather as further assurance of cross-sectional dependence not being at the basis of the effect of non-state actor violence against refugees on political trust.

6. Conclusion

In time of writing, and in fact throughout the entire writing process, Europe is no longer geographically isolated from large influxes of refugees: this has changed. But I will repeat what I stated in the introduction: we need to remember that the global total of refugees is equivalent to approximately the entire population of Ukraine, and likely double if we count internally displaced people. This serves as a stark reminder of the realities faced by a significant portion of the population of the Earth on a daily basis. And future research should likely not be eager to extrapolate findings from the Russo-Ukrainian war: interstate war, luckily, remains infrequent; and the Russo-Ukrainian war, as well as the refugees fleeing it and its consequences, might involve many highly idiosyncratic factors because of such events being rare in our time. Though perhaps it is in this sense we ought to be somewhat content with the results in this project. After all, suppose the POSVAR data was filled to the brim with an ever-increasing rate of violence against refugees or I found definitive evidence of governments deliberately attack refugees en masse to prop up populistic fervor and trust in its regime? Simultaneously, if results were significant, yet influenced political trust in such a way as to suggest people are but mildly annoyed or indifferent over the suffering of others, then this would also be a most depressive finding. Though regardless, violence against refugees does take place and in significant amounts in certain countries and its effect on political trust remains, with the exception of non-state actor violence and terrorist attacks, uncertain.

As for the problems relating to the data, it is likely too easy to criticize others while ignoring the challenges to constructing such a dataset as POSVAR; and the Afrobarometer and ESS survey data are a tough match in a number of contexts as such attitudinal data tends to be difficult to employ towards producing results. In a sense, there will thus be challenges to the data in regard to both treatment and outcome, and this tends to involve too much uncertainty to work around. Furthermore, the POSVAR dataset had not seen use – I hesitate to say this could

be for a reason – but having been produced and published, one could argue it deserved a chance at being pitched against political trust when its many specific measures on refugee related violence were aimed directly at overcoming challenges and shortages in the literature.

Nonetheless there are of course many things that could have been different both in terms of data and this project and with the benefit of hindsight I naturally contemplate the possibility of having generated the data for this project myself using for example SCAD event data as reference and appending it with emphasis on refugee related violence to create an event data set – but alas, perhaps a project for another time.

7. Literature

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8. Appendix

8.1. Afrobarometer survey question comparison between rounds

The table indicates rounds in which survey questions were phrased similarly enough to warrant pooling of survey rounds. Dash (-) indicates the round did not phrase the survey question similarly enough, and further reason is specified in its respective entry in the appendix. ~~Crossed line~~ indicate variable not used/omitted due to theoretical considerations (i.e.: people may be highly distrustful towards the *ruling party* (or opposition, or government branch) due to idiosyncratic reasons and variable may not indicate anything to extrapolate from to a systemic level. They are listed only for reference based on Afrobarometer R6 and no further entry on them are made in this appendix. A reader may question why this is necessary if indeed questions are repeated from older surveys (such as Zambia 96), and the answer is simply that questions are *adapted* from older survey rounds and not necessarily identical. This is particularly evident in the phrasing of trust in the president or PM which in R1 asks about the specific president.

Codebook publish date	DATEINT range		Pres. or PM	Parlia. Or Nat. ass.	Elect. Comiss.	~~Tax Departme nt~~	Local Assembly	~~Ruling Party~~	~~Oppos itioon parties~~	~~Police~~	~~The Army~~	Courts of Law
2016	01.03.14 – 22.11.15	6	Q52A	Q52B	Q52C	Q52D	Q52E	Q52F	Q52G	Q52H	Q52I	Q52J
2015	20.10.11 – 04.06.13	5	Q59A	Q59B	Q59C	Q59D	Q59E	Q59F	Q59G	Q59H	Q59I	Q59J
2008	04.03.08 – 31.12.08	4	Q49A	Q49B	Q49C		Q49D	Q49E	Q49F	Q49G		Q49H
2005	10.03.05 – 07.03.05	3	Q55A	Q55B	Q55C		Q55D	Q55E	Q55F	Q55H	Q55G	Q55I
2004	03.06.02 – 17.05.04	2*	Q43A	Q43B	Q43C		Q43D					Q43J
1999-2001	??.??.99 – ??.??.01	1*	-	-	-		-					-
First use source	(dd, mm, yr)		Zambia 96	Zambia 96	Zambia 96	R5	Zambia 96	Zambia 96	Zambia 96	Zambia 96	R5 (R3)	Zambia 96

