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COMMENT

Is Religion Not Prosocial at All? Comment on Galen (2012)

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Galen (2012), critically reviewing recent research on religion and prosociality, concludes that the religious prosociality hypothesis is a (congruence) fallacy. The observed effects are not real: They only reflect stereotypes and ingroup favoritism, are due to secular psychological effects, are inconsistent, and confound (e.g., by ignoring curvilinear relationships) those low in religiosity with nonbelievers. In this commentary, a distinction is first made between the already known limitations on the extent, context, and quality of the religion–prosociality link and the novel, more radical argument of Galen denying the validity and the plausibility of such a link. Second, careful examination of relevant studies shows that religious prosociality is not reduced to social desirability in self-reports, is confirmed through ratings by peers who are blind with regard to the religious status of the target, and is expressed through real prosocial behavior in controlled experiments and life decisions with long-term effects. This behavior cannot be reduced to ingroup favoritism. Finally, Galen's opposition between religious versus "secular" psychological effects is criticized as psychologically problematic, and his insistence for examination of curvilinear relationships is relativized on the basis of research confirming the linear relationship. Alternative research questions for understanding prosociality of atheists are proposed.

Keywords: prosocial behavior, altruism, ingroup favoritism, religion, atheism

The critical examination of research on religion and prosociality made by Galen (2012) is very much appreciated. It constitutes an impressive review of accumulated recent research on a topic that has increasingly been the focus of several psychological subdisciplines: social, developmental, moral, and, more recently, evolutionary (for other reviews, see also Preston, Ritter, & Hermandez, 2010; Saroglou, in press). More importantly, this article is probably unique in constituting a detailed, systematic, and probably exhaustive accumulation of evidence and arguments criticizing the idea that religion or religiosity may have any (real) positive influence on prosocial behavior.

Some of the issues and arguments raised concern the limitations of the extent and the quality of the religion–prosociality link and are already known; others are more critical and question even the validity and the plausibility of such a link. Indeed, on the basis of their research, scholars have concluded that religious prosociality (a) is *minimal* (common and low cost) and *limited* in extent, since it applies to proximal targets but not to unknown targets and outgroup members (Saroglou, 2006; see also Blogowska & Saroglou, 2011; Pichon & Saroglou, 2009). Moreover, it (b) is often motivated by *egoistic* (self-image) rather than altruistic *motivations* (Batson, Schoenrade, & Ventis, 1993) and concern with social *reputation* and divine *control* (Norenzayan & Shariff, 2008; Shariff & Norenzayan, 2007). In addition, it is already known that religious prosociality (c) is *conditional* to contextual variables that may activate it (e.g., salience of religious norms and induction of self-transcendent positive emotions; Malhotra, 2010; Van Cappellen & Saroglou, 2011) or inhibit it (other moral principles opposing care; Van Pachterbeke, Freyer, & Saroglou, 2011). Finally, religious prosociality (d) *varies* greatly, depending on the specific aspects of religion activated (coalitional vs. devotional; Ginges, Hansen, & Norenzayan, 2009; Preston et al., 2010) or the specific religious orientations involved (Batson et al., 1993).

Galen (2012) goes further, however, and even contests the validity and the plausibility of the religion–prosociality link. He should be applauded for having raised three major issues in particular: (a) prosociality as a function of religiosity is due to impression formation and relevant stereotypes and is equivalent to ingroup favoritism; (b) nothing unique to religion is present when religious aspects seem to activate or enhance prosocial behavior (similarly, religiosity has no real influence on prosocial behavior); and (c) there is need to distinguish between atheists, nonbelievers, and people who are low or moderate in religiosity when the findings are in favor of the "religious." These three issues, together with the above-mentioned ones, could suggest that the idea according to which religion overall causes prosocial behavior and that this can be broadly observed in most believers' lives is not only a serious overstatement or an unjustified conclusion, but perhaps even a myth.

