Crimes Against Nature: The Colonial Roots of Homophobia in Sub-Saharan Africa*

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Abstract

Anti-gay prejudice in Sub-Saharan Africa—where some countries have the death penalty for same-sex sexual activity—is often defended as tradition, though historians and anthropologists argue it largely resulted from colonial rule. We test this claim using a quasi-experiment that also separates the cultural and institutional roots of prejudice: Portugal was an outlier in limiting missionaries' influence over education (culture) and not fully criminalizing same-sex sexual activity until the eve of decolonization (institutions). Comparing Afrobarometer respondents within the same ethnic homeland on either side of Angola's and Mozambique's borders, we find substantially less prejudice toward homosexuals just inside the former Portuguese colonies: 0.84 points on a five-point scale, or 80% of a standard deviation. This effect is specific to homophobia, as attitudes toward other ethnicities, religions, and immigrants are no different along these borders. Proximity to colonial-era missions and present-day religious affiliations account for the largest shares of the gap, while schooling, living standards, and state presence account for essentially none. The attitudes we measure predict real-world outcomes: nationals of more homophobic African countries are granted UK asylum on sexual-orientation grounds more often, including for a third of all Ugandan applicants.

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1. Introduction

Prejudice against stigmatized groups is persistent, costly, and stubbornly hard to explain. Two accounts compete. One locates prejudice in culture: beliefs transmitted across generations through families, religion, and education, largely independent of what any state does. The other locates it in institutions: laws, courts, and schools that define who is a legitimate member of a society and what may be done to those who are not. Distinguishing between them is difficult because they almost always travel together—the same historical events that write a group out of the law also write it out of the curriculum. Colonial rule in Africa offers unusual leverage on this problem. European powers differed sharply, and for reasons largely unrelated to conditions on the ground, in both what they criminalized and what they taught. They then drew borders that cut through ethnic homelands, leaving otherwise similar people under different legal and educational regimes. We exploit one such difference—Portugal's—to ask how much of a specific, intensely held, and consequential prejudice is inherited from colonial culture and how much from colonial institutions.

Anti-gay prejudice in Sub-Saharan Africa is frequently portrayed as a longstanding cultural tradition, thus justifying why most survey respondents report strong antipathy toward homosexuals and some countries even impose the death penalty for consensual same-sex sexual activity (Afrobarometer, 2025; ILGA World, 2025). Yet historians and anthropologists dispute this narrative by pointing to evidence of diverse forms of same-sex relationships and gender nonconformity prior to colonial rule and arguing that anti-gay attitudes and laws stem primarily from Victorian-era sodomy statutes and Christian missionary education that redefined sexual diversity as deviance (Epprecht, 2008; Mehra, Lemieux and Stophel, 2019).

In this paper, we test the colonial origins of Sub-Saharan African homophobia by exploiting differences in European powers' attempts to regulate their subjects' private lives through laws and religion. In particular, Gomes da Costa Santos and Waites (2019) note that Portugal was an outlier in both when it criminalized same-sex sexual activity in its colonies (far later than in British ones) and in its missionaries' main focus ("civilizing" through labor rather than education). As such, their hypothesis is that places Portugal once ruled should have systematically less negative attitudes toward homosexuals today.

We first compile cross-country data on laws and attitudes in Sub-Saharan Africa. In a number of ways, we find that former Portuguese colonies are indeed less homophobic. With respect to legal regimes, data from the ILGA World Database show that all five of them have decriminalized same-sex sexual activity. In contrast, it is illegal in the majority

of former British and French colonies, and punishable in nine cases by sentences of life in prison or the death penalty. Using Afrobarometer survey data, we also find that countries once ruled by Portugal have among the least negative views of homosexuals but not of any other outgroup, implying a homophobia-specific effect.

We then turn to microdata from georeferenced Afrobarometer surveys (Afrobarometer, 2025), which we use in a regression discontinuity (RD) design along the national borders of former Portuguese colonies that compares respondents within an ethnic group homeland (Murdock, 1959; Michalopoulos and Papaioannou, 2014). Our RD results mirror the cross-country ones: attitudes toward homosexuals are substantially less negative just inside Angola and Mozambique.[1] The effect is large in magnitude and precisely estimated: 0.84 Likert scale points, or 80% of a standard deviation in the adjacent countries.

This effect is specific to homosexuals in a way that is difficult to reconcile with any general story about tolerance. At the same borders, using the same design and the same respondents, we find no discontinuity in attitudes toward people of a different ethnicity, immigrants and foreign workers, or people of a different religion. Those estimates are one-seventh to one-third the size of the homophobia effect and none is statistically distinguishable from zero. More tellingly, all three are *negative*: if anything, respondents just inside the former Portuguese colonies are slightly more hostile to other outgroups. Whatever colonial rule did here, it did not make people broadly more tolerant. It changed one belief.

In our investigation of proximate causes, we find that the share of Protestants decreases dramatically in former Portuguese colonies—but instead of having far more Catholics, these countries have much higher shares of respondents with no religion. Interestingly, subjective ratings of living conditions are better and rates of secondary schooling are lower on the former Portuguese side, suggesting that socioeconomic factors do not explain the discontinuity in anti-gay attitudes. Survey clusters there are also *more* likely to contain a police station, so a weaker present-day state presence does not explain it either. With respect to historical channels, we find that survey clusters just inside Angola and Mozambique are substantially farther from the nearest 1920s mission station (Hedde-von Westernhagen and Becker, 2022). Decomposing the border gap following Gelbach (2016), we find that this colonial-era variable accounts for the largest share of it—close to 10% at every bandwidth we consider—while contemporary religious affiliation accounts for a comparable share only in the narrowest window and about half as much thereafter, and state presence and living standards account for es-

[1] Guinea-Bissau data are not available, and Cape Verde and Sao Tome and Principe are island nations.

sentially none. We do not treat these shares as causal, but their ordering is stable across specifications and points toward the colonial period rather than the present as the source of the difference, in line with cross-sectional evidence linking missions to anti-gay attitudes (Ananyev and Poyker, 2021).

We also address the most direct alternative to this account: that what we detect is not a colonial inheritance but contemporary exposure to a Lusophone cultural sphere in which sexual minorities have been unusually visible. If so, the border effect should be concentrated among the young, the media-connected, and the urban. It is not. The effect is significantly *larger* among respondents aged 33 and older than among those aged 18-32, and it is statistically indistinguishable across households with and without a television or radio and across urban and rural areas, with point estimates in both cases slightly favoring the less-exposed group.

Finally, we provide evidence that these survey measures capture something consequential rather than cheap talk. The costs of anti-gay prejudice fall first and most heavily on sexual minorities themselves, in the form of criminalization, violence, economic exclusion, and—in several countries in our sample—capital punishment. These harms are not systematically measured in the region, so we turn to a revealed-preference proxy: combining our attitudes data with UK Home Office records, we show that nationals of more homophobic Sub-Saharan African countries are substantially more likely to be granted asylum on grounds that include sexual orientation, including in an astounding 33% of all claims filed by Ugandans. That a Likert scale item on a household survey predicts a decision as costly as flight, and an asylum claim that adjudicators judge to rest on a well-founded fear of persecution, indicates that the prejudice we measure corresponds to persecution risk severe enough to displace people. It also underscores why the origins of this prejudice matter: whether it is deeply rooted or historically contingent bears directly on whether it can be expected to change.

This paper contributes to four literatures. First, and most centrally, we add to work on the origins and persistence of prejudice. This literature has established that group animus is durable across centuries and travels through identifiable channels: religious persecution that resurfaces hundreds of years later (Voigtländer and Voth, 2012), institutional and economic environments that sustain or erode intergroup tolerance (Jha, 2013), assimilation policies that provoke backlash (Fouka, 2020), and settlement histories that leave lasting cultural residues (Bazzi, Fiszbein and Gebresilasse, 2020; Bazzi et al., 2023). It is also, almost entirely, a literature about ethnic, racial, and religious animus, in which the disliked group is a distinct population whose position was shaped by conquest, migration, or labor market competition, and in which intergroup contact organizes much of

the evidence (Rao, 2019; Lowe, 2021). Prejudice against sexual minorities has a different structure. The disliked group is dispersed within every community rather than concentrated in one, is largely unobservable, and competes for no distinct economic niche, so neither contact nor competition offers a natural account of why animus toward it is so much stronger than animus toward any other outgroup in the same survey—a gap we document across nearly every country on the continent. What people were taught about sexual minorities is correspondingly more visible as a channel, and beliefs about what others believe can sustain such norms long after their origins (Bursztyn, González and Yanagizawa-Drott, 2020). In studying this case we also build on work measuring the economic consequences of prejudice (Becker, 1957; Charles and Guryan, 2008) and its political uses (Glaeser, 2005; Alesina, Miano and Stantcheva, 2023).

Second, we contribute to work on the long-run effects of colonial rule (e.g., Acemoglu, Johnson and Robinson, 2001; Nunn, 2008; Dell, 2010; Michalopoulos and Papaioannou, 2014; Lowes and Montero, 2021). Most of this literature identifies effects of colonial institutions on state capacity, public goods provision, and generalized trust—outcomes plausibly mediated by the material conditions that colonial rule also shaped. What we find is a colonial effect on a moral attitude that persists even though the material conditions run in the opposite direction: respondents on the former Portuguese side rate their living conditions *better*, are *less* likely to have attended secondary school, and live in places *more* likely to contain a police station. Whatever transmits this difference across the border, it is not contemporary prosperity, education, or the reach of the state.

Third, we speak to the literature on culture and institutions (e.g., Guiso, Sapienza and Zingales, 2006; Tabellini, 2010; Alesina and Giuliano, 2015), which has generally established that both matter without being able to weigh one against the other. Our setting has a feature this work rarely gets: both channels were set by the same colonizer, but at different times. Portuguese educational policy operated for roughly seventy years, whereas the criminal prohibition arrived in 1954, barely a decade before independence. The institutional channel had very little time in which to work. The cross-colonizer evidence points the same way: we find no discontinuity in attitudes along former French-British borders, where colonial-era legal regimes differed sharply but post-independence legislation converged (Section 4.4). We therefore read our results as bounding the institutional channel from above and pointing toward the transmission of beliefs through missionary education, consistent with cross-sectional evidence linking missions to anti-gay attitudes (Ananyev and Poyker, 2021) and with work on how religious institutions shape norms (Cantoni, Dittmar and Yuchtman, 2018). Whereas much of this literature examines broad cultural traits such as trust, cooperation, or civic norms (e.g., Nunn and

Wantchekon, 2011; Lowes et al., 2017), we study a targeted moral attitude with direct political salience.

Finally, we contribute to the economics of sexual orientation and gender identity, in which the evidence base remains heavily skewed toward high-income countries (Badgett, Kreider and Meyer, 2021; Badgett et al., 2024; Nyeck and Shepherd, 2019). In those settings the margins of interest are typically discrimination in labor and marriage markets; here they are criminalization, exclusion, and physical risk, which is what makes the asylum evidence interpretable. By linking survey attitudes to administrative asylum records, we also offer a validation exercise of potential use to the wider attitudes literature: a single Likert scale item on a household survey predicts a costly and consequential action taken thousands of kilometers away.

2. Background

Murray and Roscoe (1998) and Mehra, Lemieux and Stophel (2019) provide overviews of the evidence that same-sex relationships were prevalent in a wide range of precolonial African societies. Beginning most notably with Evans-Pritchard (1970), these studies contradicted the narrative from previous centuries that if "Africans were the most primitive people in all humanity ... then they had to be the most heterosexual" because biological reproduction is the most primitive activity (Murray and Roscoe, 1998, p. xi).

2.1. Colonial Sodomy Laws and Missionary Education

Portuguese academics and officials largely subscribed to this view, believing that homosexuality was an urban and wealthy phenomenon, so there was little need to apply Portugal's sodomy law to the colonies. Indeed, when it finally happened in 1954, it was done in the context of concerns over recent white settlers paying Africans for sex (Gomes da Costa Santos and Waites, 2019). This approach was essentially the midpoint between the French and British ones: the Napoleonic Code had decriminalized same-sex sexual activity in France (and Belgium), so it was never illegal in these colonies, whereas Britain's sodomy law applied in its African territories from the late nineteenth century.

Portugal was more distinct in terms of its provision of education (or failure to do so): by 1909, there were only fifty-six primary schools in Mozambique and sixty-nine in Angola (Duffy, 1963). The schools that did exist were run primarily by Catholic missionaries, who were charged in a 1941 agreement with the Vatican with providing a "moral education [that] shall aim at curing laziness and preparing future rural workers and craftsmen to produce what they need to satisfy ... their social obligations" (as cited in

Ferreira, 1974, p. 67). In contrast, the British relied heavily on Protestant missionaries to provide education—a 1953 Colonial Office report noted that "deep and sincere religious belief [should be] the basis of all education ... [as] religious and character training[] is of the greatest importance" (as cited in White, 1996, pp. 18-19)—while the French supported Catholic missionary activity but offered a secular education in the state-run system (Daughton, 2006). As such, Gomes da Costa Santos and Waites (2019) contend that the transmission of missionary values was much stronger in British colonies than in Portuguese ones, and we view the French colonies as falling somewhere in between.

2.2. *(De)criminalization of Same-Sex Sexual Activity After Independence*

Upon independence, which generally occurred in the 1960s, many colonial-era criminal codes remained in place, so same-sex sexual activity was still illegal in former British and Portuguese colonies and legal in former Belgian and French ones. Figure 1a shows the changes in these laws over the past 65 years using information from the ILGA World Database. The first shift was in 1966 when Senegal criminalized this conduct, the maximum punishment for which was five years in prison. As detailed in Appendix A1, five more countries once ruled by France did the same over the next two decades, culminating in 1983 with Mauritania making the maximum sentence for Muslim men the death penalty by public stoning. More recently, the former Belgian colony of Burundi and five other former French ones have passed laws imposing prison terms of up to ten years.

However, there has also been movement in the opposite direction by countries once ruled by Britain and Portugal, especially the latter. The first on the continent to decriminalize same-sex sexual activity was Guinea-Bissau in 1993—at the same time as Irelan—and by 2021, the four other former Portuguese colonies had as well. Among countries that inherited British anti-sodomy laws, South Africa was the first to decriminalize same-sex sexual activity in 1998, and three of its five neighbors (Lesotho, Botswana, and Namibia) plus two island nations (Seychelles and Mauritius) repealed them by 2024.[2] But the situation is very different outside of the continent's southern tip, as none of these former British colonies has repealed its laws.

2.2.1. *Maximum Sentences*

Figure 1b shows countries' present-day maximum sentences for consensual same-sex sexual activity. For those once ruled by Britain and France, there are clear bimodal distributions: one peak is around non-criminalization but the other is around the most

[2] To keep the categories simple, we group South Africa and Namibia with countries formerly ruled by Britain because it was the source of their laws making same-sex sexual activity illegal.

draconian punishments—life sentences (five countries) and death penalties (three)—in former British colonies and five-year prison sentences in former French ones. Conversely, it is not illegal in two of the three countries formerly ruled by Belgium, though Burundi imposes a sentence of up to two years in prison.

