WORLD POLITICS

CONTENTS

HOW "FREE" IS FREE RIDING IN CIVIL WARS?
Violence, Insurgency, and the Collective Action Problem

By STATHIS N. KALYVAS and MATTHEW ADAM KOCHER*

> It had been by no means easy to flee into the mountains and to help set up what, both in my opinion and in that of friends little more experienced than myself, should have become a partisan band affiliated with the resistance movement Justice and Liberty. Contacts, arms, money and the experience needed to acquire them were all missing. We lacked capable men, and instead we were swamped by a deluge of outcasts, in good or bad faith, who came from the plain in search of a non-existent military, of political organization, of arms, or merely of protection, a hiding place, a fire, a pair of shoes.
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> —Primo Levi
> *Survival in Auschwitz*

I F rebels face an acute collective action problem, why is counterinsurgency so difficult? The war in Iraq is only the latest example on the list of long, violent, and often failed counterinsurgency efforts. In fact, recent research finds that massively violent counterinsurgency campaigns tend to correlate with enduring guerrilla movements.[1] Why does high individual risk not deter participation in rebellion?

We suggest that this puzzle reflects a serious problem with the current social-scientific understanding of individual participation in violent

[1] Benjamin A. Valentino, Paul Huth, and Dylan Balch-Lindsay, "Draining the Sea: Mass Killing and Guerrilla Warfare," *International Organization* 58 (Spring 2004). Long civil wars, however, tend to display relatively low levels of intensity. See Shivaji Mukherjee, "Low Intensity, Long Duration Conflict: The Maoist Insurgency in India" (Manuscript, Yale University,).

large-scale mobilization, an understanding that is informed primarily by the collective action paradigm.[2] This paradigm provides the theoretical foundation for influential research on civil wars. Recent studies invoke canonically the assumption that rebels face a collective action problem that must be overcome: this is the case with both qualitative microlevel research[3] and econometric large-N studies.[4] Since "the collective action problem for justice-seeking rebellion would usually be insuperable," Paul Collier argues, it follows that insurgencies would tend to be about "greed" rather than "grievance."[5] The same holds for opportunity (or supply) arguments; these assume that the collective action problem has somehow been resolved but fail to delve into exactly how this was achieved.[6]

Using novel and unique data, we revisit the collective action paradigm as specifically applied to the issue of insurgency.[7] This paradigm rests on two pillars: first, the free-riding incentive generated by the

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[2] Mancur Olson, *The Logic of Collective Action: Public Goods and the Theory of Groups* (Cambridge: Harvard University Press, 1965); Gordon Tullock, "The Paradox of Revolution," *Public Choice* 11 (September 1971).

[3] For example, Elisabeth Jean Wood, *Insurgent Collective Action and Civil War in El Salvador* (New York: Cambridge University Press, 2003); Roger D. Petersen, *Resistance and Rebellion: Lessons from Eastern Europe* (New York: Cambridge University Press, 2001).

[4] For example, S. Mansoob Mursheb and Scott Gates, "Spatial-Horizontal Inequality and the Maoist Insurgency in Nepal," *Review of Development Economics* 9 (February 2005); Paul Collier, "Economic Causes of Civil Conflict and their Implications for Policy," in Chester A. Crocker, Fen Osler Hampson, and Pamela Aall, eds., *Turbulent Peace: The Challenges of Managing International Conflict* (Washington, D.C.: United States Institute of Peace Press, 2001). For an extensive review, see Nicholas Sambanis, "Poverty and the Organization of Political Violence: A Review and Some Conjectures," in Susan M. Collins and Carol Graham, eds., *Brookings Trade Forum 2004* (Washington, D.C.: Brookings Institution, 2005).

[5] Collier (fn. 4), 150.

[6] It is implied, for instance, that low state capacity may take the form of occasional state incursions accompanied by indiscriminate violence that minimizes the rebels' collective action problem. In James Fearon and David Laitin's formulation: "Financially, organizationally, and politically weak central governments render insurgency more feasible due to weak local policing or inept and corrupt counterinsurgency practices. These often include a propensity for brutal and indiscriminate retaliation that helps drive noncombatant locals into rebel forces." Fearon and Laitin, "Ethnicity, Insurgency, and Civil War," *American Political Science Review* 97 (February 2003).

[7] Insurgency, defined as "a technology of military conflict characterized by small, lightly armed bands practicing guerrilla warfare from rural base areas," has been the dominant type of civil war in the post–World War II era; Fearon and Laitin (fn. 6), 75. This definition outlines the scope conditions for our argument. On the one hand, we expect wars fought as insurgencies to conform to the logic we specify in this article; this includes all types of civil wars (for example, ethnic and nonethnic), as well as insurgencies against occupation regimes. On the other hand, we exclude conventional interstate or civil wars (for example, Spain 1936–39). Although civilians may be systematically targeted in conventional wars and though this targeting is likely indiscriminate (typically, aerial bombing), its logic and implications diverge from the type of violence common in insurgencies for a straightforward reason: while civilians subject to aerial bombing can be demoralized, they do not have the option of "switching" their support toward a rival actor, an option available in insurgencies where frontlines are often fluid. See Stathis N. Kalyvas, *The Logic of Violence in Civil War* (New York: Cambridge University Press, 2006).

public goods dimension of insurgency and, second, the risks of individual participation in insurgent collective action.

More specifically, we explore an untheorized set of possible participation logics that has been ignored in the literature, while providing some empirical evidence to underwrite this theoretical extension. Our contribution is twofold. Theoretically, we point to an implicit assumption in the literature on insurgency, namely, that nonparticipation is relatively costless. We argue, instead, that the collective action problem applies only if insurgent collective action is risky *relative* to nonparticipation. Further, we specify a perverse selection mechanism that systematically increases the relative risk to noncombatants in insurgencies. Empirically, our data analysis suggests that while it is true that rebels run serious personal risks in war zones, so, too, do nonrebels. In other words, when free riding is costly, rebels do not *necessarily* face a collective action problem in recruiting followers.

Given the dearth of systematic data and the difficulty of observing individual private costs and benefits, we do not test this insight directly; rather, we triangulate among three different bodies of evidence, each of which has its shortcoming and advantages. We begin by pointing to historical evidence from several harsh counterinsurgency operations that indiscriminately targeted entire areas. This evidence is highly suggestive but also unsystematic. We then examine a uniquely detailed data set from a sophisticated program of targeted violence, the Phoenix Program in Vietnam. Using a simple mathematical model, we are able to provide a direct test of our selection mechanism and an indirect assessment of rebel and noncombatant relative risk. We show that even under optimal conditions, the individual risk of nonparticipation approaches that of participation, thus eliminating the collective action problem faced by rebel organizers. The main advantage of the Phoenix data set lies in its detail; its main limitation is that it covers a particular group of people rather than the population at large. Last, we supply systematic regional evidence from the Greek Civil War. The shortcoming here is the absence of external validity; the advantage is that these data cover the entire population of a region, combatants and noncombatants alike. All three bodies of evidence converge in suggesting that the risks of participation in insurgent collective action relative to those of nonparticipation are routinely overestimated. In fact, the puzzle may be the exact opposite of what the literature posits: free riding through nonparticipation is often puzzling given the costs it entails.