However, a major criticism that can be made is that the review article somehow suffers from the same weaknesses of which Galen (2012) accuses other scholars who draw a conclusion opposite to his own, that is, that religion and religiosity are responsible for,

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and lead to, prosocial behavior. These weaknesses are excessiveness and overstatement in the conclusions with respect to existing research, some confusion regarding the causal status of religion and religiosity in psychological research, and overeagerness in identifying confirming evidence and minimizing disconfirming evidence. Indeed, Galen concludes his review with a too strong statement: “The religious prosociality hypothesis, though popular in the literature and among the general public, is a manifestation of such a [congruence] fallacy” (p. 899). For Galen, in the end, there is no correspondence between people’s religious beliefs and behavior; and there is not any causal connection between religion, or religiosity, and prosociality.

Rather than being a well-balanced evaluation of the pros and cons of the religion–prosociality hypothesis, the review article constitutes a certainly rigorous and systematic, but still unidirectional, investigation to charge the suspect. To a certain extent, part of the demonstration sometimes looks like rhetorical argumentation. In this commentary, I will examine some pieces of evidence or counterevidence that at least weaken and, in some cases, even seriously question Galen’s (2012) major conclusion that the religious prosociality hypothesis is a fallacy.

Is It All About Impression Formation, Stereotypes, and Ingroup Favoritism?

Galen (2012) argues, among other things, that the religiosity–prosociality link is not real because (a) self-reports of religious people simply reflect social desirability, (b) peer ratings are provided by evaluators who are not blind toward the religious status of the targets, and (c) the use of behavioral measures overall disconfirms self-reported prosociality as a function of religiosity. The observed findings in the literature are mostly due to impression formation, endorsement of the religion–prosociality stereotype, and ingroup favoritism. As I argue below, these are overstatements that, in addition, neglect disconfirming evidence and give rise to both internal (to the arguments) and external (other evidence) criticisms.

Social Desirability

I concur with Galen (2012) that the religiosity–prosociality link, as found in self-reports, may have been contaminated by social desirability. However, there is some rhetoric in his conclusion. Previous research has shown that this link is only partly due to social desirability. When controlling for social desirability, the link decreases, but not all variance is explained by social desirability: The relation between religiosity and self-reported prosociality remains significant (e.g., Lewis, 1999, 2000; Saroglou, Pichon, Trompette, Verschueren, & Dernelle, 2005, Study 4). However, Galen concludes, “The more religious an individual is, the more likely he or she will be to inflate self-ratings of prosocial characteristics [to this point, I fully agree], rendering *any results* based on self-reports *suspect* [emphasis added]” (p. 890). The latter is a problematic rhetorical inference: The accurate inference would be “requesting caution about the size of the association.”

Moreover, social desirability is no longer seen as necessarily constituting a response bias, but as also denoting part of real personality (e.g., Uziel, 2010). (This is a different issue from the one of the ecological validity for believers of social desirability measures, an issue successfully discussed by Galen, 2012). Thus, rather than using the argument of social desirability or self-enhancement to fully dismiss the religiosity–prosociality link, it seems more appropriate to use it to question the link between religiosity and the altruistic motivation of prosocial behavior. As evidence reviewed by Galen (2012) suggests, religious people may behave prosocially rather to defend a positive self-image or to gain positive reputation. This is an important issue, but it is different from the question of whether religious people behave prosocially, that is, do things to meet others’ needs even when somehow costly, or not.

Peer Ratings

I also concur with the author that, a priori, peer ratings of religious targets can be suspected to simply translate positive stereotypes about religion and not reflect direct perceptions of the religious target’s personality. The critical issue is thus whether the religious personality is still manifest in peer ratings when (a) the religious status of the target is not activated during the evaluation and, more importantly, (b) the evaluator is blind with regard to the target’s religious status. In fact, in several studies providing findings on personality and prosocial characteristics of targets as a function of their religiosity, including studies reviewed by Galen, peers were not informed that the study was about religion; and the religiosity of the peers, if measured, was measured at the end of the survey, that is, after peers had provided their evaluations. In other words, peers were blind with respect to the goals of the study.

