

When the Dust Settles: The Consequences of Scandals for Organizational Competition

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Forthcoming, *AMJ*.

Acknowledgements: We would like to thank Scott Graffin and three anonymous reviewers for their thoughtful comments on earlier versions of this manuscript. We are also grateful for feedback from Mitali Banerjee, Rodolphe Durand, Ivana Katic, Fabrizio Perretti, Thomas Roulet, and Dan Wang. Earlier versions of this paper were presented at the 2014 AOM Annual Meeting in Vancouver, Canada and the 2016 EGOS Colloquium in Naples, Italy. The first author acknowledges generous support from Bocconi University and Columbia Business School. Errors remain our own.

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ABSTRACT

Recent works have documented the dark side of scandals, revealing how they spread, contaminate associated organizations, and taint the perception of entire fields. We complement this line of work by exploring how scandals durably affect competition within a field, translating into relative advantages for certain organizations over others. First, scandals may benefit organizations that provide a close substitute to the offerings of the implicated organization. Second, scandals pave the way for moralizing discourses and practices, shake taken-for-granted assumptions about the conduct of organizations, and result in a shift in the criteria used to evaluate organizations within the field. Our arguments suggest that organizations whose offerings are most similar to those of the implicated organization, yet perceived as enforcing stricter standards of conduct, are likely to benefit the most from a scandal. We find support for these arguments in a county-level study of membership in the Catholic Church and sixteen other Christian denominations in the United States in the wake of a series of sex abuse cases perpetrated by Catholic clergy between 1971 and 2000. This study contributes to our understanding of the competitive effects of scandals on organizations, and carries important implications for the management of organizations in scandal-stricken fields.

Keywords: Catholic Church, competition, scandals

Scandals—broadly defined as publicized transgressions that run counter to established norms—are ubiquitous phenomena across social worlds, and their unfolding is often momentous enough to become engraved in the collective imaginary in indelible ways (Adut, 2005). Organized life is certainly no exception. Media outlets are in fact rife with news of scandals that are centered on organizations, from corporations such as Enron and WorldCom (Jensen, 2006) to political entities such as the British Parliament (Graffin, Bundy, Porac, Wade, & Quinn, 2013), and religious organizations like the Catholic Church (Keenan, 2011). The nascent organizational literature on scandals largely highlights the latter's negative effects on organizations. More specifically, a growing body of literature posits a contamination effect, whereby the evaluation of the individuals, organizations, and institutions directly or indirectly involved in scandals is tarnished (Graffin et al., 2013; Sullivan, Haunschild, & Page, 2007; Wiesenfeld, Wurthmann, & Hambrick, 2008). Past studies

reveal how the fallout of scandals undermines the evaluation of perceived violators and spreads to bystander actors—i.e., belonging to the same category—for instance through association (Pontikes, Negro, & Rao, 2010; Vergne, 2012), or labeling and stereotyping (Jonsson, Greve, & Fujiwara-Greve, 2009; Paruchuri & Misangyi, 2015). Viewed in this light, turmoil diffuses through the social space, progressively contaminating those actors that are involved in the scandal (or perceived as such) and tainting the image of bystanders, particularly affecting visible, high-status members (Adut, 2005; Graffin et al., 2013). Eventually, large scandals tarnish entire industry categories in the eyes of external evaluators (e.g., investors), harming the “intangible commons” of all their members (Barnett & King, 2008; Paruchuri & Misangyi, 2015).

With only a narrow set of cases and outcomes studied so far, the literature provides critical yet limited insights into the competitive consequences of scandals *within* a field. Jonsson et al. (2009: 221), for instance, show how two scandals involving Skandia AB, a Swedish insurance firm, led individual investors to withdraw from transactions with similar organizations during “periods of high media attention”. In a recent study of corporate misconduct—an antecedent of scandal—Paruchuri & Misangyi (2015) reveal how market investors variously discounted bystander organizations operating in the same industry category during the days following the disclosure of an event. With their focus on short-term reactions to scandals, these studies do not say much, however, about long-term effects—that is, when the dust raised by a scandal settles. These cases have also limited generalizability in that the market investors being studied have low switching costs and many available investment options. Other key organizational audiences with stronger ties to the implicated organization, such as members and—in some instances—clients, might have more a limited set of alternatives, mediating how they react to scandals.

In this paper, we study how scandals durably affect the competition among organizations within a scandal-stricken field. Although scandals may have an overall negative effect on a field, the

magnitude of the effect may vary across firms, leading some organizations to enjoy a *relative* advantage—either a benefit or a smaller penalty—over others. We complement prior works by advancing three key arguments. First, we argue that the magnitude of the penalties suffered by the scandal-tarnished organization will be proportional to the publicity that the scandal receives, as misconduct alone is of no consequence for organizations unless it is widely made public (Adut, 2005, 2008). Second, we highlight a substitution effect likely to improve the relative competitive positioning of organizations that provide close alternatives to the offerings of the implicated organizations. For all the turmoil it triggered in the audit industry (e.g., generalized public suspicion, tainted public image of the profession, stricter regulation and control), for instance, the Enron scandal benefited the service firms that managed to attract former Arthur Andersen clients (Jensen, 2006). Substitution is likely to occur when a scandal does not fully undermine the legitimacy of an offering (e.g., auditing is still a legitimate activity), and is particularly pronounced when only limited alternatives are available (e.g., the “Big Four” accounting firms). Third, we build on the idea that scandals are critical events (Hudson, 2008; Sewell, 1996) that deeply transform the social structure of a field and have long-lasting consequences on the criteria audiences use to evaluate organizations (Lamont, 2012; Zuckerman, 2012). Following a scandal, we argue, organizations perceived as enforcing tighter norms of conduct, are likely to be evaluated more positively and gain a relative advantage over their competitors. In combination, these arguments suggest that scandals durably reshuffle competitive positions within a field in favor of organizations able to provide a close substitute to the offerings of the implicated organization(s), while being perceived as enforcing stricter norms of conduct.

We explore the above ideas by examining the impact of scandals on organizational membership. Members are “individuals who, in return for a variety of inducements, make contributions to the organization” (Scott, 1981: 16). Because they are critical to the functioning and

survival of organizations, members represent a critical resource organizations overtly compete for (Hatch & Dyer, 2004). With respect to market investors, organizational members typically have a strong, often exclusive, tie to an organization that shapes their social identity (Tajfel & Turner, 1986), and mediates how they react to scandals. Unlike investors who may react quickly—and rather coldly—to a scandal, organizational members are likely to take time to reevaluate their membership and consider potential alternatives before committing to exit and possibly join another organization. These features make membership an ideal object of study to explore the lasting competitive consequences of scandals on organizations.

Empirically, we build on quantitative evidence on sex abuse scandals in the U.S. Catholic Church. Because we are interested in the effects of scandals on organizational competition within a field, we leverage a panel-type dataset that tracks the variation in membership for the Catholic Church and sixteen other Christian organizations across geographic locations and over time. In line with our theoretical arguments, we find evidence that scandal publicity has deleterious effects on the implicated organization. Further, we show that Christian denominations close to the Catholic Church in terms of faith and practice, yet perceived as enforcing stricter norms of conduct benefited the most from the scandals in terms of membership. By highlighting the previously overlooked competitive consequences of scandals, our work contributes to the nascent literature on scandals in organizational fields, and more generally, to the scholarly understanding of competition between organizations. Our study also has implications for the management of organizations operating in scandal-stricken fields.

SCANDALS IN ORGANIZATIONAL FIELDS

A scandal can broadly be defined as a publicized instance of transgression which runs counter to social norms, typically resulting in condemnation and discredit (Adut, 2005). In his detailed treatment of the subject, Adut (2008) characterizes scandals as moral phenomena which

originate from a transgression, either real or alleged. Transgressions can be acts of wrongdoing (Palmer, 2012), instances of gross incompetence, or even the mere release of information about past actions that are inconsistent with a social actor's public image. Although necessary, a transgression may not however be sufficient to trigger a scandal. Publicity is another requirement, in that it allows information about the scandal to be broadcast (Thompson, 2000). Even when the transgression is already widely known, publicity acts as a focusing device that makes it difficult to ignore. In modern societies, the media serve a particularly prominent function in ensuring publicity (Gamson, Croteau, Hoynes, & Sasson, 1992; Jonsson & Buhr, 2010). Their role, however, is not simply accessory, but often a necessary requirement for a scandal to occur: in fact, by covering the scandal they also enable it; moreover, the choice of topics and the tenor of coverage can also amplify or dampen the magnitude of a scandal.

A defining feature of scandals is their ability to contaminate select others, with particular reference to “those associated personally, institutionally or even categorically with the suspect” (Adut, 2008 : 24). Extant research on scandals emphasize the dynamics of *contamination* (Hudson & Okhuysen, 2009; Pontikes et al., 2010), i.e. the idea that social actors can be affected (Devers, Dewett, Mishina, & Belsito, 2009) by mere association with a tainted alter (Pontikes et al., 2010). Such instances of contamination (Adut, 2008)—typically known as stigma by association in psychology (Pryor, Reeder, & Monroe, 2012) and in the organizational literature (Hudson & Okhuysen, 2009)—have the capacity to substantially amplify the pervasiveness of scandals. A tie between social actors need not exist, however, in order for contamination to occur: in their study of the scandal surrounding a Swedish insurance firm, Jonsson et al. (2009) in fact found that other insurance firms were cognitively categorized as similar—and thus penalized—because they share a common set of features with the focal firm. A key mechanism for contamination through

categorization or association is generalization of culpability (Paruchuri & Misangyi, 2015): actors perceived as being similar, and associates, become themselves suspects.

Although the study of scandals is predominantly rooted in sociology, the organizational literature is rife with examples of scholarship addressing the topic (see Greve et al., 2010 for a review). Scholars in this tradition, however, have mostly framed the problem in terms of organizational wrongdoing, with the aim of explaining how it originates (Mishina, Dykes, Block, & Pollock, 2010; Palmer, 2012; Palmer & Yenkey, in-press), how it might spread (Pierce & Snyder, 2008), and what organizations can do to regain their social standing in its wake (Pfarrer, Decelles, Smith, & Taylor, 2008). However, while the organizational wrongdoing perspective has been instrumental in enhancing our understanding of how transgressions are linked to the emergence and diffusion of organizational disapproval, it is also limited in three major ways.

First, most extant research has studied unethical acts without properly appreciating the boundary conditions that make them relevant. In fact, if transgressions went unreported, they would most likely be unproblematic; indeed, many transgressors are never caught (Margolis & Walsh, 2003), or if caught much later (as in our empirical setting). And even when a transgression is publicized it might still not result in sanctions, unless it runs counter to the expectation of a negatively-oriented audience (Adut, 2008). In short, any act of misconduct is not *per se* conducive to negative outcomes in the absence of the appropriate context (Desai, 2011).