Note that our objective is not to formulate a general theory of rebel mobilization or to predict rates of rebel recruitment. Recruitment into

rebel organizations is a function of many factors beyond indiscriminate violence; we also do not claim that violence in insurgencies is always so indiscriminate as to equalize the risk between participation and non-participation or reverse the collective action problem; we recognize that there is likely to be variation among both these possibilities and the classic collective action logic. Our objective is primarily theoretical: we seek to demonstrate that the microfoundations of current theorizing about recruitment do not always hold, and we explore the implications of this demonstration.

COLLECTIVE ACTION AND CIVIL WAR

The collective action problem has been the dominant paradigm for the analysis of political groups and group action in sociology, economics, and political science since it was introduced by Olson in the 1960s. Because the basic outline of this framework is so widely known, we treat it only briefly.

Individuals value many goods that can be produced only through collective action. Collective goods are nonrival and nonexcludable. That I enjoy such a good does not in any way limit your ability to benefit from it; if the good is provided, everyone can take advantage of it. Political goods like democracy, the rule of law, or collective defense are classic examples. Yet individuals also value purely personal goods, such as the time, opportunity cost, or risk involved in acting collectively. In other words, the benefits of collective action are public, while the costs are borne privately. The choice of each individual to work for the collective benefit or not usually has no bearing on its provision. Under these circumstances, every person's best move is to stay home and let someone else work for the public benefit—that is, free ride. If everyone reasons as he or she should, public goods will be systematically underprovided, even when everyone wants them. Olson's logic makes collective action for public goods a puzzle. A considerable portion of the empirical literature since Olson has focused on the use of selective incentives and other mechanisms that explain how the collective action problem may be overcome.

A large body of empirical work has applied the collection action problem to demonstrations, social protest, and social movements—that is, contexts that are not characterized by mass violence.[8] It has also

8 Sidney Tarrow, *Power in Movement: Social Movements and Contentious Politics* (New York: Cambridge University Press, 1998).

been central to theoretically informed work on antistate or antiregime violence. Gordon Tullock was the first to challenge prevailing understandings in the revolutions literature that peasant grievances automatically translate into rebellion. This approach was further elaborated by Lichbach, applied empirically by Popkin, and critically extended by Wood.[9] The application of the collective action framework to the study of insurgent mobilization differs from standard applications in one key respect: the attention it pays to violence and the subsequent shift from an exclusive focus on the public goods and free-riding aspects of rebellion to one that focuses on the costs associated with insurgent participation. As Collier put it, "Rebellion is a full-time commitment and it is dangerous."[10]

Rebel groups typically claim public goods as their goals, with secession, autonomy, democratization, and redistribution appearing to be the most common. Even putting aside the question of how authentic and widely shared these aims are, they are subject to high levels of uncertainty and very long time horizons.[11] The ills of civil war, death of self or family, economic collapse, and forced relocation are immediate, and they affect nonrebels as well as rebels. Given that by 1999, the typical post-WWII civil war lasted sixteen years[12]—with a great many of them concluding with the victory of the state—the expected value of the proffered public goods must be considered small relative to the public ills of civil war. Hence it is not surprising that studies of insurgent mobilization, unlike studies of collective mobilization and protest, have highlighted the costs of individual participation.[13] In Roger Gould's formulation: "While activists might have little trouble persuading a casual acquaintance to sign a petition, they would have great difficulty convincing such a person to risk injury, death, or imprisonment."[14] Thus, the central implication of the collective action paradigm for the study of insurgent collective action is obvious: rebel activists face tremendous obstacles in launching and sustaining insurgencies.

[9] Tullock (fn. 2); Mark Irving Lichbach, *The Rebel's Dilemma* (Ann Arbor: University of Michigan Press, 1995); Samuel L. Popkin, *The Rational Peasant: The Political Economy of Rural Society in Vietnam* (Berkeley: University of California Press, 1979); Wood (fn. 3).

[10] Collier (fn. 4), 150.

[11] Note, however, that (prewar) public order is also a collective good that individuals may prefer not to sacrifice. Failure to join an insurgency is a de facto contribution to the provision of this good. However, scholars have so far failed to give adequate consideration to the value of public order.

[12] James D. Fearon, "Why Do Some Civil Wars Last So Much Longer than Others?" *Journal of Peace Research* 41 (May 2004).

[13] Wood (fn. 3); Collier (fn. 4).

[14] Roger V. Gould, *Insurgent Identities: Class, Community, and Protest in Paris from 1848 to the Commune* (Chicago: University of Chicago Press, 1995), 204.

It is unclear whether the collective action problem applies primarily to the formation of a core political movement or to massive popular recruitment by this movement—that is, to first movers or to late joiners.[15] Our focus in this article is on the latter. First movers, whom Jon Elster aptly calls "everyday Kantians,"[16] are political entrepreneurs with strong motivations, willing to assume high risks. The world does not lack Che Guevaras ready to launch insurgencies—and likely to fail. What it does lack, however, is a mass of followers willing to take the necessary risks; it is the success of entrepreneurs in recruiting followers that results in insurgencies and that calls for investigation: "The Michigan Militia was unable to grow beyond a handful of part-time volunteers," Collier notes, "whereas the FARC in Colombia has grown to employ around 12,000 people."[17]

The puzzle, then, is explaining mass participation given costly sanctions and the public goods nature of rebellion. A first set of answers relies on private selective incentives powerful enough to overcome the cost of participation. Rebels enjoy several types of benefits that are both rival and excludable, such as "loot." The enormous literature on "selective incentives" within the collective action tradition provides ample evidence that rebels often receive private material incentives for participation above and beyond any anticipated public goods.[18] The voluminous civil war literature of the past decade on "greed" and looting is a case in point. A second body of research strays beyond the narrow rationalist paradigm by emphasizing nonrational behavior,[19] pointing to "opportunity structures,"[20] stressing "in-process" benefits,[21] and underscoring social ties.[22]

Unlike these strands, we do not depart from the core rationalist assumptions of the collective action problem, nor do we extend the theory. Our analysis, however, challenges its applicability to insurgen-

[15] This is different from the distinction between onset and duration of civil war. In the empirical literature, onset is computed on the basis of large fatalities, a computation that assumes already substantial operational rebel organizations.

[16] Jon Elster, *The Cement of Society: A Study of Social Order* (Cambridge: Cambridge University Press, 1989).

[17] Collier (fn. 4), 143.

[18] "Selective incentives" is the term used for the individual side payments that are dispensed by organizations to overcome the Olsonian logic of collective action. For evidence of the widespread existence of the private benefits that accrue to rebels, see Lichbach (fn. 9), 215–38.

[19] Edward N. Muller and Karl-Dieter Opp, "Rational Choice and Rebellious Collective Action," *American Political Science Review* 80 (June 1986).

[20] Charles Tilly, *From Mobilization to Revolution* (Reading, Mass.: Addison-Wesley, 1978); Charles D. Brocket, *Political Movements and Violence in Central America* (New York: Cambridge University Press, 2005).

[21] Wood (fn. 3).

