Investing in Angels: The Impacts of Lottery Grants to State Clergy

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Abstract

State sponsorship of religion has theoretically ambiguous effects. Financial support can increase religious participation by funding proselytization efforts, or reduce clergy effort by stifling competition and limiting accountability. We study these forces in eighteenth-century Britain using a natural experiment. During a period that saw the rapid rise of non-state ("dissenting") sects, the state issued lottery-based grants of land to support poor state clergy through a program called Queen Anne's Bounty. Winning a grant increased clergy income by 80% initially, with this effect converging to 5% 30 years later, as lottery-losers eventually won grants themselves. Winning parishes were less likely to report the presence of dissenting households in the short term, and less likely to construct dissenting churches in the long term, yet score lower on a composite index of religious service provision. We provide evidence that this latter channel lowers overall religious participation. Our results support a hypothesis going back to Adam Smith that by limiting competition in the marketplace of ideas, state sponsorship of religion can negatively affect religious life.

Keywords: religiosity, state intervention, market structure, Anglican church
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1 Introduction

States have long used religion to shape the values and social norms of citizens. One prominent approach is support for an officially recognized state religion that is endorsed by, or closely affiliated with, the government. State religions have been widespread, with a majority of today's nations having had one at some point historically. State support can take many forms, including symbolic, legal, and even bureaucratic privileges. But in most cases, state support includes material and financial advantages for the favored religion (Barro and McCleary, 2005).

The effects of financial sponsorship on religious practice are theoretically ambiguous. On the one hand, financial support may promote religiosity by subsidizing religious practices (McCleary and Barro, 2006b; Bazzi et al., 2020). With additional resources, the state religion might attract a higher quantity and quality of religious workers, motivating them with increased compensation. Positive selection on the supply-side may then be used to establish new religious organizations (Bentzen et al. 2026), and to drive greater attendance of religious services (Engelberg et al. 2016). On the other hand, material incentives might be secondary for devoted workers with high intrinsic motivation (Deserranno 2019). State support could even stifle religiosity by creating a monopoly for a favored group, preventing the emergence of a competitive religious marketplace (Iannaccone 1991; Finke and Stark 2005). External funding could also weakening the financial incentives for religious workers to exert effort (Becker and Won, 2024). Adam Smith opined about each of these channels in *The Wealth of Nations*, writing that competition from other sects would require a religious teacher to make the "utmost exertion…to increase the number of his disciples," and that the clergy would work with more "zeal and industry" when depending on the voluntary contributions of their hearers than when funded by the state (Smith 1776, Book V, Ch. 1, Part III).

We study these forces in the place and time of Adam Smith—eighteenth-century Britain—by using a long-running natural experiment given by a scheme called Queen Anne's Bounty (QAB). Aiming to strengthen the Church of England amid the spread of competing groups, QAB provided financial support to poorly funded clerical positions throughout the 1700s. Its grants came in the form of purchases of land, or interest payments, that provided additional income for recipients. Because of its large upfront costs, the program's spread was gradual, with some areas receiving funds immediately while others waited decades. Budgetary limitations prompted the state to allocate grants using a lottery system among eligible low-income parishes. The lottery system provides exogenous variation in state support during a crucial era in Britain's religious history—an era that witnessed the "Evangelical Revival" and the birth of the Methodist movement.

To conduct our analyses, we assemble a large panel dataset documenting religious life, church finances, and economic outcomes across approximately 1,300 parishes from 1700 to 1851. Most crucially, we use archival materials to reconstruct annual clergy income and associated lists of

eligible and winning parishes for around one hundred QAB lotteries. We link these parishes to a rich set of outcomes, including data on the number of Anglican (state) and non-Anglican churches, active Anglican clergy, and parish registers of baptisms, marriages, and burials. We additionally digitize cross-sectional data from a set of "visitation returns" that measure facets of religious life in a parish as reported by priests to the bishop of a diocese. These data allow us to measure the presence of the Anglican church as measured by its clergy, the spread of non-Anglican groups through the construction of their churches, the frequency of Anglican religious services as reported by priests in the visitation returns, and participation in religious rites such as baptism.

Compared with losers, grant winners enjoyed an 80% increase in clergy income initially, with this effect converging to 5% 30 years later, as losers eventually won grants themselves. This increase in funding ultimately accomplished its goal of expanding the presence of the Anglican church, as we find a significant increase in the number of clergy employed in treated parishes in the years immediately following a lottery win. Parishes with informal, untenured ministers were given permanent positions with job security.

In keeping with the goals of its designers, the Bounty slowed the spread of non-Anglican denominations, with winning parishes 27% less likely to report the presence of dissenting households in the short term, and roughly 20% less likely to construct non-Anglican churches in subsequent decades. As such, parishes that win grants earlier have a permanently lower non-Anglican presence by this measure. While we cannot directly observe the methods through which this spread was slowed, the increased presence of Anglican clergy plausibly increased social pressure to conform and made it more difficult to practice other religions.

Despite the increased resources available to the Anglican church, we show that the provision of Anglican religious services actually decreased, confirming concerns of the low-powered incentives that follow from a lack of competitive pressures. Following a lottery win, Anglican parishes decrease the frequencies of Sunday services, major religious rites, and religious instruction (catechism). Analyzing an aggregate index of these outcomes, our preferred estimate is that religious service provision declines by 0.36 standard deviations. Markers of engagement with the Anglican church also stay neutral or decline: we observe a decrease in the frequency of baptisms, and, suggestively, a fall in Easter church attendance. The effects on religious beliefs and religiosity are strongest in areas without a permanent church edifice and therefore those most affected by competition due to a less permanent presence of the Anglican church.

Taken together, our results point away from a model where the supply of religious services is constrained by a lack of resources or insufficient pay for religious workers. Neither the additional workers nor efficiency-wage incentives for better pay increase the provision of Anglican services. Our results instead support the idea that canonical models of entry deterrence by dominant incumbents apply to religious practices. In conventional markets, incumbents can discourage

competitors' entry through investments that increase their own capacity or their rivals' costs, preserving incumbent rents at the cost of aggregate output (Dixit, 1980; Salop and Scheffman, 1983). In our setting, state support reduced the competitive pressure on the favored religion, reducing its incentives to provide high-quality services. This highlights a tension in the Bounty's original goals for providing additional support to Anglican clergy. By slowing the spread of competing beliefs, the Bounty also removed an important impetus for promoting religious views and engagement with the church.

We rule out a number of alternative explanations for our results. Most importantly, pre-lottery characteristics are balanced across winning and losing areas in both levels and trends, validating both our presumption of a fair lottery and our reconstruction of the eligible entrants. We also do not detect substantial changes in the quality of clergy, with marginal hires comparable to their contemporaries in terms of educational background and the number of positions held. Finally, we do not detect changes in outcomes unlikely to be affected by the clergy, including total fertility and rates of burial. These results go against the explanation that financial support affected the ability of parishes to accurately report outcomes, leading to bias through differential attrition or data quality.

Finally, we show that state support for the state religion led to persistent consolidation in local government. Using the cross-sectional version of our analysis, we show that the winners of state support become more administratively consolidated, subdividing into fewer sub-parish units. In 1803 data, these larger local bodies impose higher rates of taxes in support of anti-poverty "poor relief." While we cannot measure the welfare effect of this expansion, it suggests an economy of scale in local government, at least for fiscal capacity. The result also underlines the importance of studying the dynamics of state religions, given their close administrative links to the state itself.

Our paper relates to several branches of economic research. Most directly, a large literature studies the connections between the state and the market structure of religious organizations. Classic contributions helped link economic notions of supply, demand, and competition to a marketplace for religious beliefs. In turn, the "efficiency" of these markets could be hampered by monopolies or excessive government regulation (Finke and Stark, 1988; Iannaccone, 1991; Chaves and Cann, 1992). Building on this framework, subsequent work documents the connection between specific state policies and the distribution of religious beliefs. Using either country-level data (McCleary and Barro, 2006b,a; Becker and Pfaff, 2022) or within-country variation (Corbi and Miessi Sanches, 2025; Bentzen et al., 2026), these studies show that the establishment of state religions, tax policy, and cooperation with non-governmental organizations can influence religious beliefs, while related work shows that the broad-based suppression of religion by Communist regimes reduced religious participation, though less so belief, with little evidence of a rebound in participation after Communism fell (Froese 2008; Barro et al. 2025). However, direct tests of the effects of state fiscal support have been harder to conduct. Our setting provides a rare histor-

ical experiment in which the state varied its level of support to its chosen church with the aim of strengthening it against competition. This allows us to investigate whether increased support for an established church can further cement its position, and also discuss the consequences of increasing a state religion's dominance. Our results show that fiscal support matters even for dominant state religions, but providing funding for this purpose can backfire when it comes to the quality of religious services and vitality of religious practices. The randomized grants allow us to cleanly identify these effects, and while the search for causal identification often involves trading off the importance of the setting, it does not in our case: we study these questions in precisely the setting of Adam Smith's initial theorizing.

A growing body of work has also studied how religion affects growth, development, and other social or political institutions. Cross-country evidence links religious beliefs and practices to growth and related outcomes (Barro and McCleary, 2003), while historical and quasi-experimental studies show that religious movements, missions, and religious competition can shape human capital, social capital, and institutional trajectories over long horizons (Nunn, 2010; Valencia Caicedo, 2019; Cantoni, 2015; Cantoni et al., 2018; Heldring et al., 2021; Desierto et al., 2025). Two of these papers are particularly related. Cantoni et al. (2018) use the Protestant Reformation to show that a competitive shock to the Catholic Church leads to secularization. Instead, we study the effects of a state's attempt to curb religious competition. Heldring et al. (2021) show that the Crown's sale of expropriated monasteries that followed the English Reformation led to industrialization. Our paper studies one form of the reverse question, by focusing on the Crown's attempt to strengthen the state religion through purchasing land for poor clergy.

Related work emphasizes competition between state and religious non-state providers in domains such as education and welfare and how these interactions can persistently affect political preferences and local institutions (Bazzi et al., 2025). We contribute to this literature by studying random variation in the strength of a state religion and its concomitant effects on social institutions. The duration of our panel allows us to consider very long-run effects over generational time spans. Our results highlight the connection between church and state, with funding to the former leading to local government consolidation in the latter. The higher anti-poverty taxes imposed by larger units show that the connection between church and state ties back to the central tradeoff in Alesina et al. (2004), where jurisdiction size trades off economies of scale against the costs of a heterogeneous population.

Finally, our analysis relates to studies in development and public economics linking the selection and effectiveness of public employees to their salaries. Field experiments and quasi-experimental evidence show that higher pay can change who applies for public positions, improve recruitment outcomes, and alter subsequent effort, while non-monetary incentives and mission motivation can interact with financial rewards in shaping performance (Ferraz and Finan, 2011;

Dal Bó et al., 2013a; Gagliarducci and Nannicini, 2013; Ashraf et al., 2014; Finan et al., 2017; Deserranno, 2019; de Pleijt and Koschnick, 2026). We contribute to research in this area by examining whether the same selection and efficiency-wage dynamics apply to the quasi-public clergy in state religions. In our context, a large and long-lasting salary boost increases clerical hiring but does not improve observable dimensions of worker quality, and appears to decrease service provision, although the expanded clergy presence did deter the entry of competing religious groups. Contrary to the beliefs of eighteenth-century Anglican reformers, poor pay was not a major constraint in the provision of religious services. Instead, our findings are consistent with the importance of intrinsic motivation and the disciplining role of dissenting beliefs.

The rest of the paper proceeds as follows. Section 2 discusses the historical background of eighteenth-century Anglicanism. Section 3 details our data and empirical strategy. Section 4 analyzes how increased state support generated by lottery wins affected the size and presence of the Anglican church, the presence of non-Anglicans, and religious practices. Section 5 discusses mechanisms and interpretation. Section 7 addresses alternative explanations. Section 8 concludes.

2 Historical Background

2.1 Christianity in Eighteenth-Century Britain

Religion in eighteenth-century Britain was dominated by the state-affiliated Church of England (Anglican Church), yet the period was also one of growing religious pluralism and contestation. Field (2012) estimates that circa 1720, 92% of the population was at least nominally Anglican, but non-Anglican "dissenting" or "nonconformist" groups comprised 23% of the population by 1840. The non-Anglican population was itself diverse, including Methodists, Evangelicals like Baptists and Quakers, and Roman Catholics, among others. These groups were subject to persecution, including their exclusion from political office and the universities,[1] which effectively barred dissenters from prominent routes to social status. Perhaps due to such constraints, dissenters played an important role in Britain's Scientific and Industrial Revolutions (Tawney, 1926; Munro, 2010). For example, dissenters accounted for roughly one half of the scientists and inventors of the Royal Society, despite forming only a small minority of Britain's population. Beyond their importance to the economy, dissenters lay behind Welsh religious revivals that reduced crime (Lowe, 2021), and the waves of emigration that laid the foundations of religious life in the US (Fischer, 1989).

The growing spread of dissenting groups helped spur calls for reform in the Anglican church. By the end of the seventeenth century, British churchmen felt that "religion was not being efficiently…taught…the clerical order was too often held in contempt and poverty, and [the] social

[1] Though the Toleration Act of 1688 at least allowed for the freedom of worship of Protestant dissenters.

order was imperiled" (Best, 2010, p. 11). At this time, ministerial incomes were set locally, leading to widespread inequality. Based on rates often set hundreds of years prior, some ministers earned thousands of pounds per year, but others could expect less than ten (about $2,500 in 2026).

2.2 Clerical Income and Effort

> Alternately I am obliged to do Duty in the Church of Woodborough that is joined with Calverton, I presume the small allowance from the Church of Southwell has been the Reason that Service could not be performed according to Canon
> — Vicar Maurice Pugh, Calverton, 1744 *Herring Visitation Returns*.

Anglican clergy, supported by some historians, tended to emphasize the harms from their low incomes. Many clerics in smaller and poorer parishes supplemented their income with additional offices, a practice known as "pluralism," or informal support from their communities (Best, 2010; Evans, 1976). As illustrated by the above quote, the competing demands of these offices could lead clergy to divide their attention and provide a lower quantity and quality of religious services. Standard theories in labor economics also offer plausible channels through which state support could increase effort. Higher salaries might attract better candidates (Dal Bó et al., 2013b; Gagliarducci and Nannicini, 2013), and efficiency wage dynamics could provide motivation (Coviello et al., 2022).

