To the Editors:

In his recent article, Stuart Kaufman subjects a set of rationalist theories of ethnic conflict to strong criticism and proposes his theory of “symbolic politics” as a superior alternative. Drawing on his earlier work, Kaufman argues that violence between ethnic groups is best understood not as a consequence of security dilemmas, informational asymmetries, commitment problems, or elite manipulation, but instead as a consequence of the content of ethnic groups’ identities, which he calls “myth-symbol complexes.” These complexes are basically mythologized narratives of an ethnic group’s culture and history, which also contain depictions of certain target groups as victimizers or inferiors (pp. 50–51). Feelings of enmity are the result of such narratives, according to Kaufman, and violence is the result of such feelings.

Kaufman tests this theory against the aforementioned rationalist theories in the context of the conflicts in southern Sudan and Rwanda. He concludes that the evidence from these cases strongly supports his theory, while disconfirming the rationalist ones. I argue in this brief comment, however, that students of ethnic conflict should not rush to endorse Kaufman’s conclusions. Even if the rationalist theories in question have problems, they should not be discarded on the basis of his critique. In fact, endorsing Kaufman’s conclusions, and especially endorsing his proposed alternative, would mean not just discarding the theories that are the subject of his criticism. It is not even the rationalist enterprise as a whole that would have to be jettisoned. The symbolic politics theory has something much more general in its crosshairs, namely, theories with structural and material causes. The symbolic politics theory itself is essentially a some-
what more systematic articulation of a popular, but erroneous, belief that ethnic conflicts result from little more than irrational hatreds rooted in culture.4

The errors in Kaufman’s version of this popular belief can be grouped into three general categories. First, his empirical test is not appropriately designed to demonstrate what the symbolic politics theory implies. Second, Kaufman fails to report evidence that could potentially disconfirm his theory. Third, he misinterprets some evidence by arbitrarily redefining certain concepts as consistent with and related to his theory when they are not.

**Design Flaws**

The problems in Kaufman’s empirical analysis become apparent even before combing through the evidence that he presents in the case studies. They begin with the empirical hypotheses Kaufman derives both from his theory and the rationalist alternatives, which are supposed to guide the search for and the analysis of appropriate evidence. He derives six hypotheses from his theory, dividing them into two groups. The first group concerns the preconditions for ethnic war, and the second the mobilization for violent conflict. The first group includes hypotheses about the existence of “widespread group myths [that] explicitly [justify] hostility toward or the need to dominate the ethnic adversary”; the presence of strong “fear[s] of group extinction”; and the existence of a territorial base (p. 58). The second group includes hypotheses about “extreme mass hostility . . . expressed in the media and . . . popular support for the goal of political domination over ethnic rivals”; use of “symbolic appeals to group myths, tapping into and promoting fear and mass hostility”; and the rise of a “predation-driven security dilemma,” where the extremism on one side results in the radicalization of the “leadership on the other” (ibid).

Even a cursory look at these hypotheses makes clear that the successful discovery of confirming evidence for any or all of them will at best demonstrate the existence of hostile myths and hate narratives, and not that a given ethnic conflict was caused by such myths and narratives—unless one assumes that the very existence of such myths and narratives confirms the symbolic politics theory because no other theory in principle can account for them. That, however, would not be a justified assumption, because of the reasonable possibility that causality may run in the opposite direction. After all, it would be difficult to find any conflict that was not accompanied by rallies around the flag, remembrances of heroes and martyrs of wars past, and hateful and hostile rhetoric directed at the opponent. Such behavior invariably accompanies conflicts, because conflicts cause it. An argument can also be made that increased nationalist fervor, attitudes of hate, and contempt directed at an adversary are instrumental, because intense identification with one’s group can be an effective way of increasing internal cohesion and fighter morale,5 while well-conditioned hostility toward or racist contempt for the

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4. Kaufman distances himself from the popularly known “ancient hatred theory,” arguing that “attitudes and even identities in question have varied over time” (p. 45). What is interesting about the ancient hatred theory, however, is the part about hatred, not the part claiming that the hatreds are ancient.

adversary may be psychologically necessary to discriminate, persecute, or kill. This is not necessarily a denial of the possibility that hatred can cause a conflict. But any claim to that effect should involve a serious effort to rule out reverse causality. That is what Kaufman’s null hypothesis should be, not whether a particular conflict was caused by the specific rationalist mechanisms under his scrutiny.