Second, and as previously noted, the nascent literature on the topic has only examined a narrow range of outcomes, which a focus on short-term market investors' reactions; examples include cumulative abnormal returns (CAR) in the short-time window following a scandal (Paruchuri & Misangyi, 2015), and individual investors' transactions during periods of high media exposure (Jonsson et al., 2009). Little attention has been devoted to understanding the effect of scandals on other audiences, more specifically how scandals might affect the core constituents of organizations,

i.e. their members. Yet, there is good reason to believe that the occurrence of scandals might have a bearing on the capacity of organizations to retain their existing members and attract new ones; in turn, these factors frequently represent key drivers of organizational performance and survival (Hatch & Dyer, 2004).

Third, and critically, the literature on organizational misconduct has predominantly been concerned with the consequences for implicated organizations, overlooking the broader competitive consequences of publicized transgressions within a field. Even though a handful of studies have now begun exploring this topic (Jonsson et al., 2009; Paruchuri & Misangyi, 2015), they are largely focused on the negative spillovers of transgression, without investigating the possibility that disapproval affecting some organizations might translate into relative advantages for other actors (e.g. Helms & Patterson, 2014). In what follows, we introduce our empirical setting and explain how it allows us to address the above mentioned limitations of the organizational literature on scandals.

EMPIRICAL SETTING

Aside from the general scholarly reluctance to tackle topics that might be seen as contentious, controversial or “taboo” (Hudson & Okhuysen, 2014), there are several methodological barriers to the quantitative study of scandals. First of all, scandals tend to be both idiosyncratic and heterogeneous, which has made the design of a quantitative, regression-based study problematic up to this point. Other issues stem from data availability, in that it is inherently challenging to collect information about multiple transgressions as well as the amount of publicity they received, so as to obtain a large enough sample. In all, this state of affairs has resulted in scandals being used more as exogenous shocks—useful for investigating other phenomena—than as objects of inquiry in their own right. This is for instance the case of Graffin et al.'s (2013) study of the British MP expense scandal and Jonsson et al.'s (2009) article on the scandal surrounding the Swedish firm Skandia,

among others. Yet, most of the above problems can be overcome through a careful choice of empirical setting; we discuss the main features of ours below.

Christian denominations in the United States. Christianity was introduced to the Americas in the 16th and 17th centuries, and it still constitutes the most popular religion in the United States: in a 2012 survey, approximately 73 percent of polled Americans identified themselves as Christian. While in most European countries Christians generally belong to a single denomination, religion in the United States is highly fragmented, with over 200 established denominations and countless independent churches. Although Protestant denominations collectively accounted for a majority of U.S. Christians, Roman Catholicism was the largest individual denomination at 23.9 percent and with almost 70 million members in 2012. On the other hand, the second largest denomination—the Southern Baptist Convention, a Protestant group—only had about 16 million, while the total number of Christians in the country is estimated to be in excess of 220 million. This suggests that the religious landscape is highly fragmented.

Because of their nationwide character, Christian churches cannot be readily assimilated to single-unit organizations. Rather, major denominations more closely resemble multi-unit organizations with individual congregations acting as local “branches” (Goldstein & Haveman, 2013). For churches, membership is typically one of the most valuable organizational resources: because they are almost exclusively reliant on the time, effort and financial support of their members, it is mainly their involvement that allows religious organizations to function and grow (Iannaccone, Olson, & Stark, 1995). Viewed from this perspective, church growth is essentially a resource mobilization problem (Goldstein & Haveman, 2013; McCarthy & Zald, 1977).

Churches in the United States are also rather fluid organizations. Data from the U.S. Religious Landscape Survey, conducted in 2007, suggests that 28 percent of American adults have transitioned from one faith to another at least once; among Protestants, this number raises to 44

percent (Pew Forum, 2007). And while denominations might exhibit steady number of adherents over years or even decades, church membership typically experiences some degree of churn: members move away, shift to another denomination, or become unaffiliated, while new converts join the church. In all, church growth can thus be seen as being driven by a denomination's combined capability to retain its members and attract new ones.

Finally, while Christian denominations in the United States differ in a wide variety of ways, social scientists have traditionally classified them based on the so-called *church-sect typology* (Johnson, 1963; Troeltsch, 1950). The theory holds that religious denominations can be placed on a strictness continuum ranging from church-like to sect-like (Barros & Garoupa, 2002; Iannaccone, 1988). Church-like denominations' members exhibit distributions of religious beliefs and practices that are comparable to those of the population as a whole—they are, therefore, less distinctive. On the other hand, sect-like denominations exhibit beliefs and practices associated with an established religious tradition, but with more onerous requirements concerning group membership as well as greater involvement and participation from members (Iannaccone, 1994). Table 1 presents a selection of Christian denominations that are active in the United States, ranked from the least strict to the strictest.

----- Insert Table 1 about here -----

Sexual abuse in the U.S. Catholic Church. Among Christian denominations, the Catholic Church has a well-documented history of sexual abuse around the world (Keenan, 2011), with cases in the United States having gained particular prominence in recent years. Most allegations involved misconduct by members of the clergy, often perpetrated on minors, and historically these mostly received coverage from small local newspapers. It was not until 2002 that the Boston Globe

coverage of a series of criminal prosecutions of five priests thrust the issue of sexual abuse of minors by Catholic clergy into the national debate, which resulted in further allegations, lawsuits, and criminal cases. To date, over 3,000 civil lawsuits have now been filed against the Catholic Church in the United States alone, with some of them resulting in substantial monetary settlements. In response to the scandals, the United States Conference of Catholic Bishops commissioned the John Jay College of Criminal Justice in New York to conduct a survey-based study of individual dioceses. The resulting John Jay Report, published in 2004, indicated that 4,392 priests in the United States had been accused and approximately 11,000 distinct allegations had been made. Otherwise stated, about 4 percent of the priests who had served during the period covered by the survey (1950–2002) were the target of allegations, and over 90 percent of Catholic dioceses in the country had at least one case.

While the overall number of clergy members against whom allegations were made is sizeable, most of them have actually been publicly accused in the past two decades. Hungerman (2013) shows that most clergymen were accused in 2002 or later: indeed, less than 150 accusations per year were made between 1988 and 2001, while their number spiked to almost 1,000 in 2002. The number of newspaper articles covering sex abuse cases follows a similar pattern: less than 50 articles were published on sex abuse cases in the 1970s, rising to a few hundred in the 1980s and almost 2,000 in the 1990s; yet in the following decade—and largely because of the phenomenon having achieved nationwide prominence—over 18,000 articles were published. These trends suggest that there was a clear discontinuity in how myriad local scandals that developed independently over decades coalesced into a nationwide one after 2002, resulting in a surge of allegations, media coverage, and legal proceedings. Figure 1 and 2 depict the temporal trends of both instances of misconduct and scandal publicity.

----- Insert Figures 1 and 2 about here -----

This feature of our empirical setting is particularly attractive for the purposes of our analysis, in that it provides us with a well-defined time window in which sex abuse scandals unfolded at the local community level and independently of each other, as geographically dispersed allegations against Catholic clergymen had yet to be perceived as a single, nationwide scandal. Another attractive empirical feature of this setting is the pervasiveness of the phenomenon being analyzed: data from the Pew Research Center suggests that the number of adherents that have left the Catholic Church is quite substantial, and that approximately one in ten Americans is a former Catholic (Pew Forum, 2009). While only some of them might have left because of scandals—21% nationwide according to the Pew data and about two million according to Hungerman (2013)—further evidence suggests that this number might be much higher in areas with a substantial, documented history of sexual abuse (Merica, 2012). In all, for the Catholic Church the loss of adherents that resulted from sex abuse scandals can be configured as a “hidden exodus” that substantially affected local congregations on a national—if not global—scale.

Given the magnitude of the phenomenon, understanding what happens to former Catholics is of particular interest. In this regard, data from the Religious Landscape Survey conducted in 2007 by the Pew Research Center suggests that among people who reported leaving the Catholic Church for any reason approximately half (46 percent) became unaffiliated, while 19 percent joined mainline denominations and 29 percent joined other Christian denominations, while the remaining 6 percent chose a non-Christian denomination (Pew Forum, 2009). These trends reveal that while the “unaffiliated” category captures a substantial share of former Catholics, suggesting disillusionment with organized religion as the main reason behind the decision to leave, about as many opt to join a different Christian denomination instead. While this might be due to a variety of factors, ranging from getting married to someone of a different religion to dissatisfaction with doctrinal teachings, it

stands to reason that scandals could have led some Catholics to leave the Church and join other denominations instead, while affecting Christian denominations' recruitment in general.

COMPETITIVE CONSEQUENCES OF SCANDALS

Scandal-driven substitution. Prior works highlight a number of highly tangible—typically undesirable—consequences of scandals, ranging from bad press (Zavyalova, Pfarrer, Reger, & Shapiro, 2012), the disengagement of key constituencies (Graffin et al., 2013), and the severance of network ties (Jensen, 2006), to decreases in key performance indicators such as sales (Rhee & Haunschild, 2006). Applied to organizational membership, this line of argument suggests that an organization implicated in a scandal may suffer losses in this area as well. While members are tied to their organization, and thus less mobile than external constituents such as market investors, they are likely to react to a scandal. Individuals classify themselves and others into social groups, with a tendency to feel psychologically intertwined with the fate of the group and to partake vicariously of its accomplishments (Tajfel & Turner, 1986). When a social group—in our case, a scandal-stricken organization—is evaluated negatively, individuals that identify with it will experience dissonance due to their positive perception of the social group being challenged (Branscombe, Ellemers, Spears, & Doosje, 1999). When deviant acts occur within organization and are made public, therefore, the discrepancy between the moral beliefs held by its members (Hitlin & Vaisey, 2013) and the negative character of the publicized transgressions makes identification problematic (Dutton, Dukerich, & Harquail, 1994; Elsbach & Kramer, 1996; Ethier & Deaux, 1994), threatening the identity of members (Ellemers, Spears, & Doosje, 2002) and resulting in various reactions, including increased turnover (O'Reilly & Chatman, 1986). In turn, when an individual decides to leave, embeddedness and social contagion dynamics might eventually result in entire groups of members leaving (Bartunek, Huang, & Walsh, 2008; Felps et al., 2009).