[22] Petersen (fn. 3).

cies, which, we argue, are characterized by patterns of violence that systematically, though variably, select nonparticipants for victimization. In fact, we expect that the costs of nonparticipation and free riding often equal or even exceed those of participation: while it is undoubtedly true that rebels run serious personal risks in war zones, war is very dangerous for nonrebels as well. It follows that obstacles to collective action are much lower than typically assumed.

In short, the classic collective action formulation of collective benefits versus private costs misdescribes the nature of a major subset of violent conflicts. While insurgent collective action may entail the expectation of future collective benefits, in fact *public* costs predominate in the short term. Put otherwise, individuals may participate in rebellion not in spite of risk but in order to better manage it. Hence, we argue, the collective action paradigm as applied to civil wars can be both descriptively inaccurate and analytically misleading.

Several scholars have attempted to understand how large-scale violence shapes individual incentives for participation. Some have argued in favor of a linear function whereby repression dampens recruitment by raising the costs to individuals of joining insurgent organizations.[23] Others have specified a concave function whereby participation in protest peaks at intermediate levels of repression but declines thereafter.[24] Finally, others have claimed that the relationship between repression and protest is convex: higher levels of repression increase resentment and, therefore, repression feeds protest and rebellion.[25] Indiscriminate state violence is a central mechanism that explains rebel recruitment in Jeff Goodwin's aptly titled *No Other Way Out*, while both Lichbach and Wood argue along similar lines that rebels successfully incorporate the government's indiscriminate violence into their appeals to recruit sup-

[23] Tullock (fn. 2), 90.

[24] Ted Robert Gurr, *Why Men Rebel* (Princeton: Princeton University Press, 1970); Edward N. Muller and Erich Weede, "Cross-National Variation in Political Violence: A Rational Action Approach," *Journal of Conflict Resolution* 34 (December 1990).

[25] Mark Irving Lichbach and Ted Gurr, "The Conflict Process: A Formal Model," *Journal of Conflict Resolution* 25 (March 1981); David T. Mason and Dale A. Krane, "The Political Economy of Death Squads: Toward a Theory of the Impact of State-Sanctioned Terror," *International Studies Quarterly* 33 (June 1989). Quoting Greene, Lichbach conjectures that violence used by a government against its own citizens may be seen as arbitrary, which would tend to "lower the government's legitimacy and raise the society's revolutionary potential." He concludes that, as a result, "the apathetic become politicized, the reformers become radicalized, and the revolutionaries redouble their efforts. Thus, when the government follows a policy of coercion, the policy itself may become the target of dissent by new challenging groups, thereby spreading conflict and engulfing the entire nation"; in short, repression "radicalizes" previous "free riders" to the revolution. In this formulation of the effects of repression, Lichbach does not consider the violence of repression per se, which may alter an individual's cost-benefit calculation. See Mark Irving Lichbach, "Deterrence or Escalation? The Puzzle of Aggregate Studies of Repression and Dissent," *Journal of Conflict Resolution* 31 (June 1987), 269.

porters. Fearon and Laitin imply a similar mechanism to explain why weak state capacity in peripheral areas, as proxied by per capita GDP, raises the probability of the onset of civil war.[26] Empirical studies using event cross-national data on various types of dissent have provided some degree of support for the "convex conjecture," but the overall picture remains inconclusive.[27] Overall, this empirical literature has refrained from engaging theoretically with the assumption that participation in violent collective action is always "risky," a risk usually conceptualized as the principal (expected) individual cost paid by rebels.

The convex conjecture is, nevertheless, important insofar as it moves beyond the classic collective action dilemma and hints at two alternative descriptions of the relationship between violence and mobilization in civil war, each of which raises distinct theoretical puzzles and explanatory possibilities. In the first scenario rebel and noncombatant risk is roughly equivalent; in the second the risk to noncombatants actually exceeds that to rebels.

The first scenario sidelines both risk and reward in favor of alternative motivational dimensions that have been largely overlooked in the theoretical literature because they fail to provide solutions to the collective action problem. It offers plausible microfoundations for the rehabilitation of strong grievance theories, but it is also consistent with Hobsbawm's classic formulation of "large opportunities and small motivations."[28] The second scenario offers a straightforward endogenous logic of participation, but at the same time it entails a puzzle that is the mirror image of the collective action problem: if nonparticipation carries greater risk, then why do we not observe universal participation in insurgencies? Once violence becomes sufficiently generalized, we should expect to see precisely the opposite of what the collective action literature predicts. Rather than organizations chasing individuals, offering incentives to get them over the risk threshold to cooperation, we should observe that organizations reject willing candidates.

The evidence we present in this article does not permit us to adjudicate rigorously among these possibilities. Because of the obvious obstacles in tackling this question directly, we formulate and supply

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[26] Jeff Goodwin, *No Other Way Out: States and Revolutionary Movements, 1944–1991* (New York: Cambridge University Press, 2001); Lichbach (fn. 9); Wood (fn. 3); Fearon and Laitin (fn. 6), 75–76.

[27] For example Lichbach (fn. 26); Ron Francisco, "Coercion and Protest: An Empirical Test in Two Democratic States," *American Journal of Political Science* 40 (November 1996); Will H. Moore, "Repression and Dissent: Substitution, Context, and Timing," *American Journal of Political Science* 42 (July 1998).

[28] Francisco Gutiérrez Sanín, "Criminal Rebels? A Discussion of Civil War and Criminality from the Colombian Experience," *Politics and Society* 32 (June 2004).

evidence for the following claim: insurgent participation is much less dangerous relative to nonparticipation than is posited by the collective action paradigm and than is generally thought. Our contribution is theoretically far-reaching. Our alternative logics of mobilization have been systematically ignored in the civil war literature; the applicability of the collective action problem has not been tested because the question of its applicability has barely been asked. We suspect that, in fact, there is considerable variation across the landscape of civil wars[29] and perhaps even greater variation over space and time within cases.

A significant obstacle to the development of a research program about the effect of violence on collective action has been the dearth of systematic data—especially disaggregated data. In fact, the problem we are addressing has not been examined systematically within the collective action literature, even though it rests on a vital central assumption of the paradigm. Indeed, it is often ignored completely. Consider, for instance, an excellent recent addition to the literature on dissidence and collective violence that uses a microlevel research design and takes its point of departure from the collective action problem. Wood asserts that the insurgent participation of the Salvadoran agricultural collectivists she studied was "risky," citing evidence that *campesinos* constituted a very high proportion of the victims of state violence.[30] While the evidence is sufficient to establish that being a poor Salvadoran farmer was very risky during this period, there is no way to judge from the evidence how participants in collective action differed from nonparticipants in terms of their average risk.