SINS OF OMISSION
The stories Kaufman tells about the conflicts in southern Sudan and Rwanda are incomplete. In the case of Sudan, he is silent about the open and intense dispute over the constitution of the country not long after it became independent in 1956. The population of the south insisted on a federal system, while the Arab-dominated government in Khartoum resisted, insisting on a unitary state. It was the failure of these negotiations that produced the first southern insurgency, which had self-determination as its explicit goal, and which led to a civil war as the Khartoum government refused to concede.6 The conflict evolved, of course, creating new problems, grievances, and demands over the next several decades, but fundamentally it remained about the degree of autonomy the south would have. The basis for such a claim is the content of the negotiations that took place periodically throughout the entire period of the civil war, and especially the content of the negotiations that eventually led to a settlement in 2005.7

In the case of Rwanda, Kaufman does not entirely ignore the long-simmering war that preceded the 1994 genocide, but he says little about its origins. The issue there was not autonomy or secession for a minority, but control of the country as a whole and the inability or the unwillingness of the Tutsi and Hutu elites to agree on a workable power-sharing arrangement following decolonization. Such an arrangement would have ended the domination of the majority Hutu by the minority Tutsi, which had characterized their relationship since the precolonial period. Also incontrovertible are the facts of both Hutu mobilization against precisely this system toward the end of the colonial period and Tutsi resistance against Hutu demands for majority rule. The tensions produced by these conflicting preferences eventually exploded into violence in 1959, which resulted in the exodus of tens of thousands of Tutsis into neighboring countries and the shift of power from the Tutsi to the Hutu.8 This was not the end of the story, however, because the Tutsis who had been forced out refused to accept either their fate or that Rwanda had come under Hutu control. Their periodic attempts both to return and to restore their control over the country, and Hutu resistance against

these attempts, kept the conflict alive in Rwanda for several decades. The conflict eventually culminated in the 1994 genocide, as a powerful segment of the Hutu elite concluded that the power-sharing agreement signed by President Juvenal Habyarimana in 1993 under strong third-party pressure amounted to handing the country back to the Tutsis.9

In an attempt to rebut some potential rationalist counterarguments, Kaufman does acknowledge this, and indeed does not dispute the conclusion that the agreement Habyarimana signed amounted to handing the country back to the Tutsis. But he argues that genocide was not a rational preemptive response, because it was obvious that it would not affect the final outcome (p. 79). Such an argument, however, depends too much on hindsight. It also depends on evidence that the perpetrators themselves clearly saw the strategic futility of genocide, which Kaufman does not provide.

I should clarify what I am not arguing here. I am not arguing that the problem of the degree of autonomy in southern Sudan or the conflicting claims of control over Rwanda were sufficient causes of the bloodshed in those countries. Problems of political status and conflicting demands for power are more ubiquitous than civil wars and genocides. Moreover, the intensity of the conflicts in both southern Sudan and Rwanda varied over time. Additional variables would be necessary to explain that variation, as well as the more general fact that not all potential conflicts of sovereignty and power degenerate into violence. What is at issue, however, is not only, or mainly, the causes of violence in ethnic disputes (including those in Sudan and Rwanda) but the causes of those disputes themselves, which is primarily what Kaufman’s symbolic politics theory is about.10 And as such, it does not demonstrate that the conflicts in question would not have happened in the absence of the hateful mythologies but with the same sort of conflicting preferences over status and power. Nor does it demonstrate that the hateful mythologies themselves and their political deployments were not the consequences of those conflicts. In fact, given the long histories of domination, exploitation, and violence that have characterized the relationships of the ethnic antagonists in southern Sudan and Rwanda, as well as the intense differences they had over the political status