2.3. *Attitudes Toward Homosexuals and Other Outgroups*

Next, we examine whether former Portuguese colonies are exceptional in survey measures of homophobia as well. We use data from Afrobarometer, which regularly assesses public opinion on a range of topics in more than thirty countries on the continent. Our outcome of interest is a respondent's Likert scale rating—would strongly dislike, somewhat dislike, not care, somewhat like, or strongly like—of the prospect of having a member of the following outgroups as a neighbor: homosexuals, people of a different ethnicity, immigrants and foreign workers, and people of a different religion. We code these responses as integers from -2 to 2, interpreting larger negative numbers as stronger prejudice toward the group and higher positive numbers as greater affinity for it.[3]

These questions were asked in Afrobarometer rounds 6 (2014-15), 7 (2016-18), 8 (2019-21), and 9 (2021-22), so we pool these datasets and calculate a country's mean Likert scale rating of an outgroup after adjusting for fixed effects for a respondent's age and sex, the survey round, and the region in which a country lies. Figure 1c shows these values ranked from lowest to highest for each of the four groups, and two facts are immediately apparent. First, effectively every country has a negative average rating of homosexuals (top left panel) but positive ratings of all other groups. Additionally, the only clear pattern is that the four former Portuguese colonies with available Afrobarometer data are among the seven countries with the least antipathy towards homosexuals—a phenomenon that is made even more striking by their below-median levels of affinity for all other outgroups. Perhaps not surprisingly, the other countries in the top quartile of attitudes toward homosexuals are formerly British colonies that are island nations (Mauritius and Seychelles) or South Africa and one of its neighbors (Namibia).

3. Main Result

In this section, we use an RD design along national borders to test whether these historical narratives and cross-country patterns are reflected in quasi-experimental evidence.

[3] We prefer this approach over, e.g., an indicator for strongly disliking a member of a given outgroup because it exploits all the available information and, as we discuss below, it avoids floor effects in our placebo tests because very few respondents express such strong antipathy toward the other groups.

3.1. *Data*

We combine georeferenced Afrobarometer round 8 data from two former Portuguese colonies in Southern Africa (Angola and Mozambique) and seven adjacent countries (Eswatini, Malawi, Namibia, South Africa, Tanzania, Zambia, and Zimbabwe). Figure 2a shows the locations of survey clusters within 250 km of a border between Angola or Mozambique and their neighbors. Given the discussion in the previous section, it is important to note that because we are limited to these national borders, two of our seven adjacent countries are arguably the least homophobic ones on the continent that were not ruled by Portugal. As such, our estimates when including them should be smaller than when the sample contains only more typical former colonies.

3.2. *Empirical Strategy*

We use the georeferenced data in an RD design that tests for discrete changes in attitudes toward homosexuals along the former Portuguese borders. The estimating equation is

$$y_{i,c} = \alpha + \tau \text{Portuguese}_c + f(\text{Distance}_c) + \mathbf{\Gamma}_i + \delta_{e(c)} + \epsilon_{i,c} \quad \text{for } c \in B^*_{\text{MSE}}, \tag{1}$$

where $y_{i,c}$ is the outcome of interest for individual i in survey cluster c, Portuguese_c indicates whether c is in Angola or Mozambique, $f(\text{Distance}_c)$ is the RD polynomial in c's distance to the nearest border between one of these countries and a neighbor, $\mathbf{\Gamma}_i$ is a vector of fixed effects for i's predetermined characteristics (age and sex), $\delta_{e(c)}$ is a fixed effect for the Murdock (1959) ethnic group homeland containing c (as in Michalopoulos and Papaioannou, 2014), and $\epsilon_{i,c}$ is the idiosyncratic error term.

The coefficient τ captures the average change in the outcome when crossing a border partitioning an ethnic group homeland between a neighboring country and a former Portuguese colony, which holds many geographic and cultural traits fixed.

The intuition is best seen through a concrete case. The Chewa homeland straddles the Mozambique-Malawi border; the Yao homeland straddles Mozambique-Tanzania; the Ovambo homeland straddles Angola-Namibia. In each case, the border was set in European capitals during the Scramble for Africa with reference to rivers, latitudes, and the balance of power in Europe—not to where one people ended and another began. A Chewa household a few kilometers inside Mozambique and a Chewa household a few kilometers inside Malawi therefore share precolonial institutions, kinship and inheritance norms, language, climate, and soil. What they do not share is which European power governed them: whether same-sex sexual activity was criminalized for seventy years or for ten, and whether the school within reach was run by Protestant missionaries

for whom education was the mission or by a Portuguese administration for which it was an afterthought.

The identifying assumption is that all other determinants of attitudes toward homosexuals vary smoothly across these borders, so that any discrete change at the border is attributable to colonial rule. Two threats deserve attention. The first is selective migration: if more tolerant individuals sorted into Angola and Mozambique, we would observe a discontinuity without any colonial effect. This is implausible in a setting where cross-border movement in the relevant decades was driven by civil conflict and labor migration rather than by social attitudes (Epprecht, 2001), and where, as we show below, the discontinuity appears for attitudes toward homosexuals alone. The second is that the colonial regimes differed in ways other than those we emphasize and that independently affect tolerance, most obviously in state capacity and in economic development. We address this directly in Section 3.4 by showing that attitudes toward other outgroups do not break at the border, and in Section 4 by showing that living conditions are rated *better* and secondary schooling is *lower* on the Portuguese side—both of which cut against a development-based explanation—and that survey clusters there are *more* likely to contain a police station, which cuts against an explanation based on the weaker reach of the state.

For estimation, we use a local linear RD specification (Gelman and Imbens, 2019), a triangular kernel that gives the highest weights to the observations closest to the border (Cattaneo, Idrobo and Titiunik, 2020), and the bandwidth that optimizes mean-squared error (MSE) to select the observations included in the sample (Calonico, Cattaneo and Titiunik, 2014). For inference, we cluster standard errors by first-level administrative unit (i.e., province or region), the minimum number of which is thirty-nine in the MSE-optimal bandwidths.

3.3. Homophobia

Table 1 Panel A column (1) contains the RD result for homophobia. We estimate that the average rating of homosexuals increases 0.84 Likert scale points just inside the former Portuguese colonies. This effect is almost two-thirds of the mean and 80% of the standard deviation (SD) in the adjacent countries, and it is very precisely estimated (SE 0.24). The corresponding RD plot in the top left panel of Figure 2b shows this discontinuity visually. Interestingly, despite the large discrete change in attitudes toward homosexuals, very few Angolans and Mozambicans living near national borders have positive views of homosexuals. Instead, the treatment effect moves them from between a strong dislike and a slight one to between a slight dislike and indifference.

3.4. *Placebo Tests: Attitudes Toward Other Outgroups*

We then examine whether the improvement in attitudes toward homosexuals is part of a general pattern of greater tolerance for outgroups. However, the estimates in columns (2)-(4) strongly cast doubt on this notion: none is distinguishable from zero at conventional levels of significance and they are one-seventh to one-third of the size of the homophobia effect. In addition, all of these estimates are negative, which if anything makes the discontinuity in attitudes toward homosexuals more striking because it is strongly in the opposite direction of these slightly greater prejudices. The corresponding RD plots in Figure 2b show the absence of discrete changes at the border.

3.5. *Robustness Tests*

We take several steps to test the robustness of these results. First, because MSE-optimal bandwidths can change across outcomes, a shifting sample composition might create differences in the respective RD estimates. To address this concern, in Appendix B1 we present the results estimating equation (1) using bandwidths ranging from 50 to 250 km in 25-km increments and the wild cluster bootstrap due to the low number of provinces and regions in the sample when bandwidths are small (Cameron, Gelbach and Miller, 2008). In all cases, the estimates are stable around the values in Table 1, and the wild cluster bootstrap p-values for Likert scale ratings of homosexuals are almost entirely below 0.10. We also do the same process after switching to a uniform weighting kernel (Appendix B2) and a quadratic RD polynomial (Appendix B3), and our conclusions once again do not change, though confidence intervals are wider for small bandwidths when using the quadratic specification.

Additionally, because Afrobarometer policies limit us to a single round of georeferenced data from multiple countries, the density of survey clusters near former Portuguese borders might be too low for reliable RD estimation. We deal with this concern in Appendix B4 by using the Cattaneo, Frandsen and Titiunik (2015) randomization inference procedure across windows ranging from one to twenty survey clusters on each side of former Portuguese borders.[4] The results are strikingly consistent with those in Table 1: the estimates for attitudes toward homosexuals are of relatively large magnitude (around 0.5) as long as there is more than one cluster on each side, and randomization inference p-values are below 0.10 in fourteen of the twenty windows. In contrast, the estimates for the other outgroups when windows contain two or more clusters are smaller,

[4] When using the maximum number of survey clusters, the randomization inference window extends 2 km into adjacent countries and 35 km into former Portuguese colonies.

almost uniformly negative, and none is statistically significant.

We also transform the outcome into an indicator for strongly disliking the prospect of having a member of an outgroup as a neighbor. The estimates in Appendix B5 are exactly in line with the main results above: the share that would strongly dislike a homosexual neighbor drops 41 percentage points (p.p.), or nearly two-thirds of the adjacent country mean, just inside Angola and Mozambique, while the estimates for the other outgroups are small, positive, and not distinguishable from zero. As anticipated, the RD plots in Appendix B6 show that floor effects may be a concern, though the mean being so high for homosexuals but so close to zero for other groups is informative on its own.

3.6. Excluding South Africa and Namibia

We view the evidence from these robustness tests as strongly supporting the internal validity of our main results. But because our data are limited to Southern Africa, the adjacent country sample includes South Africa and Namibia, which are outliers among former non-Portuguese colonies in their decriminalization of same-sex sexual activity and attitudes toward homosexuals. To understand what the effect sizes would be when comparing the former Portuguese colonies to more typical Sub-Saharan African countries, we exclude the observations nearest to the Angola-Namibia and Mozambique-South Africa borders and reestimate equation (1). In line with our prediction, the RD estimate for homosexuals in Appendix B7—a very precisely estimated 1.20—is 0.36 Likert scale points (43%) larger than the estimate in Table 1, whereas the other estimates are essentially unaffected. The RD plots in Appendix B8 show that this change arises almost entirely from lower ratings just inside the remaining adjacent countries.

4. Mechanisms

We then study the characteristics of respondents that give rise to this reduction in homophobia—but not any other prejudice—along former Portuguese borders.

4.1. Proximate Cause: Religion

Given the discussion in Section 2 of historical differences in missionary activity and their hypothesized effects on attitudes toward homosexuals, our first test of this channel is to examine present-day religious affiliations. We do so by defining the outcome as an indicator for belonging to one of four major groups (Protestants, no religion, Catholics, or Christians who do not name a denomination) and estimating equation (1). Table 1

Panel B presents these results. In column (1), there is a 34-p.p. decrease in the share of Protestants just inside the former Portuguese colonies, which is 65% of the adjacent country mean. Interestingly, the border effects in columns (2)-(4) reflect that Angolans and Mozambicans are primarily more likely to have no religion (22 p.p., or 440%), and to a lesser extent Catholic (10 p.p., or 71%), while fewer of them identify simply as Christian without naming a denomination (10 p.p., or 50%). Each of these effects is precisely estimated, and the top panels of Figure 3a show the discrete changes in being Protestant and having no religion just inside the former Portuguese colonies.

We probe the robustness of these results in the same manner as above. Specifically, in Appendices C1-C3 we show the estimates and wild cluster bootstrap confidence intervals over a range of bandwidths when using our baseline specification, a uniform kernel, and a quadratic RD polynomial. In all three cases, the effects remain large and statistically significant for Protestantism and no religion but not for the other two categories. A similar pattern is also apparent when using randomization inference in Appendix C4. Because the four categories are mutually exclusive and exhaustive, and because the two that are individually robust are also the two that move most, we enter religious affiliation into the decomposition below as a single group rather than one indicator at a time.

4.2. Proximate Causes: Living Conditions, Secondary Schooling, and State Presence

Next, we study whether outcomes related to economic development change discontinuously along former Portuguese borders. Our first measure in this domain is whether respondents rate their living conditions as fairly good or very good, as the perceived economic threat of an outgroup is an important determinant of attitudes toward it (Glaeser, 2005; Manstead, 2018).[5] Consistent with this notion, Table 1 Panel C column (1) shows a precisely estimated 22-p.p. increase in the likelihood of living conditions being assessed as good just inside the former Portuguese colonies, which is 110% of the adjacent country mean. This discontinuity is clearly visible in the bottom left panel of Figure 3a.

Because of links between Protestantism and education (Becker and Woessmann, 2009; Cagé and Rueda, 2016), the religion results above suggest that schooling levels might also differ along former Portuguese borders. If so, recent evidence from Europe suggests that the side with more schooling should have lower levels of prejudice (Cavaille and Marshall, 2019; Yang, 2022). However, we find the opposite in this context: in column (2) and the bottom right panel of Figure 3a, the share of respondents that have attended

[5] For this reason, we view this variable's subjectiveness as a strength instead of a weakness. In addition, it captures the fact that country-specific frames of reference (e.g., recency of civil war or hyperinflation) can lead to different levels of satisfaction with the same objectively measured level of development.

secondary school decreases a precisely estimated 19 p.p. (34%) just inside Angola and Mozambique. One way to understand this finding is that the difference in education suppresses the difference in attitudes toward homosexuals; another possibility is that lower secondary schooling rates imply fewer Protestant schools that transmit anti-gay attitudes to their students. We test the first proposition in the decomposition below and further examine the link—or lack thereof—between schooling and homophobia in Sub-Saharan Africa in a companion paper (Denton-Schneider and Garg, 2025).

A third possibility is that what changes at the border is the reach of the state rather than anything about beliefs. If the government were simply more distant on the Portuguese side, laws of any kind—including those governing private life—would bind less tightly there, and attitudes might follow. Our measure of this channel is whether a respondent's enumeration area contains a police station, the most visible everyday manifestation of state presence in these settings (Herbst, 2000). Column (3) of Panel C shows that the data run against this account: survey clusters just inside Angola and Mozambique are 23 p.p. more likely to contain a police station, which is 96% of the adjacent country mean, and the discontinuity is visible in the left panel of Figure 3b. The state is if anything *more* present on the side of the border where prejudice is lower.

For all three outcomes, the results are very much robust to the choice of bandwidth as well as using a uniform kernel or a quadratic RD polynomial (Appendices C5-C7), and the same is true when using randomization inference (Appendix C8). Thus, we include all of them in the decomposition.