We suspect the reason for this common elision is a lack of careful analytical and descriptive attention to the nature of warfare in civil war. War differs from other types of violence, in both quantitative and qualitative terms. It is not simply that more people die in wars but that they may be targeted in ways that differ from what happens in other violent contexts. In riots or violent protests, we can reasonably suppose that staying home dramatically attenuates individual risk.[31] When war

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[29] This empirical variation likely accounts for the contradictory results of recent microlevel studies that rely on higher-quality data: Lyall supplies evidence that indiscriminate state violence may be effective against Chechen rebels, whereas Do and Iyer conclude the opposite for Nepal. See Jason Lyall, "Disturbing Fire: A Randomized Evaluation of the Impact of Indiscriminate Violence on Insurgent Reprisals" (Paper presented at the annual meeting of the International Studies Association, Chicago, February 28–March 3, 2007); Quy-Toan Do and Lakshmi Iyer, "Poverty, Social Divisions and Conflict in Nepal" (Manuscript, World Bank, 2006).

[30] Wood (fn. 3), 8–10.

[31] Quite obviously, this claim holds only for those who can choose whether or not to participate, not for those targeted during ethnic riots and pogroms.

passes through an area, however, the risk of being victimized is distributed much more widely.

Perhaps more important is that risk varies across specific types of warfare. In conventional warfare, typical in interstate contexts, combatants almost always run greater risks than civilians. Conventional warfare exhibits frontlines that signal clearly to noncombatants where risk is maximized. Even when armies do not particularly care to discriminate, civilians can increase their security by moving away from the battlefield, while soldiers are concentrated precisely at the point of decision where individual risk reaches its maximum.[32]

By contrast, wars fought in irregular fashion tend to implicate civilians in a more direct and consequential way: rebel fighters hide among the civilian population. This feature of irregular war, also known as the "identification problem,"[33] can be found in many participant observations, including the following one by John Kerry about his experience in Vietnam: "Wherever I went and young Vietnamese men would look at me I grew scared. There really was no way to tell who was who. You could be in a room with one and not know whether he was really a Charlie [that is, a Vietcong] or not. It became easy to sense the distrust that must exist in the outlying areas. How could one really fight in the fields and know whether at any time the men beside you were not going to turn tail and train their guns on you? Whom did you begin to trust and where did you draw the line?"[34]

A consequence of the identification problem is that armed actors may target the civilian population indiscriminately, that is, by using a series of very rough "group profiles," such as ethnicity, locality, sex, or age.[35] If an individual's chances of being victimized depend on a profile rather than on his or her behavior, then shunning participation in the rebellion and free riding may actually prove deadlier than joining it, since the rebels may be able to offer a degree of protection.

That indiscriminate violence may "push the population into the arms of the enemy" has been appreciated in the literature on insurgency and civil war.[36] Yet even violence that is intended to discriminate between

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[32] While it is true that civilians are sometimes targeted systematically in conventional wars (in particular through area bombing), the relative impermeability of frontlines generally leaves them with no options in terms of participation. For this reason, we are unlikely to encounter the kind of endogenous, risk-based logic of participation contemplated here.

[33] Kalyvas (fn. 7).

[34] Douglas Brinkley, "Tour of Duty: John Kerry in Vietnam," *Atlantic Monthly* 292 (December 2003).

[35] As opposed to selective targeting based on tangible evidence about a specific individual's actions. See Kalyvas (fn. 7).

[36] For example, see Goodwin (fn. 26); Mason and Krane (fn. 25).

combatants and civilians may be sufficiently inaccurate to create similar pressures. We explore this possibility empirically in a later section by focusing on a "hard case," in which violence fell disproportionately on the innocent—despite substantial evidence of intent to discriminate.

Whether violence is indiscriminate or merely poorly selective in intent, there are good reasons to suppose that it is perversely selective in effect. Given the identification problem and assuming an actor willing to apply violence selectively, high-quality intelligence is crucial. Yet information may be in short supply for a number of reasons. In particular, the logic of malicious denunciation elaborated by Kalyvas leads us to anticipate that some significant proportion of individually targeted persons in civil war contexts is innocent.[37]

We posit an additional selection mechanism that tends to produce high levels of civilian victimization in insurgencies. Given a pool of individuals targeted for victimization on the basis of individual suspicion, we must assume that some are innocent and some guilty.[38] The proportions are unknown and undoubtedly vary considerably. Nevertheless, across varying contexts of insurgency, we expect that innocents in a pool of suspects are actually victimized disproportionately. The reason is that rebel combatants have access to skills, resources, and networks that should promote their survival relative to noncombatants. Rebel organizations warn their members of approaching raids, provide safe houses, bunkers, escape routes, and food caches, and train their members in concealment, evasion, and survival. In some civil wars, particularly in African countries with their large proportions of the population living close to the subsistence line, combatants may be the only people in a position to avoid war-induced famine.

In the next section we turn to empirics, triangulating among three different bodies of evidence: anecdotal evidence from several harsh counterinsurgency operations, the Phoenix Program in Vietnam, and regional data from the Greek Civil War.

LARGE-SCALE VIOLENCE IN CIVIL WARS

It is possible to study the homicidal violence of war by relying on a distinction between selective and indiscriminate violence based on the

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[37] See Kalyvas (fn. 7).

[38] Why should we assume this? Experience with the peacetime court systems of advanced industrial countries teaches that distinguishing guilt from innocence is an inexact science. In civil war the scale of the problem alone should increase the proportion of false positives. The logic of malicious denunciation leads us to anticipate an even higher proportion of junk intelligence in insurgencies. The selection mechanism we elaborate adds insult to injury.

level at which "guilt" (and hence targeting) is determined.[39] Violence is selective when targeting requires the determination of individual guilt; it is indiscriminate when targeting is based on guilt by association or collective guilt. Note that though indiscriminate violence is often associated with mass killing,[40] our distinction is independent of the scale of targeting: selective violence can be massive while indiscriminate violence can be limited. The Phoenix Program in Vietnam was massive despite being selective in intent, whereas recent violence in Kosovo is both limited and indiscriminate, as targeting is ascertained primarily by group membership. States and rebels engage in a variable mix of selective and indiscriminate violence, primarily as a function of their degree of local knowledge.

Indiscriminate violence is a common, though far from universal, occurrence in civil wars. For example, "scorched earth" campaigns waged by the German and Japanese armies in occupied territories during World War II, by the Guatemalan army in the 1980s, and by the Serbian army in Kosovo have been described as indiscriminate instances of mass violence. This practice has been traced to a combination of factors, including racist ideologies and lack of resources for more selective counterinsurgency.[41]

Few observations enjoy more currency among historians than the futility of the German and Japanese antipartisan reprisals. "Whatever the purpose of the German policy of reprisals," a study of occupied Greece points out, "it did little to pacify Greece, fight communism, or control the population. In general, the result was just the opposite. Burning villages left many male inhabitants with little place to turn except guerrilla bands. Killing women, children, and old men fed the growing hatred of the Germans and the desire for vengeance." German observers in neighboring Yugoslavia "frankly concluded that rather than deterring resistance, reprisal policy was driving hitherto peaceful and politically indifferent Serbs into the arms of the partisans." In occupied France, "when the acts of reprisal are added to the indiscriminate round-ups and the residue of Vichy collaborationism, the pressure on the population in a multitude of localities to look to the *maquis* as a place of refuge, or as a receptive and mobilizing organization, was high." Historians of the French resistance link its development to German forced labor dragnets: rather than go to Germany as industrial laborers, many

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[39] This discussion draws on Kalyvas (fn. 7).
[40] Valentino, Huth, and Balch-Lindsay (fn. 1).
[41] Stathis N. Kalyvas, "The Paradox of Terrorism in Civil War," *Journal of Ethics* 8 (March 2004).