This of course does not exclude the possibility that those who know the target well (family and perhaps friends) may also be aware of the target’s attitudes toward religion and thus be implicitly influenced by a religious prosociality stereotype;[1] or that the more the target is religious, the more family members and possibly friends are likely to be themselves religious and thus inclined to show ingroup favoritism. Interestingly, among peers who evaluated targets’ personality and prosociality and whose ratings were shown later on in the analyses to be associated with targets’ religiosity, one can find work colleagues (Saroglou et al., 2005, Study 4), teachers (McCullough, Tsang, & Brion, 2003; Sallquist, Eisenberg, French, Purwono, & Suryanti, 2010), and various observers (Wink, Ciciolla, Dillon, & Tracy, 2007). These peers can reasonably be considered as rather ignorant of the targets’ religious attitudes and as not selected on the basis of their own religious attitudes. As such, it seems unjustified simply to dismiss all peer-reported evidence on religious prosociality on the basis of the

[1] Note that this is a likelihood but not a certainty. Not all parents, siblings, and friends know what the religious attitudes of their adolescent children, siblings, and friends are. Surprisingly, though, Galen (2012) affirms with certainty that the peers in Saroglou et al. (2005, Studies 3 and 4) “were not blind to the target’s religiosity, and ratings must therefore be interpreted in light of this contamination by rater bias” (p. 879), although nothing from that article allows for such a conclusion to be drawn.

suspicion of ingroup favoritism and activation of relevant stereotypes.[2]

Real (Experimental) Behavior

For reasons of space limitations, I will not comment here on behavioral evidence coming from naturalistic studies (studies on volunteering and charity donations, carried out mostly by sociologists). Nor will I focus on studies on forgiveness, for which I drew a conclusion similar to the author in another review (Saroglou, in press), where I argued, in addition, that forgiving behavior within a religious context may also conflict with competing beliefs and principles (e.g., need for recognition of the fault by the perpetrator and possibly need for expiation). The focus here will be on controlled, experimental studies.

The evidence of many experimental studies measuring cooperation, trust, and generosity in the laboratory is in fact in favor of the link between religiosity and prosocial behavior. The effect is not consistent (some studies failed to provide such effects) and is modest in size; but when studies provide significant results, these confirm the religiosity–prosociality hypothesis as found in self-reports and peer ratings (Saroglou, in press; see also studies reviewed in Galen, 2012). No study, to my knowledge, has found the opposite, that is, religiosity predicting low prosocial behavior in the laboratory (generosity, cooperation, trust, and help), except when the target is an outgroup member threatening religious values.

However, Galen (2012) follows a surprising line of argumentation when reviewing that body of evidence. On the basis of studies showing that when the (religious) ingroup versus outgroup status of the target was clarified, religious prosociality was uniquely applicable to ingroup targets, Galen concludes that the religiosity–prosociality link is nonexistent because it is not universalistic and because it expresses mostly ingroup favoritism.

The first argument seems a bit idealistic, assuming that to be qualified as prosocial, a behavior would need to be proven to be universalistic. In other words, there are different levels and kinds of prosocial behavior; not all of them (perhaps, fortunately) denote "universal altruism." The second argument is based on a problematic assumption. Galen (2012) assumes that in (the many) studies where religiosity predicted prosocial behavior but the group status of the target and the participant was not clarified, the prosociality "may be attributable to an assumption that the target is another religious individual" (p. 882). Similarly, the Christian "identity constitutes a 'default' such that even those who do not disclose a religious identity are presumed to be Christian unless explicitly labeled otherwise," at least in highly religious countries such as the United States, with a strong majority of Christians (p. 879). If this were indeed the case, how could one explain that religiosity also relates to prosocial behavior in highly secularized European countries where only half of the population believe in God (European Commission, 2005), and lab participants are thus unable to make any implicit assumption on the religious status of their partner in the interaction?

Below is an example of a recent study in Belgium (only 43% of the population believe in God; European Commission, 2005) where there was no indication and possibility of assumption about the religious status of the target. Psychology students, after exiting (individually) a lab experiment, were approached by an older "student" who asked them for help with her master's thesis by immediately dedicating 30 min to filling in a questionnaire. Half the students accepted to help her, and half did not; they also provided reasons why they accepted or not. The student (experimenter) then informed participants that the request was just for research purposes. It turned out that religiosity (measured earlier during the previous lab experiment) predicted both helping behavior in a real-life context and prosocial justifications of the decision made (including apologies for unavailability to help; Blogowska, Lambert, & Saroglou, 2012). Of course, filling in religious items earlier in a previous study may have activated a religious prosociality stereotype, or simply the salience of religious norms, beliefs, and emotions (see also Malhotra, 2010; Van Cappellen & Saroglou, 2011), but the effect was real and not an artifact.