4.3. Historical Channel: Colonial Mission Presence

We then look to the colonial period itself. The historical factor we study is a survey cluster's distance to the nearest 1920s-era mission, which we view as a measure of Christian missionaries' ability to transmit the European values of that era to the African population. We use the digitized map from Hedde-von Westernhagen and Becker (2022), which contains more mission stations than other sources, a fact that can substantively affect empirical findings (Jedwab, Meier zu Selhausen and Moradi, 2022), and winsorize these values at 90 percent to limit the influence of outliers. The estimate in Table 1 Panel C column (4) is of a 29-km increase in this distance just on the Portuguese side of a former border—78% of the adjacent country mean and 0.37 SD—and it is statistically distinguishable from zero (SE 14). The right panel of Figure 3b shows this clear discontinuity.

Together with the previous subsection, this gives the pattern the rest of the paper turns on. The two things that are worse on the Portuguese side of the border are mis-

sionary reach in the colonial period and secondary schooling today; everything else we can measure—living standards, state presence—is better there. Only the first of these is a plausible carrier of beliefs about sexual minorities, and only it was determined during the period in which the two colonial regimes actually differed.

As with the previous outcomes, we examine whether these results change when varying the RD bandwidth, kernel, and polynomial order (Appendices C9-C11) and when using randomization inference (Appendix C12). The mission distance variable loses statistical significance in some of these tests, but the effect sizes are stable across bandwidths. We take this evidence to broadly support the existence of colonial-era differences in missionary presence, and thus include the variable in the decomposition.

4.4. Placebo Test: Former French Borders

Lastly, we perform an additional set of placebo tests to determine whether there are discrete changes in attitudes toward outgroups along borders between former colonies ruled by different European countries. With the Afrobarometer data, it is possible to do so for twelve countries in West and Central Africa that were once colonies of France (Benin, Burkina Faso, Cameroon, Côte d'Ivoire, Guinea, Niger, Senegal, and Togo) or Britain (Gambia, Ghana, Nigeria, and Sierra Leone). Appendix C14 contains a map of these countries and survey clusters within 100 km of a former French-British border. This exercise tests one specific competing explanation: that what we detect along former Portuguese borders is a generic difference between Catholic and Protestant colonizers, or an artifact of using former British colonies as the comparison group. We note at the outset that it is a limited test. The West African sample sits in a very different religious context—roughly seventy percent of respondents on both sides of these borders are Muslim—so it speaks to the sufficiency of a Catholic-Protestant contrast for generating anti-gay attitude gaps, and not to the role of Christian missionary education in Southern Africa.

Table 1 Panel D presents the results when estimating equation (1) after replacing Portuguese_c with an indicator for being in a former French colony. The estimates are small (0-10 p.p.) and not distinguishable from zero, especially for homophobia in column (1). The implication is that simply having countries once ruled by Britain as controls is not sufficient to generate differences in attitudes toward any outgroup. In Appendix C15, we test for discrete changes in religious affiliations (as in Panel B) and find that there are significantly fewer Protestants (5 p.p.) and more Catholics (7 p.p.) just inside former French colonies. We interpret this result as evidence that simple Protestant-Catholic differences are also not sufficient for different levels of anti-gay prejudice. Instead, the

implication is that the key faith-related difference for homophobia might be in the share with no religion. Religious affiliation enters the decomposition below as a single group, so we cannot separate that category's contribution there; what the decomposition does show is that affiliation as a whole accounts for less of the border gap than colonial mission proximity does at all but the narrowest bandwidth.

These null results raise an apparent puzzle. France never criminalized same-sex sexual activity in its colonies, whereas Britain did so from the late nineteenth century, so if colonial legal regimes were the operative channel we should expect a discontinuity along precisely these borders. The resolution is that colonial-era law is a poor description of the legal environment these populations have actually lived under. As documented in Section 2.2, beginning with Senegal in 1966 at least six former French colonies criminalized same-sex sexual activity after independence, culminating in Mauritania's 1983 statute making the maximum sentence death by public stoning, while nearly all former British colonies retained the statutes they had inherited. By the period our attitudes data cover, the legal regimes of the two groups had largely converged, and the absence of a discontinuity in attitudes is what our framework predicts rather than a challenge to it. What did not converge is the colonial educational environment, and it is there that we find the discontinuity in Southern Africa.

Read together with the timing of Portuguese criminalization in 1954, we take this as evidence that the institutional channel does not operate independently of the cultural one. Colonial-era law had little time to shape beliefs in the Portuguese case and did not do so in the French one; what varies systematically with contemporary attitudes is the reach of missionary education. A natural reading is that causation runs at least partly in the other direction, with independent states legislating in the direction their inherited beliefs already pointed—decriminalizing in the former Portuguese colonies and criminalizing in much of former French Africa.

4.5. Decomposing the Border Gap

To gauge each channel's contribution to the difference in anti-gay attitudes, we decompose the border gap following Gelbach (2016). The exercise has the structure of the omitted-variable-bias formula. We first estimate equation (1) with no channels included, obtaining the base estimate τ^{base} reported in Table 1 Panel A. We then estimate it again with all of the candidate channels included at once, obtaining τ^{full}, and allocate the difference $\tau^{\text{base}} - \tau^{\text{full}}$ across them: each channel's contribution is the coefficient from regressing it on the Portuguese indicator, multiplied by its own coefficient in the full specification.

Two features of this decomposition make it preferable to adding covariates one at a time and reading off how the treatment effect moves. First, the allocation is derived from a single full specification, so it does not depend on the order in which the channels are introduced—the path dependence that makes sequential "controlling for" exercises difficult to interpret when the channels are correlated with one another, as ours plainly are. Second, because the base and full estimates come from a system that can be estimated jointly, Gelbach (2016) provides the asymptotic distribution of each contribution, which is what allows us to attach confidence intervals to the shares rather than reporting bare point estimates. Appendix C13 gives the equations. Figure 3c reports each share of τ^{base} with its 95% confidence interval across bandwidths from roughly 100 to 300 km, grouping the four religious affiliations together and the two living-standards measures together.

We emphasize that this is a descriptive exercise rather than a causal one. Recovering channel-specific causal effects would require each covariate to be as good as randomly assigned conditional on the controls, which is not defensible here: mission placement is itself an outcome of colonial policy and is correlated with much else that colonial policy determined. What the decomposition can credibly deliver is an *ordering*, and the ordering is stable. Distance to the nearest 1920s mission accounts for close to 10% of the border gap at every bandwidth we consider, and it is the largest single contributor everywhere except the narrowest window. Contemporary religious affiliation accounts for a comparable share (about 11%) at that narrowest bandwidth but declines steadily to roughly 5% at the widest—the pattern one would expect if present-day religion partly reflects the same colonial-era variation that mission distance measures directly. The two contemporary channels account for essentially nothing: the shares for state presence and for living standards sit within a percentage point or two of zero at every bandwidth, with confidence intervals that comfortably include it. Taken together, the factors determined during the colonial era outweigh contemporary socioeconomic ones in accounting for the gap, which is the conclusion we carry forward; we do not read the individual shares as estimates of how much each channel would move attitudes if it could be manipulated.

4.6. Is It Contemporary Lusophone Visibility?

A distinct alternative to our account locates the Portuguese difference not in the colonial period but in the present. The Lusophone cultural sphere has had unusually early and prominent LGBTQ+ visibility—Brazilian *travesti* performers toured Lourenço Marques and Beira in the late 1960s and 1970s, and Angolan artists such as Titica and Coréon Dú are prominent today—and Angolans and Mozambicans consume Brazilian and Por-

tuguese media that their anglophone neighbors do not. If this is what we are detecting, the border effect should be a recent phenomenon carried by media: concentrated among younger cohorts, among respondents with access to television or radio, and in urban areas where such content circulates most freely.

Figure 4 tests these predictions by estimating equation (1) separately within each subgroup. None of them holds. The effect is *larger* among respondents aged 33 and older (1.11, SE 0.25) than among those aged 18-32 (0.73, SE 0.24), and this difference of 0.38 (SE 0.16) is the one comparison of the three that is statistically significant—pointing in exactly the opposite direction from a story about recent cultural exposure. Across the other two splits the effect is essentially flat: 0.75 (SE 0.25) for respondents in households with a television or radio versus 0.96 (SE 0.27) for those without (difference 0.20, SE 0.55), and 0.70 (SE 0.27) in urban areas versus 1.02 (SE 0.25) in rural ones (difference 0.33, SE 0.43). Neither difference is distinguishable from zero, and both point estimates run mildly against the media account rather than for it.[6]

We read this as evidence against contemporary media exposure as the primary driver, though not as a refutation of the broader Lusophone-sphere hypothesis. Cohort differences are also consistent with attitudes hardening or softening over the life cycle, and household media access is a coarse proxy for exposure to any specific cultural content. What the pattern does rule out is the simplest version of the alternative, in which the gap is produced by what people are watching now.

5. UK Asylum Grants with Basis of Sexual Orientation

The costs of the prejudice we have documented fall first on sexual minorities themselves. In the countries in our sample these include criminal sanction—up to and including life imprisonment and the death penalty—alongside violence, social exclusion, and restricted access to employment and health care (Nyeck and Shepherd, 2019; Shaw et al., 2021). These harms are the reason the subject matters, and they are also very difficult to measure systematically: there is no cross-country register of who was assaulted, dismissed, or driven out. In this section we therefore use a revealed-preference proxy to establish that our survey measure corresponds to persecution risk rather than to cheap talk. If Likert scale ratings of homosexuals capture something real about the danger of living openly, they should predict the decision to flee and to claim asylum abroad on that basis. We test this using data by nationality from the UK Home Office (2025) on the

[6] The urban-rural comparison controls for fixed effects for the nearest international border rather than for ethnic homeland, because only four partitioned homelands contain urban enumeration areas on both sides of a border.

number of asylum claims made and granted, and in how many sexual orientation was part of a claim's basis.[7]

According to these data, in the 2015-23 fiscal years there were just over 475,000 UK asylum claims, 15% of which were made by nationals of Sub-Saharan African countries. In over 6% of them, sexual orientation formed part of the claim's basis, which was more than twice the share for all other regions.

To assess whether these outcomes track the attitudes we measure, we combine the UK Home Office data with the Afrobarometer data from rounds 6-9 and regress the share of a country's nationals' asylum claims that were granted on grounds that included sexual orientation on its mean Likert scale rating of homosexuals (net of fixed effects for age, sex, round, and region), weighting countries by their number of claims.

We use grants rather than claims as the outcome because a grant records an adjudicator's judgment that the applicant faced a well-founded fear of persecution, which is precisely the quantity we want the survey item to predict. The left side of Figure 1d plots these data. A 0.1-Likert scale point improvement in attitudes toward homosexuals is associated with a 0.5-p.p. lower share of claims granted on this basis (SE 0.2), and the standardized coefficient implies that a 1-SD improvement in the explanatory variable corresponds to a 0.51-SD decrease in the outcome. The right side of the figure shows no such relationship for other forms of prejudice: pooling the Likert scale ratings of all other outgroups yields a slope that is zero to two decimal places and a standardized coefficient of -0.02.

Uganda is excluded from the estimation as an extreme outlier, and the magnitude involved is worth stating plainly: 540 asylum claims by Ugandans were granted on grounds that included sexual orientation, an astonishing 33% of all the claims they filed. The comparison with Nigeria shows that this is not simply a function of claim volume. Nigerians received almost exactly the same number of homophobia-based grants (551), but because far more Nigerians seek asylum in the UK overall, these account for only 4% of their claims.

Taken together, these patterns indicate that the survey item we use as our outcome is measuring something with substantial stakes. A one-standard-deviation difference in a country's mean rating of homosexuals corresponds to roughly a half-standard-deviation difference in the share of its nationals' UK asylum claims granted on grounds that include sexual orientation, with no comparable relationship for attitudes toward any other

[7] We could not find comparable data for other European countries. We look outside the region because it is unlikely that governments in the region would collect or publish data of this kind (Schäfer and Range, 2014).

outgroup. We do not interpret these cross-country associations causally: there are thirty-one countries in the estimation sample, the most extreme case is excluded from it, and many factors other than prejudice determine where people seek asylum and what they claim. What the exercise establishes is narrower and, for our purposes, more important: the difference in attitudes we identify at the border is of a kind that shows up in costly, consequential decisions made by the people it is about.

6. Conclusion

This paper studies whether colonial legacies help explain contemporary hostility toward sexual minorities in Sub-Saharan Africa. Using georeferenced Afrobarometer data in an RD design along the borders of Angola and Mozambique, we find substantially less negative attitudes toward homosexuals—but not other outgroups—just inside the former Portuguese colonies. The specificity of the result is as informative as its size: at the same borders, with the same design and the same respondents, attitudes toward other ethnicities, religions, and immigrants show no break at all, and if anything run slightly in the opposite direction. We also document discrete changes in present-day religious affiliation and historical missionary environments near these borders, which together account for the largest shares of the border gap while contemporary living standards and state presence account for essentially none, and show that our survey measure of homophobia predicts whether sexual minorities flee and are granted protection abroad. The effect is also no smaller among older, rural, and less media-connected respondents, which is difficult to square with an account based on contemporary cultural exposure rather than colonial history.

The central implication is that anti-gay prejudice in the region should not be interpreted as a culturally primordial trait. Instead, the evidence suggests that colonial rule shaped both the legal regulation of same-sex sexual activity and the ability of missionaries to transmit norms and values. What is often defended as long-standing tradition may thus reflect changes under colonial rule. Between the two channels, our evidence points more strongly toward the second. Portugal's criminal prohibition arrived in 1954, barely a decade before independence, and we find no attitudinal discontinuity along former French-British borders, where colonial legal regimes differed sharply but post-independence legislation converged. What separates the former Portuguese colonies from their neighbors, and persists, is the reach of colonial missionary education—which suggests that where laws have changed without a corresponding change in the beliefs that sustained them, attitudes are unlikely to follow automatically.