Without publicity, however, misconduct may not significantly alter organizational membership. As Adut (2005: 215) points out, a scandal is only such—and thus exerts its effects—”when transgressions are publicized, as opposed to when they are simply known.” In the context of sex abuse scandals, transgressions are particularly likely to go unreported because victims are likely to feel ashamed and fear ostracism. As Figure 2 illustrates, many of the allegations that were made against members of the clergy only surfaced years after the associated cases of abuse took place, often decades later, when the media took interest in them. Publicity then turns misconduct—real or alleged—into scandals by unleashing externalities on members, making even oblivious individuals aware of what is going on in their organization (Adut, 2005). The amount of publicity transgressions receive, rather than the violations themselves, is critical because it brings the transgression to the forefront of the debate (Hilgartner & Bosk, 1988), both within the organization and in the broader community, making it “costly for those who would otherwise ignore the transgression to do so” (Adut, 2005: 218). As publicity accumulates, a dark shadow is cast on the organization at large, making members’ social identity problematic due to the implications of being associated with a negatively perceived entity (Pontikes et al., 2010; Pryor et al., 2012) and creating pressure to defect (Piazza & Perretti, 2015). Because membership in the Church is an integral part to a Catholic’s social identity, and because individuals strive to maintain a positive perception of the groups they belong to, they are likely to experience a sense of threat if these positive perceptions are challenged (Dutton et al., 1994). In turn, identity threat induces dissonance (Festinger, 1957) and creates a state of tension that organizational members try to resolve; in this context, leaving the organization can be seen as an attempt to reduce threat by “cutting off reflected failure” (Snyder, Lassegard, & Ford, 1986). Publicity of misconduct further affects recruitment, as potential new members reluctant to be associated with a “tainted” organization may be discouraged from joining the Church. We thus expect Catholic membership to decrease in the wake of a scandal.

Hypothesis 1. *There is a negative relationship between scandal publicity and Catholic membership at the local level.*

Whereas the negative effects of scandals have been documented, much less is known about the opportunities scandals may offer competitors. For instance, by undermining the credibility of Arthur Andersen's audit practice and eventually causing its demise, the Enron scandal led client firms to look for alternative service providers, eventually benefitting the remaining major audit companies (Jensen, 2008). To the extent that the offerings of the suspect organization are not fully delegitimized, constituents defecting the tarnished organization will search for alternatives, and the most similar offerings are the most likely to be picked, benefiting competitors offering the closest alternatives. Such substitution effect is likely to unfold in many—if not all—scandals involving organizations that operate in competitive environments.

Although the two mechanisms may occur concurrently, scandal-driven substitution and contamination are conceptually distinct—yielding opposing effects. As previously discussed, the contamination argument posits that external audiences generalize a presumption of culpability to the organization belonging to the same category of or associated with the implicated actor (Paruchuri & Misangyi, 2015), altering how such organizations are perceived and evaluated. The substitution argument is a more functional in nature and also more restricted: it only concerns the actors directly dependent on the implicated organization—or primary stakeholders (Clarkson, 1995)—such as clients or members. Both mechanisms are likely to affect competitive positions in the field when a scandal breaks: organizations belonging to the same category or closely associated with the implicated organization may suffer from contamination; organizations with similar offerings may enjoy substitution benefits.

Applied to organizational membership, scandal-driven substitution materializes when the implicated organizations lose some of their members, and competitor organizations attract new members, fostering interorganizational mobility. Similarity is key to substitution: similar organizations may benefit the most from this phenomenon because they provide members with an organizational environment that is closest to that of the focal organization; they are also more likely to provide estranged members of the scandal-stricken organization with an opportunity to substantially reaffirm their identity while distancing themselves from the damaged entity (Hudson & Okhuysen, 2009). In the context of this study, Protestant denominations are unlikely to suffer from contamination because they belong to a conceptually distinct category within Christianity. Protestantism historically developed in opposition to Catholicism, starting with the 16th century Reformation movement. Despite all their differences, the various Protestant denominations share a common rejection of core features of Catholicism. In other words, there is an ancient, well-established, and strong categorical boundary that shields Protestant denominations from the fallouts of the sex-abuse scandals implicating the Catholic clergy—likely strong enough to prevent contamination in the eyes of the relevant audiences. As a result, we expect non-Catholic denominations to benefit from substitution and gain members as a result of the scandal. We also predict the extent of this effect to be proportional to their similarity to the Catholic Church. By joining a church characterized by similar levels of participation, involvement, and belief strength—yet categorically distinct—defecting Catholics may enjoy a comparable religious experience and repair their own identity (Elsbach & Kramer, 1996), while still dissociating themselves from the scandal. At the same time, potential new members who might have considered joining the Catholic Church—had it not been for scandals—are also likely to opt for denominations whose standing within the field is less compromised (Cialdini et al., 1976). Hence:

Hypothesis 2a. *There is a positive relationship between scandal publicity and non-Catholic membership at the local level.*

Hypothesis 2b. *The positive relationship between scandal publicity and non-Catholic membership at the local level is lower the more dissimilar the denomination is to the Catholic Church.*

Scandal-driven evaluative shifts. Scandals can have long-lasting transformative consequences that go far beyond the circle of directly affected social actors. For instance, the Enron and Worldcom scandals shook the confidence of U.S. investors for years to come, eventually resulting in much tighter regulation of financial reporting for all companies by means of the Sarbanes-Oxley Act of 2002. And more recently, the 2015 Volkswagen's emission test scandal cast a shadow of doubt and suspicion on the practices of the entire global auto industry. As Adut (2008: 35) notes, scandals are "historical events transforming social structures." As critical events (Sewell, 1996), they create a discontinuity in the way social actors are evaluated (Zuckerman, 2012), thereby affecting the social perception of all the organizations in the field (Hudson, 2008). While evaluation criteria may gradually shift, for instance as the result of collective mobilization (Weber, Heinze, & DeSoucey, 2008) or progressive changes in institutional logics (Dunn & Jones, 2010), extant literature supports the idea that they can occasionally be triggered by exogenous shocks that suddenly and durably affect the evaluations underlying cultural schemas (Sewell, 1996).

We further this line of argument to suggest that scandals trigger shifts in the criteria used to evaluate organizations by increasing the amount of attention given to organizational misconduct. A central feature of scandals is that previously committed acts, potentially known by a limited set of actors, become publically denounced for being unacceptable (Adut, 2008). When a scandal breaks out, deviance is made known and denounced, societal structures are reaffirmed and reinforced, while moral boundaries are redrawn (Goode & Ben-Yehuda, 1994). The media and other interested parties

(e.g., politicians, activists) pass on judgments about what is good and what is wrong, engaging in moralizing—i.e. the “practice of offering moral lessons” (Hopkins, 2015: 11-12). As a consequence of the scandal, the taken-for-granted assumption that organizations in the field behave in an acceptable and appropriate manner becomes a topic for public debate; in the extreme case, the assumption is reversed, such that all organizations are regarded as potential perpetrators and asked to provide evidence of appropriate conduct. In the aftermath of the 2015 Volkswagen’s emission scandal, for instance, investors group turned to all major carmakers, asking them to disclose more detail about their lobbying practices (FT, 2015)¹. As misconduct becomes a central focus of attention, the enforcement of strict norms of conduct is likely to be regarded as a desirable organizational attribute such that organizations that are perceived as enforcing stronger norms are likely to benefit the most, or suffer the least, from a scandal—thus gaining a relative advantage.

Applying this line of argument to our case, we contend that, in the wake of scandals affecting the Catholic Church, stricter non-Catholic denominations will gain more members than less strict ones. Strict denominations may in fact appeal to estranged Catholics that left the Church following a scandal for several reasons. Strictness results in a tighter enforcement of norms and a low tolerance for deviance and dissent, with the implicit promise of higher standards of organizational conduct and a lower risk of malfeasance (Coleman, 1988; Granovetter, 1985). To estranged Catholics looking for a new church, strict denominations may thus appear relatively safer than laxer alternatives. Further, strictness is typically associated with a general “attitude of ethical austerity” (Iannaccone, 1994: 1192) that may help estranged Catholics repair their self-image and identity, potentially damaged by the scandal (Grandey, Krannitz, & Slezak, 2013). Conversely, the more laissez-faire attitudes permitted by liberal mainline churches may be seen as a sign of looser

¹ <http://www.ft.com/intl/cms/s/0/16110904-70a1-11e5-9b9e-690fdae72044.html#axzz3ojyQATX3>

norm enforcement and organizational discipline. If this is the case, we would then expect stricter churches to benefit more as a result of Catholic scandals than less strict ones, *ceteris paribus*. Thus:

Hypothesis 3. *The positive relationship between scandal publicity and organizational membership at the local level is stronger for stricter non-Catholic denominations than for less strict denominations.*

DATA AND MEASURES

Dependent variable. Our dependent variable for this study is the logged number of adherents for the seventeen Christian denominations in our study, including the Catholic Church. We coded it based on information contained in the Religious Congregations and Membership Study, carried out in 2000, and the Churches and Church Membership in the United States study, carried out in 1971, 1980 and 1990. These datasets are maintained by the Association for Religion Data Archives (ARDA), and they include statistics for 149 religious bodies on the number of congregations within each county of the United States. We used the above data to construct county-level measures of church membership for each of the seventeen Christian denominations in our sample for 1971, 1980, 1990 and 2000. We chose to terminate our analysis in 2000 because sex abuse scandals in the U.S. Catholic Church rose to national prominence shortly after, first in 2002 with the Boston Globe coverage and then with the publication of the John Jay Report in 2004. Conversely, before 2002 sex abuse scandals were predominantly covered by local newspapers, a fact which allows us to examine the effects of scandals on churches at the county level while benefitting from a relatively clean setting.

Independent variables. For data on sex abuse we turn to *BishopAccountability.org*, a watchdog website where an accurate record of every Catholic sex abuse case in the United States is kept. Because our main independent variable must model the amount of publicity that transgressions by Catholic Church affiliates receive, for each time period we used this data to construct a measure

of *scandal publicity* for each diocese by means of a count of articles reporting cases of abuse within the diocese during the previous decade; diocese-level information was then matched with the counties each diocese comprises. This variable parsimoniously captures the magnitude of the scandal within a diocese, correlating highly with both the total number of victims ($\rho = 0.86$) and the total number of accused priests within the same diocese ($\rho = 0.80$), while simultaneously ensuring that transgressions are known to both organizational members and the local community.