Finally, there is need to maintain a conceptual distinction between ingroup favoritism and prosocial behavior, even when the latter is selectively oriented to proximal and ingroup members, or at least does not extend to outgroup members that threaten the group's values. Not all behaviors indicating ingroup favoritism require the costs and sacrifice of time, money, and other resources, nor empathy, control of impulsivity, low hedonism, and high commitment to others, that prosocial behavior does (Mikulincer & Shaver, 2010).

Internal Incoherence and External Counterevidence

The religious prosociality as fallacy hypothesis can also be criticized on the basis of some internal incoherencies in arguments of the review article as well as counterevidence on religion and behaviors related to prosociality. For reasons of space limitations, only three points will be raised here.

Why is the stereotype of believers as being prosocial, as Galen (2012) acknowledges, "ubiquitous," that is, not only shared by religious people, but even found to be endorsed by nonbelievers (Saroglou, Yzerbyt, & Kaschten, 2011)? Is this stereotype so strong and pervasive throughout human history that even nonbelievers endorse it although it is not true? An alternative, more plausible and prudent, explanation is that nonbelievers, who accurately judge believers as conservative and low in impulsivity and hedonism (Saroglou et al., 2011), also accurately give some credit to believers for prosociality.

Moreover, if religious people were only willing to appear prosocial—to themselves or to others—but were in fact not at all prosocial, as suggested by Galen (2012), why do they indeed discriminate between ingroup and outgroup targets already at the level of expression of willingness to help them in hypothetical scenarios (Saroglou et al., 2005; see also Blogowska & Saroglou,

[2] Here is another example of what seems to be a rhetorical reappropriation of existing research. Galen (2012) states: "Although Saroglou et al. (2005) argued that self-reports of morality are *veridical* [emphasis added] and not contaminated by self-deception or self-enhancement (because they are validated by peer and family reports), the experimental evidence suggests otherwise" (p. 890). However, Saroglou and coll. were cautious in their discussion and stated that "peer ratings should still only be considered as indirect indicators of prosocial behavior; between-judges agreement is a question partially independent from the question of the accuracy of the judgment and the latter can be established when behavioral measures are used" (p. 344).

2011) and not only at the behavioral level? Would it be so difficult for them to be a bit more hypocritical and pretend, since they believe they are universalistic in prosociality (Blogowska & Saroglou, 2011), that they would also help the gay person to visit his grandmother (Batson, Floyd, Meyer, & Winner, 1999) or assist the single mother and the gay person who are unemployed (Jackson & Esses, 1997)? An alternative, again more plausible and prudent, explanation is that willingness to help is here, as in other research areas, a valid indicator of real behavior, and thus religious people really tend to be prosocial toward proximal targets and ingroup members but not necessarily toward other people.

An additional argument comes from real-life behavior. Prosocial tendencies of people can be found in some important life decisions such as the choice of a field of study and consequently of a corresponding professional career. I analyzed the data of the European Social Survey (2006–2007), Round 3 (23 countries; $N = 43{,}000$), on religiosity (three indicators: "how religious you are," religious attendance, and frequency of prayer) and fields or subjects of study with the highest education reached (a choice out of 14 categories). It turned out that people who chose fields of study related to the care for others ("teaching, education," "personal care services," and "medical, health services, nursing etc.") had the highest mean scores on religiosity (respectively, 3.41, 3.35, and 3.30 on a 7-point scale from 1 to 7). These fields indeed attracted people with the highest mean scores on valuing benevolence. The fields of study where mean religiosity was the lowest (2.76, 2.65) were "technical and engineering" and "sciences, mathematics, computing etc.," that is, domains that were independent from prosocial concerns. Even if we assume that the religious people made these choices just for self-presentation concerns or to conform to a religion–prosociality stereotype, we have to acknowledge at least that these were choices with long-term consequences for everyday life.

Summary

In sum, self-reported prosociality of religious people is not a simple reflection of social desirability; and peers that are blind with respect to the religious status of the target confirm the religiosity–prosociality link. Moreover, although not consistently, when results are significant, studies using behavioral measures confirm the religiosity–prosociality link, except when the target is an outgroup member. The links between religiosity and prosocial behavior become clearer after religious norms are made salient or relevant positive emotions are induced. Interpreting these findings as consistent with religious prosocial values, emotions, and group-related practices seems in addition more prudent than dismissing them as simple artifacts of (a) a panhuman, transhistorical stereotype of religious prosociality and (b) mere group favoritism.