10. Kaufman writes that ethnic myths that justify predatory policy are “a necessary precondition for ethnic war” (pp. 52–53, 81). His theory does have two additional variables, fear and opportunity, that are supposed to explain when these myths are translated into violence (p. 63). He does not, however, operationalize opportunity in a way that would allow one to make systematic predictions about when to expect violence. Any evidence of a myth-symbol complex that fails to produce violence can always be ascribed to the lack of opportunity, which makes the theory at least partially immune to criticism. The opportunity variable plays no role in Kaufman’s empirical analysis anyway. He admits this explicitly by stating that “in Sudan, lack of opportunity is rarely a constraint on the outbreak of ethnic war” (p. 63), and that in Rwanda, “the Hutu elite obviously had the opportunity to organize ethnic violence at any time, as long as they held government power” (p. 73). His treatment of fear has a similar problem: the operationalization of its intensity seems to depend on the outcome.
of southern Sudan and the control over Rwanda, a real puzzle would have been the absence of hate narratives in their cultures.

**SINS OF COMMISSION**

One possible answer Kaufman can offer in response to the criticism above is that he not only accounts for conflicting preferences over status and power, but that he also emphasizes them as integral to his theory, as well as in response to the rationalists’ tendency to assume peaceful, status quo preferences. He argues, more specifically, that “chauvinist leaders always claim to be driven by security motives, but what makes them chauvinists is that they define their group’s security as requiring dominance over rival groups—which is, naturally, threatening to the others. If two or more competing groups feel they need to dominate over the same territory, the result is a security dilemma: neither group feels secure unless its status needs are met, but both sets of demands cannot be satisfied at once” (p. 54). Even assuming for a moment that the claim is empirically justified and ignoring the misunderstanding of what it means to describe a situation as a security dilemma, the claim makes Kaufman’s theory inconsistent ontologically. We are being told simultaneously that ethnic conflicts are the result of hate narratives rooted in ethnic groups’ “myth-symbol complexes” and that they are caused by competition for territory and status. The fallback position would be to argue that power and status maximization are also consequences of certain narratives, but such a move would require a procedure for ruling out power and status maximization for noncultural reasons, which is missing from Kaufman’s theory. Moreover, given that so many cultures seem to be afflicted by the same pathology, it is highly unlikely that culture is the problem.

The more important problem, however, is that dismissing defensive motives in ethnic conflicts is not justified empirically either. Kaufman may be correct in arguing that security is not the sole concern motivating ethnic groups in conflict, but it is unreason-

11. The “dilemma” in the security dilemma is that an actor motivated by security faces the competing risks of doing nothing and being attacked or attacking unnecessarily when there are advantages to attacking first. If actors are motivated by the “need to dominate over the same territory,” there is no dilemma.

12. Some scholars maintain that the universality of certain behaviors and patterns is not inherently problematic for the culturalist paradigm. Alexander Wendt, for example, has framed social identity theory in culturalist terms despite its universalistic character. See Wendt, *Social Theory of International Politics* (Cambridge: Cambridge University Press, 1999). As Jack Snyder has pointed out, however, theories about universal tendencies fit “uneasily with the basic ontology the cultural theorists propound.” See Snyder, “Anarchy and Culture: Insights from the Anthropology of War,” *International Organization*, Vol. 56, No. 1 (February 2002), p. 19.