To differentiate among denominations we took advantage of the strictness measure used by Iannaccone (1994: 1182), which was originally developed by Hoge (1979, see also Table A1 in the Appendix) as part of his multidimensional scale used to classify churches in the United States. In his study of whether church strictness results in increased membership and higher commitment from members, Iannaccone (1994: 1190) leveraged Hoge's (1979) distinctiveness item to place Christian denominations on a strictness continuum based on the degree to which they “emphasize maintaining a separate and distinctive life style or morality in personal and family life, in such areas as dress, diet, drinking, entertainment, uses of time, marriage, sex, child rearing” or whether they “affirm the current American mainline life style in these respects”. Table 1 reports the seventeen denominations included in our study with their strictness scores in the first column. With a score of 3.0, the Catholic Church is fairly close to the center of the 1-6 scale. It must also be noted that churches with similar traits also tend to score similarly. For instance, the mainline Protestant denominations (Episcopal, Presbyterian, United Church of Christ, Disciples of Christ, Unitarian, American Baptist, and Evangelical Lutheran) are at the same end of the spectrum; these churches are thus the least strict. At the other end of the spectrum we find sect-like churches—such as the Church of Jesus Christ of Latter-day Saints, Jehovah's Witnesses, Seventh-Day Adventists, and the Assemblies of God—that originated in America in the 19th and early 20th century due to dissatisfaction with mainline Christianity; these churches are also the strictest.

It must be noted, however, that churches with similar scores are not necessarily alike in terms of doctrine and rites. Rather, similarity here is to be interpreted as the extent to which two denominations offer analogous religious experiences with respect to the involvement they require of their members, thus providing them with a similarly strong sense of community, identification and moral expectations. For instance, the Episcopal Church overlaps substantially with the Catholic Church as far as beliefs are concerned, yet the two denominations offer remarkably different experiences in terms of the participation they demand, the extent to which they impose stringent behavioral norms on their members, and the openness to different belief systems. In this regard, the Catholic Church is akin to relatively more conservative denominations such as Southern Baptists.

In practical terms, churches with similar scores tend to have equivalent levels of church attendance, charitable giving, involvement in outside activities, integration with the broader society, and so forth. Despite their belief diversity, these churches thus offer their members religious experiences that are behaviorally comparable. Indeed, Iannaccone (1994: 1192-4) finds that churches with similar strictness scores also tend to attract similar adherents; for instance, the strictest denominations are found to have members that “are poorer and less educated, contribute more money and attend more services, hold stronger beliefs, belong to more church-related groups, and are less involved in secular organizations”; because of the above arguments, churches with similar scores “should therefore display fundamental behavioral similarities, despite the peculiarities of their individual histories, theologies, and organizational structures.”²

Since one of the goals of our study is to investigate the effects of both strictness and similarity between the Catholic Church and non-Catholic denominations, we use the strictness score to construct two additional independent variables: 1) the absolute value of the difference in

² To the best of our knowledge, this measure and its associated arguments are fairly well-established in the so-called “new sociology of religion” (Iannaccone et al., 1995; Warner, 1993)

strictness between the focal denomination and the Catholic Church, which we label *dissimilarity*; and 2) a dummy for whether the focal denomination is stricter than the Catholic Church, which we also label *strictness* for brevity. These variables—which are assumed to be stable and not to vary over time, and whose meaning is fairly intuitive—are reported in the second and third column of Table 1, respectively. For our study we used all of the eighteen denominations included in Iannaccone (1994) with the exception of Jehovah’s Witnesses, which had to be dropped due to insufficient availability of membership data in the ARDA archives, reducing the total number to seventeen. Table 1 also includes some basic statistics about these seventeen religious bodies, which we coded based on ARDA information. Taken together, these seventeen denominations had almost 120 million members in 2009 and represented almost half of all U.S. Christians; the remaining half is distributed across approximately 180 other established denominations as well as an unspecified number of unaffiliated, non-denominational congregations. In light of the available information, these denominations can thus be seen as fairly representative of organized Christianity in the United States.

Control variables. We also coded a number of control variables for use in our models. Because some socioeconomic characteristics might affect religious preferences and the likelihood of joining a particular church, we control for the percentage of *individuals of Hispanic descent* and *individuals of African descent* in each county, as well as for the percentage of *individuals below the poverty line*. Similarly, religious adherence is likely to be correlated with population shifts and patterns of increased secularization over time, so we coded the total number of *religious adherents* in the county, as well as the *county population*.³ Nationwide trends in the growth and decline of individual denominations might also confound our estimates; therefore, we also coded a variable for the

³ Because county population and the number of religious adherents were highly correlated, we orthogonalized them through a Gram-Schmidt procedure implemented by means of the *orthog* command in STATA. In models that include the number of Catholics as a control variable—such as those in Table 5—this latter variable was orthogonalized too.

number of *focal denomination adherents nationwide*.⁴ Finally, the choice of leaving a church and joining a different one might be affected by the availability of alternatives and their relative size. For instance, in counties with substantial religious concentration, estranged Catholic Church members might have a hard time finding a different church offering a religious experience similar to what they are used to; under these circumstances, we might expect a greater share of these alienated members to reluctantly remain in the church or become unaffiliated. We control for this effect through the inclusion of a *Herfindahl-Hirschman index* of religious concentration for each county in our regression models. Additionally, some models—notably those used for testing Hypotheses 2a, 2b, and 3—also include the *number of Catholics* in the county as a control variable. Finally, for each accused priest in the database, we used the BishopAccountability articles to code the decade in which the alleged misconduct took place, and we used this information to construct a measure for the *total number of Church-affiliated individuals accused of misconduct* within the diocese.

MODELS

----- Insert Table 2 about here -----

In the first part of the study, we model the number of adherents to the Catholic Church across counties and over time to assess the effect on scandal on the focal organization at the local level. Table 2 reports descriptive statistics and pairwise correlations for our variables.⁵ Because our data is an unbalanced panel, and because the number of Catholics at time t is strongly dependent on the number of Catholics at time $t-1$, our dataset is likely to suffer from serial correlation.

Wooldridge's (2002) test rejected the null hypothesis, indicating that this is indeed the case. Dynamic

⁴ Including a variable for the total number of adherents in each county is necessary because we only consider seventeen denominations here; as a result, the sum of these denominations' adherents is not equal to the total number of religious adherents.

⁵ Being both denomination-specific and time-invariant, the pairwise correlations between dissimilarity and strictness are not shown. However, the two variables were found to be correlated .38.

panel data analysis typically overcomes this problem through the inclusion of the lagged dependent variable among the covariates; yet this is known to bias OLS results because the lagged dependent variable is typically correlated with the cross-sectional component of the error term. We thus opted for Arellano-Bond generalized method of moments (GMM) estimation (Arellano & Bond, 1991) for our models. Arellano-Bond estimation in fact overcomes this problem by differencing the equation in order to remove the unit-specific effects and by using lagged differences of the endogenous and exogenous covariates as instruments. It must be noted that this technique results in a smaller number of observations because the first time period must be dropped.⁶

In the second part of our analysis, we proceed to examine the effect of scandals on membership for the other sixteen Christian denominations. To do so, we consider the denomination-county dyad as the level of analysis, while retaining a variable for the number of Catholic adherents in each county. Doing so allows us to look for evidence of transfer between the Catholic Church and other churches at the local level. At this stage, however, additional estimation challenges arise: because of the presence of time-invariant covariates such as dissimilarity and strictness, first-differenced models such as Arellano-Bond GMM cannot be used, as the effects of such variables would be differenced away. Furthermore, the number of adherents to a given denomination in a given county is likely to be once again serially correlated over time. We therefore chose generalized estimating equations (GEE)—after Liang & Zeger (1986)—to estimate our models, specifying an exchangeable correlation structure.⁷ This family of models has several desirable properties: in particular, it accommodates correlation across time periods and it provides consistent estimates even when the correlation structure is misspecified, while allowing for time-

⁶ Using generalized estimating equations (GEE) for these models generated a similar pattern of results.

⁷ The models were found to be robust to alternative specifications for the correlation structure.

invariant covariates. Finally, we applied Huber-White estimators so as to obtain robust standard errors in both parts of the study (White, 1980).

RESULTS

----- Insert Table 3 about here -----

Table 3 presents Arellano-Bond GMM estimates of the number of adherents to the Catholic Church in every county of the United States between 1971 and 2000, with spells of approximately ten years (1971, 1980, 1990, and 2000). Model 1 includes just control variables and the lagged dependent variable, while Model 2 adds time dummies, Model 3 adds the misconduct measure, and Model 4 is our full model inclusive of the main independent variable, logged scandal publicity. The coefficient for the misconduct variable is marginally significant in Model 3 and not significant in Model 4; considering the fact that both coefficients are very small and lack statistical significance, this suggests that there is no appreciable effect of misconduct on membership rates. Further, the coefficient for logged scandal publicity in Model 4 is negative and significant, indicating support for Hypothesis 1; in terms of effect sizes, a one percent increase in scandal publicity is associated with a 0.116 percent decrease in number of Catholics in the county. This means that if cumulative scandal publicity were to double (a 100 percent increase), we would see Catholic membership fall by 11.6%.

----- Insert Table 4 about here -----

Table 4 reports the results of a set of GEE models estimating the number of adherents to non-Catholic denominations only. Once again, the level of analysis here is the denomination-county

dyad, resulting in 50,276 observations whose evolution we follow over time. Here, Model 5 is our basic model with control variables, while Model 6 adds logged scandal publicity; Models 7 and 8 include state and time dummies, respectively; Model 9 and 10 add the two interaction effects separately, while Model 11 is our full model with the interaction effects between scandal publicity and: 1) dissimilarity, i.e. the extent to which the focal denomination differs from the Catholic Church; 2) strictness, a dummy for whether the focal denomination is stricter than the Catholic Church. The effect of Catholic membership is negative and significant in all models, indicating the tendency for non-Catholic denominations to gain members in areas where the Catholic Church loses members. Moreover, Models 7–11 show a positive and significant effect of Catholic scandal publicity on non-Catholic denominations, which is robust to the inclusion of time and state dummies to control for unobserved geographical and year-specific characteristics, providing support for Hypothesis 2a. According to Model 8, a one percent increase in scandal publicity results in a 0.029 percent increase in membership for non-Catholic denominations; otherwise stated, if scandal publicity were to double (a 100 percent increase) we would expect individual non-Catholic denominations to gain 2.9 percent more members on average. It is also worth noting that the coefficient for dissimilarity is positive and significant in all models, indicating that the most dissimilar denominations tend to gain the most members over time; this is consistent with the available empirical evidence, because the two most dissimilar denominations—Seventh-Day Adventists and Church of Jesus Christ of Latter-day Saints—are also the fastest growing nationwide.