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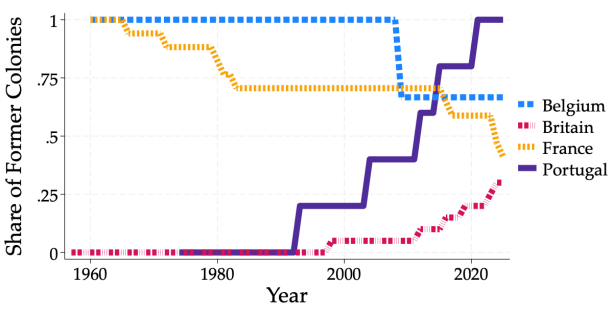
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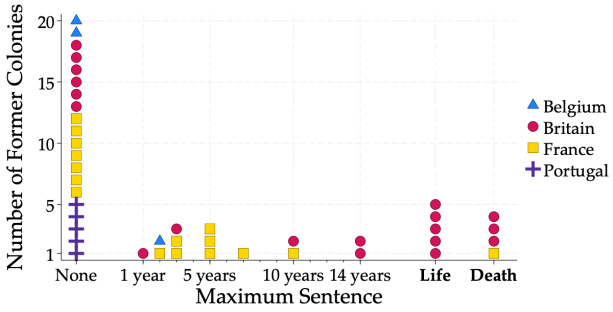
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Figures and Tables

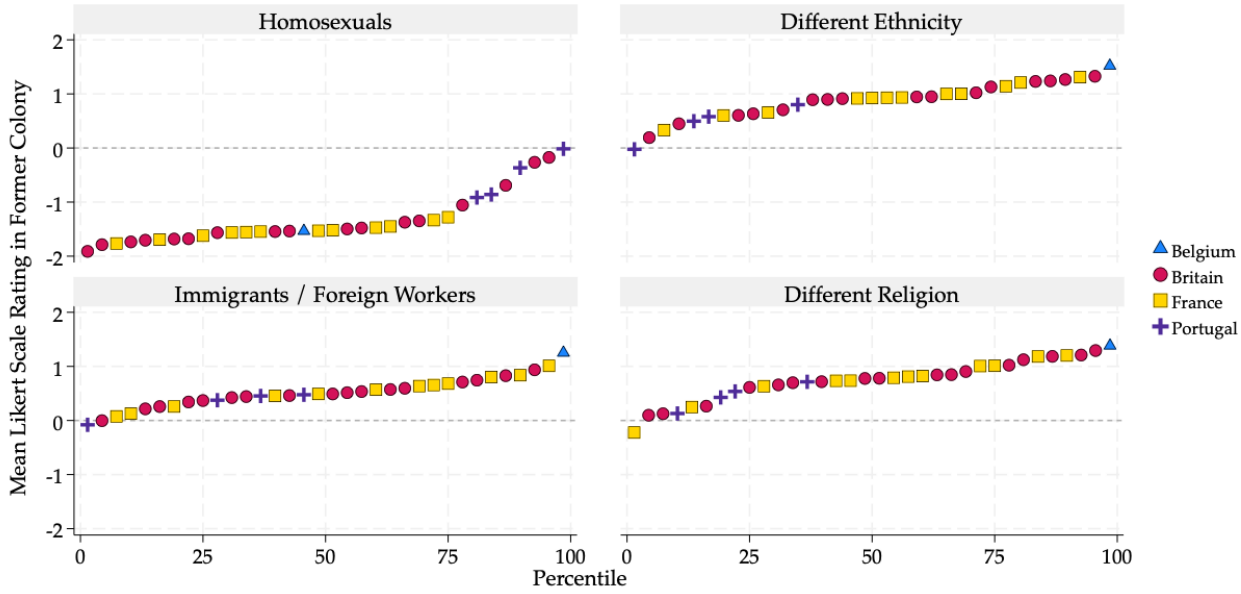
Figure 1: Laws and Attitudes in Sub-Saharan African Countries and UK Asylum Grants [6, 7, 17]



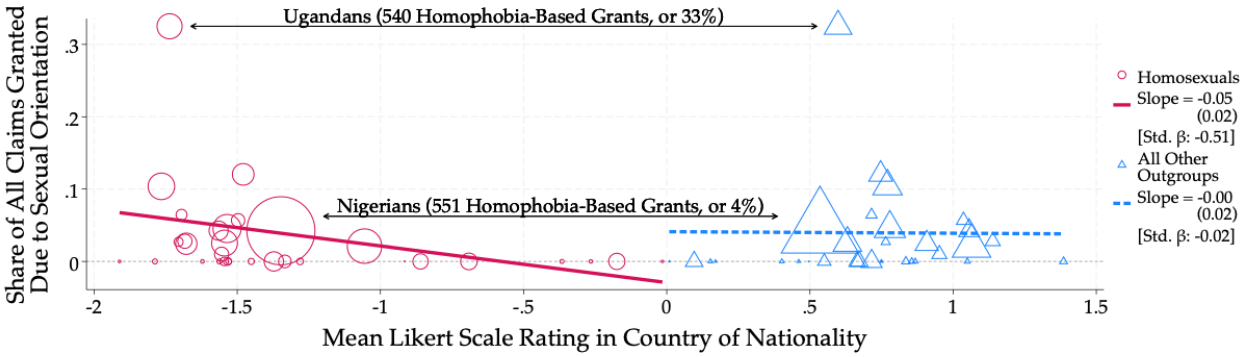
(a) Same-Sex Sexual Activity Legal, 1960-2025



(b) Sentence for Same-Sex Sexual Activity, 2025



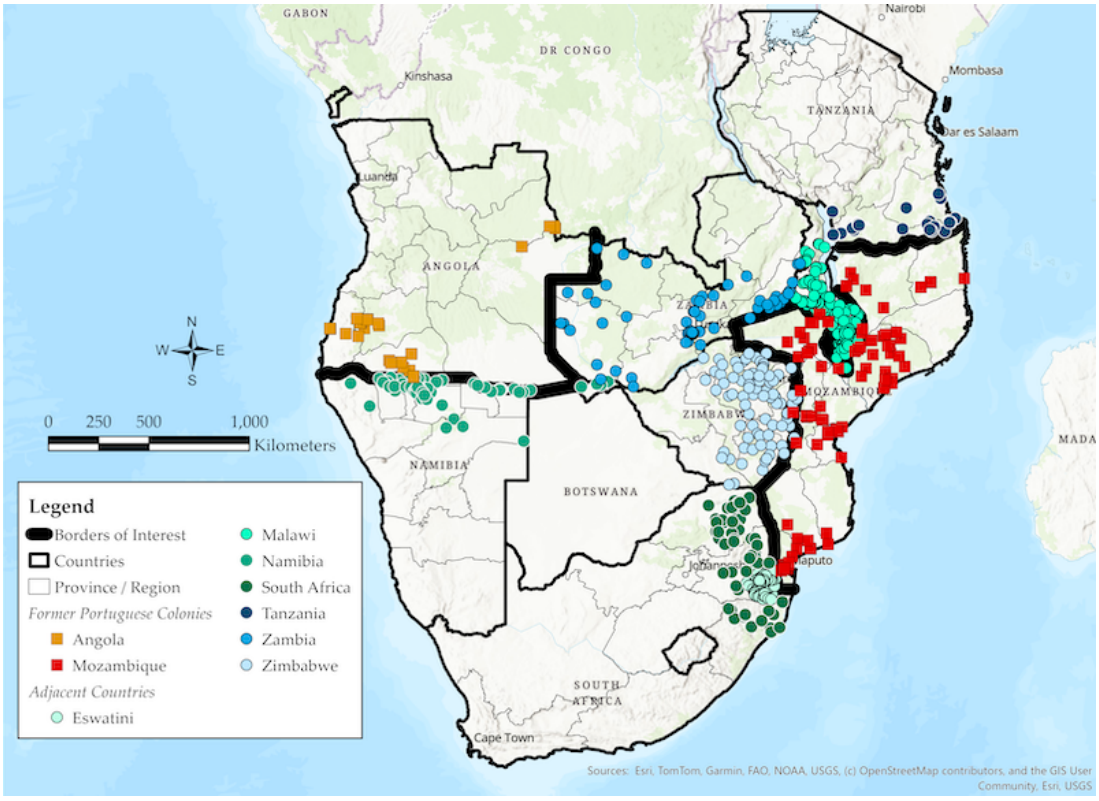
(c) Attitudes Toward Outgroups in Afrobarometer Surveys, 2014-22



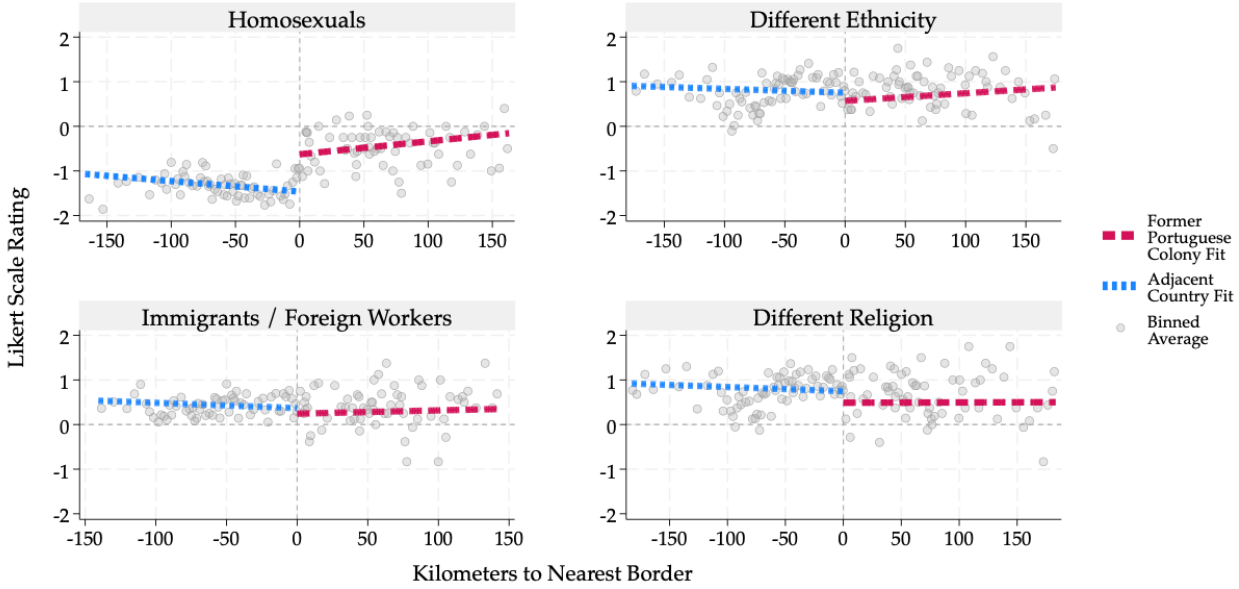
(d) Asylum Grants Based on Sexual Orientation to Nationals of Sub-Saharan African Countries

Notes: Data are from ILGA World Database, Afrobarometer rounds 6-9, and the UK Home Office. Mean Likert scale ratings are net of fixed effects for age, sex, round, and region using around 162,000 observations. In the bottom panel, estimated slopes, robust standard errors in parentheses, and standardized coefficients in brackets are from regressing the share of all UK asylum claims by nationals of 31 Sub-Saharan African countries (excluding Uganda) granted due to sexual orientation on the mean ratings in those countries of homosexuals and all other outgroups. Countries are weighted by the number of claims.

Figure 2: Map of Survey Clusters and RD Plots for Main Result and Placebo Tests [8, 9]



(a) Afrobarometer Survey Clusters Within 250 km of Former Portuguese Borders



(b) Main Result and Placebo Tests: Attitudes Toward Outgroups

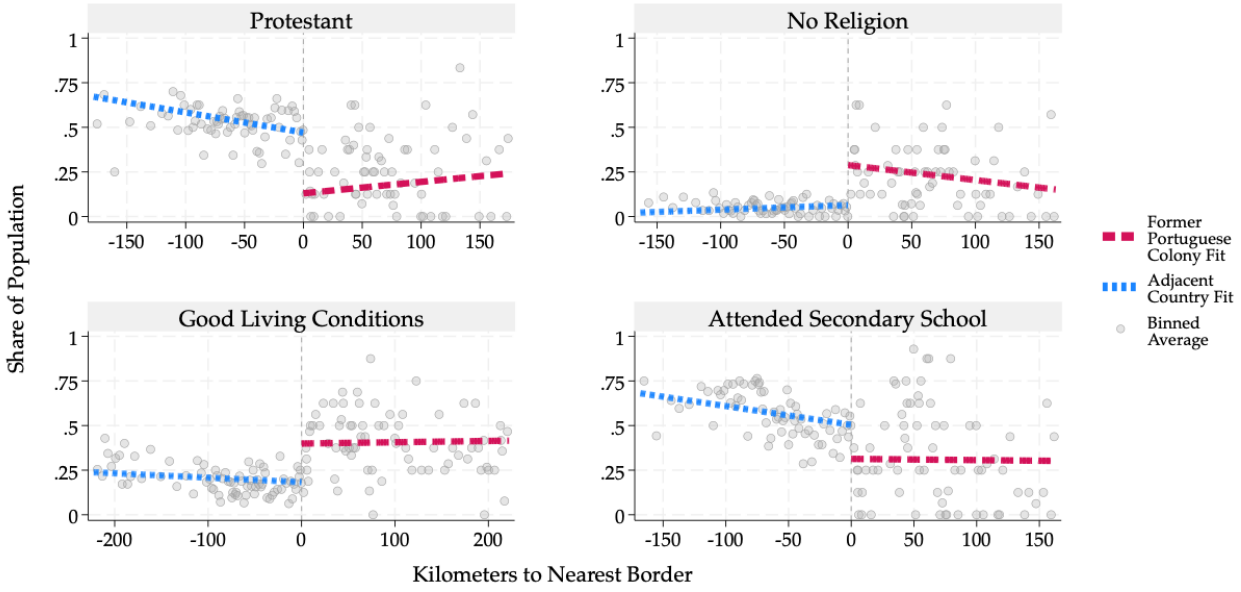
Notes: Map shows Afrobarometer round 8 survey clusters within 250 km of former Portuguese colonies' borders. RD plots show means (net of fixed effects for age, sex, and ethnic group homeland) in quantile-spaced variance-mimicking bins within MSE-optimal bandwidths, with local linear fits estimated using a triangular kernel. Corresponding RD estimates and standard errors are shown in Table 1 Panel A.