Frenchmen opted to join the Resistance, which they had not considered before the German initiative. Participation in the French *maquis* took off only after the Germans began to forcibly recruit laborers.[42] Nazi reprisals produced a similar effect all over occupied Europe, while Japanese reprisals had similar effects in Asia.[43] Several mechanisms underlie these counterproductive effects of indiscriminate violence, including emotional reactions and norms of fairness, an ambiguous structure of incentives, reverse discrimination, the production of selective incentives for rivals, and the systematic overestimation of the strength of ties between political actors and civilians.[44]

When the state relies on indiscriminate violence, insurgent organizations may respond by providing protection to the targeted civilian population. Survival-maximizing civilians will then be more likely to join such an organization than they would otherwise have been. This process has been systematically studied in the case of the Salvadoran Civil War: using a rare representative sample of former combatants and noncombatants, Viterna finds that a significant number of women who joined the insurgency did so primarily on survival grounds. They were pushed to join by the army's scorched-earth campaign and thus became what she calls "reluctant" guerrillas.[45] Clearly, in the face of indiscriminate state violence, not joining an insurgency (an act framed in most studies as free riding) can be more costly than participation.

Three observable implications follow. First, many rebel organizations are likely to welcome and even provoke indiscriminate state reprisals—as, indeed, several observers have noted.[46] Second, indiscriminate

[42] Respectively: D. M. Condit, *Case Study in Guerrilla War: Greece during World War II* (Washington, D.C.: Special Operations Research Office, American University, 1961), 268; Christopher R. Browning, "Germans and Serbs: The Emergence of Nazi Antipartisan Policies in 1941," in Michael Berenbaum, ed., *A Mosaic of Victims: Non-Jews Persecuted and Murdered by the Nazis* (New York: New York University Press, 1990), 68; H. R. Kedward, *In Search of the Maquis: Rural Resistance in Southern France, 1942–1944* (Oxford: Oxford University Press, 1993), 190.

[43] Mark Mazower, *Dark Continent: Europe's Twentieth Century* (London: Allen Lane, 1998); Lincoln Li, *The Japanese Army in North China, 1937–1941: Problems of Political and Economic Control* (Tokyo: Oxford University Press, 1975).

[44] Kalyvas (fn. 41).

[45] Two snapshots from El Salvador: when asked why he joined, an insurgent answered that he "had no choice. . . . It was a matter of survival. Those were the days when *not* to go meant getting killed." Likewise, a woman said that "in this war, you don't get involved because you want to, but because you have to. Because if you don't, they kill you. Even though you didn't know anything about the war." Quoted respectively in Jon Lee Anderson, *Guerrillas: Journeys in the Insurgent World (*New York: Penguin, 2004), 222; and Jocelyn Viterna, "Pulled, Pushed, and Persuaded: Explaining Women's Mobilization into the Salvadoran Guerrilla Army," *American Journal of Sociology* 112 (July 2006), 24.

[46] Paul Aussaresses, *Services Spéciaux Algérie 1955–1957: Mon témoignage sur la torture* [Algeria Special Services 1955–1957: My Testimony on Torture] (Paris: Perrin, 2001), 62; William Hayden, "The Kosovo Conflict: The Strategic Use of Displacement and the Obstacle to International Protection," *Civil Wars* 2 (Summer 1999), 57.

state violence may give rise to club goods for the rebels, as protection against it can be used selectively and can be made available only to certain individuals and communities.[47] For instance, as a sanction for tax evasion, the Vietcong forced offenders to stay in hamlets that were shelled by the government army.[48] In this way indiscriminate state violence may turn into an extremely counterproductive weapon, as the decision by insurgents not to protect a village that is unfriendly to them amounts to exposing it to state violence—that is, to using one's enemies as one's own enforcers. Third, weak rebel organizations will fail to offer protection to civilians. Consequently, civilians will flee from areas targeted by the army, as was the case most recently in Darfur, or (given the option) they will defect to the government side, as witnessed in Guatemala, Peru, or Chechnya.[49]

This last point suggests that indiscriminate state violence in and of itself does not automatically result in the growth of an insurgency: the rebels' ability and willingness to capitalize on it are necessary factors intervening between indiscriminate violence and rebel recruitment. For instance, the introduction of rebel violence into the equation may tilt the balance even more toward the rebels: faced with a mix of selective violence and protection on one side and indiscriminate violence on the other, most civilians are likely to join the rebels.[50]

A final issue concerns the nuances of indiscriminate violence. Could it be the case that what appears as indiscriminate violence against an undistinguished civilian population may, in fact, be targeting primarily civilian collaborators of the rebels as opposed to civilians in general? There is no reason to believe that indiscriminate violence will target primarily individuals who are actively collaborating with the rebels as opposed to nonparticipants. Precisely because indiscriminate violence reflects the absence of local information about individual behavior, we should expect the distribution of victims to be at the very least random in terms of the degree of their participation. The sad truth is that in-

[47] Odoric Y. K. Wou, *Mobilizing the Masses: Building Revolution in Henan* (Stanford, Calif.: Stanford University Press, 1994), 231; Timothy P. Wickham-Crowley, *Exploring Revolution: Essays on Latin American Insurgency and Revolutionary Theory* (Armonk, N.Y.: M. E. Sharpe, 1991), 43; Edward G. Lansdale, "Viet Nam: Do We Understand Revolution?" *Foreign Affairs* 43 (October 1964), 85.

[48] David W. P. Elliott, *The Vietnamese War: Revolution and Social Change in the Mekong Delta, 1930–1975* (Armonk and London: M. E. Sharpe, 2003), 873.

[49] David Stoll, *Between Two Armies: In the Ixil Towns of Guatemala* (New York: Columbia University Press, 1993); Carlos Iván Degregori, "Harvesting Storms: Peasant *Rondas* and the Defeat of Sendero Luminoso in Ayacucho," in Steve J. Stern, ed., *Shining and Other Paths: War and Society in Peru, 1980–1995* (Durham and London: Duke University Press, 1998); Valery Tishkov, *Chechnya: Life in a War-Torn Society* (Berkeley: University of California Press, 2004); Lyall (fn. 29).

[50] Kalyvas (fn. 7).

nocent people get killed all the time in civil war contexts.[51] If anything, there is evidence suggesting that less motivated collaborators and the non-collaborators may be easier targets in an instance of perversely reverse discrimination. For example, it turns out that the people killed by the Salvadoran army during the massacre of El Mozote "were the least *decidido* ("persuaded," "convinced," but meaning, in this context, "politically committed"). . . . Prior to the massacre, about 70 percent of the prewar inhabitants of El Mozote left; several dozen of these had enlisted in the ranks of the [insurgent] ERP or supported the government. Those who did none of these things were murdered."[52] A Greek villager recounts that "one evening the Germans raided our village and caught all the men they found at home. In fact, they found and caught precisely those men who were not associated with [the partisans] and had, thus, no reason to fear. They did because those who had made up their minds [and were associated with the partisans] used to leave the village at night and sleep outside."[53] In such contexts, the belief in one's own innocence can be fatal, unlike the awareness of one's own guilt. A Greek man recalls how he warned several suspected communist collaborators of an imminent raid by the Germans. A former communist did not flee, reasoning that because he had severed his links to the party and was not involved in any clandestine activity, he had no reason to run away. He was arrested and executed, whereas the real communist collaborators were able to escape.[54]

In short, anecdotal evidence from large-scale state violence suggests a pattern consistent with our argument: nonparticipation (free riding) is not necessarily a costless alternative to participation. In the next section, we turn to systematic evidence from Vietnam.