----- Insert Figure 3 about here -----

Both interactions included in Model 11 are statistically significant. The first one, graphically depicted in Figure 3, shows that logged scandal publicity has a significant and positive effect on

denominations that are closer to the Catholic Church, with the most benefits accruing to the closest denominations, in line with Hypothesis 2b. Conversely, for the most dissimilar denominations the effect becomes non-significant. As for the second interaction—the one between logged scandal publicity and the strictness dummy—it is positive and significant, indicating support for Hypothesis 3. To gauge the relative effect size for the two groups, we split our sample into stricter and less strict denominations and we ran Model 8 again on the two subsamples.⁸ A one percent increase in logged scandal publicity was found to result in a 0.1 percent increase in membership for stricter, non-Catholic denominations and in a 0.01 percent increase for less strict denominations. Otherwise stated, the effect size is on average ten times larger for stricter denominations, further corroborating Hypothesis 3.

Further analyses. While the evidence we have gathered so far might be compelling, drawing inferences about individual behavior from aggregate membership data is often challenging. In fact, because church membership is fluid and a certain degree of turnover is normal, we cannot be sure that increases in membership for churches that are similar to the Catholic Church are actually due (at least in part) to an influx of former Catholics. To be certain, we would need some kind of information about the religious choices of individuals, and specifically of individuals that left the Catholic Church due to scandals. To achieve this, once again we leverage data by the Pew Research Center. The Faith in Flux Survey (FFS), conducted in 2009, identified 802 former Catholics out of a sample of 2,867 respondents: of these, 401 had become unaffiliated, 39 joined a non-Christian denomination and 362 joined a different Christian denomination (Pew Forum, 2009). Among the latter, 68 specifically mentioned sex abuse scandals as a reason for leaving the Catholic Church; their breakdown by denomination at the time of the survey is reported in Table 5.

⁸These models are not reported for the sake of brevity and are available from the authors upon request.

----- Insert Table 5 about here -----

Although the classification scheme used in the Faith in Flux Survey is different from the one used in our study, we can nonetheless leverage this data to draw a few conclusions. First of all, we divide our sixteen non-Catholic denominations in two groups based on their dissimilarity score: the eight most *similar* (Methodist, Disciples of Christ, American Baptist, Evangelical Lutheran, Reformed Church, Missouri Synod Lutheran, Southern Baptist and Quaker) and the eight most *dissimilar* (the remaining churches). Looking at the categories in Table 1, Lutheran (encompassing Evangelical Lutheran and Missouri Synod Lutheran), Baptist (comprising American Baptist and Southern Baptist), Quaker, Methodist, Reformed Christian and Disciples of Christ all fall within the former category, while Presbyterian, Episcopalian, United Church of Christ, Adventists and Church of Jesus Christ of Latter-day Saints fall within the latter category. Adding up the number of former Catholics within the two categories, we observe that 24 individuals (i.e. 35 percent) chose similar denominations, while 12 (i.e. about 18 percent) chose dissimilar denominations.⁹ Otherwise stated, twice as many former Catholics chose similar denominations over dissimilar ones; this provides further support for Hypothesis 2b.

Our results concerning Hypothesis 3 face similar challenges: it might simply be that the variance in membership for individual denominations over time is a spurious byproduct of serial correlation or the result of some unobserved, time-invariant characteristics we cannot control for, rather than relative strictness. If this was the case, the validity of our conclusions might be called

⁹ The remaining 47 percent chose Christian denominations not included in our study, for which similarity to the Catholic Church cannot be determined; this figure is consistent with the fact that our 17 denominations taken together only account for about half of the Christian denominations in the United States. Denominations labeled as “other” include: nondenominational Christian, Pentecostal, Holiness Christian, Evangelical or Fundamentalist Christian, generic Protestant, Orthodox Christian, Anabaptists, Pietists, and Jehovah’s Witnesses. However, we might take “Holiness Christian” to stand for the Church of the Nazarene, the largest Holiness denomination; similarly, “Pentecostal” might refer to the Assemblies of God. Even when these two are included in the dissimilar category, however, we still find that a relative majority of former Catholics choose to join similar churches over dissimilar ones (among those that are included in our study).

into question. Unfortunately, Pew survey data does not allow us to test Hypothesis 3 further, because some of the denominations in our study are conflated in the FFS data.¹⁰ We can however set up a relatively simple empirical test of whether Catholic scandal publicity actually results in membership gains for strict churches, independently of time effects and denomination-specific factors. To this end, we run two separate ordinary least squares (OLS) models to estimate the logged number of adherents to stricter and less strict denominations; moreover, since most of the scandal publicity occurred between 1990 and 2000, to keep the model as simple as possible and to rule out time effects, we restrict our analysis to the last time period, while controlling for prior coverage.

----- Insert Table 6 about here -----

Table 6 reports estimates for our OLS models, which include denominational dummies to control for unobserved, time-invariant heterogeneity within individual churches, as well as a variable for the number of denomination members nationwide to account for country-level trends of growth and decline. Model 10 shows no significant effect of scandal publicity in the 1990-2000 period on less strict churches as a whole, while Model 11 shows that the effect is positive and significant for stricter denominations, corroborating Hypothesis 3. Further, we ran a Z-test to compare the coefficients for scandal publicity between the two models (Clogg, Petkova, & Haritou, 1995). We found the p-value to be 0.037, and we can therefore reject the null hypothesis that the two coefficients are equal.

Robustness checks. In order to further corroborate our findings, a handful of issues that might potentially invalidate our results still need to be addressed. The first one is *spatial autocorrelation*,

¹⁰ For instance, “Baptists” likely include both American Baptists and Southern Baptists, while “Lutheran” encompasses both Evangelical Lutheran and Missouri Synod Lutheran.

i.e. the possibility that observations referring to counties that are geographically proximate might not be independent, thereby violating one of the key assumptions of regression analysis. More specifically, media coverage of a scandal occurring within a given diocese might have effects on church membership patterns in other, neighboring dioceses. This is of particular concern because our measure of scandal publicity is based on local newspapers, which can often have statewide circulation. To control for this, we re-estimate our models reported in Tables 4 and 5 while including a variable for *scandal publicity in other dioceses within the same state*. The results—available from the authors—show that our findings are unaffected when accounting for spatial autocorrelation, at least as far as scandal publicity is concerned.

Another issue of concern is the content validity of our dissimilarity measure. The use of this measure hinges on the assumption—common in economic and sociological studies of religion—that churches occupying similar positions on the church-sect continuum offer a comparable religious experience. For instance, Barros & Garoupa (2002: 562) assume that “preferences can be represented by a unidimensional variable that we have defined as religious strictness. In other words, the church’s decision-making problem is solved by defining a position in that unidimensional space. It seems to us that such assumption is at least as compelling as the usual assumption in political science that parties choose a political platform from left to right.” Churches that are similarly on a strictness continuum should therefore be similar in other ways, as well, and offer comparable religious experiences. We believe this assumption to be reasonable; however, if it were to prove untrue, our arguments would be invalid.

To test whether this assumption actually holds, we implement two distinct robustness checks. First, we examine whether our unidimensional measure is indeed representative of several facets of religious life. To do this, we went back to Hoge (1979), in which religious denominations in America are categorized based on eight dimensions: ethnic identity, conservative/liberal,

ecumenism, polity, distinctiveness, evangelism, social action, and pluralism. A panel of 21 experts was asked to rate each denomination on a 1-7 Likert scale for each dimension, and the results were then averaged. Hoge's data contains information for 14 of the 17 denominations included in our study, reported in Table A1. For each pair of denominations, we built a cosine similarity measure across all eight dimensions to operationalize dissimilarity. For each of the 91 resulting pairs, we then compared this measure with our original measure—the absolute value of the difference in strictness—using a simple correlation coefficient, whose value turned out to be 0.89 ($p < .001$). The fact that these two measures are strongly correlated adds support to Iannaccone's core argument: churches that are similar in strictness are also similar in other core dimensions of religious life.¹¹

As a further robustness check, we also devised an alternative similarity measure based on pooled data from the General Social Survey. Following Smith (1990), we created a dataset of all respondents affiliated with one of the 17 denominations in our study between 1984 and 2014 (Protestant denominations were not individually coded before 1984). We then used the survey items suggested by the author to set up a multidimensional analysis analogous to the one described in the previous paragraph. The items were: feelings about the Bible; frequency of prayer; church attendance; belief in the afterlife; views on homosexuality, abortion and premarital sex, free speech for atheists. Running our models once more while using this alternative variable generates results which do not differ in any substantial manner from those previously reported.¹²

Our strictness measure, which we derive from Iannaccone (1994) and Hoge (1979), is also a potential source of concern in that it is based on a single item; the validity of single-item measures—especially in the case of ambiguous constructs—has often been called into question. To address this concern, we leveraged data from the National Congregations Study Panel Dataset obtained from the

¹¹ We could not employ this measure in regression models as it does not cover all denominations in our study.

¹² These results are also available from the authors.

Association of Religion Data Archives (ARDA), which contains nine items on behavioral norms that can be assembled to create a multi-item measure of strictness: alcohol use, cohabitation of unmarried adults, dancing, diet, smoking, dating, homosexuality, membership requirements, and charitable giving. Using these 9 items, we constructed a strictness measure on a 0-1 interval, and we compared it to our measure for each of the denominations included in our study. We found that the strictness measure thus obtained is correlated 84% with our pre-existing, single-item measure, which is highly suggestive of convergent validity.¹³

A final issue of concern has to do with a limitation of our data, and specifically with the fact that our main data source—the Churches and Church Membership in the United States survey by the Glenmary Research Center—is only carried out once per decade. It could therefore be argued that the effects of scandal publicity at the beginning of the decade might not have any significant effect on membership rate at the end of the decade and that our results could be explained entirely based on historical trends. To rule out this alternative explanation, we construct a *decaying* measure of media coverage: that is, we assume that the effect of publicity is greatest in the year in which the article is published, and then decays by a fixed rate every year (Mitchell, 2014; Watt, Mazza, & Snyder, 1993). This way, while an article published in 1979 would still carry most of its weight in 1980, an article published in 1971 would hold little weight by then. If the alternative explanation described above were true, we would expect our pattern of results to weaken or disappear outright when such measures are used. Yet this is not what we observe: the models with decaying media coverage—not reported here for the sake of brevity—show that our results still hold, even when different decay rates are used, with no meaningful differences between rates.

¹³ Further details about this robustness check are available from the authors.

DISCUSSION

While scandals are gaining traction as an object of inquiry in management studies, and scholars have begun to explore their consequences for the focal organization as well as others in the same field, their competitive implications are not yet fully understood. In this article, we leveraged a quantitative study of the effects of sex abuse scandals on Christian denominations in the United States to develop and test a theory of the competitive implications of scandals, using organizational membership as a dependent variable. In so doing, we found evidence that increased publicity of sex abuse scandals was positively associated with a decrease in membership for the Catholic Church at the local level. Furthermore, we found that competing organizations belonging to a different category gained members as a result of the scandal, and all the more so the more similar they were to the scandal-stricken one. Finally, we hypothesized that scandal publicity would trigger an audience-level evaluative shift which would put stricter churches at an advantage; our pattern of results is broadly consistent with this account. Overall, our study adds nuance to our understanding of the competitive consequences of scandals in organizational fields, making distinct contributions to the organizational literature which we would situate along four main lines.