Table 1: RD Estimates for Main Result, Mechanisms, and Placebo Tests [9, 11, 12, 13, 14]

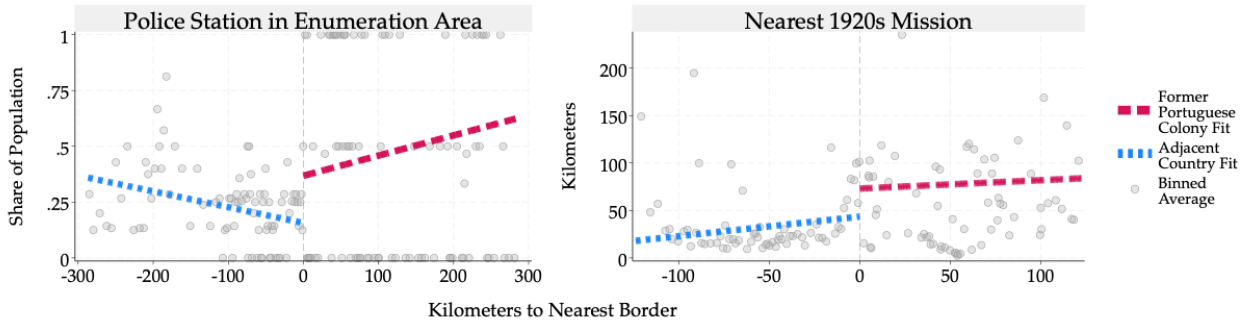
	(1)	(2)	(3)	(4)
Panel A. Main Result and Placebo Tests: Likert Scale Ratings				
	Homosexuals	Different Ethnicity	Immigrants / Foreign Workers	Different Religion
Former Portuguese Colony	0.84	-0.18	-0.12	-0.26
	(0.24)	(0.14)	(0.14)	(0.18)
Observations	4,479	4,604	4,294	4,721
Provinces	42	43	39	44
Bandwidth	167	178	142	184
Adjacent Country Mean	-1.32	0.77	0.42	0.76
Adjacent Country SD	1.09	1.17	1.23	1.21
Panel B. Mechanisms: Religion				
	Protestant	No Religion	Catholic	Christian Only
Former Portuguese Colony	-0.34	0.22	0.10	-0.10
	(0.06)	(0.04)	(0.04)	(0.02)
Observations	4,646	4,496	4,429	5,491
Provinces	43	42	41	49
Bandwidth	180	166	155	220
Adjacent Country Mean	0.52	0.05	0.14	0.20
Panel C. Mechanisms: Living Standards, State Presence, and Distance to Colonial Missions				
	Good Living Conditions	Secondary Schooling	Has Police Station	Km to 1920s Mission
Former Portuguese Colony	0.22	-0.19	0.23	29
	(0.04)	(0.04)	(0.08)	(14)
Observations	5,513	4,534	6,182	4,204
Provinces	49	43	56	39
Bandwidth	223	168	289	126
Adjacent Country Mean	0.20	0.56	0.24	37
Adjacent Country SD				79
Panel D. Placebo Tests: Likert Scale Ratings Along Former French Borders				
	Homosexuals	Different Ethnicity	Immigrants / Foreign Workers	Different Religion
Former French Colony	-0.00	-0.07	0.04	-0.10
	(0.07)	(0.07)	(0.07)	(0.09)
Observations	4,859	5,372	4,848	6,911
Provinces	71	75	71	83
Bandwidth	57	63	57	84
Adjacent Country Mean	-1.75	1.37	0.98	1.12
Adjacent Country SD	0.79	0.96	1.19	1.17

Notes: Data are from Afrobarometer round 8. Standard errors clustered by first-level administrative units (provinces) in parentheses. Bandwidths in kilometers are MSE-optimal and local linear fits are estimated using a triangular kernel. Regressions include fixed effects for age, sex, and ethnic homeland.

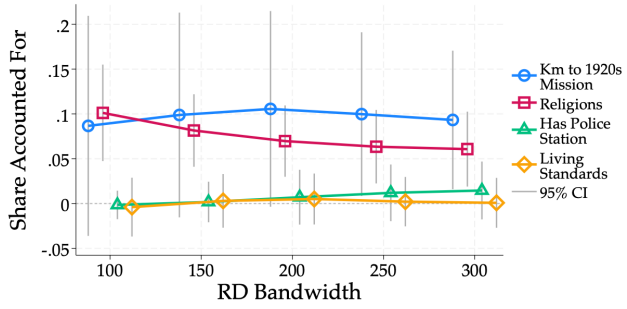
Figure 3: RD Plots for Mechanisms and Decomposition of Main Effect [11, 12, 13, 15]



(a) Religion and Living Standards



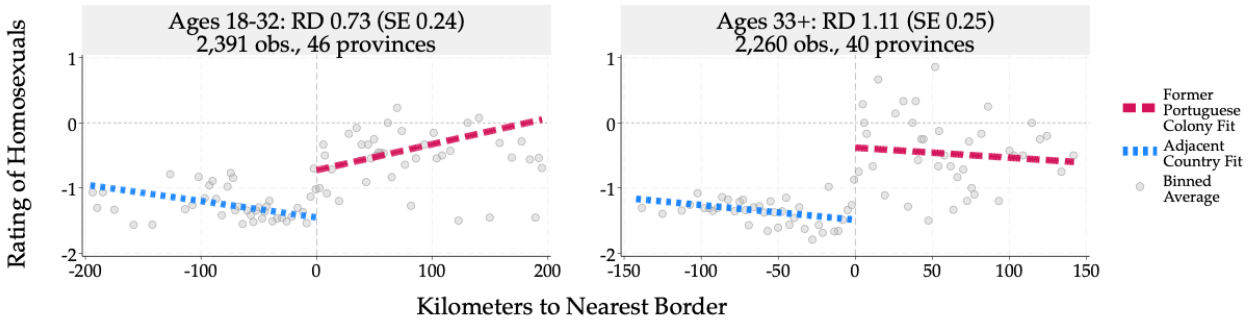
(b) State Presence and Distance to Colonial Missions



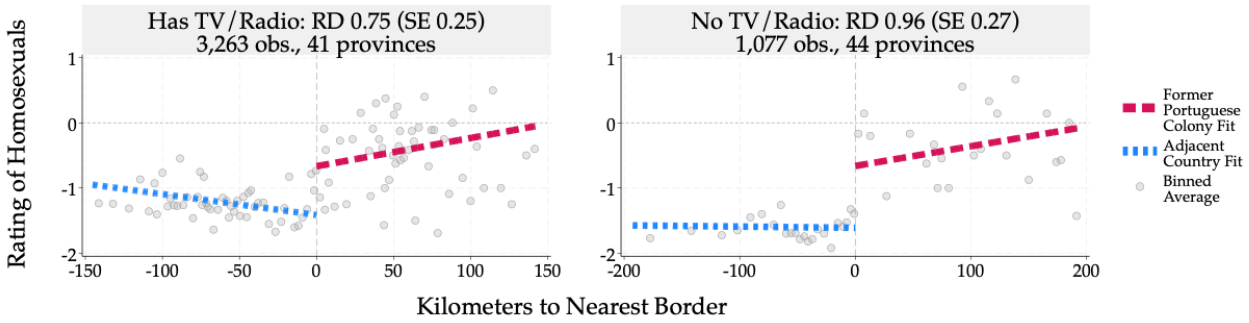
(c) Decomposition of Effect on Likert Scale Rating of Homosexuals Across RD Bandwidths

Notes: RD plots show means (net of fixed effects for age, sex, and ethnic group homeland) in quantile-spaced variance-mimicking bins within MSE-optimal bandwidths, with local linear fits estimated using a triangular kernel. Corresponding RD estimates and standard errors are shown in Table 1 Panels B and C. The bottom panel shows shares of RD estimates for Likert scale ratings of homosexuals accounted for by each covariate or group of them at a given bandwidth, with 95% confidence intervals. The groups are religions (Protestant, no religion, Catholic, and Christian only) and living standards (good living conditions and secondary schooling).

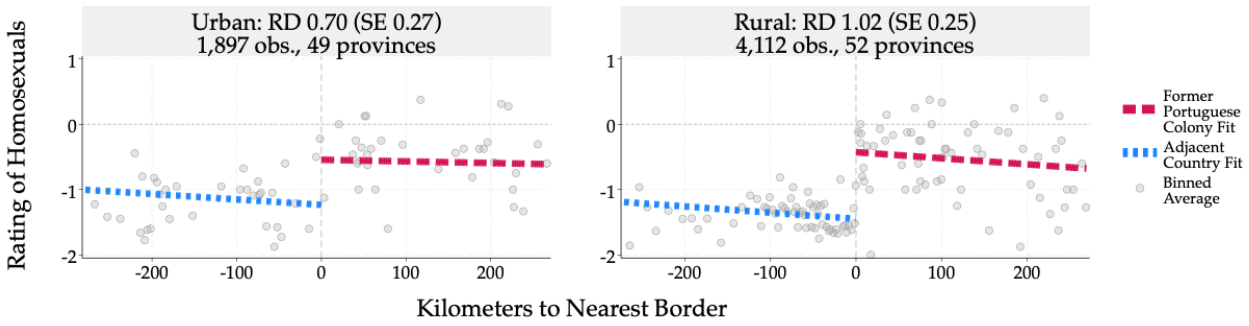
Figure 4: RD Plots and Estimates by Age Group, Media Access, and Urban Residence [16]



(a) Attitudes Toward Homosexuals by Age Group: Difference 0.38 (SE 0.16)



(b) Attitudes Toward Homosexuals by Household TV or Radio Access: Difference 0.20 (SE 0.55)



(c) Attitudes Toward Homosexuals by Urban Residence: Difference 0.33 (SE 0.43)

Notes: The top and middle panels show means (net of fixed effects for age, sex, and ethnic group homeland) in quantile-spaced variance-mimicking bins within MSE-optimal bandwidths, with local linear fits estimated using a triangular kernel. The bottom panel controls for fixed effects for the nearest international border instead of ethnic group homeland because only four partitioned homelands have urban enumeration areas on both sides of a border. Standard errors for differences in RD estimates account for covariances that arise from provinces appearing in both subsamples.

Online Appendix for:

Crimes Against Nature: The Colonial Roots of Homophobia in Sub-Saharan Africa

Jon Denton-Schneider
Clark University

Teevrat Garg
UC San Diego

September 1, 2026

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Appendix A. Additional Tables: Background

A1. Laws Regarding Homosexuality in Former Colonies in Sub-Saharan Africa

Table A1: Laws Regarding Homosexuality in Former Colonies in Sub-Saharan Africa [5]

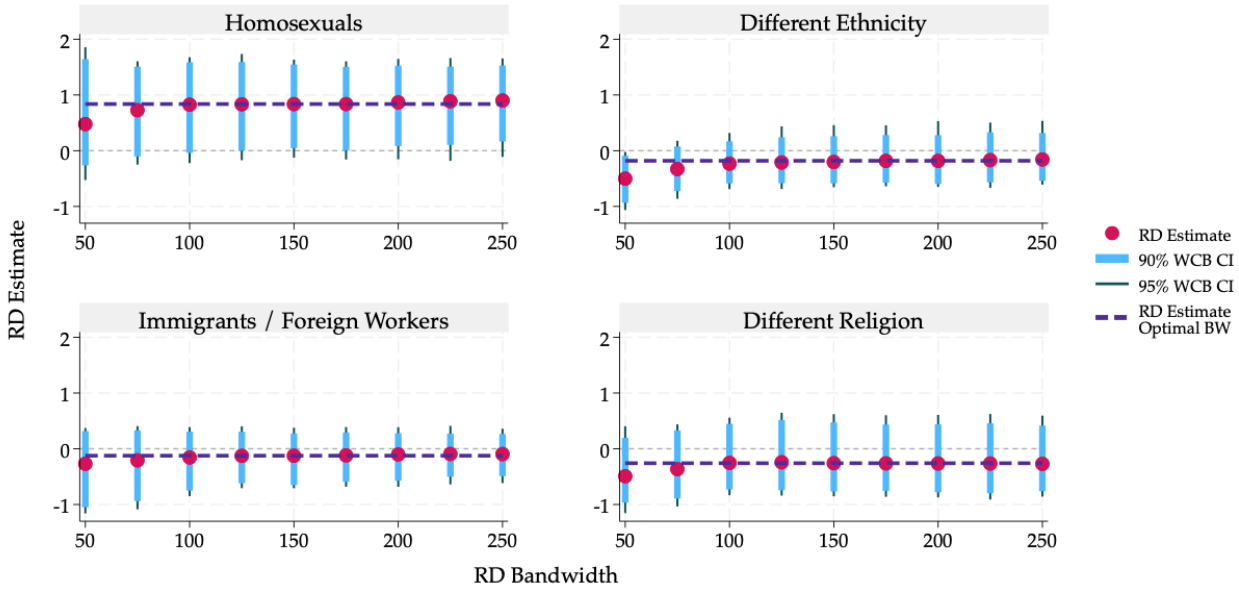
		Consensual Same-Sex Sexual Activity			Sexual Orientation
Country	Colonial Ruler	Criminalized	Decriminalized	Maximum Sentence	Employment Protections
Guinea-Bissau	*Portugal*	*Colonial period*	*1993*		
South Africa	Britain	Colonial period	1998		1996
Cape Verde	*Portugal*	*Colonial period*	*2004*		*2008*
Lesotho	Britain	Colonial period	2012		
Sao Tome and Principe	*Portugal*	*Colonial period*	*2012*		*2019*
Mozambique	*Portugal*	*Colonial period*	*2015*		*2007*
Seychelles	Britain	Colonial period	2016		2006
Botswana	Britain	Colonial period	2019		2010
Angola	*Portugal*	*Colonial period*	*2021*		*2021*
Mauritius	Britain	Colonial period	2023		2012
Namibia	Britain	Colonial period	2024		
Eswatini	Britain	Colonial period		Unclear	
Gambia	**Britain**	**Colonial period**		**Life**	
Ghana	Britain	Colonial period		3 years	
Kenya	Britain	Colonial period		14 years	
Malawi	Britain	Colonial period		14 years	
Nigeria	**Britain**	**Colonial period**		**Death**	
Sierra Leone	**Britain**	**Colonial period**		**Life**	
Somalia	**Britain**	**Colonial period**		**Death**	
South Sudan	Britain	Colonial period		10 years	
Sudan	**Britain**	**Colonial period**		**Life**	
Tanzania	**Britain**	**Colonial period**		**Life**	
Uganda	**Britain**	**Colonial period**		**Death**	
Zambia	**Britain**	**Colonial period**		**Life**	
Zimbabwe	Britain	Colonial period		1 year	
Senegal	France	1966		5 years	
Cameroon	France	1972		5 years	
Togo	France	1980		3 years	
Comoros	France	1981		5 years	
Mauritania	**France**	**1983**		**Death**	
Burundi	Belgium	2009		2 years	
Guinea	France	2016		3 years	
Chad	France	2017		2 years	
Burkina Faso	France	2024		10 years	
Mali	France	2024		7 years	
Niger	France	2025		Unclear	
Benin	France	Never, but proposed			
Congo, Dem. Rep.	Belgium	Never, but proposed			
Gabon	France	Never, but proposed			
Rwanda	Belgium	Never, but proposed			
Central African Rep.	France	Never			
Congo, Rep.	France	Never			
Cote d'Ivoire	France	Never			
Djibouti	France	Never			
Madagascar	France	Never			

Notes: Data are from the ILGA World Database. Former Portuguese colonies are in italics and countries with maximum sentences of life imprisonment or death are in bold.