THE PHOENIX PROGRAM

Although the historical evidence is highly suggestive, it remains unsystematic. Estimating the relative risk faced by rebels versus nonrebels raises an exceptionally difficult empirical problem, because civil war tends to degrade data-collection capabilities and because rebel actors

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[51] For instance, a U.S. marine was given a sentence of eight years for killing an Iraqi civilian "who was known to support the American occupation"; *New York Times,* February 19, 2007, A6.

[52] Leigh Binford, *The El Mozote Massacre: Anthropology and Human Rights* (Tucson: University of Arizona Press, 1996), 115.

[53] Alexandros Svolos, *Andartis sta vouna tou Moria: Odoiporiko (1947–49)* [Guerrilla in the Mountains of Moria: A Journey (1947–49)] (Athens: Self-published, 1990), 22.

[54] Michalis Papakonstantinou, *To chroniko tis megalis nichtas* [Chronicle of the Long Night] (Athens: Estia, 1999), 313.

often conceal their identities intentionally. Even systematic knowledge of the identities of the victims of large-scale violence—information that is now available for some civil wars—will generally not be sufficient for our purposes, because organizational memberships often remain unknown. Absent extremely detailed ethnographic/historical work, which is feasible only on a small scale, it can be very difficult to distinguish combatants from civilians.

In this section we examine a unique data source from the Vietnam War to try to improve our grasp—for at least one civil war—of the rate of victimization of participants in violent collective action relative to the larger population. By analyzing the individualized targeting data left behind by the notorious U.S. Phoenix Program (and making some fairly cautious assumptions), we derive an estimate of the proportions of Vietcong agents and civilians killed. Although Phoenix accounted for only a fraction of the overall violence of the war (in particular, it covers only state violence), there is a reasonable argument to be made that it represented the *best* attempt of the U.S. and South Vietnamese governments to target the Vietcong selectively and to avoid civilian casualties. Consistent with the qualitative literature on the subject, we find that Phoenix was wildly inaccurate, killing or otherwise victimizing numerous civilians for every legitimate Vietcong member.

The Phoenix Program was a joint intelligence-gathering and coordination system designed to identify and "neutralize" clandestine agents of the Vietcong in South Vietnam. In essence, it was a clearinghouse for information gathered by numerous military and police organizations operating as part of the U.S. alliance. The intelligence gathered by Phoenix could then be used to target individuals for capture or assassination more accurately.[55] Phoenix was oriented toward identifying and disposing of the most important Vietcong agents, "executive cadre at all levels of the communist apparatus."[56]

The program emerged as a response to the need for discrimination in targeting. As a CIA operative recalls in his memoirs, by 1971 the war was transformed into "one in which whom we killed was far more important than how many we killed."[57] The best evidence for the explicit intent toward selectivity of targeting comes from the actual data:

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[55] Ralph William Johnson, *Phoenix/Phung Hoang: A Study of Wartime Intelligence Management* (Ph.D. diss., American University, 1982).

[56] Thomas C. Thayer, *War without Fronts: The American Experience in Vietnam* (Boulder, Colo.: Westview Press, 1985), 208. Note that the intended targets of the Phoenix Program were not full-time combatants but rather were clandestine operatives.

[57] Stuart A. Herrington, *Stalking the Vietcong: Inside Operation Phoenix—A Personal Account* (Novato, Calif.: Presidio Press, 1997), 69.

alleged Vietcong agents were identified by name, alias, date, and place of birth. Where available, fingerprints and photographs were compiled, and an organizational profile was assembled. The information was then widely disseminated to the various military forces, police organizations, and official militia forces involved in prosecuting the war, sometimes in the form of "wanted posters." According to Johnson, the Phoenix Program "put the United States and the Government of Vietnam eyeball to eyeball with the Viet Cong Infrastructure on a daily basis."[58]

At the end of the war Phoenix left behind a database, the National Police Infrastructure Analysis Subsystem II—NPIASS-II—containing a summary of all the information held on each individual, including a record of each person's status as of the close of the program: captured, killed, defected, or "at large."[59] These data are completely unique. We are unaware of any other civil war combatant that has left behind a systematic (if partial) record of its intended victims and their eventual fates. The Phoenix Program identified 73,697 individuals as members of the clandestine Vietcong infrastructure in South Vietnam. By the end of the war 15,438 people, or about 21 percent of those selected, had been killed. We use this and other information in the database to derive an estimate of the percentage of Phoenix's victims who were innocent.[60]

It is important to keep in mind at the outset that we have no way of knowing for sure how representative the individuals targeted by the Phoenix Program were of the South Vietnamese population as a whole. We do not know how the people targeted were chosen, though we have strong theoretical priors and considerable anecdotal evidence suggesting that actual guilt was, at best, weakly correlated with selection. Thus, our analysis can be only an indirect assessment of the overall relative risk to civilians and Vietcong members posed by state violence. Our analysis is, however, a direct test of the perverse selection mechanism outlined above. That is, we are able to show that, given a pool of individuals targeted for violence, those most likely to be innocent were precisely the most likely to be victimized.[61]

[58] Johnson (fn. 55), 307.

[59] National Archives and Records Administration (NARA), *National Police Infrastructure Analysis Subsystem—II* (Records Group 330. Accession no. 3-349-79-002-D).

[60] It is important to keep in mind that the Phoenix Program counted *all* of the individuals on the list as Vietcong agents. It is our assumption, based on a reasonable understanding of insurgency processes, that many of these individuals were incorrectly identified. Our task is to determine how many.

[61] Note, as well, that we have no way of ascertaining the effect of this perverse selection on insurgent recruitment. Kalyvas (fn. 7) suggests that a key variable in the efficacy of selective violence is not accurate selection per se but, rather, is the perception among the population that targeting is based on accurate selection. Such analysis would require data currently not available in the case of the Vietnam War.

Although the full extent of the suspicion against each individual is unknown, at the beginning of 1971 Phoenix began to self-evaluate the quality of its evidence using a single binary variable. An individual who had been identified as a Vietcong agent either by three or more independent sources or by a single "irrefutable source" was labeled "confirmed," while those under a lesser degree of suspicion were labeled "unconfirmed." We do not know what counted as a source; nor do we know the standard of confirmation for independence among sources. About 11 percent of all the persons identified in the Phoenix list met this standard of confirmation.