On the other hand, contemporaries including Adam Smith were skeptical that increased state support would improve religious services. As we quoted in our introduction, Smith felt that guaranteed state support, and a lack of competitive pressures, weakened the incentives for clergy to exert effort. Other theories of the behavior of religious workers would lead to similar dynamics. For example, if religious workers are primarily motivated by intrinsic beliefs, financial support might crowd out effort (Bénabou and Tirole, 2006; Becker and Won, 2024) or select in candidates with weaker pro-social motivations (Deserranno, 2019).

2.3 Queen Anne's Bounty

It was in this context of frequent ministerial poverty that Queen Anne's Bounty was incorporated in 1704, named for Queen Anne (r. 1702-1714). The program devoted state funds[2] to raise the incomes of the poorest Anglican clergy, ultimately proving to be a century-long enterprise. While there were many motives for the Bounty's creation, a significant concern was that underpaid clergy would be unable to maintain the dominance of the Anglican church against other groups: "Queen

[2]Specifically, the funds came from "First Fruits and Tenths"—payments paid by richer clergy to the Crown. First Fruits comprise a newly-appointed cleric's first year of income, while Tenths comprise one-tenth of a cleric's subsequent annual income.

Anne's Bounty was a product of the strong Anglican resurgence during her reign, caused partly by concern at the apparent progress of dissent since the Glorious Revolution" (Cannon, 1997).

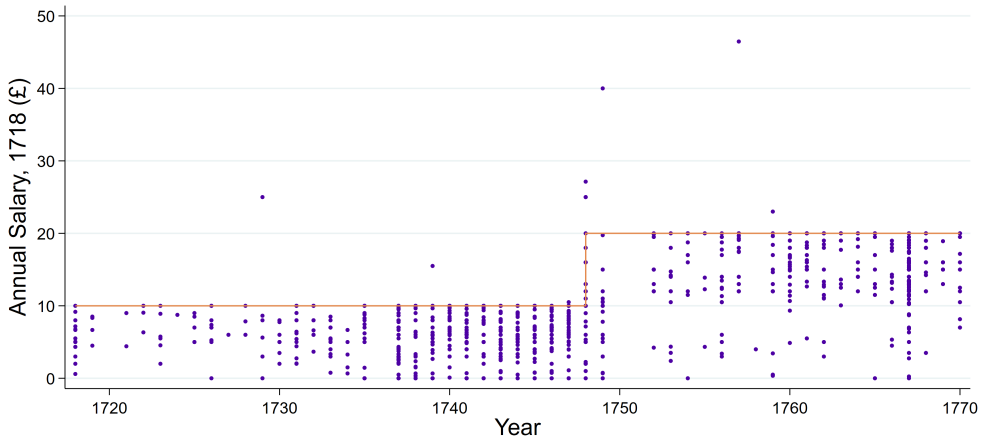
The Bounty began awarding grants in 1714, observing several general rules. First, it would raise clerical salaries through standard £200 grants invested in land that produced an annual income, though in later years, difficulties and delays in land purchases saw many lottery winners receive interest payments instead. The awards were granted to specific offices ("livings"), meaning the income generated went in perpetuity to the holders of that office. In most cases, the eligible offices were synonymous with "parishes": the smallest administrative units in the Church of England, typically home to a few hundred people. In some cases, grants were given to "chapels" that covered communities not yet granted formal parish status, but for simplicity we often use "parish" to describe eligible offices.

The Bounty aimed to gradually raise all parish incomes to a specified minimum level. To identify the low-income parishes, the Governors of the Bounty established an inquiry into parish incomes, requiring local ministers and witnesses to report incomes under oath, including income from land, tithes, fixed payments (e.g. pensions), and ecclesiastical dues, but excluding voluntary contributions (Ecton, 1718). Parishes with incomes below an initial cutoff were eligible for lottery-based state grants. Those below a second, higher cutoff could also receive state support, but only through matches of donations from private benefactors; all other offices were ineligible. In our analysis, we focus exclusively on the lower income threshold where grants were determined by the state.

Initially, the Bounty targeted parishes with incomes below £10, making the £200 asset grant a substantial boost to clergy earning potential. This process was completed in 1748, and the Bounty turned its focus to incomes below £20. In 1792, the target increased to £30. We depict these transitions along with our reconstructions of valuations and QAB grants in Figure 1. Upon winning a lottery, the Governors of the Bounty would again assess the income of the living to verify that its income remains below the required threshold. In practice, this re-assessment disqualified relatively few livings—roughly 85% of grant winners actually received the grant.

Because the Bounty's funding was not sufficient to augment all eligible parishes at once, grants among eligible parishes were distributed by lottery each year. While some clergy received an immediate income increase, others waited decades. Conditional on eligibility, these annual randomized lotteries mimic the procedures of a randomized controlled trial (RCT), enabling us to directly compare lottery winners with losers.

Figure 1: Parish Year of First Win



Notes: this depicts a scatterplot of parishes' initial income (y-axis) and first QAB lottery win (x-axis) according to our data. Horizontal lines depict the evolving QAB income targets through our eligibility reconstruction in 1770.

3 Data and Empirical Approach

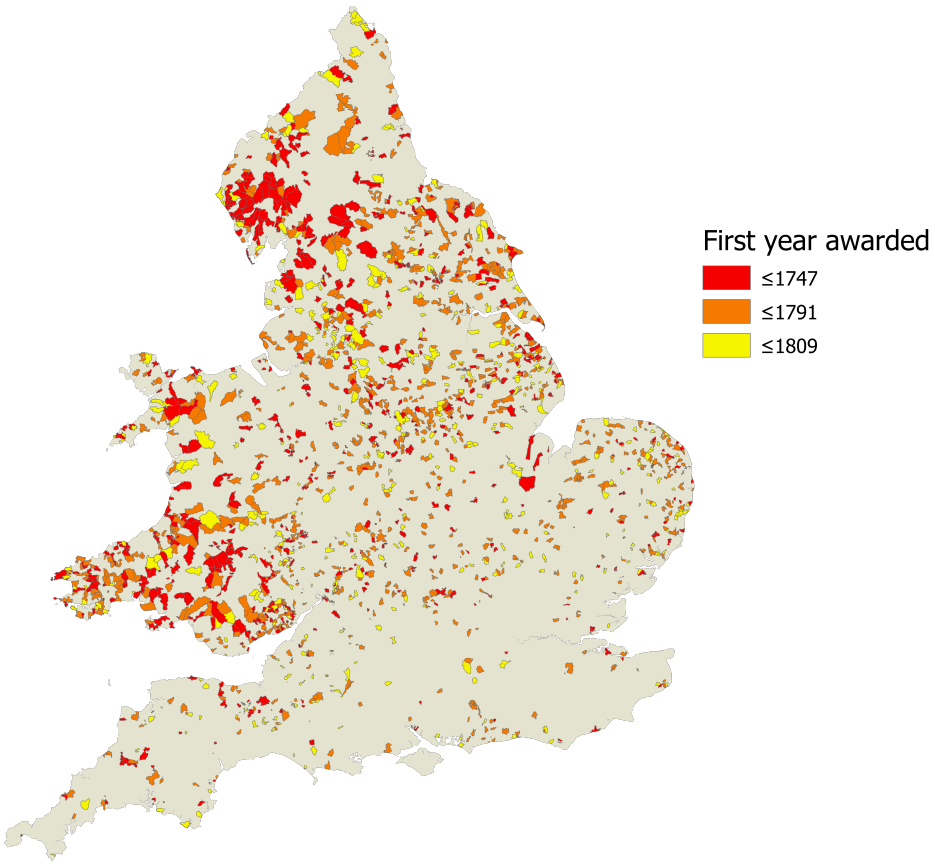
3.1 Data

We assemble annual lottery- and parish-level data on QAB results, religious life and practices, and economic characteristics from a combination of eighteenth-century ecclesiastical finance records, administrative archives, and multiple independent measures of religious activity and competition. We begin by reconstructing the eligible lottery livings based on valuations of parish salaries. We use other archival QAB records to reconstruct increases to these values updated over time, the eligibility rules for each lottery, and the winning set of parishes.

We link eligible parishes to a rich set of outcomes covering the Anglican church, the presence of non-Anglican (dissenting) religions, and economic data. These include panel data on the number of active Anglican clergy from the Clergy of the Church of England Database (CCEd), Anglican and non-Anglican churches, and "parish register" data on baptisms, marriages, and burials. We additionally digitize cross-sectional data from a set of "visitation returns" that measure facets of religious life in a parish as reported by priests to the bishop of a diocese. While offering important variables unavailable in other sources, the decentralized nature of this source means we cannot collect panel data and are instead limited to exploring these measures in particular dioceses in specific years. Although the questions in the visitation returns were largely standard, responses are comprised of short essays. We standardize these into quantitative values using large language models (LLMs). For complete documentation and description of each source, see Appendix A.

Our final dataset consists of parish-level data from 1700 to 1851 and QAB lotteries from 1718 to 1770.[3] Figure 1 maps the set of parishes by the year when they secured a first-time lottery win. The parishes in our sample have a wide geographic spread across England and Wales, and due to the QAB eligibility rules, they were poorer and typically rural. The gradual distribution of the grants also meant that there was large variation in when parishes received funding.

Figure 2: First-time Lottery Winners, 1714-1809



3.2 Single Lottery Empirics Example

To build intuition, we begin with a hypothetical case of a single lottery. In this situation, we would estimate an event study specification according to

$$y_{p,t} = \sum_{k \neq -1} \alpha_k [\text{Won}]_p \times (t - x = k) + \beta_p + \gamma_t + \varepsilon_{p,t} \tag{1}$$

where p is a parish, t is a year, and x is the year in which the single lottery took place, making $t - x$ the number of years relative to the lottery. Here, the α_k are the main coefficients of interest,

[3] Our data on initial valuations begins in 1718, making this a natural starting point. This excludes a small number of lotteries from 1714-1717 from our sample.

representing the intent-to-treat effects of winning the lottery (recall that 85% of winners actually received the grant) k years after it takes place, and a test of parallel pre-trends where $k < 0$. This specification includes both parish and year fixed effects, with the latter necessary to remove time trends.[4] Note that unlike a standard difference-in-difference design, the lottery randomization means that parish fixed effects are not necessary for unbiasedness, though they are still useful for power.

3.3 Main Specification: Stacked Panel

Our full dataset involves approximately one hundred lotteries, with losers in a given lottery typically showing up and eventually winning a subsequent one. We thus face two decisions on how to structure our primary panel results. First, how should parishes that appear in multiple lotteries be represented in the data? Second, how should the single-lottery equation (1) be extended to accommodate multiple lotteries?

For the first question, we adopt a stacked structure where each observation represents a lottery l, a lottery-eligible parish p, and a year t. We do so primarily because it allows us to straightforwardly extend the event study logic of equation (1), essentially pooling estimates from each lottery into a single sample. However, note that most parish-year data appear multiple times in the data, once for each relevant lottery. This is not a source of bias, though it does mean that each parish does not receive equal weight in our regressions and this increases our nominal sample size.

Our primary regression equation is given by (2) below, and it is similar to equation (1) except all controls are interacted with lottery fixed effects

$$y_{p,l,t} = \sum_{k \neq -1} \alpha_k [\text{Won}]_{p,l} \times (t - t(l) = k) + \beta_{p,l} + \gamma_{l,t} + \varepsilon_{p,l,t} \tag{2}$$

where l is a lottery, $t(l)$ is the year of lottery l, $\beta_{p,l}$ represent parish-lottery fixed effects and $\gamma_{l,t}$ are lottery-year fixed effects. By default, each time period is one year, but for some specifications we group years for visual clarity or for statistical power. Including controls for each lottery is necessary to restrict to within-lottery comparisons over time. Note that cross-lottery comparisons are likely biased since each lottery includes different groups of eligible parishes with different lottery-specific probabilities of winning.[5]

[4] Even with random variation in $[\text{Won}]_p$, the event study terms will still correlate with general time trends in the absence of year fixed effects.

[5] Lottery characteristics and time trends are likely correlated with lottery win probability. Later lotteries often had higher win probabilities since previous winners became ineligible and the pool of parishes was smaller, inducing a correlation between win chance and lottery year that our fixed effects remove.

3.3.1 Clustering Standard Errors

Based on the lottery randomization structure, we cluster our standard errors at the lottery level by default. In contrast to the typical stacked panel, our lottery-based design means treatment is set at random at the parish-lottery level (conditional on eligibility). With a fixed win probability within a lottery, draws across lotteries are independent even for the same parish. However, since each lottery allocated a fixed number of grants, parishes' wins have a small negative correlation within a given lottery, leading to our preference for lottery clusters. However, we explore adding parish-level clusters as a robustness check to match the standard in non-randomized panels and find largely similar results.

3.4 Secondary Cross-Section Specification

Some of our data are only available as a cross-section of a single or small set of years, necessitating an alternative specification. In these situations, we cannot include parish fixed effects as we typically observe a parish only once. Fortunately, in our setting parish fixed effects are not necessary for identification.

For these cases, we analyze lotteries that occurred within a fixed number of years of the outcome data, by default from zero to ten years beforehand. Then, we compare winners and losers within lotteries using regressions of the form

$$y_{p,l,t} = \alpha[\text{Won}]_{p,l} + \beta_l + X_{p,l}\delta + \varepsilon_{p,l,t} \tag{3}$$

where β_l are lottery fixed effects and $X_{p,l}$ are (ex-ante) parish or lottery controls. As with equation (2), we cluster standard errors by lottery.

3.5 Balance

We provide evidence to support the assumption that the QAB lotteries operated fairly, with winners and losers balanced on observable characteristics. Table 1 compares winners and losers within lotteries using equation (3) on a single pre-lottery year. Given the goal of examining balance, we only include lottery fixed effects as controls.

Across a range of variables, lottery winners and losers are statistically similar before their statuses are determined. The point estimates on an entrant's salary, status as a formal parish, number of observed Anglican clerics, number of Anglican churches, and number of non-Anglican churches are all small and statistically insignificant. These results support both our assumption that the lotteries were conducted fairly and that we have properly reconstructed the set of eligible entrants.