Appendix B. Additional Figures and Tables: Main Results

B1. Main Linear RD Results Using Different Bandwidths

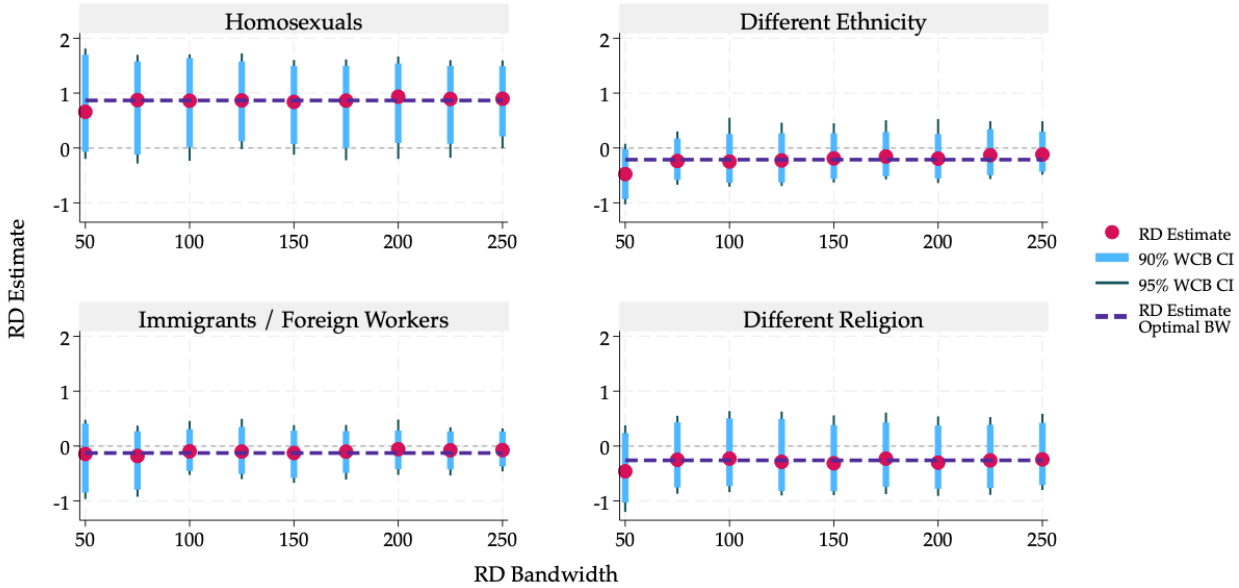
Figure B1: Main Linear RD Results Using Different Bandwidths [10]



Notes: RD plots show estimates and standard errors clustered by province using bandwidths between 50 and 250 km in 25-km increments.

B2. Main Uniform Kernel RD Results Using Different Bandwidths

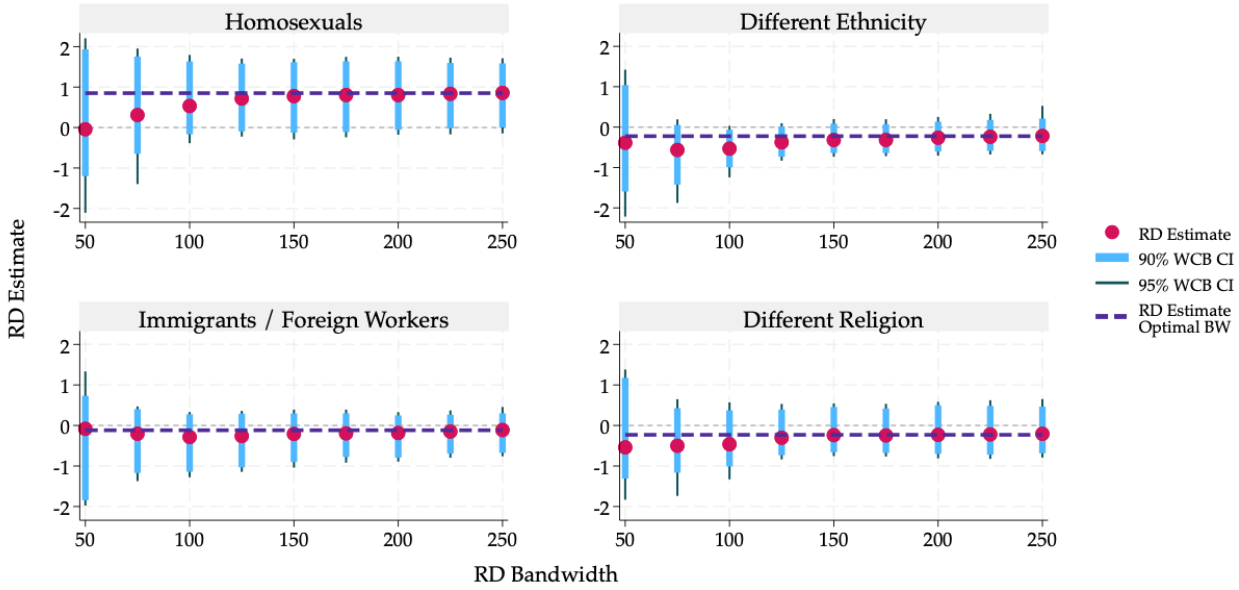
Figure B2: Main Uniform Kernel RD Results Using Different Bandwidths [10]



Notes: RD plots show estimates and standard errors clustered by province using uniform kernels and bandwidths between 50 and 250 km in 25-km increments.

B3. Main Quadratic RD Results Using Different Bandwidths

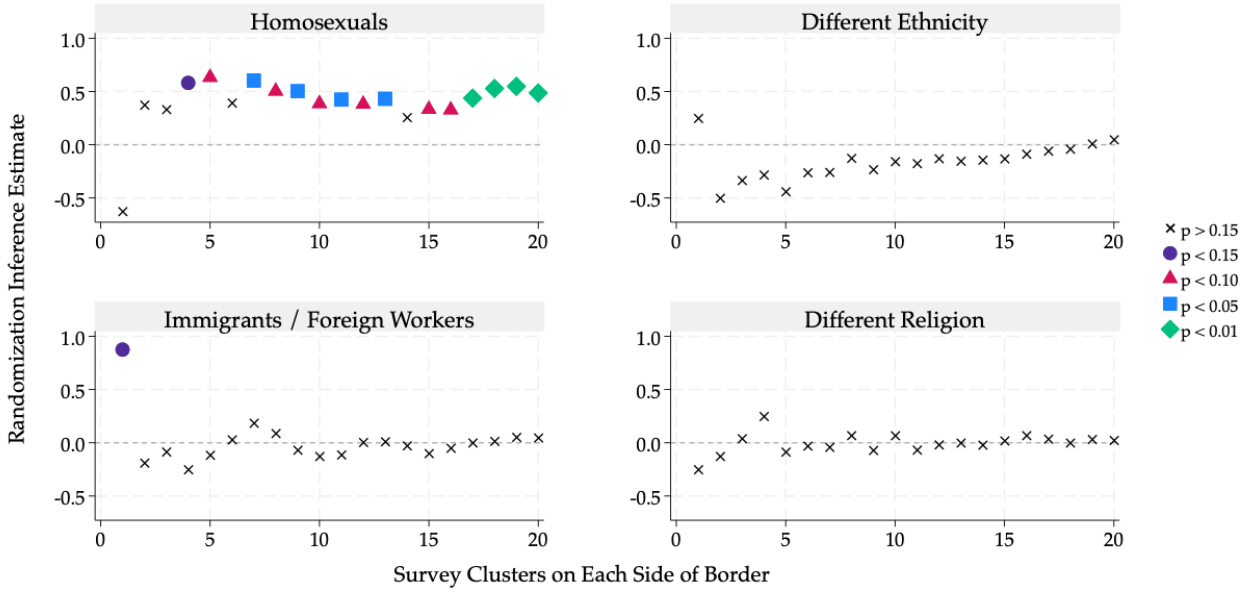
Figure B3: Main Quadratic RD Results Using Different Bandwidths [10]



Notes: RD plots show estimates and standard errors clustered by province using quadratic RD polynomials and bandwidths between 50 and 250 km in 25-km increments.

B4. Main Randomization Inference Results Using Different Windows

Figure B4: Main Randomization Inference Results Using Different Windows [10]



Notes: Plots show estimates and randomization inference *p*-values using windows from 1 to 20 survey clusters of each side of the former Portuguese borders.

B5. RD Estimates Using Strongly Dislike as Outcome

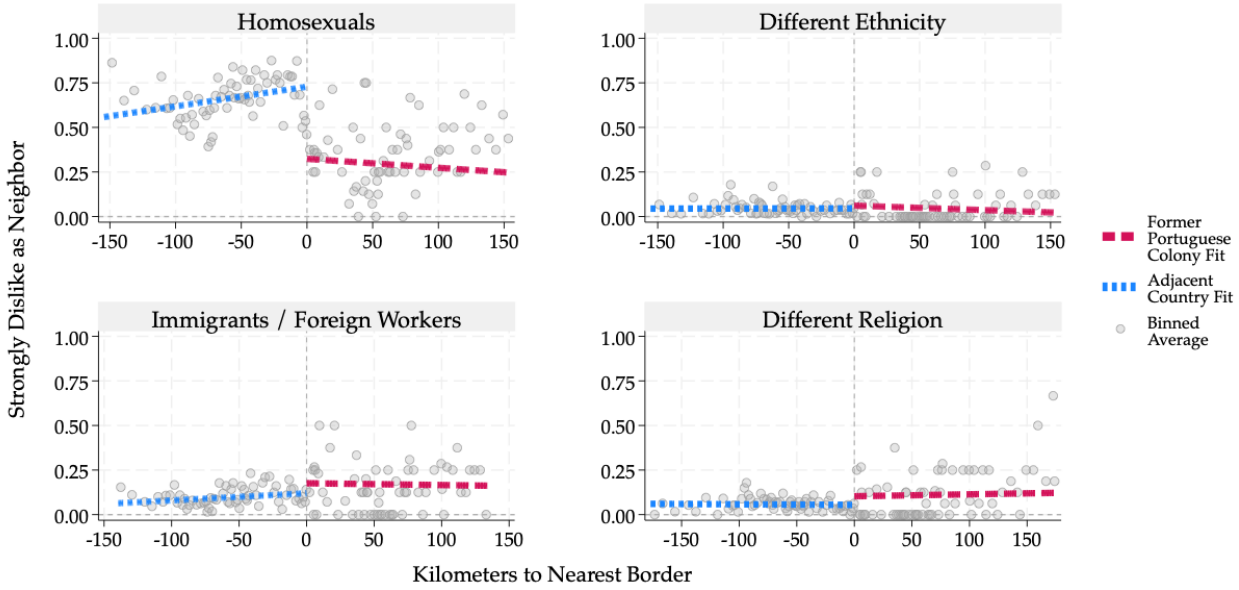
Table B1: RD Estimates Using Strongly Dislike as Outcome [10]

	(1)	(2)	(3)	(4)
Panel A. Main Result and Placebo Tests: Strongly Dislike as Neighbor				
	Homosexuals	Different Ethnicity	Immigrants / Foreign Workers	Different Religion
Former Portuguese Colony	-0.41	0.01	0.06	0.05
	(0.08)	(0.02)	(0.04)	(0.04)
Observations	4,383	4,410	4,270	4,609
Provinces / Regions	41	41	39	43
Bandwidth	155	156	140	177
Adjacent Country Mean	0.66	0.05	0.10	0.06

Notes: Data are from Afrobarometer round 8. Standard errors clustered by provinces / regions in parentheses. Bandwidths in kilometers are MSE-optimal and local linear fits are estimated using a triangular kernel. Regressions include fixed effects for age, sex, and ethnic homeland.

B6. RD Plots Using Strongly Dislike as Outcome

Figure B5: RD Plots Using Strongly Dislike as Outcome [10]



Notes: RD plots show means (net of fixed effects for age, sex, and ethnic group homeland) in quantile-spaced variance-mimicking bins within MSE-optimal bandwidths, with local linear fits estimated using a triangular kernel. Corresponding RD estimates and standard errors are shown in Appendix B5.

B7. RD Estimates Excluding South Africa and Namibia

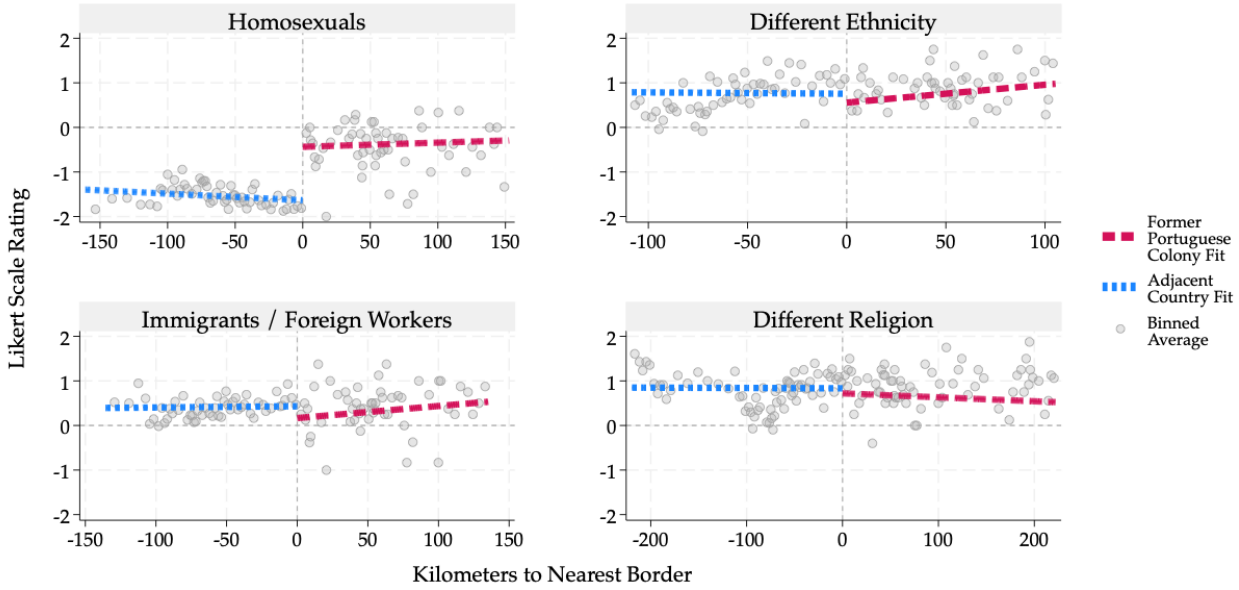
Table B3: RD Estimates Excluding South Africa and Namibia [11]

	(1)	(2)	(3)	(4)
Panel A. Main Result and Placebo Tests: Likert Scale Rating				
	Homosexuals	Different Ethnicity	Immigrants / Foreign Workers	Different Religion
Former Portuguese Colony	1.20	-0.19	-0.26	-0.11
	(0.15)	(0.13)	(0.16)	(0.16)
Observations	3,611	3,242	3,450	4,536
Provinces / Regions	28	25	26	33
Bandwidth	162	109	136	222
Adjacent Country Mean	-1.53	0.73	0.40	0.79
Adjacent Country SD	0.91	1.20	1.24	1.22

Notes: Data are from Afrobarometer round 8. Standard errors clustered by first-level administrative units in parentheses. Bandwidths in kilometers are MSE-optimal and local linear RD polynomials are estimated using a triangular weighting kernel. Regressions include fixed effects for age, sex, and ethnic homeland.