13. Kaufman may be exaggerating the significance of the assumption, however. The assumption of benign motives should not be interpreted to mean literally that greed is never the motive. The idea rather is to demonstrate that conflicts can occur even when greed is not the motive. In fact, the possibility of greedy motives is indirectly built into the security dilemma. See Andrew Kydd, “Sheep in Sheep’s Clothing: Why Security Seekers Do Not Fight Each Other,” *Security Studies*, Vol. 7, No. 1 (Autumn 1997), pp. 114–154. In addition, there are rationalist theories of ethnonationalist violence that allow for motives that are less benign than security seeking. See, for example, Paul
able to claim that security is never a concern. Note that Kaufman’s claim does not have
the form of a convenient assumption to prove some analytical point. It is fundamentally
an empirical claim. The problem is that the research design of the article, which
consists of two case studies, in addition to passing references to a couple of other
conflicts, is ill suited to demonstrate the veracity of such a claim.14 Moreover, the groups
that are the targets of greed-driven, “chauvinist” mobilizations would likely have in-
centives to countermobilize and (correctly) justify it as necessary to their own security,
which they invariably do using the same hostile and hyperpatriotic language. Reactive
and defensive mobilization is actually one of Kaufman’s empirical hypotheses (p. 58),
but he fails to realize that ruling out security-driven nationalist mobilization in prin-
ciple makes such a hypothesis problematic.

A related tension is evident in the attempt to represent countermobilizations as con-
sequences of myth-symbol complexes. Even though the southern Sudanese were the
targets of the north’s predatory behavior, Kaufman argues that their mobilization was
not simply a defensive response to it, but was rather due to the Dinka “myths” about
their history of enslavement by the Arabs (pp. 62–63). Kaufman does not explain, how-
ever, why the north’s predatory behavior was not enough for the Dinka to counter-
mobilize, if indeed the Arab behavior was predatory. Elsewhere Kaufman describes the
Azerbaijani mobilization against the Armenian demands in Nagorno-Karabagh as mo-
bilization against the “symbol” of Armenian threats.15 Again, if the problem had been
Armenians’ predatory behavior, which is what Kaufman claims, Azerbaijanis would
have mobilized against it with or without any “symbols” of Armenian threats. This ex-
ample also makes one wonder what would not be a symbol or an example of symbolic
politics.

A word is also in order about the treatment of fear in Kaufman’s argument. He
defines all fear-driven behavior as irrational, because fear is an emotion, and goes so far
as to claim that the title of the article by Rui de Figueredo and Barry Weingast—“The
Rationality of Fear”—is an oxymoron (p. 83). Some fears—such as hypochondria—are
definitely irrational. Other fears—such as the fear of dying in a plane crash—are
definitely exaggerations given the statistical risks. But are no fears rational? Surely
some fears result from a correct assessment of threats and dangers. Kaufman goes even
further with the following claim: “Because the fear is subjective, a probabilistic un-
derstanding of its effect is appropriate” (p. 53). It is not entirely clear what the aim of this
sentence is, but the context seems to imply that even the concept of uncertainty belongs

Collier and Anke Hoeffler, “Greed and Grievance in Civil Wars,” Oxford Economic Papers, Vol. 56,
No. 4 (October 2004), pp. 563–595; and Benjamin A. Valentino, Final Solutions: Mass Killing and
14. For how to pose the question regarding the motives behind ethnic civil wars correctly, as well
as the more appropriate methodology for answering such a question, see Nicholas Sambanis, “Do
Ethnic and Nonethnic Civil Wars Have the Same Causes? A Theoretical and Empirical Inquiry
(Part 1),” Journal of Conflict Resolution, Vol. 45, No. 3 (June 2001), pp. 259–282. The argument here is
not against the case study method, but rather for its proper employment.
15. See Kaufman, Modern Hatreds, p. 94.
to the “symbolist” camp, which, of course, is a bizarre claim. That, however, does not stop Kaufman from confidently describing all fear-driven behavior as irrational and chalking up examples of it as supporting evidence for the symbolic politics theory.

CONCLUSION
Kaufman’s article has confused much and illuminated little about the causes of ethnic conflicts. It has essentially dressed up in academic garb an old, but mistaken, belief about the fundamental irrationality and cultural roots of these conflicts. In the process, Kaufman has failed to identify and rule out the correct null hypothesis; he has presented narratives of the conflicts in southern Sudan and Rwanda that fail to account for a number of important facts; and he has introduced a substantial amount of intellectual contraband into the symbolic politics theory to account for certain inconsistencies that it otherwise could not. Students of ethnic conflict, therefore, should treat Kaufman’s findings and conclusions with a healthy dose of skepticism.