First, our study provides a robust organizing framework for analyzing the effect of scandals in organizational settings. While most extant studies (e.g. Graffin et al., 2013; Jonsson et al., 2009) had typically considered a single scandal and its consequences, to our knowledge ours is the first study to examine a large number of geographically dispersed, comparable scandals over the course of several decades, and as such it represents a substantial improvement in terms of external validity and generalizability. Our framework also brings order to the study of scandals by empirically distinguishing the mechanisms behind the various effects that scandals have on the focal organization, as well as other organizations. Second, our findings contribute to scholarly understanding of scandals by providing empirical support for the idea that scandals can be

conceived as identity threats looming over the members of the stricken organizations. Although our data is not granular enough to observe individual coping mechanisms, and we cannot be certain of the extent to which decreases in Catholic membership are due to adherents defecting vis-à-vis increased difficulty in recruiting converts, taken as a whole our data suggests that sex abuse scandals in the U.S. Catholic Church led at least some of its members to defect, cutting off their relationship with the organization to reduce dissonance (Festinger, 1957) and engage in identity repair. Third, our results strengthen the idea that scandals might translate into (relative) competitive advantages for other organizations. More specifically, we have advanced the argument that publicized transgressions undermine the identification link between the scandal-stricken organizations and its current members, which will result in some of them leaving. At the same time, closely similar organizations offer these estranged members an opportunity to experience analogous levels of involvement and identification, while distancing themselves from the stigmatized organization; as a result of this substitution effect, these organizations will gain the most members in the wake of the scandal. Indeed, we found that former Catholics who joined other Christian denominations predominantly chose churches characterized by similar levels of involvement and participation—a way to reaffirm their religious commitment while disassociating themselves from the Catholic Church. In a way, this result is particularly surprising because the two denominations that are the most unlike the Catholic Church based on our score—the Church of Jesus Christ of Latter-day Saints and Seventh Day Adventists—are also the fastest-growing denominations nationwide.

With respect to the points made above, one of the attractive features of our empirical setting is that we can observe how scandals affect organizations that are both closely similar attractive yet belong to a different category from, and are not associated with, the implicated organization. Whereas most of the recent literature in this area has emphasized the contamination effect that undermines bystander organizations belonging to the same category (Jonsson et al., 2009; Paruchuri

& Misangyi, 2015), our study helps bring to light the substitution effect triggered by the weakening of the implicated organization's competitive position. Although past studies provide examples of such substitution (e.g. Jensen, 2006), this is to our knowledge the first study documenting on a large scale the positive spillovers that competitor organizations might experience as a result of scandals. And while the literature examining the linkages between misconduct, scandals, and interorganizational competition is still in its infancy (see Bennett, Pierce, Snyder, & Toffel, 2013; Bertrand & Lumineau, 2016 for notable examples), we believe our study makes a notable contribution in shifting the attention from the contamination aspects of scandals, which have been extensively explored by organizational scholars, to the competitive aspects, which are arguably just as consequential for organizations and can often be exploited to the latter's advantage. Indeed, while scandals might be enduring and far-reaching enough to durably tarnish entire organizational fields, the fact that some organizations are penalized more than others affects the distribution of resources in such a way that competitors exhibiting particular features—in our case, similarity to the scandal-stricken organization and strictness—are poised to benefit in the aftermath of a scandal.

Finally, by positing that the field-level effects of scandals are due to a discontinuity in the way in which audiences evaluate organizations, our study invites further attention to the tangible effects that valuation and evaluation processes exert on the working mechanisms of organizational life, an area of inquiry that has been experiencing a surge in interest in recent years (e.g. Lamont, 2012; Zuckerman, 2012). Our findings suggest that scandals can be understood as ruptures of the social structure that introduce a “durable transformation” of evaluative schemas (Sewell, 1996: 843); in fact, by exposing instances of norm transgression, sex abuse scandals made standards of conduct and discipline salient, to the benefit of stricter churches. In a sense, then, “scandals renew societies” (Adut, 2005: 217), with durable effects on organizational fields, as well. On a general level,

understanding the conditions under which evaluative shifts might occur, as well as their consequences, has the potential to open up new avenues for organizational scholarship.

Future research on these issues could further explore the boundary conditions of our results. For instance, scholars may investigate how the type of organization (e.g., market-based, non-profit) as well as the identity of the relevant constituents (e.g., members, customers) affect the relative strengths of the competitive effects of scandals. The relative durability of the various effects may also deserve further scholarly exploration: while within-category contamination may dissipate after organizations react to disprove the presumption of culpability that affect them (Pfarrer et al., 2008), the substitution effect might be more durable. If this is true, organizations with similar offerings operating in the same category as the implicated organization may be at first penalized by a scandal (e.g., suffer a loss in market value), as documented in the literature (Jonsson et al., 2009; Paruchuri & Misangyi, 2015), but eventually benefit from it after the dust has settled (e.g., by durably gaining market share). Along the same lines, an organization known for being strict might enjoy a more or less durable advantage after a scandal over its laxer competitors, as audiences focus their attention on misconduct issues. All of these represent intriguing avenues for future empirical research.

In a similar fashion, the field-level effects of scandals have not yet been thoroughly explored. While our study offers initial evidence that scandals have pervasive effects on the way in which all actors are evaluated within a field, and that such effects likely run counter to the salient features of the transgressions that become public during the course of the scandal, such issues deserve further attention. In particular, scholars could examine the conditions under which scandals are especially likely to trigger a backlash, and if so, its direction and intensity. Finally, scope conditions that might enhance or dampen the effect of scandals (Adut, 2008: 28-29), such as information asymmetry, lack of internal control, closure and other network factors, and the existence of emotional solidarity and collective liability, cannot be tested with our data and would also deserve further inquiry.

Our results come with some caveats, however. Chief amongst them is the fact that, because churches are classified on a unidimensional continuum (as shown in Table 1), similarity and strictness go together; this is not necessarily the case in other settings. For instance, in the Volkswagen example we have cited earlier, similarity and strictness would likely be independent, as there is no apparent correlation between a manufacturer's strictness and the range of its product offerings. While this feature of our empirical setting potentially limits the generalizability of our findings, none of the arguments we have made hinges on this assumption. In a similar vein, while churches are among the most ancient and widespread organizational forms (Tracey, 2012), they differ from the market-based organizations typically studied by management scholars in a variety of ways. At least two key differences appear relevant to the study of scandals. First, churchgoers tend to have a deep connection to their organization—which may make them relatively more immune to the effects of scandals than, for instance, customers of more market-oriented organizations. The examples of Skandia and Arthur Andersen suggest that customers may be more mobile and quickly dissociate themselves from an organization tarnished by a scandal (Jonsson et al., 2009; Paruchuri & Misangyi, 2015). In that regard, our findings might even be seen as conservative, since the competitive effects of scandals—with respect to both contamination and substitution—may be stronger and unfold more rapidly in market settings. Second, issues of behavioral norm enforcement might be particularly salient in religious organizations, raising questions about the generalizability of our findings relating to strictness. Yet, such issues are all but absent from market settings: the public revelation of concealed misconduct typically triggers a re-evaluation of the organizations accused of untrustworthiness and their conformity to established norms of conduct. The “cheating” label widely applied in the media to the 2015 Volkswagen emission test scandal provides a striking illustration of the vivid reaction that accompanies corporate scandals, often expressed with a strong

moral tone. We would thus expect scandals in market settings to be associated with evaluative shifts, as well.

Practical implications

Independently of their scholarly relevance, it is our contention that our results can be generalized beyond our empirical setting and condensed into useful lessons for practice. Although scandals *per se* are largely exogenous, and therefore beyond administrative control, we argue that their consequences can be managed by the focal organization and appropriately leveraged by competitor organizations. In the former case, the scandal-stricken organization can implement measures to contain the leakage of members ensuing from the identity threat induced by the scandal, thereby reducing turnover. Such remedial measures might include a public apology, CEO succession (Connelly, Ketchen, Gangloff, & Shook, 2015), impression management techniques, symbolic practice adoption, prosocial claims, or a combination of the above (Marquis, Glynn, & Davis, 2007; McDonnell & King, 2013). In the latter case, competitors can tailor their offerings by making them similar to those of the scandal-stricken organization, while simultaneously highlighting the fact that they belong to a different category—if such claim is credible—and also enforce stricter standards of conduct, thereby capturing many of the scandal-stricken organization’s former members. When a scandal affects a firm in a competitive environment, this dynamic could turn out to be a key factor in the so-called “war for talent”, in which organizations compete for human capital.

Our findings are likely to extend beyond organizational membership. In the case of the recent (and well-publicized) Volkswagen emissions scandal, for instance, based on our results we would predict that automakers offering products similar to the vehicles recalled by Volkswagen and with a reputation for strict norm enforcement in their emission testing process—and, more generally, in their standard of conduct—are likely to reap the greatest rewards in terms of sales. While many manufacturers offer a product line that is similar to Volkswagen’s, and are therefore

well positioned to gain customers from the scandal, those with a strong reputation for strict norm enforcement are likely to benefit the most. It might not be purely coincidental that, in the aftermath of the scandal in November 2015, Volvo—a European car manufacturer known for its rigor—had record sales the U.S.¹⁴ It is therefore in these firms' best interest to emphasize their strictness and make it salient, so as to gain market share. Past work also suggests that bystanders in the same category as Volkswagen will, at least in the short term, be penalized: other German manufacturers such as Daimler and BMW might suffer from the fallout of the scandal due to contamination, regardless of the extent of their overlap in product line and commitment to environmental goals.¹⁵

In all, our study provides valuable lessons to practitioners in that it sheds further light on what happens in the aftermath of scandals, with particular reference to their competitive implications in organizational settings. In so doing, we underline that scandals cause losses for the focal organization, which can be leveraged by competitors to obtain highly tangible gains. The relative magnitude of such gains, however, is a contingent on: 1) the extent to which a competitor can provide a close substitute to the scandal-stricken organization's offerings; 2) the competitor organization being perceived as enforcing stricter norms of conduct. While the occurrence of scandals is, in itself, beyond organizational control, in the wake of a scandal organizations within the field can nonetheless position themselves optimally to maximize the advantages arising from it. Further, we show the scandal-stricken organization is particularly vulnerable to the loss of key constituencies, which should prompt its leadership to take action and minimize the extent of this dynamic.