B8. RD Plots Excluding South Africa and Namibia

Figure B6: RD Plots Excluding South Africa and Namibia [11]

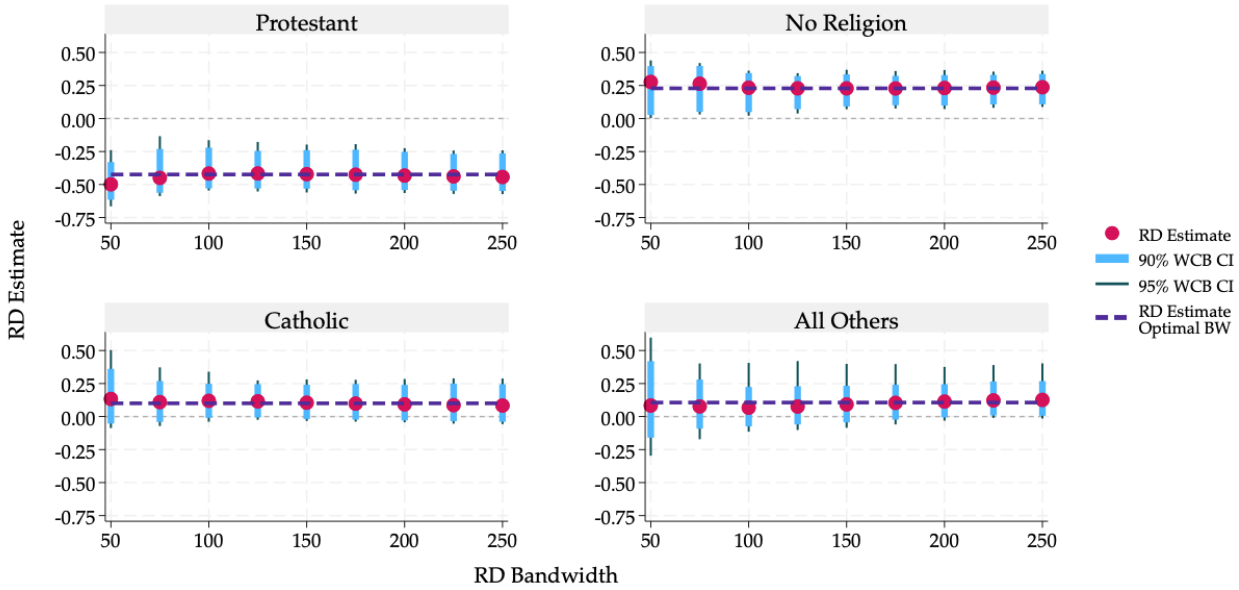


Notes: RD plots show means (net of fixed effects for age, sex, and ethnic group homeland) in quantile-spaced variance-mimicking bins within MSE-optimal bandwidths, with local linear fits estimated using a triangular kernel. Corresponding RD estimates and standard errors are shown in Appendix B7.

Appendix C. Additional Figures and Tables: Mechanisms

C1. Religion Linear RD Results Using Different Bandwidths

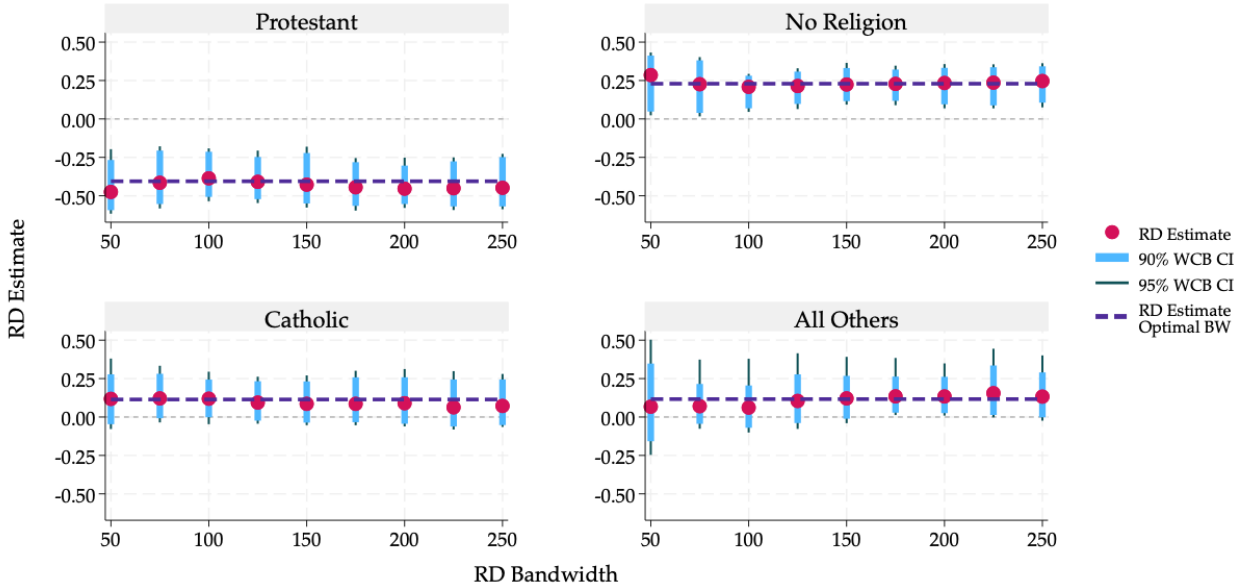
Figure C1: Religion Linear RD Results Using Different Bandwidths [11]



Notes: RD plots show estimates and standard errors clustered by province using bandwidths between 50 and 250 km in 25-km increments.

C2. Religion Uniform Kernel RD Results Using Different Bandwidths

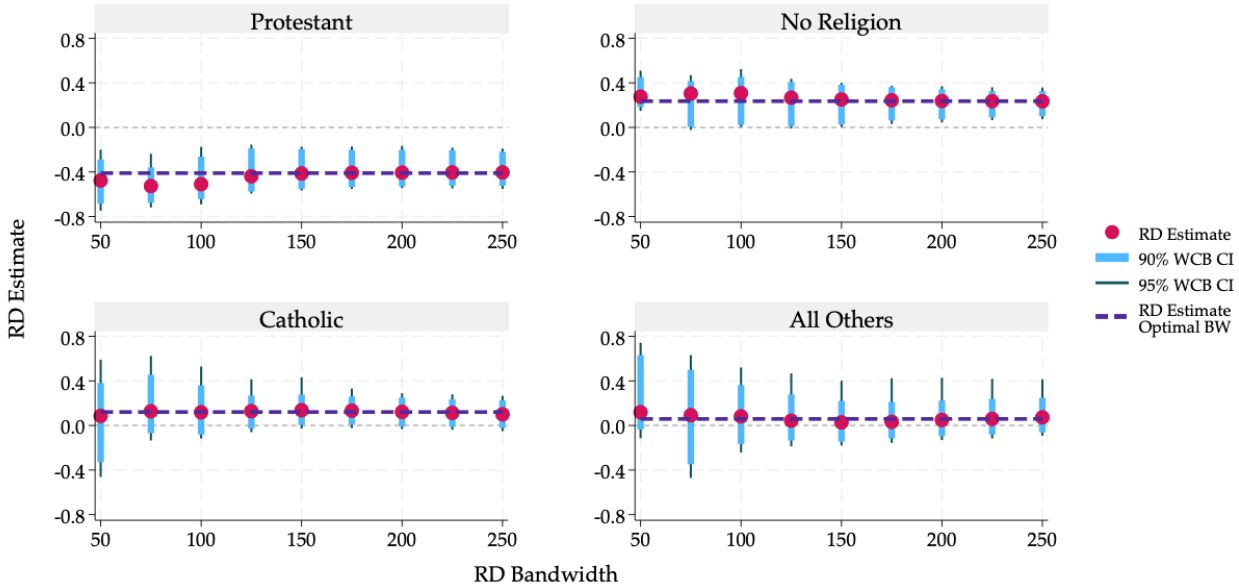
Figure C2: Religion Uniform Kernel RD Results Using Different Bandwidths [11]



Notes: RD plots show estimates and standard errors clustered by province using uniform kernels and bandwidths between 50 and 250 km in 25-km increments.

C3. Religion Quadratic RD Results Using Different Bandwidths

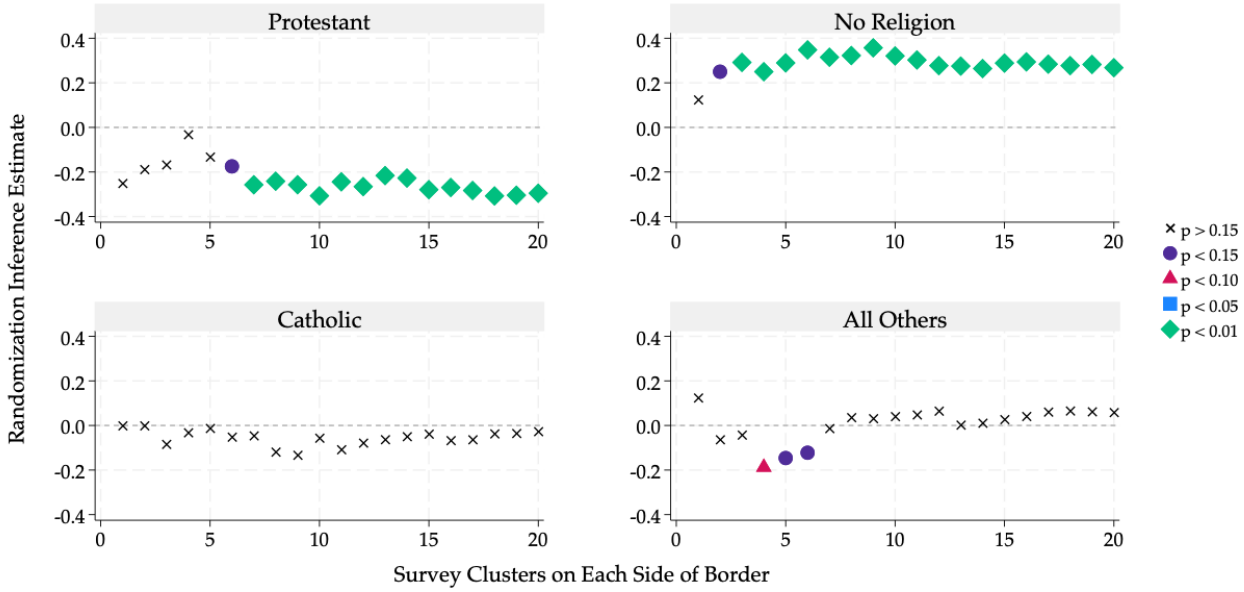
Figure C3: Religion Quadratic RD Results Using Different Bandwidths [11]



Notes: RD plots show estimates and standard errors clustered by province using quadratic RD polynomials and bandwidths between 50 and 250 km in 25-km increments.

C4. Religion Randomization Inference Results Using Different Windows

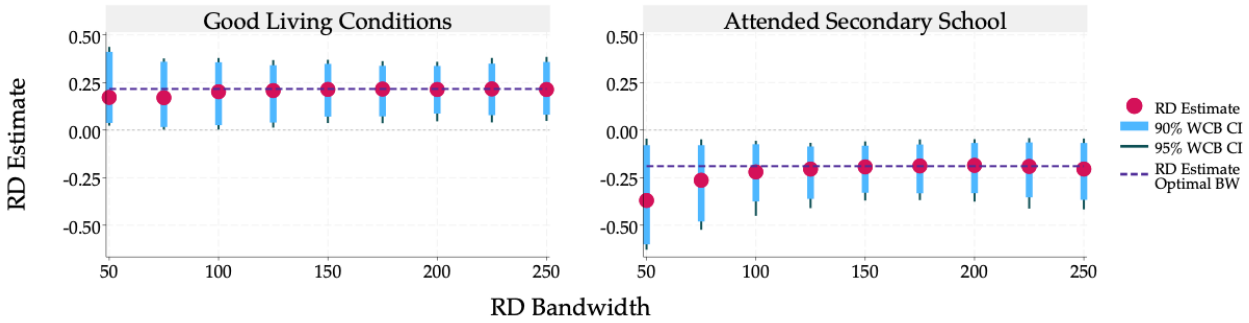
Figure C4: Religion Randomization Inference Results Using Different Windows [11]



Notes: Plots show estimates and randomization inference *p*-values using windows from 1 to 20 survey clusters of each side of the former Portuguese borders.

C5. Development Linear RD Results Using Different Bandwidths

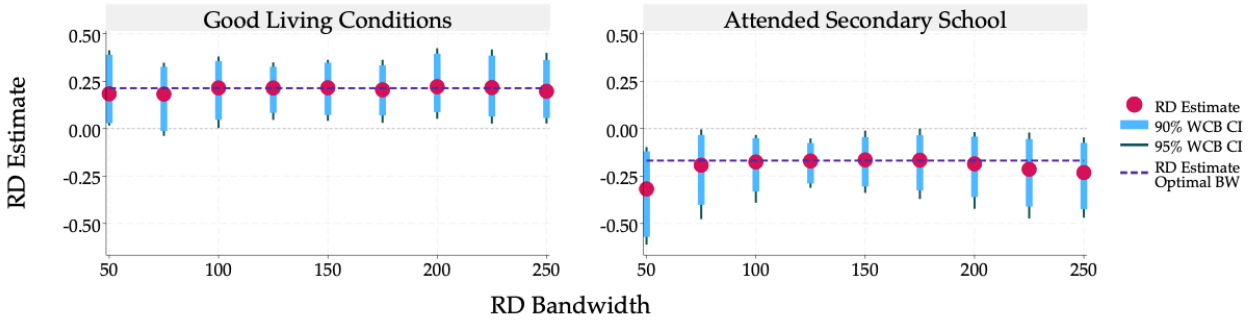
Figure C5: Development Linear RD Results Using Different Bandwidths [12]



Notes: RD plots show estimates and standard errors clustered by province using bandwidths between 50 and 250 km in 25-km increments.

C6. Development Uniform Kernel RD Results Using Different Bandwidths

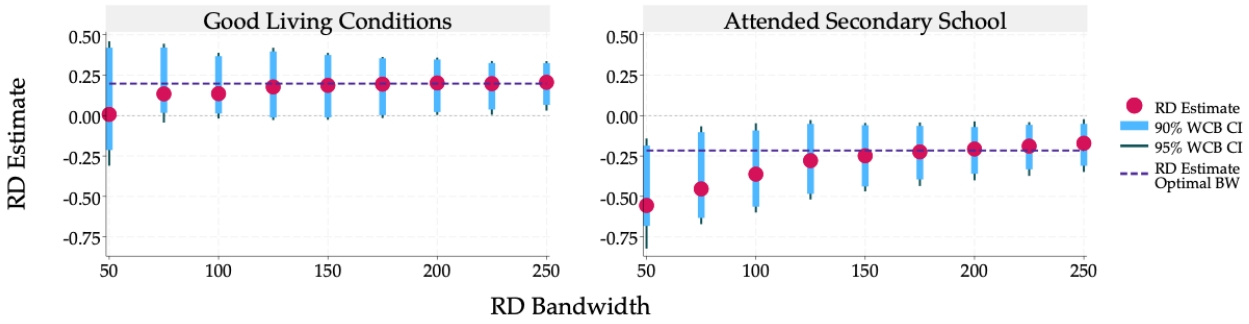
Figure C6: Development Uniform Kernel RD Results Using Different Bandwidths [12]



Notes: RD plots show estimates and standard errors clustered by province using uniform kernels and bandwidths between 50 and 250 km in 25-km increments.