—Arman Grigorian
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Stuart J. Kaufman Replies:
In “Symbolic Politics or Rational Choice,” I argue that the outbreak of ethnic war in Sudan in 1983 and the Rwanda genocide in 1994 can best be understood from the perspective of symbolic politics theory. Symbolic politics theory identifies six causal variables for explaining extreme ethnic violence: ethnic myths justifying hostility, fears of group extinction, opportunity to mobilize, extreme mass hostility, chauvinist political mobilization, and a predation-driven interethnic security dilemma. The article further shows that rational choice models fail to account for what occurred in the Sudan and Rwanda cases. Arman Grigorian offers a number of criticisms of this article. In this response, I identify two key areas in which he misunderstands my argument and respond to three other general issues he raises.

WHAT ARE THE ISSUES?
In Grigorian’s view, “What is at issue . . . is not only, or mainly, the causes of violence in ethnic disputes . . . but the causes of those disputes themselves.” Based on this assumption, Grigorian concludes that the point of my article is to advocate the wholesale abandonment of the rational choice enterprise and of all “theories with structural and material causes.”

Grigorian is wrong, however, about both my dependent variable and my theoretical target. As stated in the title and throughout the article, the dependent variable is extreme ethnic violence (i.e., ethnic war and genocide), not the causes of the initial disputes. Nowhere do I deny that conflicts over material goods such as money and power

are the basic stuff of nonviolent politics, including ethnic politics; thus I am not attacking all theories with material causes. Rather, my symbolic politics model, like the competing rationalist models, takes as given the existence of such disputes and asks why in some cases these peaceful disputes escalate to extreme violence.

Because symbolic politics theory is a theory of elite-mass relations, the broader implications of my argument affect primarily scholarly understanding of how politicians communicate with the mass public. Thus, for example, it would challenge theories of the rational American voter, suggesting that primarily symbolic appeals on issues such as abortion and school prayer may explain why many Americans vote against their material interests. It is less applicable to the essentially distributive ethnic politics in African “hegemonic exchange” regimes. In other words, symbolic politics theory does not challenge rationalist bargaining theory where issues are purely distributive or regulatory or when bargaining is confined to elites.

WHAT ARE THE INDEPENDENT VARIABLES?
Grigorian’s second error is his charge of monocausality. He claims that my article defends a “belief that ethnic conflicts result from little more than irrational hatreds rooted in culture.” As stated above, however, my theory identifies six causal variables. Opportunity, chauvinist mobilization, and security dilemmas are just as important as myths, fears, and hostility in explaining extreme ethnic violence according to symbolic politics theory. Thus, according to the theory, it takes much more than just irrational hatreds rooted in culture for ethnic tensions to escalate to extreme violence.

This pretense that my theory is monocausal is required for Grigorian’s claim that I am hostile to all theories with structural causes. In fact, opportunity is a structural variable, and it is a key part of my theory. My argument is that theories that focus exclusively on structural and material causes—for example, the rationalist focus on information failures and commitment problems—are inadequate for explaining extreme ethnic violence. In the cases I examine, information failures and commitment problems were unimportant; what explains the outcomes is a combination of opportunity structure and the myths, fears, hostility, and chauvinist mobilization at the center of symbolist theory.

Grigorian’s assumption of monocausality also seems to underlie his objection to the notion that both “hate narratives rooted in ethnic groups’ ‘myth-symbol complexes,’” and “competition for territory and status” contribute to causing extreme ethnic violence. In fact, all of these factors are comfortably included in symbolic politics theory, which argues that myths, fears, and hate help to explain why disputes over material or nonmaterial goods may turn violent. Grigorian identifies the logic: “power and status maximization are also consequences of certain narratives.” More precisely, symbolic and emotive factors help to explain where groups seek power and status maximization.

tion—and are willing to fight for these maximal goals—and where they remain content with compromise on such issues. Furthermore, status, as distinct from power, is a purely symbolic, nonmaterial good. If Grigorian agrees that pursuit of status motivates political behavior, then he accepts the core of symbolic politics theory.