Table 1: Balance on pre-Lottery Measures

	(1) Salary (1718)	(2) Salary	(3) Formal Parish	(4) Any Cleric	(5) Any Curate	(6) # Churches (Anglican)	(7) # Churches (Non-Anglican)
Win	0.26	0.024	0.017	-0.0075	0.010	-0.0018	-0.00049
	(0.18)	(0.100)	(0.019)	(0.012)	(0.0094)	(0.013)	(0.0083)
N	36,140	36,140	29,574	29,574	29,574	36,140	36,140
SEs / Clusters	Lottery	Lottery	Lottery	Lottery	Lottery	Lottery	Lottery
N (clusters)	95	95	95	95	95	95	95
Lottery FEs	Y	Y	Y	Y	Y	Y	Y
$\mathbb{E}[y\|\text{Win}=0]$	8.6	11	.75	.21	.084	.29	.048

Notes: within-lottery comparisons of winners and losers in a single pre-lottery year using estimates of equation (3). Column (1) is measured in 1718, which we use as our initial pre-lottery salary estimate. Columns (2)-(7) are measured one year prior to the lottery. Columns (1)-(7) respectively measure the entrant's salary for both (1) and (2), whether the entrant had formally attained parish status, whether we observe any active clergy in the CCEd, whether we observe any CCEd curate, the number of Anglican churches, and the number of non-Anglican churches.

4 Main Results

Here we document the effects of additional financial support for state religion on a range of religious and economic outcomes.

4.1 Zeroth Stage

We begin by documenting that a QAB lottery victory substantially increased Anglican clergy salaries (livings) for roughly a generation. Several forces influence our estimated income effects. First, roughly 85% of lottery winners actually receive the grant, with the others disqualified upon being re-assessed as having ineligibly high income. For those that receive the grant, the Bounty-recorded living value increases by the expected return on the land (3 or 3.5%, depending on the time period).[6] Second, parishes may receive matched benefactions, which increase income by twice that of a lottery win. In principle these benefactions could mute our first stage, if they are more common among lottery losers. Third, lottery losers eventually win lottery grants themselves—a channel that drives our dynamic effects of lottery wins towards zero over time.

Figure 3 displays our event study results of equation (2) that estimate the effect of a lottery victory on the value of clergy salary. In the first post-lottery year, salaries rise by 79%, an amount that shrinks to 8.7% after twenty years. Most of the effects dissipate after about thirty years,

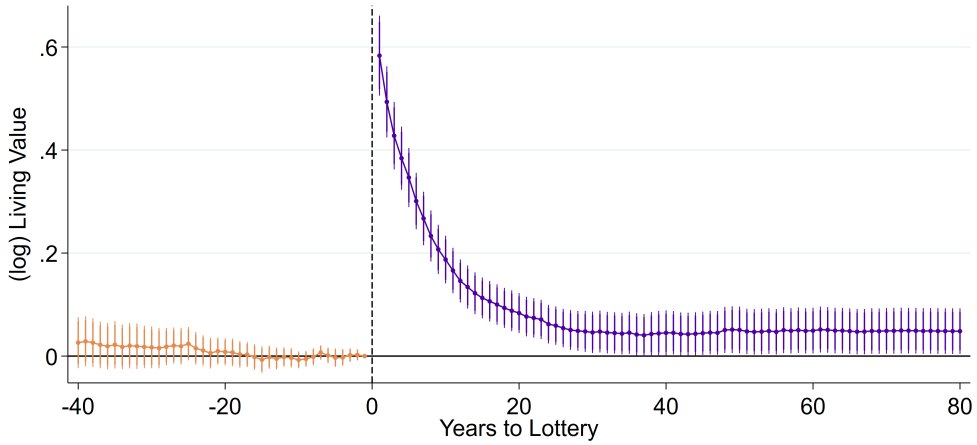
[6]The actual income gain for a parish would sometimes be smaller and sometimes larger than the imputed return on land. For example, Austin (1972) writes that a 10% return on land was not uncommon in Derbyshire. In cases like this, we would underestimate the actual income effects of a lottery win.

reflecting the fact that lottery losers in one year typically win in a subsequent lottery, though there is some evidence of a small permanent effect.

The permanent effect can be rationalized by an institutional detail: when a lottery-eligible parish receives a matched benefaction, they become ineligible for subsequent lotteries until the income threshold for the lottery increases. The reverse is not true: a lottery-winning parish remains eligible for a benefaction. As a result, if lottery wins do not reduce the chances of receiving a future benefaction, lottery winners will have persistently higher income than lottery losers, since not all lottery losers will eventually win the same income-threshold lottery.[7]

Overall, the effects in Figure 3 shows that our effective treatment is substantial, amounting to decades of higher clergy income.

Figure 3: Lottery Winners Enjoy Substantially Higher Incomes for Several Decades



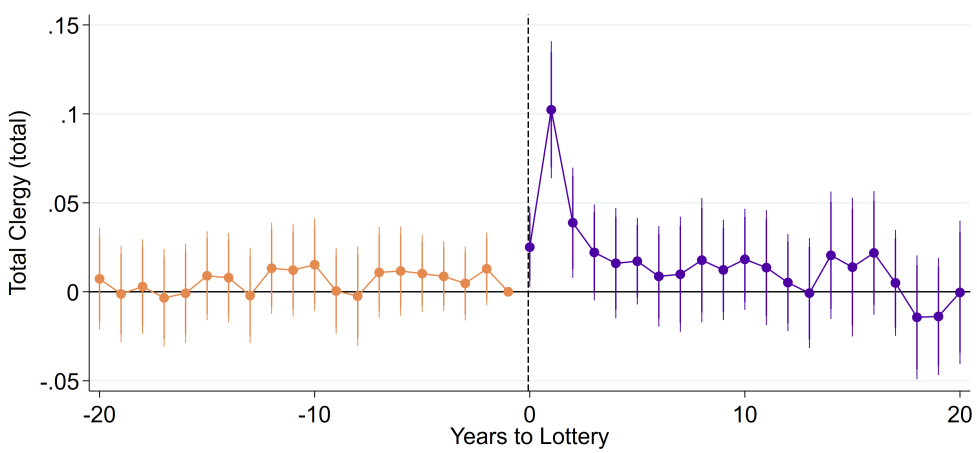
Notes: the effects of a QAB lottery victory on the logarithm of a parish's Anglican clergy salary ("living"). Each point represents a coefficient estimate in the event study equation (2). The x-axis marks the years relative to the lottery, with the last pre-lottery period forming the omitted category. 90% and 95% confidence intervals are shown.

4.2 Anglican Presence

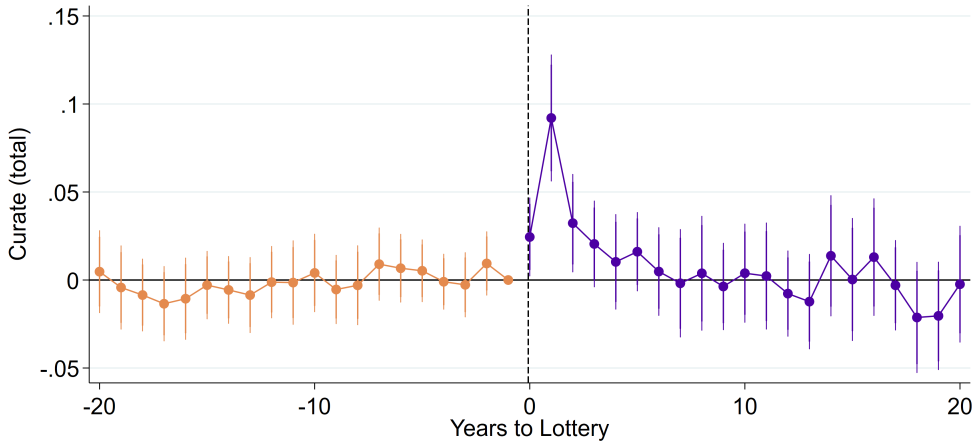
The additional funds from a lottery win successfully expanded the presence of the Anglican church, as intended by its architects. This expansion is evident in both the number of Anglican clergy and the upgrading of informal chapels into formal parishes.

[7]For a concrete example, assume that parish A and B begin with an income of 7 pounds, making them both eligible for the initial set of lotteries with an income threshold of 10 pounds. Suppose parish A wins the lottery in 1729, and parish B loses. The former now has a value of 14 pounds, the latter remains at 7 pounds. Both could win a benefaction in 1730, making the value of A 28 pounds and the value of B 21 pounds. Now neither parish is eligible for the 10-pound threshold lottery, or even for subsequent 20-pound threshold lotteries. As a result, parish A's lottery win in 1729 leads to enduring positive income effects.

Figure 4: Lottery Wins Increase the Presence of Anglican Clergy



(a) All clergy



(b) Curates

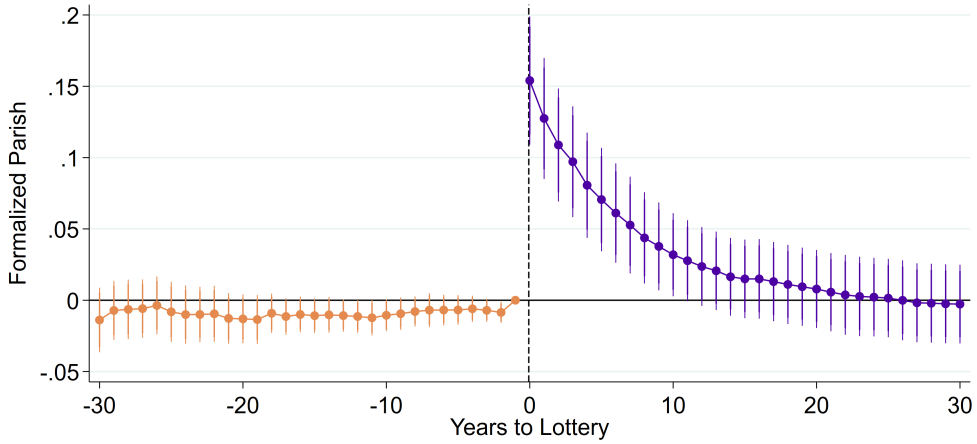
Notes: estimates of the impact of winning a QAB lottery on the total number of (a) clerics, and (b) curates active in a parish. Each point represents a coefficient estimate in the event study equation (2). The x-axis marks the years relative to the lottery, with -1 forming the omitted year. 90% and 95% confidence intervals are shown.

Figure 4 shows that a QAB lottery win quickly led parishes to hire additional Anglican clergy. Panel (a) shows the effect on the total number of clergy active according to the CCEd data. This rises by 0.1 in the year following the lottery, a 46% increase relative to lottery-losing parishes, with smaller positive effects in the lottery year itself. Panel (b) shows that essentially all of this impact is due to the hiring of junior curates, with the point estimates in both panels nearly identical.

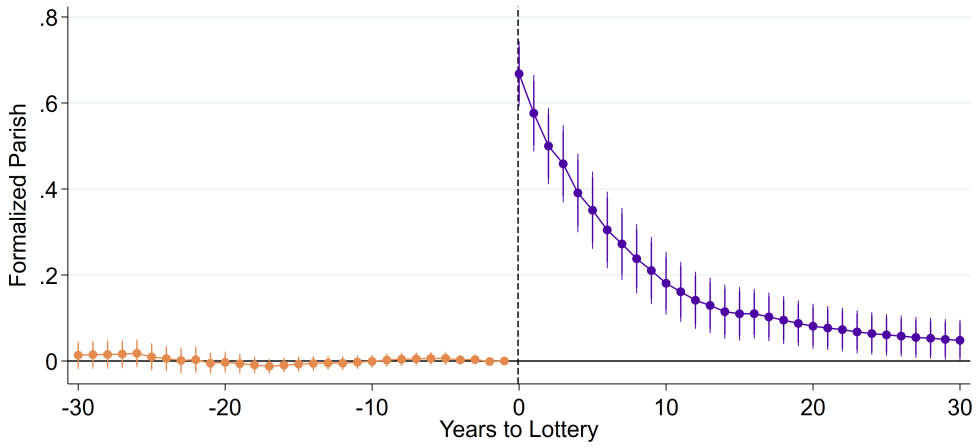
These results show that state support increased the number of Anglican clergy, though due to limitations in the CCEd data, we cannot directly estimate how long the differences in employment

persist. For most curates, the data only include a hiring date, with departures recorded more infrequently. In these cases, we code the cleric as being active for only one year, though their actual tenure was almost surely longer, with ten to fifteen years being typical in cases where we observe start and end points. Finally, the CCEd likely does not contain records on all clergy, further attenuating results. We thus interpret Figure 4 as illustrating an initial expansion that attenuated within one to two decades even though we cannot directly estimate the time path.

Figure 5: Lottery Wins Lead to Parish Formalization



(a) All parishes



(b) Initially informal parishes

Notes: estimates of the impact of winning a QAB lottery on whether an entrant achieved the administrative designation of a parish. Panel (a) shows results for all entrants, panel (b) restricts the sample to those that were not formalized in the year prior to the lottery. Each point represents a coefficient estimate in the event study equation (2). The x-axis marks the years relative to the lottery, with -1 forming the omitted year. 90% and 95% confidence intervals are shown.

Figure 5 demonstrates that a lottery win also upgraded the status of informal places of worship

into administrative parishes. In the Anglican system, this entailed assigning a permanent cleric to preach in the area rather than relying on the discretion of a higher-level cleric to assign a temporary one. As expected, the effect is concentrated among the parishes that were informal at the time of the lottery draw, as shown in panel (b). Immediately following a lottery win, 67% of these entrants formalized, a difference that lasted for roughly twenty years before attenuating.

Combined with the salary effects shown in Figure 3, our results show that the resources provided by QAB afforded the Anglican church the ability to meaningfully expand in winning parishes. A lottery win resulted in more clergy, better-paid clergy, and a more permanent presence. While initially high effects attenuated over time, the gaps between winners and losers remained for several decades, reflecting the long duration of the QAB process.

4.3 Reduced non-Anglican Presence

The increased presence of the Anglican church successfully reduced the practice of non-Anglican denominations, another key goal of the QAB program. We establish this result using two outcome measures: Anglican priests' reports of "dissenters" in their communities (from visitation returns to bishops), and the construction of non-Anglican churches (from GENUKI).