* Round 2 does contain similar questions, but the scaling of respondent options changes from round 2 to 3 and thereon. In round 2, options are: not at all, a little bit, a lot and a great deal; in round 3 options change to: not at all, just a little, *somewhat*, and a lot. There is a significant difference between these options, and as such rounds 3-6 cannot be merged with rounds 1-2 even if questions are similar.

Note that the date published can be quite distinct from the DATEINT-range, where DATEINT-range indicates the timeframe which within interviews took place.
In summary, this indicates that the selected survey questions for trust in the president/PM, parliament/national assembly, electoral commission, local assembly, courts of law and election fairness are comparable between round 3, 4, 5 and 6.

Finally, round 7 is omitted as the POSVAR lacks data beyond 2016.

8.2. *Political Trust* index statistics and tests

8.2.1. Afrobarometer

8.2.1.1. Correlation matrix Afrobarometer

Variables	(1)	(2)	(3)	(4)	(5)
(1) Q52a. Trust president	1.000				
(2) Q52b. Trust parliament/national assembly	0.809	1.000			
(3) Q52c. Trust national electoral commission	0.701	0.822	1.000		
(4) Q52e. Trust your elected local government council	0.708	0.889	0.772	1.000	
(5) Q52j. Trust courts of law	0.536	0.665	0.727	0.598	1.000

8.2.1.2. Principal component analysis for Political Trust Index Afrobarometer

(obs=102)

Factor analysis/correlation	Number of obs	=	102
Method: principal-component factors	Retained factors	=	1
Rotation: (unrotated)	Number of params	=	5

Factor	Eigenvalue	Difference	Proportion	Cumulative
Factor1	3.905	3.393	0.781	0.781
Factor2	0.512	0.214	0.102	0.883
Factor3	0.298	0.099	0.059	0.943
Factor4	0.199	0.112	0.040	0.983
Factor5	0.087	.	0.017	1.000

LR test: independent vs. saturated: chi2(10) = 455.72 Prob>chi2 = 0.0000

Factor loadings (pattern matrix) and unique variances

Variable	Factor1	Uniqueness
Trust Pres	0.851	0.275
Trust Parlia	0.953	0.092
Trust ElecC	0.913	0.167
Trust Local	0.904	0.183
Trust Courts	0.789	0.377

8.2.1.3. Cronbach's Alpha test Afrobarometer

Test scale = mean(unstandardized items)
Average interitem covariance: .1003007
Number of items in the scale: 5
Scale reliability coefficient: 0.9278

8.3. ESS *Political Trust* index statistics and tests

8.3.1. ESS

8.3.1.1. Correlation matrix

Variables	(1)	(2)	(3)	(4)
(1) Trust in the legal system (trstlgl)	1.000			
(2) Trust in the police (trstplc)	0.894	1.000		
(3) Trust in politicians (trstplt)	0.865	0.756	1.000	
(4) Trust in country's parliament (trstprl)	0.915	0.809	0.929	1.000

8.3.1.2. Principal component analysis for Political Trust Index ESS

(obs=173)

Factor analysis/correlation	Number of obs	=	173
Method: principal-component factors	Retained factors	=	1
Rotation: (unrotated)	Number of params	=	4

Factor	Eigenvalue	Difference	Proportion	Cumulative
Factor1	3.587	3.308	0.897	0.897
Factor2	0.278	0.198	0.070	0.966
Factor3	0.080	0.026	0.020	0.986
Factor4	0.055	.	0.014	1.000