WHAT EVIDENCE IS NEEDED?

Grigorian asserts that I am guilty of “sins of omission,” that is, of telling the history of the conflicts “incomplete[ly].” He is right: space limitations prevent any article from mentioning all potentially relevant historical details. Grigorian does not show that any of the omitted historical details undermine the argument in the article, however. And I do discuss the key issues he raises, such as southern Sudan’s desire for autonomy and the Hutu-Tutsi competition for power in Rwanda.

Grigorian further asserts that there is a methodological problem: that the analysis fails to account for the possibility that hostile myths may be the result, not the cause, of the conflicts. Again, however, he has the dependent variable wrong, and his analysis is too simplistic. The myths in question are, of course, historical myths in many cases; therefore the myths, at any particular time, are the result of the previous conflictual history. But in both case studies, I identify myths that existed before the outbreaks of violence under study. For example, I identify myths recorded by René Lemarchand before 1974 as among the causes of the Rwandan violence of 1994. This timing rules out the possibility that the myths were created by the politics of genocide in 1993–94. The key Sudanese myths also existed before the events I use them to explain.

Grigorian later concedes the importance both of the conflictual history and of the myths, stating: “given the long histories of [conflict]... in southern Sudan and Rwanda... a real puzzle would have been the absence of hate narratives in their cultures.” Again, his analysis is too deterministic. Anti-American hate narratives are very weak in Japan and Germany, despite the U.S. leveling of several of their cities and occupation of their homelands in the 1940s. Most Tatars do not cultivate hate narratives toward Russians despite centuries of domination and discrimination, and surveys show that they resist ethnic stereotyping. The symbolist insight is that myth-symbol complexes vary in the degree to which they justify hostility toward others, even if past history is conflictual; the symbolist hypothesis is that the probability of violence varies with the degree of hostility in the myths.

WHAT IS A SECURITY DILEMMA?

Grigorian objects to my argument that the predatory motives, not uncertainties about security, drove the security dilemmas in Sudan and Rwanda. He states, “It is unreasonable to claim that security is never a concern,” and asserts that the article “rule[s] out security-driven nationalist mobilization in principle.” The first assertion is right: it would be absurd to claim that security is never a concern, but nowhere does the article

suggest this. Nor does it rule out security-driven mobilization: I concede that a security dilemma driven purely by security uncertainties is logically possible, but it is not what happened in the cases studied.

My argument is that a group’s (or state’s) security needs can be defined narrowly or broadly, and the more control a group (or state) thinks it needs over its neighbors, the more intense the security competition. This insight—that security dilemmas vary in the degree to which they are driven by predatory motives—has long been established in the literature, and has been applied to civil wars by Jack Snyder and Robert Jervis. I am content to share this “misunderstanding” of the security dilemma with Jervis and Snyder.

Empirically, Grigorian’s argument is that southern Sudan’s mobilization should be understood as “simply a defensive response” to northern aggression, not as a result of myth-symbol complexes. Again, the problem is his insistence on a monocausal explanation. Countermobilization in cases such as southern Sudan are certainly defensive responses to enemy attack, but they are not “simply” so. While the Dinka, southern Sudan’s largest ethnic group, have a powerful anti-northern mythology, their Nuer rivals seem to dislike Dinka as much as they dislike northerners. These myth-symbol complexes help to explain who countermobilizes and who does not: whereas the main Dinka reaction to northern attack was defensive countermobilization, the Nuer split, with some collaborating with the northern-dominated government and others resisting it.

**IS GENOCIDE RATIONAL?**

Grigorian’s final concern is to defend the rationality of the Rwandan genocide. First, he asserts that the article claims on page 79 that “genocide was not a rational preemptive response because it was obvious that it would not affect the final outcome.” There is no such assertion on page 79; but on page 80, I argue the opposite: genocide was not a rational preemptive response to the situation in 1994 because it was obvious that it would affect the outcome—adversely from the perspective of the *genocidaires*. If they wished to retain power (and evidence suggests they did), scattering their army to massacre unarmed civilians—while allowing the militarily superior Rwandan Patriotic Front army to drive them out of the country virtually unopposed—was not a rational act. If such behavior can be called rational, then the threshold for rational behavior is so absurdly low that it can mean anything, and therefore it has no analytical power because it excludes nothing. If rationalism is to be scientific, it must be falsifiable, and must therefore concede the existence of some political behaviors that are not rational.