Table 2 shows that priests in winning parishes were less likely to report the presence of dissenters compared to priests in losing parishes.[8] Because we have a limited number of years for which we observe reports, we use the cross-section equation (3) and explore robustness both to the number of prior lotteries in the sample and the inclusion of pre-lottery controls. Our preferred estimates in column (2) show that QAB winners were 17 percentage points (27%) less likely to report dissenters in the next ten years. Reassuringly, we obtain similar, though slightly smaller effects when using twenty years of data, and the estimates are insensitive to the inclusion of controls, as we would expect given randomization.

We find a similar reduction in non-Anglican presence using independently-measured data on the foundation of non-Anglican churches. While it is likely that church construction lagged the spread of dissenting religions, this metric is not subject to the concern that priests biased their reports out of self-interest, and we can observe this metric annually across the sample. Figure 6 shows our equation (2) panel estimates on this measure. Consistent with Table 2, we find that a lottery win reduces the presence of non-Anglicans as measured by churches. The raw treatment effect increases over time, although this is a period in which the control mean is also increasing over time—from roughly 5% of parishes having a non-Anglican church at the time of the average lottery, to almost 30% 80 years later (panel (b), Figure C.1). As a result, the negative effect as

[8] We focus on the binary distinction of presence versus absence for two reasons. First, in this period, a large majority of the population was at least nominally Anglican, making formal dissent rare. Second, the reports clearly state when there is no dissent, but are often vaguer in enumerating them.

Table 2: Winning Parishes Are Less Likely to Have Dissenting (non-Anglican) Households

	(1) ≤10 years	(2) ≤10 years	(3) ≤20 years	(4) ≤20 years
Win	-0.17*	-0.17**	-0.15**	-0.14**
	(0.094)	(0.080)	(0.066)	(0.060)
N	964	964	1,770	1,770
SEs / Clusters	Lottery	Lottery	Lottery	Lottery
N (clusters)	46	46	71	71
Lottery FEs	Y	Y	Y	Y
Source×Year FEs	Y	Y	Y	Y
Pre-lot Controls		Y		Y
$\mathbb{E}[y\|\text{Win}=0]$	.63	.63	.57	.57

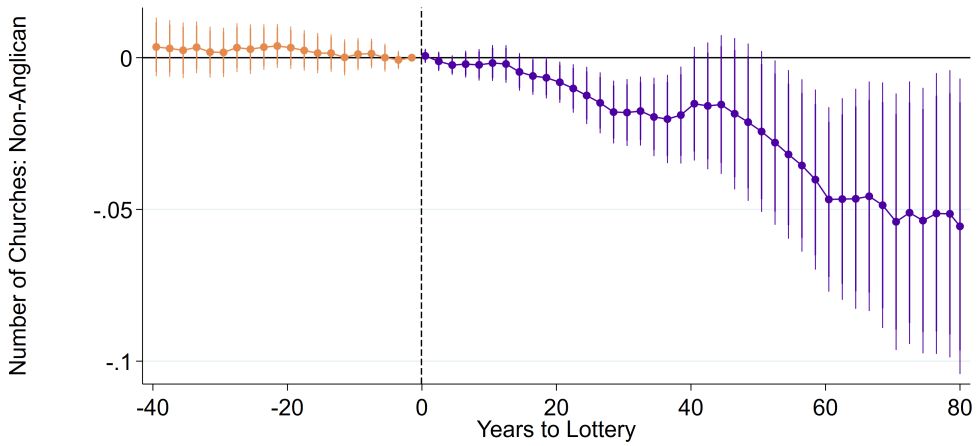
Notes: this table reports estimates of cross-section equation (3) with the outcome as a binary indicator for the presence of non-Anglicans (dissenters), according to our visitation returns data. Columns (1)-(2) include all lotteries up to ten years prior to the date of the outcome measure. Columns (3)-(4) include all lotteries up to twenty years before. The sample size is considerably smaller than the full set of lotteries and eligible parishes due to the limited availability of visitation returns. Source×Year fixed effects are dummies for each volume of returns interacted with the reporting year. Pre-lottery controls are listed in Appendix Section B.1.

a percentage of the control mean is somewhat more stable over time, at 10%, 13%, 23%, and 18%, 20, 40, 60, and 80 years post-lottery, respectively. Robustness to two-way parish and lottery clustering is shown in Appendix Figure C.4.

Notably, the graph shows that the negative effects on non-Anglican church presence were long-lasting, despite the income effects of a lottery win converging nearly to zero 30 years post-lottery (Figure 3). The persistent effects could either be because the lotteries occurred in critical years for the spread of dissenting beliefs, or because church construction represented a durable and cumulative investment by non-Anglicans in an area, meaning even a temporary disruption could have permanent effects.

Together, these results show that financial support for the Anglican church was effective at limiting the spread of dissenting religious views. There are several non-exclusive explanations for this result. The more numerous and better-paid clergy could have been more effective at promoting Anglican views, increasing religiosity. Their presence could also have made it more socially costly to hold dissenting views, both because they directly applied pressure to dissenters or because they made it more difficult to privately hold those views. In the first case, state support would increase overall religiosity, while the second case would decrease it. We turn to these questions in the next section.

Figure 6: Lottery Wins Persistently Curb the Entry of Dissenting (non-Anglican) Churches



Notes: estimates of the impact of winning a QAB lottery on the number of non-Anglican churches in a parish. Each point represents a coefficient estimate in the event study equation (2). The x-axis marks the years relative to the lottery, with -1 forming the omitted year. 90% and 95% confidence intervals are shown.

4.4 Anglican Religious Services and Religiosity

Despite the increased resources available to the Anglican church, we show that the provision of religious services and depth of religious feeling were either unaffected or declined. These results point away from an efficiency-wage explanation for clergy effort. They are, however, consistent with a competition-based story where the reduced presence of other religious groups also reduced effort.

We begin by analyzing data from the visitation returns, our most direct measure of the effort made by Anglican clergy in providing religious services. We create an aggregate index that combines the measures of how often several key services were performed: Sunday services, religious instruction for children ("catechism"), and major rites ("Sacrament of the Lord's Supper," i.e., the Eucharist). While the Anglican church issued standards for service frequency, in practice it was both common to fail and exceed these.[9] To create a single index, we calculate z-scores for each variable and average them across measures as described in Appendix Section A.2.

Table 3 shows that clergy effort declined following a lottery win. In our preferred estimates in column (2), a lottery within up to ten years prior reduces our effort index by 0.36 standard deviations. As with Table 2, the table includes several other specifications for robustness. Including or excluding pre-lottery controls has little effect, though our estimates are somewhat weaker in the twenty-year sample. Finally, Appendix Table C.1 shows that we find the same direction of results

[9]For example, the standard would be two Sunday services in the morning and afternoon. However, many returns list rates both above and below this amount. Clergy who fell below these standards often cited low pay or multiple officeholding as reasons.

Table 3: Winning Clergy Exert Less Effort

	(1) ≤10 years	(2) ≤10 years	(3) ≤20 years	(4) ≤20 years
Win	-0.36**	-0.36**	-0.27**	-0.23*
	(0.15)	(0.15)	(0.12)	(0.12)
N	943	943	1,745	1,745
SEs / Clusters	Lottery	Lottery	Lottery	Lottery
N (clusters)	42	42	71	71
Lottery FEs	Y	Y	Y	Y
Source×Year FEs	Y	Y	Y	Y
Pre-lot Controls		Y		Y
$\mathbb{E}[y\|\text{Win}=0]$	-.66	-.66	-.65	-.65

Notes: this table reports estimates of cross-section equation (3) on an index of Anglican clergy effort using our visitation returns data. The index is constructed by averaging the z-scores for the frequency of Sunday services, catechism, and the Eucharist. Columns (1)-(2) include all lotteries up to ten years prior to the outcome date. Columns (3)-(4) include all data up to twenty years before. Source×Year fixed effects are dummies for each volume of returns interacted with the reporting year. Pre-lottery controls are listed in Appendix Section B.1.

on each of the index components individually, though some are less statistically precise. While the self-reported nature of the effort measure could raise some concerns, we note that misreporting would need to be driven by a lottery win to affect our results. However, we find similar effects in measures of parishes' religiosity which we discuss below.

We next turn to how the religious attitudes of parishioners were affected by a lottery win. As with Table 3, we turn again to priests' visitation reports where they discuss the level of engagement with their parish. We focus on two measures: whether the priest reports parishioners cooperate in sending their children to be catechized and the (log) church attendance at the prior Easter. Table 4 provides suggestive evidence that these measures decline following a QAB win. Both measures fall in the ten- and twenty-year post-lottery samples, though at most these estimates reach 10% significance.

We supplement the visitation results with administrative data on baptisms. As with other results in this paper, the administrative measures are less direct than the visitation returns, but they have the advantage of having a much larger sample size for parishes and years that enables panel results. In our context, we treat participation in Anglican baptism as a signal of adherence to Anglican beliefs. While a large majority of children in the eighteenth century were baptized in the Anglican church, a number of exceptions did occur. These included parents who delayed baptism (possibly missing it if the child unfortunately died), who chose informal or non-Anglican baptisms, and who chose to baptize in another Anglican church nearby.

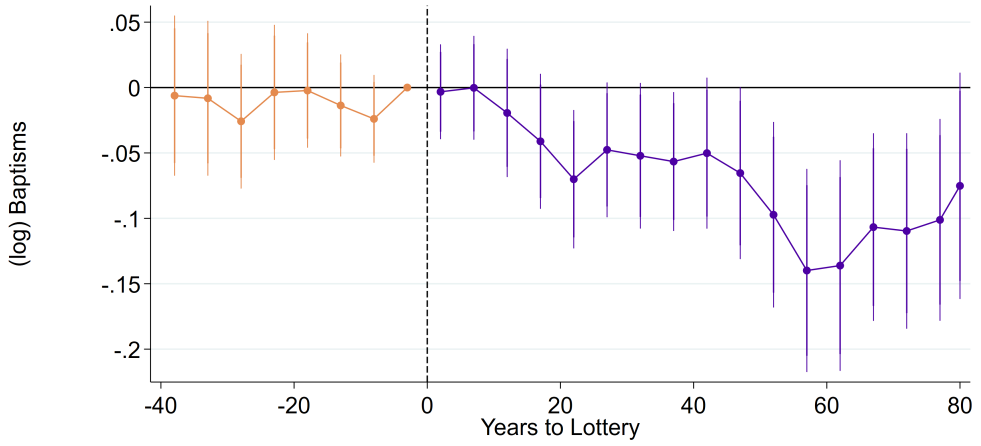
Consistent with a decline in Anglican sentiment, Figure 7 shows that baptism rates fell in

Table 4: Anglican Religious Practices Decline in Winning Parishes

	Catechism Cooperation (0/1)		log(Easter Attendance)	
	(1)	(2)	(3)	(4)
	≤10 years	≤20 years	≤10 years	≤20 years
Win	-0.16*	-0.087	-0.19	-0.28*
	(0.088)	(0.071)	(0.14)	(0.16)
N	969	1,681	770	1,340
SEs / Clusters	Lottery	Lottery	Lottery	Lottery
N (clusters)	39	61	31	59
Lottery FEs	Y	Y	Y	Y
Source×Year FEs	Y	Y	Y	Y
Pre-lot Controls	Y	Y	Y	Y
$\mathbb{E}[y\|\text{Win}=0]$	.82	.8	3.7	3.6

Notes: this table reports estimates of cross-section equation (3) with outcome measures of religiosity using our visitation returns data. Columns (1)-(2) use a binary indicator of whether the priest reported cooperation with catechism. Columns (3)-(4) use the logarithm of the attendance at the prior Easter. Columns (1), (3) include all lotteries up to ten years prior to the outcome date and columns (2), (4) include all data up to twenty years before. For the prior Easter variable, we additionally do not include the lottery year itself as the response indicates prior-year attendance. Source×ear fixed effects are dummies for each volume of returns interacted with the reporting year. Pre-lottery controls are listed in Appendix Section B.1.

Figure 7: Winning Parishes Have Persistently Fewer Anglican Baptisms



Notes: estimates of the impact of winning a QAB lottery on the log number of Anglican baptisms in a parish each year, using data from Findmypast. Before taking logs, baptism numbers are bottom-coded at $\frac{1}{2}$, equalizing the post-log change from zero to one baptisms with that of one to two baptisms. Each point represents a coefficient estimate in the event study equation (2). For power, time periods are coarsened into five-year bins. The x-axis marks the years relative to the lottery, with the final pre-lottery period forming the omitted category. 90% and 95% confidence intervals are shown.

parishes following a QAB win. The effects increase over time before largely stabilizing, 50 years post-lottery. This pattern suggests that a QAB win eroded a community's relationship with the Anglican church, and this eventually translated into a reduced rate of baptisms. The results are robust to two-way parish and lottery clustering as shown in Appendix Figure C.4. Finally, we see a similar downward trend in the number of non-Anglican baptisms as shown in Appendix Figure C.3. The latter analysis draws on a much smaller set of records due to the fact that relatively few non-Anglican baptisms were preserved and digitized, with about 2% of observations in our panel containing a positive number.

An alternative explanation is that the erosion occurred via reduced interaction with the church during formative childhood years, consistent with the reduced frequency of catechism shown in Table 3. This affected their attitudes in adulthood, where they became less enthusiastic about their own children's baptisms. Although we cannot directly test for this explanation, it is consistent with an approximate twenty-year period for the effect to materialize. Both of these dynamics would be consistent with our narrative of reduced religiosity. A less related, though intriguing, explanation would be that fertility or population itself declined. As discussed in Section 7.4, we find little evidence for this dynamic in either population figures or proxies for outmigration from a parish.

5 Discussion and Mechanisms

The pattern of results is consistent with a market-based view of religion in which the intensity of competition shapes the incentives of religious institutions. The state support provided to the Anglican church under Queen Anne's Bounty increased the effective resources of the established church and, in our data, is followed by a contraction in non-Anglican presence alongside declines in measured Anglican effort. Treated parishes exhibit lower indicators of dissenting activity and fewer signs of non-Anglican expansion. At the same time, clergy in these parishes reduce their effort in providing religious services. This result is inconsistent with a model where the supply of these services is constrained by a lack of resources or insufficient pay for religious workers. Instead, they support the idea that the dynamics of monopolistic competition apply to religious practices. As in conventional markets, incumbents can deter new entry through increased capacity or targeted actions against rivals (Dixit, 1980; Salop and Scheffman, 1983). Both are consistent with reduced output in the aggregate, with additional capacity remaining unused when entry is successfully discouraged. While we cannot directly observe how the competition reduction proceeds in our setting, the historical record confirms that Anglican clergy regularly deterred local dissent via a variety of methods, including legal prosecution, social pressure, and economic retaliation, as documented in Appendix D. Notably, these instruments rested on the clergy's institutional position within local networks of authority – which the expansion of Anglican presence and resources resulting from

Bounty augmentation presumably would have reinforced – rather than on the provision of religious services.