Two Additional Issues: The Causal Status of Religion and the Curvilinearity Problem

Does Religion Cause Prosociality?

Galen (2012) makes an important point when concluding that "it is therefore premature to suggest that religiosity itself is responsible for such [prosocial] effects" (p. 890) and that, in studies of religious priming and prosociality, "in most cases the causal mechanism is not religious content itself, but the effect of other, more general, secular pathways" (p. 878). More prudently, Galen also points out the importance of validly assessing whether "religiosity itself is a *unique* [emphasis added] causal influence in prosociality" (p. 899). To some extent, this conclusion parallels another one:

> Unfortunately, there is almost no research on the psychological mediators of the religion–prosociality relation. However, there is indirect evidence suggesting multiple possible processes, as religion relates or leads to most of the psychological factors known to play a role in building and promoting prosociality. (Saroglou, in press)

Moreover, regarding religiosity in particular, there is convergent evidence from a variety of research traditions (genetic studies, longitudinal studies, personality theory) suggesting the relevance of even the opposite direction, that is, the one going from prosocial tendencies to religion. A very plausible pathway is that people who are genetically and environmentally predisposed to be prosocial in terms of their personality (agreeable and conscientious) may turn, or remain attached, to religion—if the latter is provided by the environment—as a system that, through beliefs, rituals, norms, and communities, seems to correspond to and possibly solidify people's prosocial aspirations (Saroglou, 2010).

However, although I share Galen's (2012) broad conclusion regarding the causal status of religion and religiosity with respect to prosociality, one should be cautious about some of his underlying assumptions. Indeed, Galen seems to oppose religious explanations of prosociality versus "secular," or "general psychological," explanations. This may be problematic. From a strictly psychological research perspective, it is meaningless to ask whether religious variables (e.g., religious beliefs, religious practice, religious belonging) have "unique causal" effects, that is, beyond the ones of common psychological variables, on any kinds of outcome. By theoretical and methodological self-constraint, psychological research is only interested in finding out through which specific psychological mechanisms different human activities (e.g., art, sport, religion, and attitudes related to them) have some impact on individuals' attitudes and behavior in various domains.

Consequently, the fact that religious priming increases prosocial behavior because it activates fear of authority, as does a similar prime of secular authority (Shariff & Norenzayan, 2007), is not surprising, and it does not dismiss the religion–prosociality causal (or better, to avoid misunderstanding, "influential") link. The same is true when religious priming increases submission to the experimenter among submissive people because it activates submission-related concepts (Saroglou, Corneille, & Van Cappellen, 2009), as probably many other sources of authority can do; or when group belonging, social support, attachment to norms, and many other mechanisms may mediate the religiosity–prosociality link.

Arguing that religiosity itself should not be considered as "responsible" for prosocial effects "unless secular comparisons are used" (Galen, 2012, p. 890) is thus misleading. Secular comparisons may indicate mediators to be used; and they do not rule out the role of religiosity in people's lives. What does, however, make the psychological understanding of religion intellectually interesting is to study what the unique "profile" or configuration is of the psychological mechanisms that together explain religion's and religiosity's role with regard to prosocial behavior. This configuration may, for instance, make religious belonging a sui generis

case in predicting prosociality compared with belonging to an atheist group or identifying with one's own national group. One of the specificities of religion is that it concentrates many psychological functions (Kirkpatrick, 2005), very likely in a unique way compared to other domains of human activity (Hinde, 2009).

Atheists, Low in Religiosity, and Highly Religious: The Curvilinearity Issue

Galen (2012) convincingly argues that the analyses made in the existing research do not allow for clarifying whether the observed difference on prosociality is mainly due to the very religious, compared to the low religious, or to the affiliated or strongly convinced (religious or atheists), compared to the moderately religious or religiously unaffiliated. This is an important issue worthy of much investigation, not only because it will possibly allow for criticizing the atheist–low prosociality stereotype, but also because it will indirectly help to clarify some of the underlying psychological processes.