C7. Development Quadratic RD Results Using Different Bandwidths

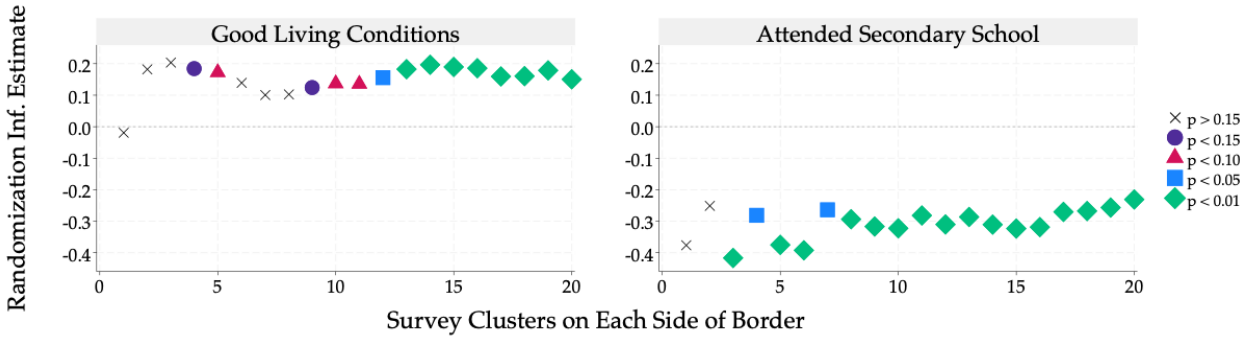
Figure C7: Development Quadratic RD Results Using Different Bandwidths [12]



Notes: RD plots show estimates and standard errors clustered by province using quadratic RD polynomials and bandwidths between 50 and 250 km in 25-km increments.

C8. Development Randomization Inference Results Using Different Windows

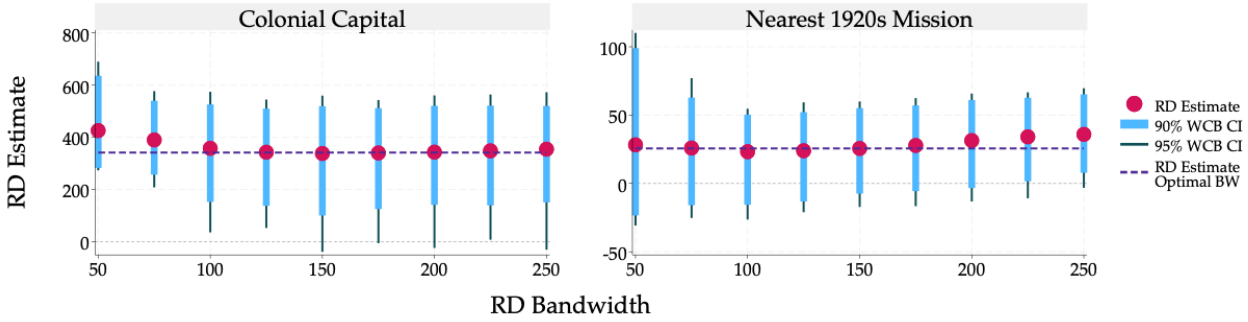
Figure C8: Development Randomization Inference Results Using Different Windows [12]



Notes: Plots show estimates and randomization inference *p*-values using windows from 1 to 20 survey clusters of each side of the former Portuguese borders.

C9. Historical Linear RD Results Using Different Bandwidths

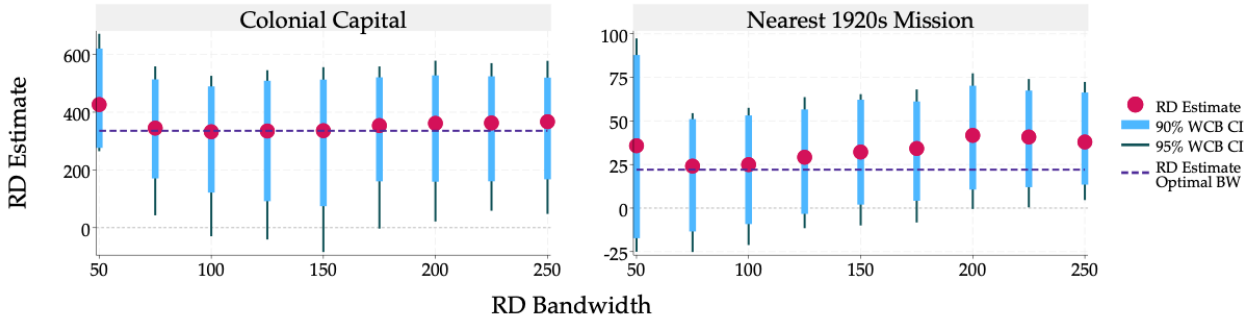
Figure C9: Historical Linear RD Results Using Different Bandwidths [13]



Notes: RD plots show estimates and standard errors clustered by province using bandwidths between 50 and 250 km in 25-km increments.

C10. Historical Uniform Kernel RD Results Using Different Bandwidths

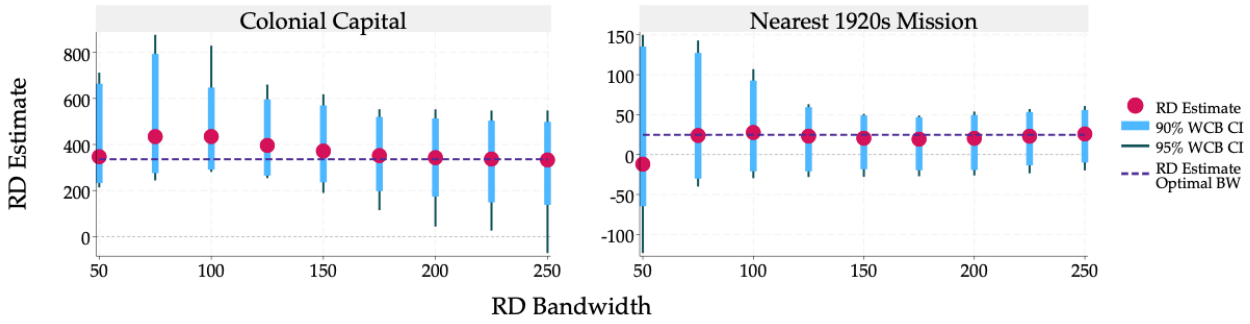
Figure C10: Historical Uniform Kernel RD Results Using Different Bandwidths [13]



Notes: RD plots show estimates and standard errors clustered by province using uniform kernels and bandwidths between 50 and 250 km in 25-km increments.

C11. Historical Quadratic RD Results Using Different Bandwidths

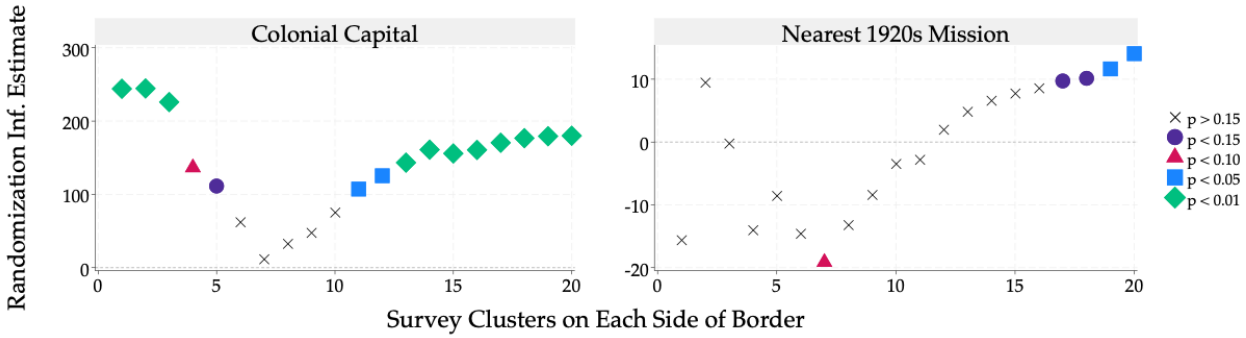
Figure C11: Historical Quadratic RD Results Using Different Bandwidths [13]



Notes: RD plots show estimates and standard errors clustered by province using quadratic RD polynomials and bandwidths between 50 and 250 km in 25-km increments.

C12. Historical Randomization Inference Results Using Different Windows

Figure C12: Historical Randomization Inference Results Using Different Windows [13]



Notes: Plots show estimates and randomization inference p-values using windows from 1 to 20 survey clusters of each side of the former Portuguese borders.

C13. Decomposition of the Border Gap in Attitudes Toward Homosexuals

This appendix gives the decomposition summarized in Section 4.5. We follow Gelbach (2016). Let B denote the set of survey clusters within the chosen RD bandwidth. The base specification is equation (1) estimated without any of the candidate channels,

$$y_{i,c} = \alpha^{\text{base}} + \tau^{\text{base}}\text{Portuguese}_c + f(\text{Distance}_c) + \mathbf{\Gamma}_i + \delta_{e(c)} + \epsilon_{i,c} \quad \text{for } c \in B, \tag{C1}$$

and the full specification adds the vector of K candidate channels $\mathbf{m}_{i,c}$,

$$y_{i,c} = \alpha^{\text{full}} + \tau^{\text{full}}\text{Portuguese}_c + \mathbf{m}'_{i,c}\boldsymbol{\beta} + f(\text{Distance}_c) + \mathbf{\Gamma}_i + \delta_{e(c)} + \epsilon_{i,c} \quad \text{for } c \in B. \tag{C2}$$

For each channel k we also estimate the auxiliary regression

$$m_{k,i,c} = \alpha_k + \gamma_k\text{Portuguese}_c + f(\text{Distance}_c) + \mathbf{\Gamma}_i + \delta_{e(c)} + \nu_{k,i,c} \quad \text{for } c \in B, \tag{C3}$$

whose coefficients γ_k are the border discontinuities in the channels themselves, reported in Table 1 Panels B and C. The omitted-variable-bias formula then decomposes the difference between the two treatment effects exactly,

$$\tau^{\text{base}} - \tau^{\text{full}} = \sum_{k=1}^{K} \gamma_k \beta_k, \tag{C4}$$

and we report each term $\gamma_k\beta_k$ as a share of τ^{base}. Two properties of this decomposition matter for how the shares should be read. First, it is not path dependent: because β

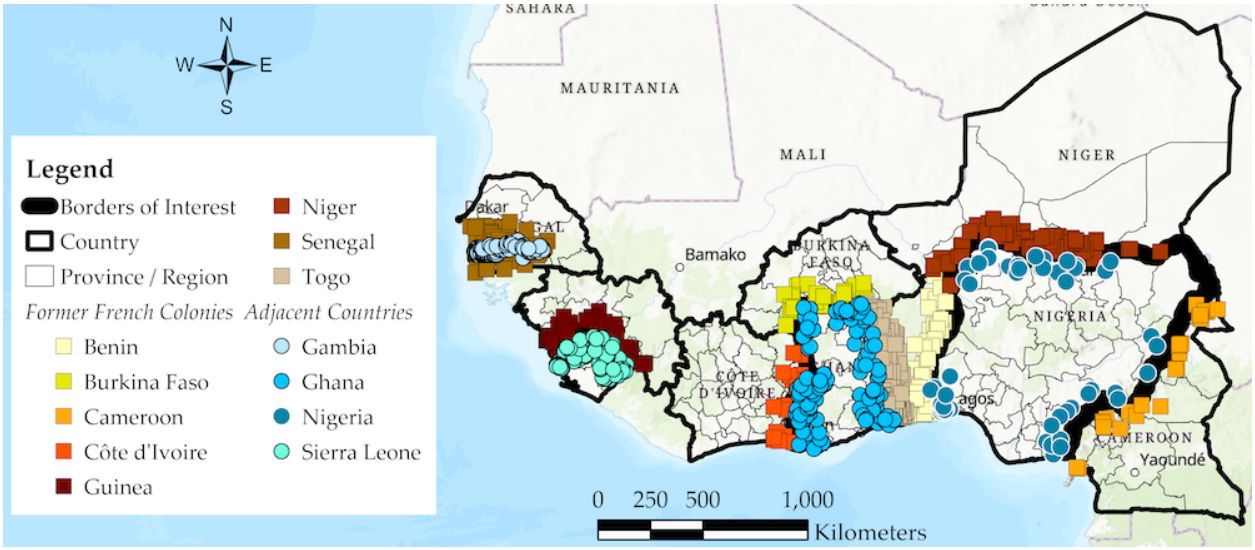
comes from the single full specification (C2) rather than from adding channels sequentially, the contribution assigned to channel k does not depend on the order in which the channels are introduced, even though they are correlated with one another. Second, equations (C1)-(C3) can be stacked and estimated jointly, so Gelbach (2016) delivers the asymptotic distribution of each $\gamma_k \beta_k$; the confidence intervals in Figure 3c come from that distribution. Where a group of related channels is reported together—the four religious affiliations, and the two living-standards measures—the reported contribution is the sum of the corresponding terms in (C4).

As discussed in the main text, we treat these shares as descriptive. Identifying them as channel-specific causal effects would require the elements of $\mathbf{m}_{i,c}$ to be as good as randomly assigned conditional on the controls, which is not defensible in this setting; what the decomposition delivers credibly is the ordering among them.

Because the MSE-optimal bandwidths for attitudes toward homosexuals and for each channel differ, Figure 3c presents the shares over a series of bandwidths. Distance to a 1920s mission accounts for close to 10% of the border gap throughout and is the largest single contributor at all but the narrowest bandwidth; religious affiliation accounts for about 11% at that narrowest bandwidth and declines to roughly 5% at the widest; and the shares for state presence and for living standards are within a percentage point or two of zero everywhere, with confidence intervals that include it.

C14. Survey Clusters along Former French Borders in West Africa

Figure C13: Survey Clusters along Former French Borders in West Africa [14]



Notes: Map shows locations of Afrobarometer round 8 survey clusters within 100 km of borders between 8 former French colonies and 4 adjacent former British colonies in West Africa.

C15. Former French Border Religion RD Estimates

Table C1: Former French Border Religion RD Estimates [14]

	(1)	(2)	(3)	(4)
Panel B. Mechanisms: Religion	Protestant	No Religion	Catholic	Muslim
Former French Colony	-0.05	0.01	0.07	-0.01
	(0.02)	(0.01)	(0.01)	(0.02)
Observations	5,067	7,610	5,217	4,931
Provinces / Regions	73	91	75	72
Bandwidth	60	99	62	57
Adjacent Country Mean	0.21	0.01	0.05	0.70

Notes: Data are from Afrobarometer round 8. Standard errors clustered by provinces / regions in parentheses. Bandwidths in kilometers are MSE-optimal and local linear RD polynomials are estimated using a triangular weighting kernel. Regressions include fixed effects for age, sex, and ethnic homeland.