Drawing from theoretical work on entry deterrence, we provide additional evidence for this mechanism by examining the heterogeneity of our main results by the pre-lottery proxies for competition. In many models of monopolistic competition, the incumbent's behavior and equilibrium outcomes only shift when entry is ex-ante plausible (Ellison and Ellison, 2011). When competitors cannot profitably enter a market, incumbents have no incentive to reallocate resources toward deterrence. In our setting, we consider two proxies for this condition. First, we look at the pre-lottery presence of an Anglican church. While almost all communities in this period had access to Anglican services, some of these were informal "chapels" rather than established churches; we consider the latter less at risk of the growth of dissent. Second, we consider the pre-lottery presence of dissenters in visitation returns. As shown in Figure C.2, the existence of a nascent dissenting community, typically just several individuals, predicts the rise of more formalized communities a half-century later, as measured by the presence of established non-Anglican churches. We thus consider entry difficult in parishes whose visitation returns recorded no dissenters.[10] Note that the cross-sectional nature of the visitation data imposes some constraints on our analysis, most notably that we cannot analyze effects on outcomes like Anglican services that are reported simultaneously.

5.1 Pre-lottery Anglican Churches

Table 5 shows that our main effects are present only in areas without a recorded Anglican church and weaker or absent elsewhere. In particular, areas without an established Anglican church experience reductions in dissent and Anglican clergy effort in the visitation returns, alongside a reduction in baptisms over the long run. Sample sizes are smaller for the parishes with an established presence, but the point estimates for the visitation data are no longer negative, and the effect on baptisms appears more muted. Since (pre-lottery) living values are similar for both samples, these effects are not due to differences in the lottery grouping or general income differences. Finally, the mean outcome values columns (1)-(2) support our use of (pre-lottery) Anglican churches as reflecting lower competition, with dissent substantially lower in observations with a church compared to those without.

Several pieces of evidence also suggest that, via reduced clerical effort, state support lowered religiosity. After winning a QAB lottery, treated parishes record fewer baptisms, and visitation returns point toward lower participation in Anglican rites. This result supports a competition-based story, where a QAB win helped shield the Anglican church from dissent primarily in areas

[10] An alternative analysis that used the pre-lottery presence of non-Anglican churches is not practical in our context, as this removes 96% of observations.

Table 5: Heterogeneity by Pre-Lottery Anglican Church Presence

	Any Dissent		Effort Index		log(Baptisms)	
	(1)	(2)	(3)	(4)	(5)	(6)
	No Church	Church	No Church	Church	No Church	Church
Win×Post (< 20 years)	-0.15**	0.26*	-0.21*	0.0086	-0.012	0.017
	(0.058)	(0.14)	(0.12)	(0.10)	(0.024)	(0.034)
Win×Post 20+ years					-0.088**	-0.045
					(0.035)	(0.056)
N	1,591	171	1,559	185	2,815,703	1,037,910
SEs / Clusters	Lottery	Lottery	Lottery	Lottery	Lottery	Lottery
N (clusters)	71	31	71	31	84	83
Pre-lot Controls	Y	Y	Y	Y	Y	Y
Parish×Lottery FEs					Y	Y
Lottery×Year FEs					Y	Y
$\mathbb{E}[y\|\text{Win}=0]$	.61	.22	-.61	-.95	1.2	1.7
Salary (pre-lottery)	8.58	7.57	8.55	7.25	10.7	11.4

Notes: this table replicates other main results after splitting the sample by the pre-lottery presence of an Anglican church ("Church") or the absence of one ("No Church"). Columns (1)-(4) use the cross-section equation (3) on the visitation data with the twenty years post-sample. (1)-(2) use the presence of any dissenters as in Table 2. (3)-(4) use the Anglican clergy effort index as in Table 3. Columns (5)-(6) use the baptism sample as in Figure 7.

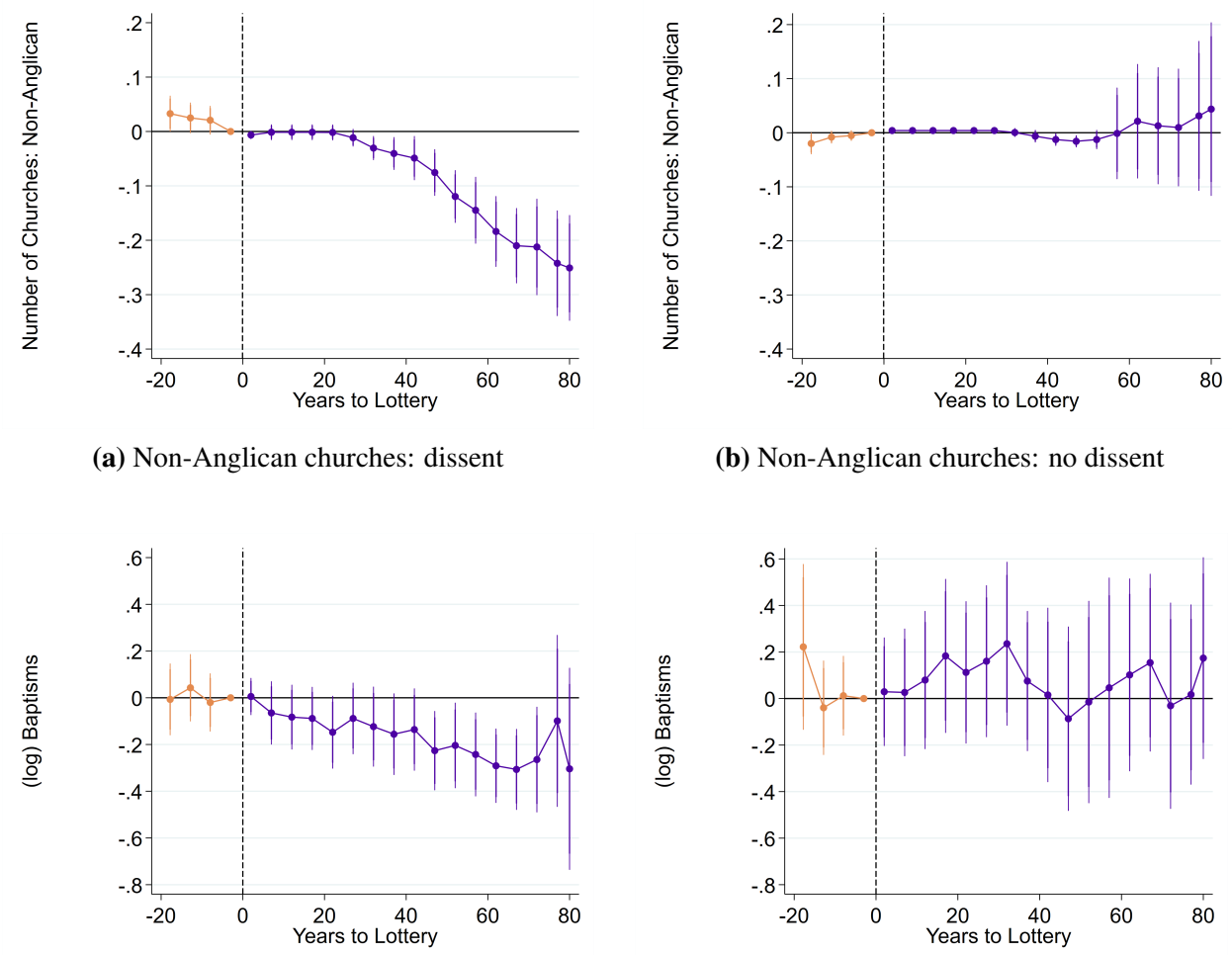
where it had a weaker presence. Areas where the church had an ex-ante stronger presence were already shielded from competition, leading to less costly, anti-competitive reallocations of effort.

5.2 Pre-lottery Dissent

Heterogeneity analyses by pre-lottery recorded dissent also support a competition story. As noted above, splitting the data by this outcome restricts us to using panel data; we typically only see visitation data once for each parish, meaning that Anglican services and reports of dissenters cannot be directly split by prior dissent. Figure 8 shows panel event studies for non-Anglican churches and baptisms. Panels (a) and (c) show that our main effects hold in the set of parishes with preexisting dissent, with non-Anglican churches and baptisms declining in winners relative to losers. However, the effects are essentially absent in parishes where dissent was historically absent, as shown in panels (b) and (d). While our confidence intervals cannot rule out modest decreases, the point estimates are either positive or close to zero, qualitatively differing from the negative impacts in the other sample.

Both sets of results tie the effects of state funding of religion on religious practices to the presence of competition. In cases where the Anglican church was more established or dissenters were absent, additional state funding did nothing to deter entry by competing groups who were

Figure 8: Winning Effects are Concentrated in Parishes with Pre-lottery Dissent



(a) Non-Anglican churches: dissent

(b) Non-Anglican churches: no dissent

(c) (log) Baptisms: dissent

(d) (log) Baptisms: no dissent

Notes: estimates of the impact of winning a QAB lottery on the number of non-Anglican churches in (a)-(b) and log baptisms in a parish in (c)-(d). Baptism are bottom-coded to $\frac{1}{2}$ equalizing the post-log change from zero to one with that of one to two. All samples are restricted to parishes and lotteries where we have visitation returns data for a prior year. Panels (a), (c) use parishes where that return reported the presence of dissenters. Panels (b), (d) use parishes where that return reported no dissenters. Each point represents a coefficient estimate in the event study equation (2). The x-axis marks the years relative to the lottery, with -1 forming the omitted year. 90% and 95% confidence intervals are shown.

already unlikely to enter. Correspondingly, we do not observe changes in Anglican services or baptisms in those areas. Those effects are limited to places where entry was plausible ex-ante but dissuaded by a QAB lottery win. To further support this story, we address and rule out several alternative explanations in Section 7.

6 Political Economy and Governance

Illustrating the connection between church and state, state funding for the Anglican church led to administrative consolidation and local governance on a larger scale. Parishes constituted the basic unit of government in this period, and in a majority of cases an eligible living was synonymous with a single one. However, some communities were granted autonomous status as "townships."[11] Beginning in 1662, townships were formally granted several local governance roles, notably including raising and spending money on "poor relief." Parishes and townships, as applicable, levied "poor rate" taxes that were one key source of funding for anti-poverty spendin (Webb and Webb, 1906).

Table 6 shows that a QAB win reduced the number of administrative units within a living. Columns (1) and (5) show that in both 1811 and 1831, the number of distinct census units within a living decreased. Columns (2)-(3) show this effect is driven by increased centralization, with fewer townships and fewer livings split into multiple units. Column (4) illustrates that this effect is limited to the township domain, with the number of parishes suggestively increasing, likely the result of fewer being divided into smaller township units. Columns (6) and (7) show that administrative consolidation shaped local fiscal policy, leading to higher tax rates for poor relief. They respectively show this result for the rate overall and the imposition of any poor rate.

Our results show that the expansion of the Anglican Church promoted administrative consolidation. Classic political economy theories have studied the tradeoffs between larger jurisdictions with scale economies and smaller ones that match homogenize preferences (Alesina et al., 2004). In our context, the Church served as the backbone of local government, and additional resources likely allowed it to perform administrative functions for a wider area. While our data do not permit us to assess welfare, our results are consistent with scale economies in governance. At a minimum, QAB wins allowed consolidated governments to levy greater resources for anti-poverty programs.

7 Alternative Mechanisms

7.1 Additional Robustness and Balance Tests

In this section, we present additional robustness and balance tests to validate our results. While our cross-sectional result tables typically show robustness to sample and control choices, the panel designs have less flexibility. The inclusion of large numbers of fixed effects absorbs a large amount of variation, potentially obscuring problems with our design. Event studies also do not show the differences in levels between winners and losers.

[11] Despite the name, townships, like the majority of our sample, were small, rural villages and not urban areas.

Table 6: Administrative consolidation and poor rating

	1811 census units				1831	Poor rate, 1803	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	All units	2+ units	Townships	Parish rows	Townships	Rate (%)	Any rate
Win	-0.067***	-0.024***	-0.082***	0.015	-0.073***	1.60***	0.034***
	(0.019)	(0.0078)	(0.025)	(0.012)	(0.025)	(0.52)	(0.0072)
N	36,133	36,133	36,133	36,133	35,968	36,017	36,017
SEs / Clusters	Lottery	Lottery	Lottery	Lottery	Lottery	Lottery	Lottery
N (clusters)	88	88	88	88	88	88	88
Lottery FEs	Y	Y	Y	Y	Y	Y	Y
Diocese FEs	Y	Y	Y	Y	Y	Y	Y
Pre-lot Controls	Y	Y	Y	Y	Y	Y	Y
$\mathbb{E}[y\|\text{Win}=0]$	1.2	.11	.46	.73	.47	24	.9

Notes: this table reports estimates of cross-section equation (3) on an index of Anglican clergy effort using our visitation returns data. The index is constructed by averaging the z-scores for the frequency of Sunday services, catechism, and the Eucharist. Columns (1)-(2) include all lotteries up to ten years prior to the outcome date. Columns (3)-(4) include all data up to twenty years before. Source×Year fixed effects are dummies for each volume of returns interacted with the reporting year. Pre-lottery controls are listed in Appendix Section B.1.

Notes: this table reports estimates of cross-section equation (3) on measures of administrative consolidation. Columns (1)-(5) study a livings' number of constituent units in the 1811 and 1831 censuses. Column (1) counts the total number of census units, (2) uses a binary for whether the living contains two or more (3), (5) count townships and (4) counts parishes. (6)–(7) studies the "poor rate" tax on land rents. (6) studies the rate itself and (7) studies whether any rate was levied. Pre-lottery controls are listed in Appendix Section B.1.