LR test: independent vs. saturated: chi2(6) = 927.75 Prob>chi2 = 0.0000

Factor loadings (pattern matrix) and unique variances

Variable	Factor1	Uniqueness
trstlgl	0.971	0.058
trstplc	0.912	0.168
trstplt	0.938	0.120
trstprl	0.966	0.068

8.3.1.3. Cronbach's Alpha test ESS

Test scale = mean(unstandardized items)
Average interitem covariance: 1.207125
Number of items in the scale: 4
Scale reliability coefficient: 0.9602

8.4. Factiva search results:

Repeating search string:

hlp=(Algeri) AND hlp=("refugees") AND hlp=("killed") NOT(internal OR IDP*) AND SN=("BBC")*

BBC MONITORING

Tuareg movements admit Malians involved in violence in southern Algeria

644 words
23 August 2013
09:59
BBC Monitoring Middle East
BBCMEP
English

Text of report by Mohamed Benahmed entitled "Forty **killed**, Azawad groups reveal Malians took part in the Bordj Badji Mokhtar incidents" published privately-owned **Algerian** newspaper El-Khabar website on 22 August

Azawad political movements have revealed a serious aspect of negligence in the official treatment of the incidents in Bordj Badji Mokhtar and stressed that Malian citizens who had come to **Algeria** as **refugees** had participated in the incidents and serious acts of violence which was not pointed out by the public authorities which seemed to be completely oblivious to the serious security scene taking place currently in the southernmost parts of **Algeria**.

A statement attributed to the Supreme Council for the Azawad Unity, the National Movement for the Liberation of the Azawad and the Arab Azawad Movement admitted the participation of Malian nationals in the acts of violence and fierce quarrels witnessed in the Bordj Badji Mokhtar Department.

In a statement published on electronic sites and Malian and Mauritanian newspapers, the three Azawad movements, which represent the Arabs and Tuareg of the Azawad region, urged the Azawad who are resident in neighbouring countries, including Algeria, to denounce violence and respect the laws and security of the host countries, which was an official admission that the acts of violence which have been recurring in Bordj Badji Mokhtar between Aydnan [as transliterated] and Berabish tribes since 13 August were carried out with the participation of Malian nationals living in Algeria.

The statement which was adopted by the biggest three political factions in the Azawad region urged those it referred to as the Azawad people to exercise self-control and unite ranks in confronting what it described as the schemes aimed at pushing them into fighting each other. The statement also urged the Azawad living abroad to denounce violence.

In a related development, field sources stressed that acts of killing and elimination which took place in Bordj Badji Mokhtar were carried out in a savage way and that a citizen was run over by a truck repeatedly until he died and another citizen, a young man not older than 22 years, was stabbed with a blunt metal bar, according to a forensic report, which meant that he was tortured while he was being killed. Most of the deaths were carried out with cold steel with the exception of three deaths in which firearms which were smuggled out of northern Mali were used.

Eyewitnesses confirmed that the true figure of people killed in Bordj Badji Mokhtar may exceed 40, while the official sides insisted that the official figure was only 10 people killed. Information from Bordj Badji Mokhtar conflicted about the exact number of the victims in the violence which broke out on 13 August. The official story which was based on the number of notifications of murders insisted that 10 people were killed, while stories circulating in the town mentioned a large number of deaths. The sources said that it was unexpected to declare victims considering the tribal nature and the deep-rooted rationale of tribal vengeance adopted by the majority of the region's residents. Furthermore, many citizens in Bordj Badji Mokhtar would not report a death because of the widespread practice of selling identity documents whereby the identity document of a deceased would be sold to a relative, a member of his tribe or a Malian acquaintance.

Substantive evidence indicate that reality; the Bordj Badji Mokhtar Municipality recorded less than 10 deaths in a population of 12,000 during 2005 which is a totally unrealistic figure, in addition to the discrepancies recorded by the Bordj Badji Mokhtar Department services between the age of people applying for an identity card and the actual age in the files pertaining to the holder of the card.