Nevertheless, it may be interesting to make two comments here. The first is that the author places enormous emphasis on this issue and tends, through it, to almost invalidate most previous research. However, it is empirically totally premature to make any assumptions on whether curvilinear analyses of the religiosity–prosociality links would provide clear and consistent findings across studies, and, in addition, in line with Galen's hypotheses.

Indeed, I reanalyzed previous data of five studies (Saroglou et al., 2005, Studies 2–4; Blogowska & Saroglou, 2011, Studies 1 and 2) by computing each time the linear and curvilinear regressions of prosociality—applied to ingroup members—on religiosity. In all five studies, both linear and curvilinear regressions were significant. However, the curvilinear shape was variable across the studies, and often contrary to what was hypothesized by Galen (2012), with the moderately religious or nonreligious being more prosocial than strict nonbelievers. Moreover, consistently across the five studies, those who were highly religious (i.e., scored 6 and 7 on 7-point scales of importance of God and religion in life) were more prosocial in both self-reports and prosocial intentions compared to nonbelievers (i.e., those who scored 1 in the same items; 1 = *not at all important*, 7 = *very important*). I also computed curvilinear regressions of volunteering and the value of benevolence on religiosity in the European Social Survey (2006–2007) data, Round 3, distinctly for each country. Many of the significant curvilinear shapes confirmed a difference between the low religious and the high religious; and the nonreligious were not necessarily the lowest. However, in almost all cases, the nonreligious were lower than the high religious. This pattern of results is in line with a study by Altemeyer (2010), where Canadian atheists and agnostics reported giving the least (respectively, 1.7% and 1.7%) of their income to social charities, compared to fundamentalists (3.8%) and regular churchgoers (3.1%). (In that study, participants were asked not to count gifts to church and church activities, such as missionary work.)

The second comment is that to increase our understanding of the prosociality of nonbelievers and atheists, other questions may be even more interesting to investigate. As argued elsewhere (Saroglou, in press), atheists and nonbelievers, probably because of lower predisposition for agreeableness and conscientiousness (if we trust the linear associations of these traits with religiosity; Saroglou, 2010), may show prosocial attitudes and behavior less easily, frequently, and intensely. However, because of predisposition to openness to experience (Saroglou, 2012; Streib, Hood, Keller, Csöff, & Silver, 2008), when they show prosociality, they may be less selective and more universalistic than the (very) religious with regard to many kinds of targets. Moreover, because of their high attachment to the value of autonomy and their low authoritarianism (again, if we trust the linear associations; Schwartz & Huismans, 1995; Wink, Dillon, & Prettyman, 2007), nonbelievers may be more autonomous, less submissive to authority, and less conventional in moral reasoning than the very religious, who are often confined to the conventional stages of moral reasoning.[3]

Conclusion

In conclusion, on the basis of the existing evidence, it is empirically premature, even unjustified, and theoretically problematic to conclude that the religion–prosociality link is a fallacy or only exists in (religious) people's mind. The existing evidence urges us to distinguish between three levels of analysis and synthesis when concluding on this research area: (a) the existence of a link between religion or religiosity and prosociality, including prosocial behavior; (b) the possible causal direction between the two and, more importantly, the mediating role of common psychological variables; and (c) the contextual limitations (type of the target, religious aspects made salient, emotions involved, type of religiosity, other moral principles in conflict, elicitors of positive self-image) in the manifestation of this link and the underlying processes. Mixing up these three levels when reviewing the existing literature leads to overstatements such as that, overall, religion causes prosociality and religious people are mostly prosocial, or at least more prosocial than the nonreligious; or, to the contrary, that the religion–prosociality link is overall a fallacy. The truth seems to be somewhere in between.

[3] The same hypotheses (extended and altruistically motivated prosocial behavior, high and autonomous moral reasoning) may apply to people high in spirituality, especially nonreligious spirituality. Indeed, the later construct, similarly to religiosity, reflects agreeableness and conscientiousness but, differently from traditional religiosity, is also associated with low authoritarianism, high openness to experience, and high universalistic values (Saroglou & Muñoz-García, 2008; Saucier & Skrzypińska, 2006; Wink et al., 2007). Therefore, studies investigating the link between spirituality and prosocial behavior are encouraged.

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