We thus replicate our primary panel results with plots of outcome means adjusted only for lottery fixed effects in each period.[12] Appendix Figure C.1 shows this for log salary value, the number of non-Anglican churches, and the log baptisms. Prior to the lottery year, winner and loser outcomes are very similar in both trends and levels. Note that this is not a mechanical result of our adjustment process as winner- and loser-values within a lottery receive the same adjustment. Following the lottery, winners and losers diverge: winners' salary values immediately increase while their baptisms and non-Anglican church construction decrease. The graphs illustrate that this divergence is not driven by absolute losses among winners but rather a slowed rate of increase. Overall, the pre-lottery balance and post-lottery divergence in these graphs are consistent with their event study counterparts.

It is also possible that subsamples of data used in our analyses, particularly the visitation re-

[12] Specifically, in any period we shift each lottery's values by a constant so its mean matches the sample mean. Note that even with pure random assignment, lottery-level adjustments are still required for identification as we pool results from lotteries with different samples and win probabilities; see Section 3.3.

turns, might be imbalanced even if the full sample is not. This could occur by chance given the small sample of visitation data or if a lottery win selectively affected the probability of a parish completing a return. However, Table 7 finds no detectable imbalance across lottery winners and losers in this subsample with point estimates remaining small, albeit with wider confidence intervals. This supports our contention that the visitation sample retains the statistical balance of the full sample and cuts against the idea of differential entry into a recorded visitation return.

Table 7: Balance on pre-Lottery Measures (Visitation Sample)

	(1) Salary (1718)	(2) Salary	(3) Formal Parish	(4) Any Cleric	(5) Any Curate	(6) # Churches (Anglican)	(7) # Churches (Non-Anglican)
Win	0.20	0.52	0.015	0.032	0.049	0.087	-0.016
	(0.45)	(0.43)	(0.081)	(0.062)	(0.056)	(0.054)	(0.0096)
N	1,031	1,031	1,031	1,031	1,031	1,031	1,031
SEs / Clusters	Lottery	Lottery	Lottery	Lottery	Lottery	Lottery	Lottery
N (clusters)	53	53	53	53	53	53	53
Lottery FEs	Y	Y	Y	Y	Y	Y	Y
$\mathbb{E}[y\|\text{Win}=0]$	7.2	8.2	.54	.15	.11	.096	.024

Notes: within-lottery comparisons of winners and losers in a single pre-lottery year using estimates of equation (3), restricted to the baseline visitation returns sample of Table 2. Column (1) is measured in 1718, which we use as our initial pre-lottery salary estimate. Columns (2)-(7) are measured one year prior to the lottery. Columns (1)-(7) respectively measure the entrant's salary for both (1) and (2), whether the entrant had formally attained parish status, whether we observe any active clergy in the CCEd, whether we observe any CCEd curate, the number of Anglican churches, and the number of non-Anglican churches.

7.2 Clergy Selection and Quality

If the skills or other quality of clergy declined as a result of state support, that would constitute a different mechanism than the one presented above. For example, a head cleric might use the funds to subcontract religious services to a less experienced and less effective curate. This explanation would still leave our estimates as reflective of the effects of state support, but the mechanisms would be more related to selection effects than incentive effects.

However, we do not find empirical support for this idea, as the marginal clergy hired following a lottery win are observably similar to the overall population. Table 8 demonstrates this by examining clerical hires of different types of clergy. Consistent with the results in Section 4.2, hiring is a short-term result, and we consider our primary estimate the pooled coefficient for zero to three years post-lottery.[13] Columns (1)-(2) replicate our earlier results that a lottery win caused immediate

[13] Other specifications are similar. For completeness, the table includes estimates of effects on all subsequent years, but as these are relatively precise zeros, we do not focus on them.

hiring, essentially all consisting of curates. Columns (3)-(4) estimate the effect on curates with a bachelor's degree and "full-time" curates whom we do not observe working multiple positions simultaneously in the CCEd data. The ratio of these estimates to the total curate increase in (2) gives us the estimate that 44% of marginal curates have a BA and 58% work full time. These figures are relatively similar to the 53% and 40% of all curates who have a bachelor's or worked full time during the same period, with marginal clerics modestly less likely to have a degree and modestly more likely to have a single position. Finally, comparing columns (4) to (5), we see that almost all the movement in full-time employees comes from curate hiring, implying little change from the senior clergy.

Table 8: Clerical Selection

	(1) Clergy (all)	(2) Curate (all)	(3) Curate (BA)	(4) Curate (Full-time)	(5) Clergy (Full-time)
Win×Post (< 4 years)	0.043***	0.045***	0.020***	0.025***	0.024***
	(0.013)	(0.011)	(0.0069)	(0.0056)	(0.0085)
Win×Post 4+ years	-0.0049	-0.0063	-0.0053	0.0025	0.0012
	(0.013)	(0.011)	(0.0082)	(0.0071)	(0.0090)
N	1,803,587	1,803,587	1,803,587	1,803,587	1,803,587
SEs / Clusters	Lottery	Lottery	Lottery	Lottery	Lottery
N (clusters)	88	88	88	88	88
Parish×Lottery FEs	Y	Y	Y	Y	Y
Lottery×Year FEs	Y	Y	Y	Y	Y
$\mathbb{E}[y\|\text{Win}=0]$	.26	.13	.062	.053	.12

Notes: this table reports estimates of panel equation (2) with the outcome as the total counts of clerics in the CCEd database. The sample includes data from thirty years before to thirty years after the lottery. We report estimates for a short-term effect of winning (zero to three years out) and a long-term (four or more years out). Columns (1)-(5) respectively report results on all clergy, all curates, curates with a bachelor's (BA), curates who do not appear as working elsewhere in the data ("full-time"), and full-time clergy of any rank.

We find a similar lack of effects in a comparison of clergy in lottery winners and losers according to visitation data. Appendix Table C.2 finds no detectable lottery effect on whether the responding priest is a curate, whether they reside in the parish, or whether resident curates have proper licensing. In addition to providing evidence against clerical selection as a channel through which state support affected religiosity, these non-results are historically important. The QAB program was successful in expanding the presence of the Anglican church, but even relatively large salary increases did not achieve the program's goal of reducing multiple office-holding and non-residence among the clergy. Furthermore, the lack of effects on clergy selection are once again consistent with the views of Adam Smith. He writes that "In England…the lottery of the church is in reality much more advantageous than is necessary…the hopes of much more moderate

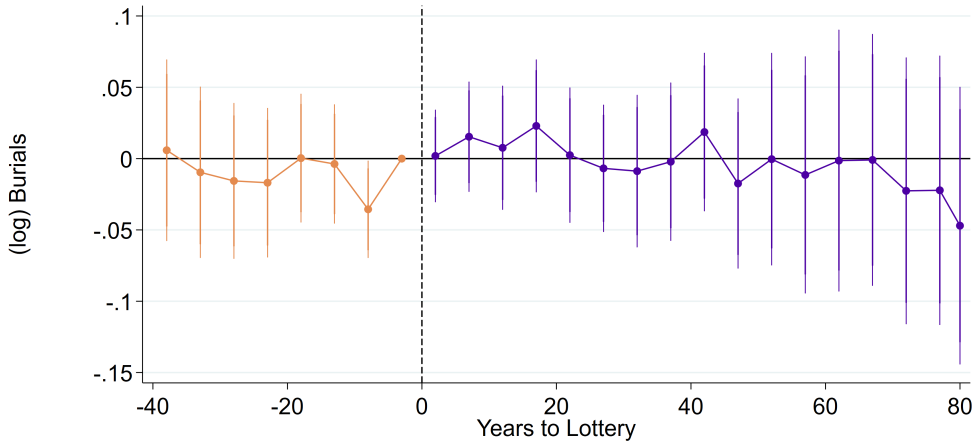
benefices will draw a sufficient number of learned, decent, and respectable men into holy orders" (Smith 1776, Book I, Ch. 10, Part II).

7.3 Differential Misreporting

Another explanation for some of our results is that a lottery win changed clerics' propensity to report certain events. The higher salary from a win could, for example, increase the value of a position to its holder, making them less likely to report unflattering news. Since baptisms and any variable from the visitation returns were both recorded by the Anglican clergy, any change in reporting behavior induced by a win would lead to a spurious result.

We do not find evidence for differential misreporting in several tests of this dynamic. We first consider the possibility of misreporting in the parish registers that list baptisms in addition to other major life events for parishioners. As a quasi-placebo test for misreporting, we instead test for changes in reported burials in the parish. In contrast to baptisms, delayed or informal burials were rare in this context, meaning state support would be unlikely to affect the true rate of burials. However, this measure would be subject to misreporting in the parish register. Figure 9 finds no evidence for changes in recorded burials following a QAB win, with most point estimates close to zero. As such, we cannot detect a differential reporting effect in these data.

Figure 9: Lottery Wins Did Not Affect Reporting of Burials



Notes: estimates of the impact of winning a QAB lottery on the log number of burials in a parish each year. Before taking logs, burial numbers are bottom-coded at $\frac{1}{2}$, equalizing the post-log change from zero to one burials with that of one to two burials. Each point represents a coefficient estimate in the event study equation (2). For power, time periods are coarsened into five-year bins. The x-axis marks the years relative to the lottery, with the final pre-lottery period forming the omitted category. 90% and 95% confidence intervals are shown.

We also provide support for priests' visitation reporting on non-Anglicans in their parish. Note

that our main result that state support inhibited the spread of competing beliefs is supported by the administrative data on non-Anglican churches in Figure 6, making misreporting unlikely as a general explanation of our results. Still, supporting the validity of the visitation responses remains important given the variety of outcomes they record. Appendix Figure C.2 shows that priests' reports of dissent correlate strongly with the church measure. First, in the 1740s, when our visitation data were collected, all parishes with a non-Anglican church reported dissent. Second, the report remains predictive of future events. Parishes with reported dissent are much more likely to establish a non-Anglican church in the subsequent decades. The reports made to bishops thus align with our administrative data on church construction. As an additional check, Appendix Table C.3 finds no detectable difference in the response length or reported population of winning and losing parishes. We consider the former a direct proxy for response quality and the latter as a test for selective non-response in the visitation data,[14] but find no evidence for either dynamic.

7.4 Fertility and Outmigration

A natural explanation for the decreased baptism rates is that fewer children were born, ultimately lowering the population. Such a dynamic could have occurred either due to changes in social norms (lowering births per person) or outmigration (lowering the number of people). However, using several measures, we do not detect any evidence of either. Table C.3 reports equation (3) cross-section effects on both short-run population effects from the visitation returns and a long-run effect for our full sample according to the 1811 census. We find no statistically significant evidence of population loss in either sample, though we cannot rule out modest effects.

We also do not see changes in outmigration, using linkages across baptism and burial records as a proxy. Because our data only code name and location for a specific set of parishes, we cannot confident link individuals across time. However, the lack of a burial record matching a baptism provides an indication that the person moved outside of their parish at some point. Using this metric, Appendix Figure C.5 shows that there are no indications of migration around a QAB win for either Anglican or non-Anglican baptism records.[15] While the limited number of non-Anglican baptism records leads to large confidence intervals, the positive point estimates cut against a story where non-Anglicans were displaced following a parish's QAB win.

[14] For example, a lottery win could have increased response rates among very small parishes that normally did not complete a visitation return. In that case, we would expect a spurious negative "effect" on population driven by selective non-response.

[15] Given the limited number of non-Anglican burial options for our period, we allow either type of record to match to any burial record in our data.

8 Conclusion

Economic theories offer competing predictions of the effects of a state's financial sponsorship of religion. This paper studies the effects of state support in a setting where the British government attempted to improve the standing of its established church against the spread of competing beliefs. Using the lottery-based allocation of grants from Queen Anne's Bounty, we obtain random variation in clergy income during a period of rapid religious change. We document a large first-stage effect: winning a lottery increases clerical income by roughly 80% initially, with differences attenuating as support became widespread. The program achieved an important objective of its designers by slowing the expansion of non-Anglican groups as measured by priests' reports and non-Anglican church construction. Yet, we also find that Anglican religious service provision declines in treated parishes. Our measures of religious participation are flat to negative, including a reduction in baptisms and suggestive declines in participation reported in visitation returns.

Taken together, the evidence supports a market-driven mechanism described by Adam Smith. By weakening competition in the religious "marketplace of ideas," state support can reduce the incentives of incumbent providers to exert effort, even when it expands organizational capacity. These findings highlight a tension inherent to policies that use fiscal tools to promote a state religion: such support can increase the dominance of the favored church while failing to increase religious practice more broadly. We find no evidence for other explanations, including imbalanced lotteries, clergy selection, and differential misreporting in our setting.

While the effects of state support on the quantity and pay of Anglican clergy were largely temporary, albeit long-lived, other impacts appear to have left a permanent mark. Most notably, the durable action of founding a non-Anglican church shows that winner and loser gaps persisted decades after the Bounty's salary boost had faded. This last piece of our work suggests that cultural institutions, like religion, may not only reflect current conditions but also the accumulation of historical events. As such, states have significant ability to influence the beliefs of their populations, though the consequences of doing so may not align with the goals that motivated such interventions.