Grigorian later objects to my assertion that de Figueiredo and Weingast’s title, “The Rationality of Fear,” is an oxymoron: some fears are rational, Grigorian argues, resulting “from a correct assessment of threats and dangers.” Here, however, Grigorian con-

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fuses the assessment and response to threat, which may be rational, with fear, which—
according to any dictionary—means entrance into an emotional state. Although it may
be understandable and human to respond to a threat with strong emotions, the rational
response is not to get scared, but to get ready for self-defense. In a complex society, the
emotional instincts of fight or flight are more likely to disrupt than to enhance the prepara-
ations needed for group self-defense. The point is important because it is not the
calm, rational assessment of threat, but the emotional state of agitation against a back-
ground of hostile myths, that explains the distorted logic of people such as Rwanda’s géno-
cidaires and their followers.

Grigorian’s defense of such extreme and false assertions as the rationality of geno-
cide (which is uniformly self-destructive for the perpetrating group), and his equation
of fear with rationality, are disturbing. The Rwanda genocide was both depraved by
virtually any modern moral standard and predictably self-defeating for the géno-
cidaires. It was, in short, stark, raving mad, so the claim that it was nevertheless rational
not only is absurd but also raises serious moral qualms.

The source of the qualms is that rationalists often assert that the rational thing to
do—with rationality usually defined in selfish, short-run, and material terms—is also
the normatively correct thing to do. Even though de Figueiredo and Weingast explicitly
disavow any normative endorsement of the Rwanda genocide—they label the plan for
genocide as “diabolical”—the distancing does not quite work. A logic that simulta-
neously asserts that rational behavior is normative and that even monstrous behavior is
rational is, at best, vulnerable to being hijacked by those who would justify the mon-
strosity. Given that there is already a literature that seems to show that studying ratio-
nal choice theory tends to make students more selfish and less likely to vote, the moral
impropriety of insisting on the rationality of genocide becomes clearer.

CONCLUSION
In sum, Grigorian’s letter has confused much and illuminated little about my article.
He repeatedly misstates the argument in the article, getting the dependent variable
wrong and claiming that the explicitly multicausal argument is somehow monocausal.
He inflates other arguments into straw men, inferring claims the article does not make.
In a rare case in which he correctly states the argument he opposes (that the “rationality
of fear” is an oxymoron), he fails to engage my logic while implicitly endorsing a posi-

5. Emotion, according to Webster’s II, is “1.a. a complex, usu. strong subjective response, as love or
fear,” whereas fear is “alarm or agitation caused by the expectation or realization of danger.” Web-
6. On the link between studying rational choice economics and selfishness, see Robert H. Frank,
of Economic Perspectives, Vol. 7, No. 2 (Spring 1993), pp. 159–171; and Robert H. Frank, Thomas
Gilovich, and Dennis T. Regan, “Do Economists Make Bad Citizens?” Journal of Economic Perspec-
(1999), pp. 39–55. For evidence against the selfishness hypothesis, see Bruno S. Frey and Stephan
Meier, “Are Political Economists Selfish and Indoctrinated? Evidence from a Natural Experiment,”
Economic Inquiry, Vol. 41, No. 3 (July 2003), pp. 448–462.
tion (the rationality of genocide) that is both empirically false and normatively problematic. This is not helpful. The way forward for understanding ethnic conflict, and toward learning how to manage it, is not to insist on the superiority of one’s preferred theory in all circumstances, but to assess in an open-minded way the strengths and weaknesses of all useful perspectives. Rational choice theory is one of them. Symbolic politics theory is another.

—Stuart J. Kaufman
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