¹⁴ <http://www.cnn.com/2015/09/24/as-volkswagen-loses-other-automakers-could-benefit.html>

¹⁵ <http://www.forbes.com/sites/greatspeculations/2015/09/28/the-domino-effect-of-volkswagens-emissions-scandal/>

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TABLES

Table 1. Strictness score and membership for the 17 Christian denominations included in the study (column 1 adapted from Iannaccone, 1994).

Denomination	Strictness Score	Dissimilarity ⁺	Strictness (dummy) ⁺⁺	Membership*	% of Christians**
Episcopal	1.1	1.9	0	2,006,343	0.81%
United Church of Christ	1.3	1.7	0	1,080,199	0.44%
Presbyterian	1.6	1.4	0	2,775,464	1.12%
Unitarian	1.6	1.4	0	221,367	0.09%
Methodist	1.8	1.2	0	7,774,931	3.15%
Disciples of Christ	2.1	0.9	0	658,869	0.27%
American Baptist	2.5	0.5	0	1,310,505	0.53%
Evangelical Lutheran	2.7	0.3	0	4,542,868	1.84%
Reformed Church	2.8	0.2	0	250,938	0.10%
Catholic	3	-	-	68,503,456	27.76%
Missouri Synod Lutheran	3.6	0.6	1	2,312,111	0.94%
Southern Baptist	4	1	1	16,160,088	6.55%
Quaker	4.1	1.1	1	~ 80,000	0.03%
Church of the Nazarene	4.5	1.5	1	645,846	0.26%
Assemblies of God	4.8	1.8	1	2,914,669	1.18%
Church of Jesus Christ of Latter-day Saints	5.4	2.4	1	6,058,907	2.46%
Seventh Day Adventist	5.8	2.8	1	1,043,606	0.42%
TOTAL				118,340,167	47.95%

⁺ Dissimilarity in strictness score relative to the Catholic Church.

⁺⁺ Equals 1 if the denomination is stricter than the Catholic Church, 0 otherwise.

* Membership in the United States (2009).

** Estimated percentage of Christians in the United States (2009).

Table 2. Descriptive statistics and pairwise correlations.¹⁶

	Mean	S.D.	Min	Max	1	2	3	4	5	6	7	8	9
1 Logged number of Catholics	7.12	2.64	0	15.152	1								
2 Individuals of Hispanic descent (percent)	0.054	0.119	0	1.568	0.21	1							
3 Individuals of African descent (percent)	0.09	0.147	0	0.909	-0.231	-0.099	1						
4 Individuals below the poverty line (percent)	0.169	0.083	0	0.672	-0.409	0.173	0.446	1					
5 Religious adherents (orthogonalized)	0	1	-0.295	39.372	0.399	0.129	0.072	-0.118	1				
6 County population (orthogonalized)	0	1	-16.53	51.35	0.069	0.038	0.006	-0.035	0	1			
7 Herfindahl–Hirschman index	0.319	0.164	0.014	1	-0.182	0.177	0.115	0.275	0.048	-0.123	1		
8 Individuals accused of misconduct	1.425	2.951	0	58	0.238	0.01	-0.063	-0.119	0.252	0.056	0.044	1	
9 Scandal coverage (logged)	1.172	1.619	0	7.112	0.151	0.124	-0.032	-0.129	0.12	0.036	-0.104	-0.121	1

	Mean	S.D.	Min	Max	1	2	3	4	5	6	7	8	9	10	11
1 Logged number of adherents to (non-Catholic) denomination	3.604	3.324	0	13.129	1										
2 Individuals of Hispanic descent (percent)	0.047	0.114	0	1.568	0.004	1									
3 Individuals of African descent (percent)	0.09	0.148	0	0.909	-0.026	-0.103	1								
4 Individuals below the poverty line (percent)	0.169	0.088	0	0.672	-0.227	0.185	0.445	1							
5 Focal denomination adherents nationwide (logged)	14.087	2.203	0	16.805	0.491	0.003	-0.006	-0.051	1						
6 Herfindahl–Hirschman index	0.326	0.164	0.071	1	-0.181	0.188	0.154	0.312	-0.027	1					
7 Individuals accused of misconduct	1.733	3.213	0	58	0.164	0.04	-0.071	-0.129	-0.007	0.026	1				
8 County population (orthogonalized)	-0.017	1.021	-16.409	50.993	0.065	0.022	0.012	-0.034	-0.005	-0.115	0.071	1			
9 Religious adherents (orthogonalized)	-0.012	0.958	-0.297	39.109	0.25	0.106	0.071	-0.131	0.007	0.062	0.301	-0.02	1		
10 Catholic adherents (orthogonalized)	0.022	0.935	-18.576	22.557	-0.113	0.078	-0.116	0.054	-0.012	0.194	0.173	0.006	0.099	1	
11 Scandal publicity (logged)	1.015	1.653	0	7.024	-0.015	0.066	-0.017	-0.213	0.065	-0.028	-0.059	0.011	0.102	0.009	1

¹⁶ This table does not include dissimilarity and strictness as reported in Table 1 because these do not vary across counties and over time, but are denomination-specific.

Table 3. Arellano-Bond dynamic panel data estimation of Catholic membership in each county (logged).

VARIABLES	Model 1	Model 2	Model 3	Model 4
Lagged number of Catholics (logged)	0.280*** (0.058)	0.099* (0.044)	0.094* (0.044)	0.053 (0.047)
Religious adherents (orthogonalized)	0.124 (0.085)	0.049 (0.070)	0.068 (0.067)	0.101 (0.082)
County population (orthogonalized)	0.024 (0.027)	-0.005 (0.021)	-0.002 (0.021)	0.006 (0.021)
Herfindahl–Hirschman index	1.706* (0.665)	1.313+ (0.671)	1.194+ (0.658)	0.827 (0.670)
Individuals of Hispanic descent (percent)	3.983*** (0.556)	3.383*** (0.536)	3.369*** (0.532)	3.377*** (0.531)
Individuals of African descent (percent)	-1.853* (0.888)	-2.748** (0.920)	-2.722** (0.912)	-2.637** (0.913)
Individuals below the poverty line (percent)	-0.377 (0.357)	-0.062 (0.359)	-0.064 (0.357)	0.078 (0.345)
Time dummies	No	Yes	Yes	Yes
Individuals accused of misconduct			0.007+ (0.004)	-0.003 (0.006)
Scandal coverage (logged)				-0.116* (0.054)
Constant	4.869*** (0.554)	6.349*** (0.412)	6.482*** (0.419)	7.056*** (0.490)
Observations	5,668	5,668	5,668	5,660
Number of counties	2,868	2,868	2,868	2,864

Robust standard errors in parentheses

*** p<0.001, ** p<0.01, * p<0.05, + p<0.1

Table 4. Generalized estimating equations (GEE) estimates of the logged number of adherents to non-Catholic denominations at the county level.

VARIABLES	Model 5	Model 6	Model 7	Model 8	Model 9	Model 10	Model 11
Individuals of Hispanic descent (percent)	-0.791*** (0.075)	-0.791*** (0.077)	-0.320*** (0.079)	-0.301** (0.093)	-0.300** (0.093)	-0.302** (0.093)	-0.301** (0.093)
Individuals of African descent (percent)	-0.941*** (0.070)	-0.941*** (0.070)	-0.733*** (0.070)	-0.258** (0.094)	-0.258** (0.094)	-0.259** (0.094)	-0.258** (0.094)
Individuals below the poverty line (percent)	0.000*** (0.000)						
Focal denomination adherents nationwide (logged)	1.430*** (0.012)	1.430*** (0.012)	1.420*** (0.011)	1.408*** (0.011)	1.416*** (0.011)	1.405*** (0.011)	1.412*** (0.011)
Herfindahl–Hirschman index	-1.574*** (0.044)	-1.573*** (0.044)	-1.468*** (0.047)	-1.302*** (0.050)	-1.302*** (0.050)	-1.301*** (0.050)	-1.301*** (0.050)
Individuals accused of misconduct	0.022*** (0.001)	0.022*** (0.001)	0.011*** (0.001)	0.003+ (0.001)	0.003+ (0.001)	0.003+ (0.001)	0.003+ (0.001)
County population (orthogonalized)	0.221*** (0.010)	0.222*** (0.011)	0.220*** (0.010)	0.200*** (0.010)	0.200*** (0.010)	0.200*** (0.010)	0.200*** (0.010)
Religious adherents (orthogonalized)	0.721*** (0.031)	0.723*** (0.032)	0.714*** (0.031)	0.625*** (0.028)	0.625*** (0.028)	0.625*** (0.028)	0.625*** (0.028)
Dissimilarity (orthog.)	0.353*** (0.016)	0.354*** (0.016)	0.342*** (0.016)	0.340*** (0.015)	0.355*** (0.015)	0.340*** (0.015)	0.355*** (0.015)
Strictness (dummy, orthog.)	0.116*** (0.012)	0.116*** (0.012)	0.113*** (0.012)	0.112*** (0.012)	0.113*** (0.012)	0.106*** (0.012)	0.107*** (0.012)
Catholic adherents (orthogonalized)	-0.150*** (0.011)	-0.158*** (0.011)	-0.172*** (0.011)	-0.199*** (0.011)	-0.199*** (0.011)	-0.199*** (0.011)	-0.199*** (0.011)
Scandal publicity (logged)		0.001 (0.002)	0.045*** (0.004)	0.029*** (0.004)	0.028*** (0.004)	0.029*** (0.004)	0.029*** (0.004)
Time dummies	No	No	Yes	Yes	Yes	Yes	Yes
State dummies	No	No	No	Yes	Yes	Yes	Yes
Scandal publicity (logged) x Dissimilarity					-0.008*** (0.002)		-0.008*** (0.002)
Scandal publicity (logged) x Stricter denom. (dummy)						0.005*** (0.001)	0.005*** (0.001)
Constant	-17.06*** (0.169)	-17.06*** (0.169)	-17.14*** (0.164)	-16.91*** (0.191)	-17.03*** (0.188)	-16.85*** (0.192)	-16.98*** (0.189)
Observations	169,698	169,390	169,390	169,390	169,390	169,390	169,390
Number of denomination-county dyads	50,249	50,149	50,149	50,149	50,149	50,149	50,149

Standard errors in parentheses

*** p<0.001, ** p<0.01, * p<0.05, + p<0.1

Table 5. Breakdown of former Catholics that joined other Christian denominations and mentioned the sex abuse scandal as a reason for doing so (data from the 2009 Faith in Flux Survey).