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Online Appendix

"Investing in Angels: The Impacts of a Century of Randomized Grants to British Clergy"

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A Data Appendix

A.1 Sources

Ex-ante valuations and the eligible set (Liber/Ecton). To reconstruct the universe of livings that were potentially eligible for Queen Anne's Bounty (QAB) lotteries, we digitize early-eighteenth-century printed valuations of ecclesiastical benefices. In practice, we use Ecton's *Liber Valorum & Decimarum* to obtain each living's valuation. We additionally consult Ecton's *Thesaurus rerum ecclesiasticarum* to recover poor livings that are missing from (or inconsistently named in) the Liber lists and to harmonize parish names across sources. These lists provide a 1718 valuation of the value of the living as assessed for Queen Anne's Bounty. As such, we take 1718 as the last pre-treatment year for our analysis even though lotteries began in 1714. Note that this was an administrative definition of income that involved estimating the returns on land and other assets provided to clergy. This is the definition relevant for lottery eligibility, but actual incomes must have fluctuated from year to year. (Ecton, 1718, 1728)

Lottery rules, annual entrant sets, and winners (QAB minute books). We obtain the hand-written minute books of the Governors of Queen Anne's Bounty from Lambeth Palace Library (London), and digitize the relevant pages. The minute books record (i) eligibility rules and threshold changes adopted by the Board (including exceptions and special ballots), and (ii) annual ballot outcomes listing lottery winners. We transcribe these entries to create a living-by-year panel of winners, constructing the list of losing entrants by replicating the eligibility rules. We use the minute books to validate (and, where necessary, correct) information in printed compilations. (Queen Anne's Bounty Governors, 1704)

Augmentations by lot, benefaction, and parliamentary grant (parliamentary compilation). To cross-check lottery outcomes and to maintain updated estimates of living value, we incorporate data on "benefactions": non-random private or public donations to a parish. QAB considered these benefactions as raising living values, meaning they are necessary in order to reconstruct eligibility. We digitize the parliamentary return *Papers Relating to Queen Anne's Bounty, and to Parliamentary Grants, for the Augmentation of the Maintenance of the Poor Clergy, 1703–1815*. This source lists augmentations and their year, and indicates whether an augmentation was funded by lot, benefaction, or parliamentary grant. We use these tables both as a validation source and to code benefactions in years when minute-book coverage is incomplete. (Great Britain. Parliament. House of Commons, 1815)

Clergy careers and parish status (CCEd). We use the Clergy of the Church of England Database (CCEd, 1540–1835) to construct parish-by-year measures of Anglican presence and clerical characteristics. The CCEd provides information on events surrounding the careers of Anglican clergy. Most importantly for our case, it includes information on appointments and vacancies for clerical positions, allowing a reconstruction of their professional history. It is important to note that this reconstruction is often partial, especially for more junior positions such as curates. For example, the database might record when a cleric was hired but have no information about when they departed the position. Other records show a cleric held the position at a particular date, but not when their term began or ended. Secondarily, the records often include information on the cleric's background, including education. (Clergy of the Church of England Database , CCEd)

Church formation and locations (GENUKI). To measure the spread of religious groups, we use the GENUKI Church Database to compile the locations and founding dates of churches. We scrape county-level church records files from GENUKI and harmonize them to our parish identifiers. The data include the church's denomination, allowing us to compute the number of both Anglican and non-Anglican churches active in a particular parish in a particular year. (GENUKI, 2021)

Visitation returns (religious life and practice) We collect and digitize a set of bishop's visitation returns: standardized surveys answered by local clergy to their bishop. While the exact set of questions varied by diocese and year, they typically included questions about religious life and practices in the parish. For example, the frequency of Sunday services, the presence of non-Anglican (dissenting) families, and whether the responding priest resided in the parish. For each return, we process the scan using optimal character recognition (OCR) and use an LLM model (typically GPT 4.1) to convert text-based responses into quantitative figures. Our data include visitation returns for York Dioecese (1743, 1764 partial); Wiltshire (1783); Surrey (1717, 1725, 1764, 1788); Oxford (1738); Bedfordshire (1706-12, 1717, 1720); Devonshire (1744, 1779). These choices were driven by the availability of digitized copies of these works.

Parish registers: baptisms, marriages, and burials (Findmypast). To proxy for engagement with Church of England rites over time, we compile parish-register data on baptisms/christenings, marriages, and burials from the public information on Findmypast. Each entry typically contains the parish in which it occurred, the name of the relevant person, the year of the event. We use the names to remove duplicate records stemming from the collection of data from multiple sources. (Findmypast, 2026)

Religious census (1851). To study longer-run religious adherence and the cross-sectional religious landscape in the mid-nineteenth century, we use the published report of the 1851 Census of Religious Worship for England and Wales, which records denomination, seating capacity, and attendance measures for places of worship. We digitize church-level entries and link them to our parish geography. (Great Britain, 1854)

Historical GIS and geocoding (CAMPOP; Google Maps). Finally, to link livings, parishes, and churches across sources with inconsistent spellings and shifting administrative boundaries, we use the Cambridge Group's historical GIS shapefile of parishes and places as enumerated in the 1851 census. We supplement this with automated geocoding for ambiguous place names and to validate matches using the Google Maps Geocoding API. (Cambridge Group for the History of Population and Social Structure, 2022)

A.2 Variable Construction and Definition

Anglican Services

Sunday Services: using GPT 4.1, we ask the model to calculate how many times Sunday services would occur in a typical year of fifty-two weeks. Most responses discuss weekly levels (twice a week, once a week, once every other week) but some describe more complex situations (e.g., additional services during Lent).

Annual Sacraments of the Lord's Supper: using GPT 4.1, we ask the model to calculate how many times the sacrament of the Lord's Supper (i.e., the Eucharist) would occur in a typical year. Since this question was framed annually, conversion is rarely needed except in cases of ambiguous ranges. Most respondents state three or four times.

Catechism: answers regarding the frequency of catechism were typically vaguer than other variables we collected (e.g., "I Catechise Children, & Servants in the spring & Especially in time of Lent, & other Convenient seasons"). Consequently, we coded responses in a categorical fashion: 3 (more than 6 times a year); 2 (about six times a year, equivalent to once a week at Lent – by far the most common response); 1 (sometimes, but less than six times a year); 0 (never).

Effort Index

For each variable within the final sample, we compute its z-score by subtracting the sample mean and dividing by the sample standard deviation (i.e., in the entire collected sample). Then, we average non-missing variable z-scores into an aggregate "effort index" for each parish-year observation

in the visitation data.

We perform two additional transformations of the effort index for ease of analysis. First, we topcode the variable at the 99^{th} percentile due to the existence of a small number of outliers that report exceptionally frequent Sunday services. Finally, we linearly rescale the index so it has a sample mean of zero and sample standard deviation of one. This allows the interpretation of changes in the index itself to represent standard deviations rather than an average of standard deviations.

B Additional Results and Description

B.1 Pre-lottery Cross-Section Controls

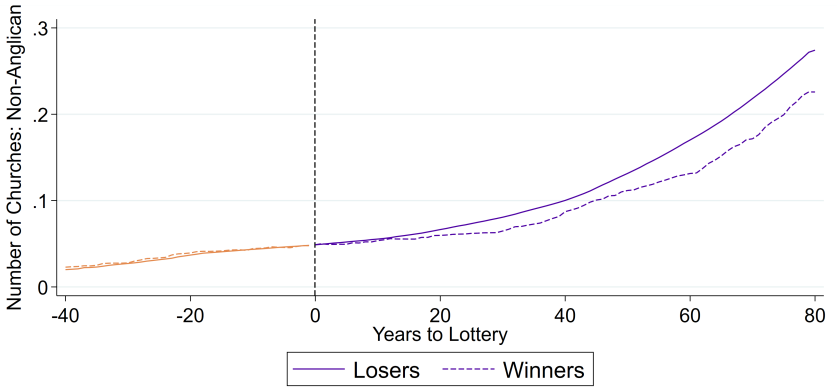
For our cross-sectional results using equation (3), including analyses of visitation data, we frequently include a set of pre-lottery controls in lieu of parish fixed effects. By default, these consist of: the living (salary) value in 1718, the living value the year before the lottery, a binary indicator for formal parish status the year before the lottery, the total active clergy the year before the lottery.

C Appendix Tables and Figures

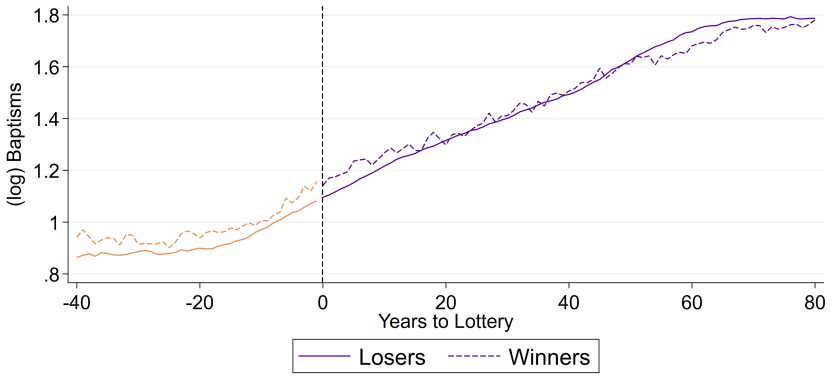
Figure C.1: Lottery-Adjusted Means Over Time



(a) (log) Salary Value



(b) Number of non-Anglican Churches



(c) (log) Baptisms

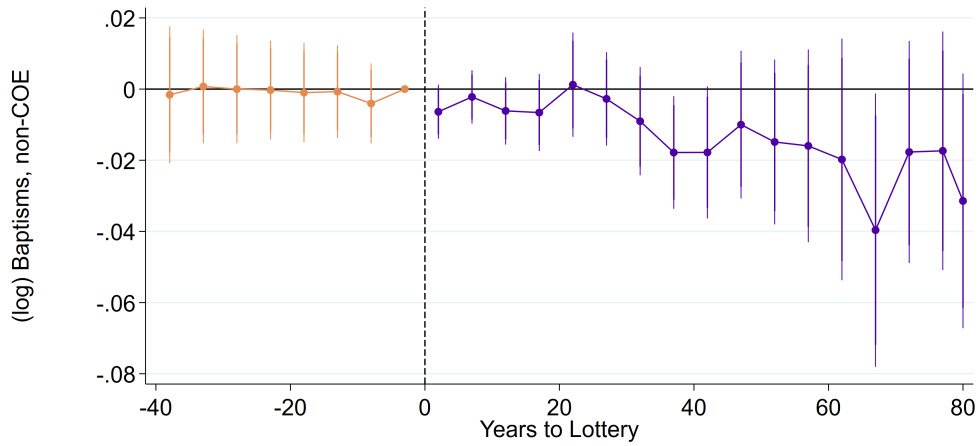
Notes: these graphs plot the average value of the listed outcome for lottery winners and losers. To equalize differences across lotteries, the values for each lottery are shifted by a constant in each period to match the sample mean. Values for parishes within a lottery, i.e., for winners and losers separately, are not adjusted.

Figure C.2: Visitation-Reported Dissent and non-Anglican Churches



Notes: this graph plots the growth in non-Anglican churches in parishes represented in our visitation data from the 1740s. The sample is split into parishes where the priest reported the existence of dissenters and those that did not and lines report the average number of non-Anglican churches open in each group in each year.

Figure C.3: Winning Parishes Have Persistently Fewer non-Anglican Baptisms



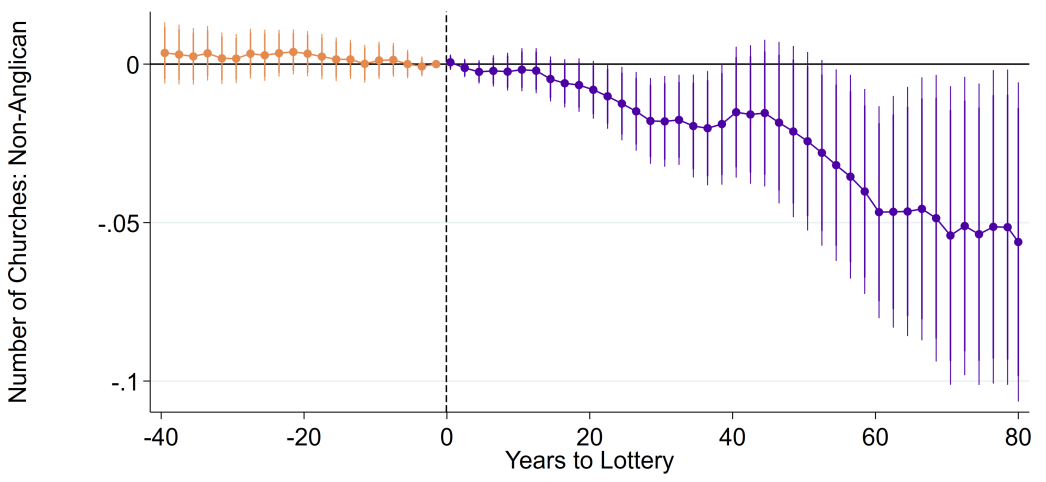
Notes: estimates of the impact of winning a QAB lottery on the log number of non-Anglican baptisms in a parish each year, using data from Findmypast. Before taking logs, baptism numbers are bottom-coded at $\frac{1}{2}$, equalizing the post-log change from zero to one baptisms with that of one to two baptisms. Each point represents a coefficient estimate in the event study equation (2). For power, time periods are coarsened into five-year bins. The x-axis marks the years relative to the lottery, with the final pre-lottery period forming the omitted category. 90% and 95% confidence intervals are shown.

Table C.1: Effort Index and Components

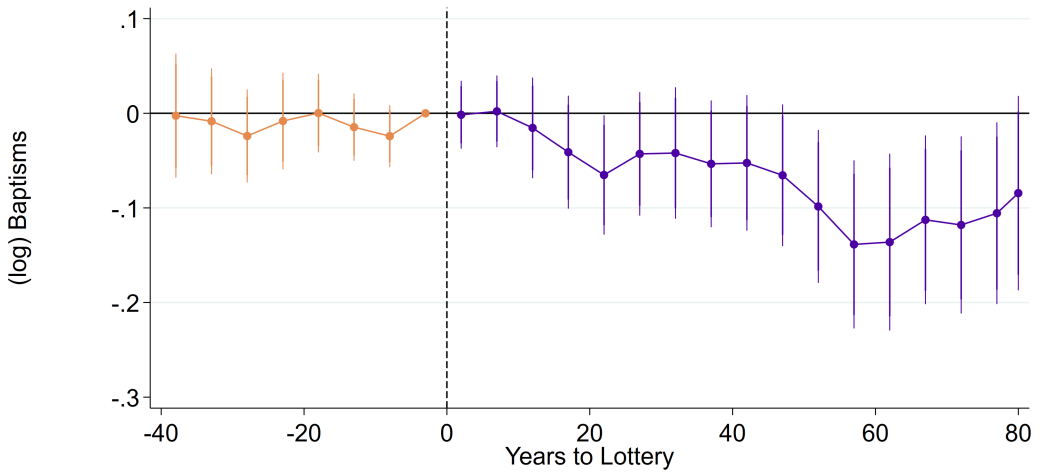
	Index (z-score)		Sunday Services / year		Sacraments / year		Catechism (0-3 scale)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Win	-0.36**	-0.36**	-7.26	-7.14	-0.35	-0.36	-0.29**	-0.27**
	(0.15)	(0.15)	(6.30)	(5.86)	(0.26)	(0.26)	(0.12)	(0.13)
N	943	943	943	943	879	879	909	909
SEs / Clusters	Lottery	Lottery	Lottery	Lottery	Lottery	Lottery	Lottery	Lottery
N (clusters)	42	42	42	42	37	37	37	37
Lottery FEs	Y	Y	Y	Y	Y	Y	Y	Y
Source×Year FEs	Y	Y	Y	Y	Y	Y	Y	Y
Pre-lot Controls		Y		Y		Y		Y
$\mathbb{E}[y\|\text{Win}=0]$	-.66	-.66	49	49	3.7	3.7	1.7	1.7

Notes: this table reports estimates of cross-section equation (3) with the outcome as an index of Anglican clergy effort, based on our visitation returns data. The index is constructed by averaging the z-scores for the frequency of Sunday services, the Eucharist, and catechism. The table analyzes each of those variables in the order listed. Pre-lottery controls are listed in Appendix Section B.1.