Source: El-Khabar website, Algiers, in Arabic 22 Aug 13

Document BBCMEP0020130823e98n003bh

BBC MONITORING

Polisario, Algerian troops chase Saharan refugees returning to Morocco - paper

482 words
8 May 2010
12:46
BBC Monitoring Middle East
BBCMEP
English

Excerpt from report by Ihsane El Hafidhi, headlined: "Security mobilization and manhunt on the borders. Polisario vehicles used to watch for Saharans returning to Morocco, and an **Algerian** helicopter **killed** seven Saharans from Tindouf", published by Moroccan privately-owned newspaper Assabah website on 8 May

Assabah has learned from informed sources that Polisario members and **Algerian** gendarmes have established monitoring posts on the borders with Mauritania and Morocco to arrest anyone coming from the Tindouf camps and likely to enter Morocco through the border checkpoint located near Dakhla city.

The same sources indicated that the Polisario militias have stationed a number of four-wheel-drive Toyota vehicles in the region to monitor the movements of people coming from the camps to Mauritania. They added that checkpoints were established along the roads and lanes used by the escapees from the Tindouf camps, while Algerian gendarmerie helicopters are combing the region every day. Other units search cars using the road being built between the camps and the Mauritanian border.

The same sources revealed that Algerian gendarmerie forces stationed in the region are arresting anyone suspected of seeking to cross into Morocco, and handing them over to the Polisario. The latter returns them immediately to the camps.

The sources stressed that the increasing number of returnees to Morocco has prompted the security services in the camps to form a joint security committee including Algerian gendarmes and Polisario army personnel. The committee is in charge of controlling the situation in the camps, and includes Mohamed Ould Akik, Polisario director of security and military intelligence, Hamma Malou, director of Saharan gendarmerie and the director of police, Cheikh Ould Errihim. Moreover, a strict control has been imposed in the camps on the families of some of those who returned to Morocco recently.

[Passage omitted: details]

The same sources underlined that those who escape from the Tindouf camps and want to return to Morocco find themselves in danger because Algerian gendarmes open fire on anyone getting close to the Moroccan border with Algeria. In fact, the sources added, an Algerian People's Army helicopter strafed a Toyota vehicle carrying Saharan Polisario members, killing seven of them. The victims included five from the Bihat tribe, two from El Faqra and one from Ouled Dlim tribe. All the bodies were carbonized after the vehicle had overturned in the region in El Hfira, 20 km away from Tifariti.

The Algerian helicopters are using the watch for smugglers and suspected Al-Qa'idah elements as a pretext to target Saharans who infiltrate across the border into Mauritania, on their way to Morocco. In fact they are safe from Algerian bullets and Polisario detention camps only when they arrive in Mauritania where Moroccan consular services make arrangements for their

entry into the national territory.

Source: Assabah website, Casablanca, in Arabic 8 May 10

abea49ee

Document BBCMEP0020100508e658002ut

8.5. Other statistical tests.

8.5.1. linktest for potential omitted variable bias

Linktest for table 5.	Political Trust Afrobarometer	Political Trust ESS
Gov. V. against ref.		
_hat	×	×
_hatsq	×	×
Civ. V against ref.		
_hat	×	×
_hatsq	×	×
Non-state V against ref.		
_hat	×	×
_hatsq	×	×
Ref. on ref. V.		
_hat	×	-
_hatsq	×	-
Ref. V against the gov.		
_hat	×	×
_hatsq	×	×
Ref. V against civ.		

_hat	×	×
_hatsq	×	×

Interpretation: ✓ indicates a significant result; × insignificant.

Insignificant _hat: not a good model

Insignificant _hatsq: no model specification problems

Linktest for table 6.	Political Trust Afrobarometer	Political Trust ESS
Ref. recr. by non-state act.		
_hat	×	×
_hatsq	×	✓
Terrorist attack by ref.		
_hat	×	×
_hatsq	×	✓
Terrorist attack against ref.		
_hat	×	×
_hatsq	×	✓
Refugee riots		
_hat	×	×
_hatsq	×	✓

Interpretation: ✓ indicates a significant result; × insignificant.