	Count	%
Baptist	7	10.3
Methodist	4	5.9
Lutheran	11	16.2
Presbyterian	5	7.4
Episcopalian	4	5.9
Disciples of Christ	1	1.5
United Church of Christ	2	2.9
Reformed Christian	0	0.0
Adventist	0	0.0
Quaker	1	1.5
Church of Jesus Christ of Latter-day Saints	1	1.5
Other Christian	32	47.1
TOTAL	68	100.0
SIMILAR CHURCHES	24	35.3%
DISSIMILAR CHURCHES	12	17.6%
OTHER CHURCHES	32	47.1%

Table 6. OLS estimates of the logged number of adherents to non-Catholic denominations at the county level in 2000.

VARIABLES	Model 12 <i>Less strict churches</i>	Model 13 <i>Stricter churches</i>
Logged number of adherents to focal denomination in county, 1990	0.939*** (0.002)	0.855*** (0.004)
Focal denomination adherents nationwide (logged), 1990	-0.601** (0.200)	-1.492*** (0.178)
Focal denomination adherents nationwide (logged), 2000	0.581** (0.207)	1.718*** (0.181)
Individuals of Hispanic descent (percent), 1990	0.338 (0.237)	-0.501 (0.367)
Individuals of Hispanic descent (percent), 2000	-0.211 (0.226)	0.896** (0.331)
Individuals of African descent (percent), 1990	0.353 (0.297)	-1.043 (0.649)
Individuals of African descent (percent), 2000	-0.203 (0.298)	1.195+ (0.643)
Individuals below the poverty line (percent), 1990	-0.515*** (0.145)	-0.739* (0.304)
Individuals below the poverty line (percent), 2000	-0.330+ (0.172)	-0.545 (0.369)
Herfindahl–Hirschman index, 1990	0.318*** (0.079)	0.549*** (0.149)
Herfindahl–Hirschman index, 2000	-0.451*** (0.075)	-0.963*** (0.139)
Religious adherents (orthogonalized), 1990	-0.078 (0.074)	-0.071 (0.106)
Religious adherents (orthogonalized), 2000	0.141* (0.065)	0.194* (0.094)
County population (orthogonalized), 1990	0.005 (0.022)	0.000 (0.031)
County population (orthogonalized), 2000	0.015 (0.017)	0.030 (0.025)
Catholic adherents (orthogonalized), 1990	0.109*** (0.023)	0.145*** (0.026)
Catholic adherents (orthogonalized), 2000	-0.149*** (0.026)	-0.207*** (0.029)
Logged cumulative scandal publicity up to 1990	0.028** (0.009)	0.017 (0.016)
Logged scandal publicity, 1990-2000	0.004 (0.005)	0.025* (0.010)
Denomination dummies	Yes	Yes
State dummies	Yes	Yes
Constant	0.564*** (0.140)	-2.379*** (0.175)
Observations	27,198	21,154
R-squared	0.952	0.850

Robust standard errors in parentheses

*** p<0.001, ** p<0.01, * p<0.05, + p<0.1

FIGURES

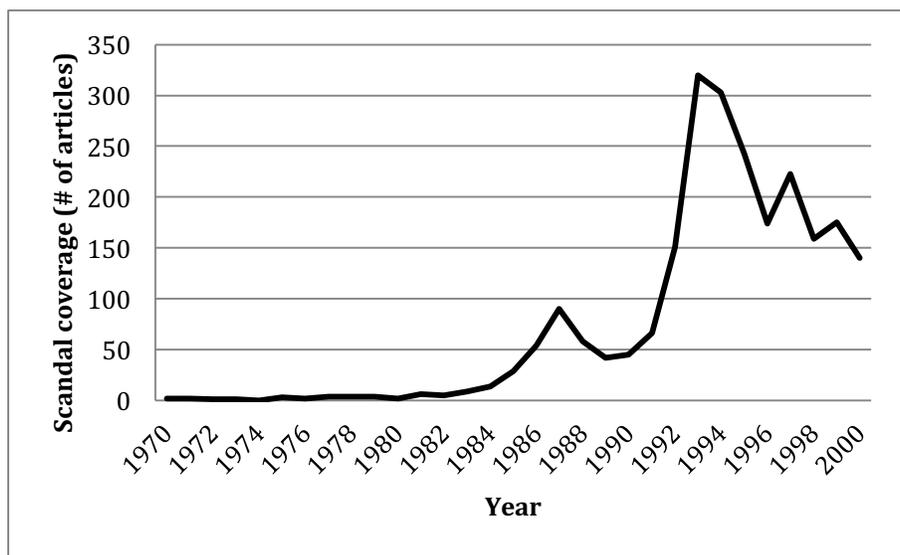
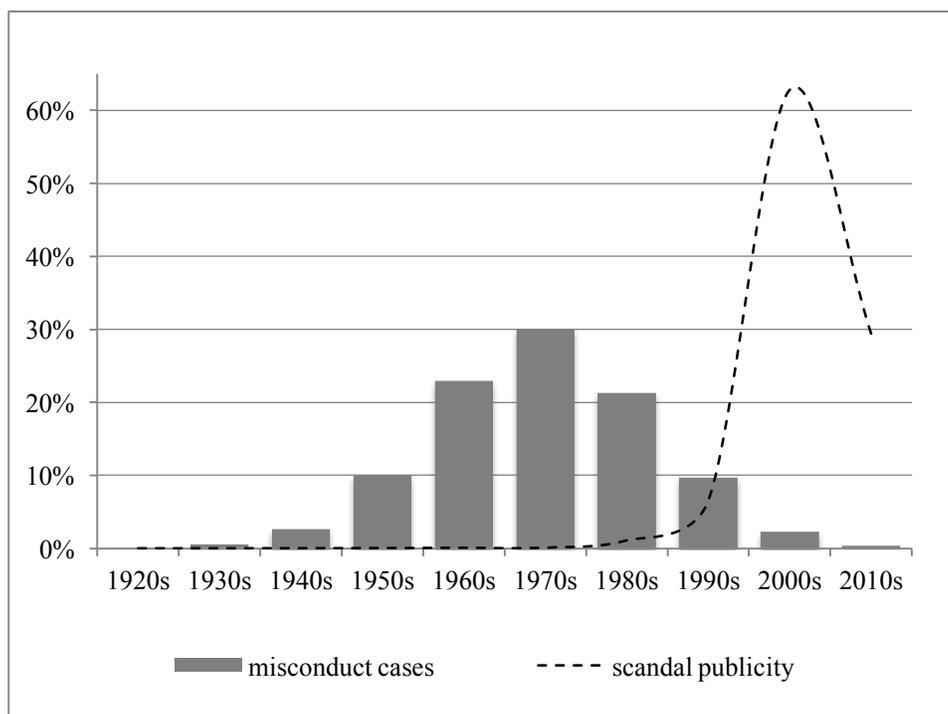


Figure 1. Temporal evolution of Catholic sex abuse scandal publicity in U.S. newspapers, 1970-2000.



Note— Distribution of the 2,251 alleged sex-abuse cases where the decade of (first) misconduct was reported in the press.

Figure 2. Distribution of reported misconduct cases and scandal publicity by decade, 1920s-2010s.

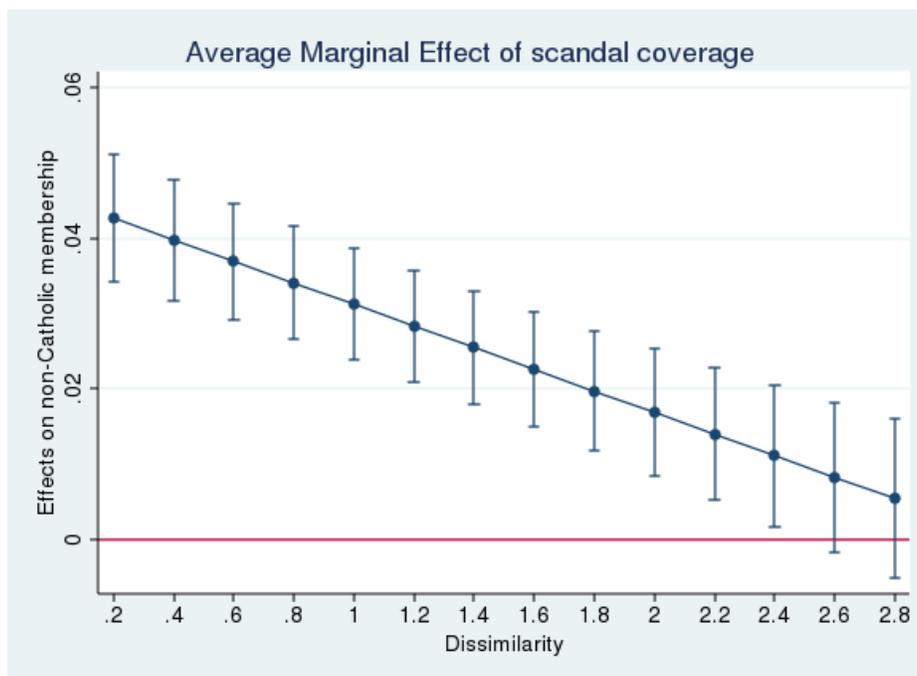


Figure 3. Marginal effect of Catholic scandal publicity on non-Catholic membership as a function of dissimilarity to the Catholic Church.

APPENDIX

Table A1. Denominational scores from Hoge (1979).

Denomination	Ethnic Identity	Cons /Lib	Ecumenism	Polity	Evangelism	Social Action	Distinctive Lifestyle	Pluralism
American Baptist	1.71	4.24	3.05	5.71	3.86	4.91	2.95	5.14
Assemblies of God	1.76	1.15	6.17	5.63	6.48	2	6	1.48
Church of the Nazarene	2	1.8	5.89	4.26	5.67	2.6	5.3	2.24
Disciples of Christ	1.9	4.67	2.33	5	3.26	5	2.95	5.5
Episcopal	2.67	5.57	2.63	2.24	1.52	5.33	1.55	5.52
Evangelical Lutheran	4.45	3.925	3.58	3.42	3.19	4.56	3	3.85
Methodist	2	5.57	2.16	2.05	3.14	5.76	2.25	5.91
Missouri Synod Lutheran	5.29	1.62	6.32	2.86	4.11	2.85	3.85	1.38
Church of Jesus Christ of Latter-day Saints	2.67	1.56	6.87	1.87	6.32	2.57	6.28	1.4
Presbyterian	1.95	5.05	1.95	2.95	3	5.76	2.05	5.19
Reformed Church	4.81	3.29	3.83	3.3	3.5	4	2.95	3.57
Seventh Day Adventist	2.05	1.48	6.37	2.92	6.05	2.71	6.26	1.15
Southern Baptist	1.95	2	6.21	5.38	6.57	2.76	4.45	2.71
United Church of Christ	2.05	6.62	1.32	4.57	1.76	6.31	1.6	6.52

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