Figure C.4: Event Studies are Robust to Parish-level Clustering



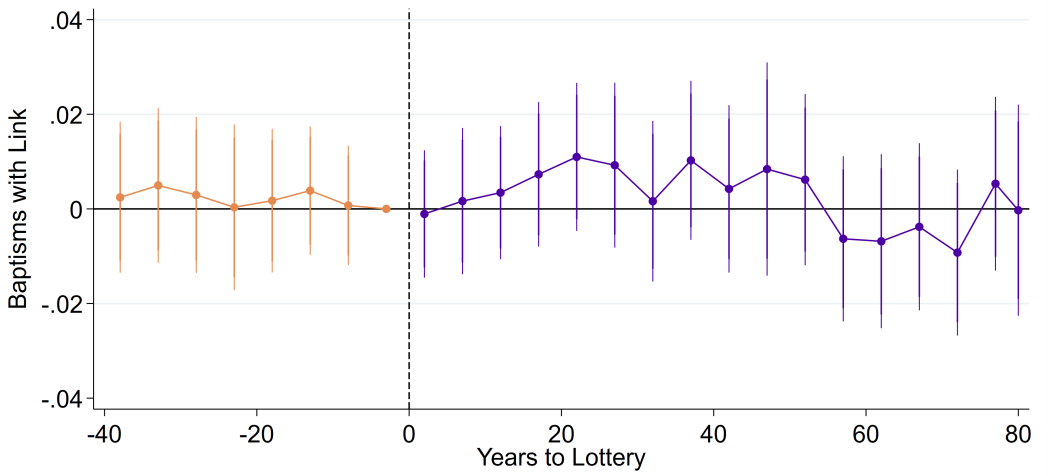
(a) Number of non-Anglican Churches



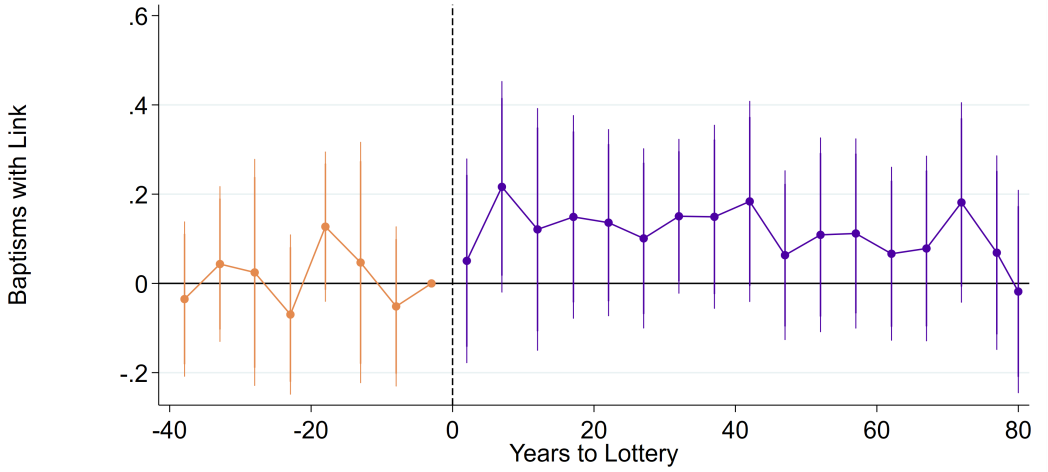
(b) (log) Baptisms

Notes: these graphs replicate the panel even studies of Figure 6 [non-Anglican churches] and 7 [baptisms] using two-way clustering at the parish and lottery levels.

Figure C.5: No Detectable Change in Baptism/Burial Links



(a) Anglican Baptisms



(b) Non-Anglican Baptisms

Notes: event study graphs of equation (2) where the outcome is the fraction of baptisms in a parish match the name of a later burial in the parish. Panel (a) covers Anglican baptisms, (b) covers non-Anglicans. Parish-year cells without relevant baptisms are excluded from the sample.

Table C.2: Cleric Residence and Seniority (Visitation)

	Respondent is Curate (0/1)		Respondent is Resident (0/1)		Residing Curate Licensed (0/1)	
	(1)	(2)	(3)	(4)	(5)	(6)
	≤10 years	≤20 years	≤10 years	≤20 years	≤10 years	≤20 years
Win	-0.014	-0.020	-0.030	-0.0022	-0.018	0.037
	(0.081)	(0.062)	(0.087)	(0.060)	(0.091)	(0.091)
N	918	1,681	952	1,764	371	658
SEs / Clusters	Lottery	Lottery	Lottery	Lottery	Lottery	Lottery
N (clusters)	42	71	43	71	35	58
Lottery FEs	Y	Y	Y	Y	Y	Y
Source×Year FEs	Y	Y	Y	Y	Y	Y
Pre-lot Controls	Y	Y	Y	Y	Y	Y
$\mathbb{E}[y\|\text{Win}=0]$	.84	.8	.19	.21	.77	.78

Notes: this table reports estimates of cross-section equation (3) on characteristics of Anglican clergy according to our visitation returns data. Odd columns include all lotteries up to ten years prior to the outcome date and even columns include all data up to twenty years before. The outcomes analyzed are, respectively, a binary variable for whether the respondent is a curate; whether the respondent identifies themself as a resident of the parish; and whether a locally resident curate has proper licensing (where applicable). Source×year fixed effects are dummies for each volume of returns interacted with the reporting year. Pre-lottery controls are listed in Appendix Section B.1.

Table C.3: Differential Reporting, Visitation Returns

	(1) Length [Visitation +10]	(2) Length [Visitation +20]	(3) (log) Population [Visitation +10]	(4) (log) Population [Visitation +20]	(5) (log) Population [Census 1811]
Win	-5.13	0.97	0.013	-0.070	-0.018
	(16.2)	(11.8)	(0.18)	(0.14)	(0.031)
N	1,017	1,857	952	1,733	36,020
SEs / Clusters	Lottery	Lottery	Lottery	Lottery	Lottery
N (clusters)	43	71	46	71	88
Lottery FEs	Y	Y	Y	Y	Y
Diocese FEs	Y	Y	Y	Y	Y
$\mathbb{E}[y\|\text{Win}=0]$	284	270	3.8	3.7	5.8

Notes: this table reports estimates of cross-section equation (3) on characteristics of the visitation returns responses for either the following 10 or 20 years following the lottery and census population in 1811. The outcomes analyzed are the length of the response and the log reported population. Diocese fixed effects are included as sourcebook fixed effects for visitation and administrative groups for the census. Pre-lottery controls are listed in Appendix Section B.1.

D Anglican Clergy and the Suppression of Dissent

As the leaders of the official church within their communities, Anglican ministers in the 18th and early 19th centuries often wielded tremendous local influence. While one obvious source sprung from their role as religious teachers, they held considerable social and institutional power as well. This appendix documents contemporaneous examples of Anglican clergy using such power to deter dissenting religious beliefs. The instruments they applied to this end – legal, social, and economic – all relied on the Anglican incumbent's standing within their community's networks of authority, which in turn was a function of the Anglican Church's local resources and manpower. The examples provided are meant merely to illustrate the types of methods Anglican ministers had at their disposal to deter dissent, and by no means represent the universe of such cases; most of those described survive only through the accident of a victim's journal or other such fragmentary archival sources. The Protestant Society for the Protection of Religious Liberty, which collected reports of such incidents, was only founded in 1811, and only summaries of its earliest annual meetings survive; nonetheless, reported caseloads in its documents suggest dissenter persecution was a relatively common occurrence at the time, with dozens and sometimes over a hundred cases reported annually.[16]

[16] *The Protestant Society for the Protection of Religious Liberty* (1915), pg. 372-373. Not every case reported to the Society involved clergy, but clergy figure prominently among the instigators in the itemized examples provided in the annual summaries.

One instrument used by Anglican ministers to suppress dissenters was the law. The voluntary offices which administered local government in England – in particular, the local magistrate positions that underpinned its legal system – were very often filled by the local Anglican clergy themselves. While dissenting worship was not illegal – the 1689 Toleration Act allowed dissenters to gather in licensed places of worship – there were still many avenues available to legally suppress such beliefs, and where the local Anglican minister was himself the magistrate, accuser and enforcer could be one and the same. For example, in 1816 the Reverend Augustus Peyton, serving concurrently as the local magistrate and Anglican minister, convicted the Methodist preacher Robert Newstead of preaching "in a field which had not been licensed," imposing a fine of £30 or three months' imprisonment (*Religious Liberty under the Regency* (1922), pg. 102-104). Ecclesiastical courts – a separate legal track – offered another avenue for suppression, most famously employed by the minister of St James Clerkenwell against the Countess of Huntingdon's unconsecrated chapel in the area in a series of cases running from 1777 to 1784. Licenses already granted could also be attacked: when a Baptist group at Blisworth had a house licensed for worship in 1784, the local Anglican minister summoned the dissenters, threatened to "leave no stone unturned" to destroy them, and eventually succeeded in having their license revoked (Dyster 2007).

Another method of keeping dissenters out was to threaten or bully those who might have supplied them with premises or livelihoods. When local farmer William Prichard provided a building for a dissenting school in the Welsh parish of Llannor in 1741, John Owen, the local Anglican minister, accused Prichard of making defamatory remarks about one of his sermons and brought him before an ecclesiastical court; the litigation lasted three years, and though Prichard was eventually acquitted, the situation drove him to sell his farm and abandon the area (Jenkins n.d.. At Bury, Lancashire, Methodist attempts in the 1770s to build a local church were blocked: the local Anglican cleric had an agent occupy the original chosen site, and when another site was found the Methodists found themselves unable to purchase building materials, as local suppliers feared association (Barr 1916, pg. 83).

Perhaps the crudest method at the disposal of the local cleric was to drum up a riot. At Colne, Lancashire, in 1748, George White – the local Anglican incumbent – issued the following mock military proclamation bribing would-be rioters with alcohol: "Notice is hereby given that, if any man be mindful to enlist in his Majesty's service, under the command of the Reverend Mr. George White, Commander in Chief . . . for the defense of the Church of England . . . each man shall have a pint of ale for advance" (Barr 1916, pg. 163). The resulting mob proceeded to break up a Methodist service and attempt to drive the attendees from the parish (Hempton 1988, pg. 99). Similarly, the opening service of a new Presbyterian meeting house at Petersfield, Hampshire, in 1772 was disrupted by a hostile demonstration – likely fomented by the local Anglican cleric – that nearly burned the building down (Johnson 2013, pg. 210-212). At Great Bardfield, Essex,

in 1793 a clergyman was among the instigators of a riot against the local Methodist chapel, and subsequently stood surety for the convicted offenders at court (Simon 1918, pg. 106-107). The Wednesbury riots – one of the most famous instances of anti-dissenter violence, occurring 1743-1744 – notably were blamed by Methodist leader Charles Wesley on the provocations of the local Anglican minister (Barr 1916, pg. 163).

Anglican clergy also targeted dissenting preachers specifically. Crowds acting with clerical sanction beat the Methodist preacher William Darney at Almondbury, Yorkshire in 1770 (Barr 1916, pg. 82), dragged a licensed Baptist preacher towards the stocks at Sawley, Derbyshire in 1766 (Wood 1847, pg. 169), and broke up Wesley's preaching at Taunton, Somerset in 1765 with a huntsman and hounds (Barr 1916, pg. 20). The law was again used as a weapon to this end: the Methodist minister Jeremiah Gilbert was repeated jailed in 1819-1820 at the instigation of the Anglican minister of Eckington, Derbyshire (Campey 1901, pg. 413), and in 1825 two Baptist preachers were sent to the penal treadmill for soliciting donations toward a chapel (*The Protestant Society for the Protection of Religious Liberty* 1915, pg. 369). In other cases dissenting ministers were removed via forced military conscription. In Madron, Cornwall between 1744 and 1746, the Reverend Walter Borlase, serving as both the local Anglican minister and senior magistrate, issued warrants under which Methodist preachers were seized for military service (Wesley 1903, pg. 474–475). At Birstall, Yorkshire in 1744 the dissenting preacher John Nelson was forced into military service, again at the urging of the local Anglican incumbent (Barr 1916, pg. 138-140).

Finally, Anglican clergy could use their power to make life more difficult for dissenters in general, most notably through the poor law system. While the outlay of poor relief was not technically a church function by this period, clergy very often served as the effective local administrators of the poor law – directing churchwardens and overseers, or sitting as magistrates who supervised them – and were known to use this economic lever to their advantage. For example, in 1742 the ministers of several Wiltshire parishes forbade their overseers to grant relief to those who attended Methodist meetings, "notwithstanding some of them were very poor and had large families to support" (Barr 1916, pg. 11). This practice continued into the next century, with a Protestant Society report in describing three separate instances in 1827 alone of parishes withholding relief from dissenters (*The Protestant Society for the Protection of Religious Liberty* 1915, pg. 370). The same report complained of clerical magistrates refusing charitable assistance to families who attended dissenter services or sent their children to dissenter Sunday schools, and also noted a clergyman near Coventry who evicted tenant farmers for attending a dissenter gathering (*The Protestant Society for the Protection of Religious Liberty* 1915, pg. 368-374).

Taken as a whole, these examples demonstrate that Anglican ministers were well able to apply social and institutional pressure to deter competition from dissenting religious sect, and likely did so not infrequently.