Insignificant _hat: not a good model

Insignificant _hatsq: no model specification problems

_hatsq indicates omitted variable bias: running ovtest:

Significant results:

×

✓

Conclusion: some model specifications for ESS suffer from potential omitted variable bias

In order of _hat, _hatsq

Linktest for model table 7 – gov. v. against. ref. (only running for first full model): ××

Linktest for model table 8 – civ. Against against (only running for first full model): ××

Linktest for model table 9 – non. state v against (only running for first full model): ××

Linktest for model table 10 – terror against (only running for first full model): ××

Linktest for model table 11 – Drisc. Kray. Std. (only running for first full model): NA

8.5.2. Breusch-pagan tests for heteroskedasticity

For table 5 and 6

	Afrobarometer	ESS
Gov. V. against ref.	×	×
Civ. V against ref.	×	×
Non-state V against ref.	×	×
Ref. on ref. V.	×	-
Ref. V against the gov.	×	×
Ref. V against civ.	×	×
Ref. recr. by non-state act.	×	✓
Terrorist attack by ref.	×	✓
Terrorist attack against ref.	×	✓
Refugee riots	×	✓

Interpretation: ✓ indicates a significant result; × insignificant.

Significant result = problem with heteroscedasticity

Table 7 full model: ×

Table 8 full model: ×

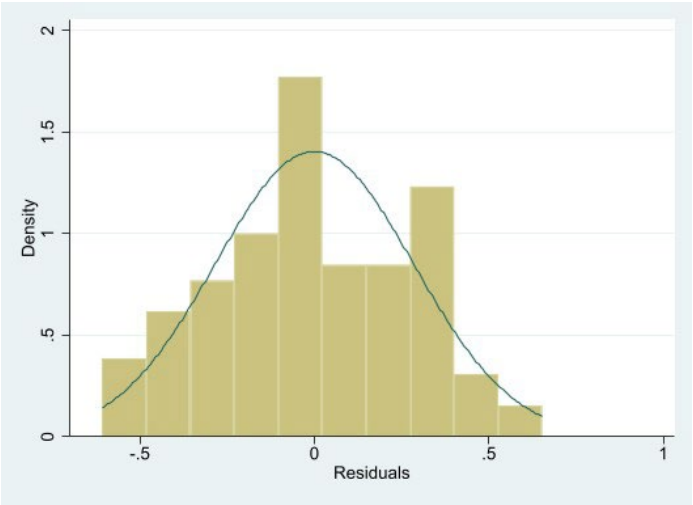
Table 9 full model: ×

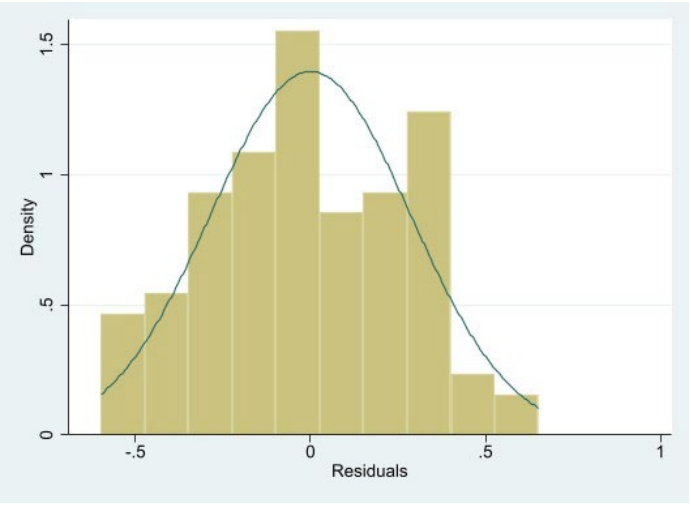
Table 10 full model: ×

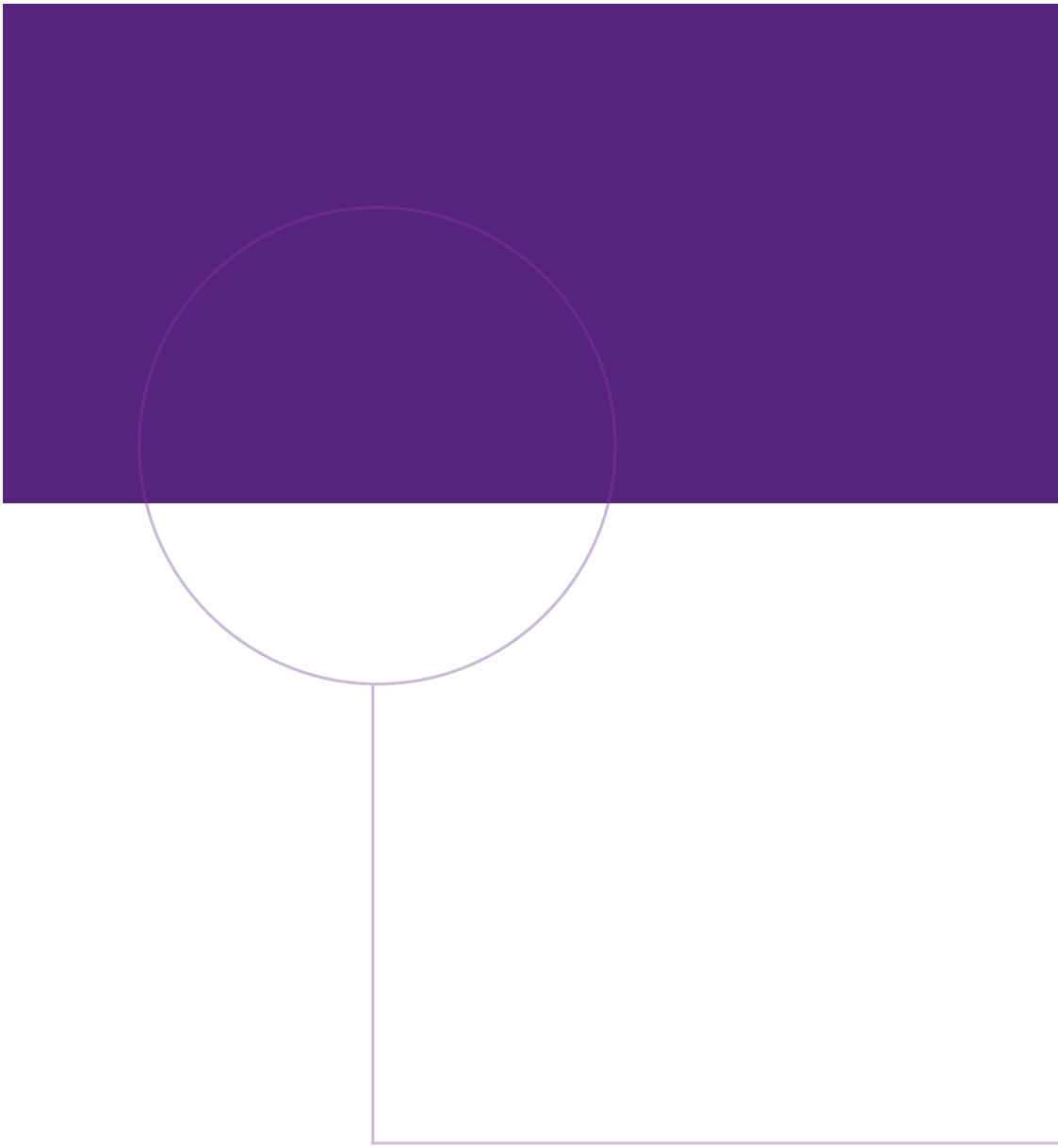
Table 11 full model: ×

8.5.3. Distribution of errors in models which relate to findings

Examining full models from table 9 and 10: = good enough







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