A simple cross-tabulation of confirmation and status tells a remarkable story of capricious violence. Table 1 compares the eventual fates of those in the confirmed and unconfirmed categories. From the beginning of 1971 until the time the database was closed, about 4.5 percent of those in the confirmed category had been killed, while roughly 94 percent remained at large. By contrast, 20 percent of the unconfirmed had been killed; additional large percentages had been captured or had defected to the government—roughly 32 percent and 15 percent, respectively—while only 32 percent remained at large. In other words, Phoenix divided its pool of supposed Vietcong agents into two categories, one of high suspicion and one of low suspicion. In a truly awesome process of perverse selection, those under low suspicion were almost five times more likely to be killed than those under high suspicion. The unconfirmed were nearly twenty-four times more likely to be captured than the confirmed, while nearly ninety-four out of every hundred highly suspicious individuals escaped the U.S. and South Vietnamese net entirely. Of those killed, nearly 97 percent came from the unconfirmed category.

In somewhat different terms, the odds ratio of killed/at large in the unconfirmed category to killed/at large in the confirmed category is approximately 13.11, while the same odds ratio for killed + captured is about 26.14. That is, an individual in the unconfirmed category had close to thirteen times greater odds of being killed and twenty-six times greater odds of being captured or killed. The simplest and most plausible explanation for these data is that the confirmation process was reasonably successful at distinguishing real Vietcong agents from innocents. To elaborate: the Phoenix Program had two selection mechanisms. The first divided the population into two groups: those on the list and those off the list. The second mechanism divided those on the list into confirmed and unconfirmed. It seems unreasonable to assume that the U.S./GVN directed *more* effort toward locating and "neutraliz-

TABLE 1
PHOENIX PROGRAM RESULTS BY CONFIRMATION STATUS FROM JANUARY 1971

Status	Confirmed Vietcong	Unconfirmed Vietcong	Total
Killed	4.53%	20.32%	18.16%
	(366)	(10,341)	(10,707)
Captured	1.35%	32.21%	27.98%
	(109)	(16,392)	(16,501)
Defected	0.21%	15.33%	13.26%
	(17)	(7,801)	(7,818)
At Large	93.91%	32.14%	40.60%
	(7,587)	(16,355)	(23,942)
Total	100.00%	100.00%	100.00%
	(8,079)	(50,889)	(58,968)
Killed + Captured	**5.88%**	**52.53%**	46.14%
	(475)	(26,733)	(27,208)

SOURCE: U.S. Department of Defense, NPIASS-II.
$\Pr(\chi^2) = 0.000$

ing" unconfirmed persons than confirmed ones.[62] A reasonable (though conservative) assumption is that effort levels were identical for the two groups.

How, then, can we explain the much higher survival and escape rates for confirmed persons? The most sensible assumption to make is that "confirmation" was an effective selection procedure, in the sense that it chose real Vietcong members from the larger list in much higher proportion to their total numbers than it did innocents. Real Vietcong agents had a range of organizational resources they could draw upon to avoid capture or assassination at the hands of counterinsurgency forces, whereas incorrectly identified innocents were completely exposed. Unconfirmed persons were victimized at higher rates not because counterinsurgents wanted it that way but rather because it was easier to find them and to subdue or kill them. A further point in support of this interpretation is that virtually nobody in the confirmed category defected, while defection was common among the unconfirmed—precisely what we would expect if the confirmed category were largely composed of actual, committed Vietcong.

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[62] According to NPIASS-II, Phoenix was aware of the "current address" of nearly 64 percent of confirmed persons but less than 1 percent of unconfirmed persons. Likewise, nearly 23 percent of confirmed persons but less than 1 percent of unconfirmed persons were the subjects of individualized arrest warrants. These data strongly suggest that significantly greater effort was oriented toward those under higher suspicion.

Given this general interpretation of the data, we use a simple mathematical model to estimate the ratio of civilians to Vietcong victimized by Phoenix. We make three key assumptions. First, we assume that the individuals in the Phoenix database can be partitioned into two stable and mutually exclusive groups: Vietcong (V) and innocents (I). It follows from this assumption that the "confirmed" and "unconfirmed" categories can likewise be partitioned into Vietcong and innocents, resulting in four integer-valued variables and two equations as follows:

V_u $\equiv$ number of Vietcong in unconfirmed category
I_u $\equiv$ number innocents (civilians) in unconfirmed category
V_c $\equiv$ number of Vietcong in confirmed category
I_c $\equiv$ number innocents (civilians) in confirmed category

$$V_u + I_u = 50889 \tag{1}$$

$$V_c + I_c = 8079 \tag{2}$$

The equations follow analytically from the first assumption and the totals of confirmed and unconfirmed persons in the data (see Table 1). The first assumption is the most controversial, and we offer a more detailed defense of it below. Second, we assume a constant proportion of victims for Vietcong (p_{vn}) and a constant proportion for innocents (p_{in}), in each case independent of their confirmation status.[63] That is, we stipulate not that confirmation itself affected rates of victimization but rather that these were determined by the characteristics of the actors (whether they were Vietcong agents or not). Note that this is a conservative assumption, since we should expect more effort to be directed toward those under greater suspicion. The assumption is operationalized in equations 3 and 4:

$$p_{vn}(V_c) + p_{in}(I_c) = 475 \qquad 0 < p_{vn}, p_{in} < 1 \tag{3}$$

$$p_{vn}(V_u) + p_{in}(I_u) = 26733 \quad 0 < p_{vn}, p_{in} < 1 \tag{4}$$

In other words, equation 3 tells us that the total number of confirmed persons victimized—captured or killed—can be expressed as the sum of two quantities: the number of Vietcong agents in that category times their rate of victimization plus the number of innocents in the category

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[63] p_{vn} stands for "proportion of Vietcong neutralized" and p_{in} stands for "proportion of innocents neutralized."

times their rate of victimization. Equation 4 follows the same logic for unconfirmed persons.

Third and finally, we assume that the odds ratio of innocents/Vietcong in the unconfirmed group to innocents/Vietcong in the confirmed group is the same as the odds ratio derived from Table 1 of victimized/at large in the unconfirmed group to victimized/at large in the confirmed group.[64] This assumption is summarized in equation 5:

$$\frac{\dfrac{I_u / 50889}{V_u / 50889}}{\dfrac{I_c / 8079}{V_c / 8079}} = \frac{I_u V_c}{I_c V_u} = 26.14 \tag{5}$$

The figures 8079 and 50889 are derived from the data and represent the total number of confirmed and unconfirmed persons, respectively. The odds ratio compares (captured + killed)/at large across the confirmed and unconfirmed categories.[65] Substantively, this assumption tells us that it is the composition of the two categories, confirmed and unconfirmed, and in particular their respective ratios of Vietcong and innocents that account for the observed difference in rates of victimization. The assumption is natural in the following sense: given that confirmation was a more selective process than simple inclusion on the Phoenix list, we should expect it to have been more accurate. Hence, the confirmed group should have a higher ratio of Vietcong to innocents than the unconfirmed group.

Returning to the first assumption, that membership and nonmembership are well-defined, stable, and exclusive categories, is an important simplifying stipulation that we need to make in order to gain leverage. The Vietcong was a highly sophisticated military/bureaucratic structure, differentiated according to both ranks and functions. Following from this insight, another possible interpretation of the data is that

[64] There is some disagreement in the methodological literature over the use of the odds ratio to express the size of effects. Note that this issue has no bearing on our analysis, because we do not attempt to use the odds ratio as an absolute measure of anything. We merely equate the odds ratios of two different comparisons.

[65] Why use killed + captured instead of just killed? Both capture and assassination require that an individual be physically located and identified by forces sufficient to take action against him. Still, this is a conservative assumption. Since defection was a good way to avoid being killed or captured (and sentenced to prison), we might expect innocent but threatened individuals to take this option at higher rates than would highly committed Vietcong agents, essentially shrinking the pool of potential unconfirmed victims faster than the pool of potential confirmed victims.

important Vietcong agents were able to escape capture or assassination, while rank-and-file members were killed at rates similar to, or only slightly lower than, innocents. This interpretation—that Phoenix was good at locating rank-and-file Vietcong agents but poor at getting high-ranking agents—is the one adopted by Thayer.[66]

Perhaps important agents were more likely to be confirmed but less likely to be caught than rank-and-file participants. Although the latter supposition seems quite plausible, the former seems less so. If the physical security of Vietcong was better protected at higher levels of the organizational chart, we would expect their identities to be more closely guarded secrets as well. Consequently, we would expect, if anything, that high-ranking agents were less likely to be confirmed.

In any case, to evaluate this supposition rigorously would require more fine-grained data than we have available. NPIASS-II does, however, include some information about the beliefs held by Phoenix managers themselves regarding the importance of the people on the list. The data tell us, for instance, whether or not a suspect was believed to be a full or probationary member of the Communist Party. Phoenix also includes agents' supposed "echelon," or the scale of operations in which they were involved (for example, region, province, district, village). Individuals operating at a higher scale were generally assumed to be more important.

Forty percent of those on the Phoenix list were believed to be Communist Party members. According to the data, roughly the same percentage of confirmed and unconfirmed persons were party members. Controlling for party membership has little effect on our main results. While among full party members, the inverse relationship between neutralization and confirmation is weaker than among ostensible nonmembers, it is much stronger among probationary party members. The inverse relationship is strongest among those whose party membership is unknown. The bivariate data on party membership and "neutralization" are equally inconclusive. Full party members were less likely to be captured than nonparty members but were more likely to be killed. Probationary party members were more likely to be killed or captured than full members, nonmembers, or those whose membership status was unknown. Similarly, controlling for echelon does not affect the

[66] Thayer (fn. 56). As an important Defense Department analyst during the Vietnam War, Thayer had access to all the data we use here, as well as to much that remains classified. His book does not consider the possibility we suggest, perhaps because it was simply inconceivable to him that the Phoenix Program was not merely unproductive but was actually counterproductive. Although it is true that the majority of people "neutralized" by Phoenix were believed to be low ranking, this result follows straightforwardly from the pyramidal form of any military or bureaucratic organization.

results. While there are differences in the relationship between confirmation and neutralization across the echelon categories, they are small and do not correspond to a clear pattern. The data suggest at best a very weak relationship between perceived importance and the rates at which individuals were victimized.

A second alternative interpretation of the Phoenix Program data is that our findings about confirmation status arise from systematic measurement error. On this account, confirmed persons were less likely to be victimized overall only because confirmation took time. Everyone entered the database unconfirmed; some were later confirmed, but only if they remained alive long enough for sufficient evidence to accumulate against them. Thus, confirmation is a consequence of survival, rather than (as we assert) the consequence of individual characteristics.

We cannot rule out this possibility definitively, because the Phoenix database reports neither individuals' dates of entry nor their dates of confirmation. However, there are important hints that this particular form of measurement error is minimal. First, we know from the information contained in the database that Phoenix continued to collect information on captured persons, many of whom were subsequently brought to trial and imprisoned. Both the circumstances of capture, as well as information obtained through interrogation, would have been highly relevant to confirmation.[67] If survival were causing confirmation, we would expect a higher proportion of captured than of killed to be confirmed. Yet as we have seen, captured persons were much *less* likely to be confirmed than those who had been killed. Second, were this story true, we would expect the rates of neutralization of confirmed and unconfirmed persons to converge over time, reaching rough equality in the final months before the database was closed. Figure 1 shows the monthly trend in the proportions of confirmed and unconfirmed persons killed or captured. While there is a slight increase in the proportion of confirmed persons victimized and a slight trend toward convergence in the middle of this period, the respective rates of victimization remain very different throughout.

Returning to summarize our model, we have the following system of five equations in six variables:

V_u ≡ number of Vietcong in unconfirmed category
I_u ≡ number innocents (civilians) in unconfirmed category

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[67] Indeed, the files of captured persons were updated an average of 1.3 times, much more often than were the files of those who were killed (0.33 times), who rallied (0.33), or who remained at large (0.59).

V_c ≡   number of Vietcong in confirmed category
I_c ≡   number innocents (civilians) in confirmed category
p_{in} ≡   proportion of innocents victimized
p_{vn} ≡   proportion of Vietcong victimized

$$V_u + I_u = 50889 \tag{1}$$

$$V_c + I_c = 8079 \tag{2}$$

$$p_{vn}(V_c) + p_{in}(I_c) = 475 \qquad 0 < p_{vn}, p_{in} < 1 \tag{3}$$

$$p_{vn}(V_u) + p_{in}(I_u) = 26733 \qquad 0 < p_{vn}, p_{in} < 1 \tag{4}$$

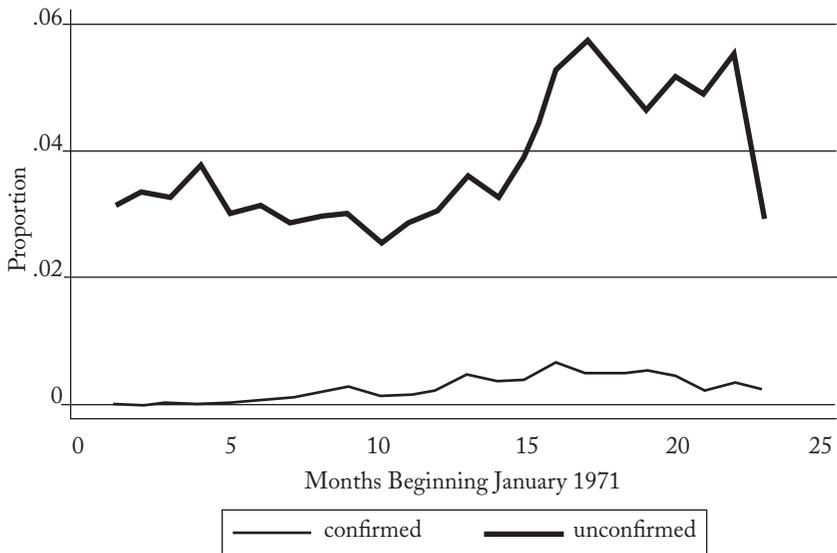$$\frac{\dfrac{I_u / 50889}{V_u / 50889}}{\dfrac{I_c / 8079}{V_c / 8079}} = \frac{I_u V_c}{I_c V_u} = 26.14 \tag{5}$$

FIGURE 1
PROPORTION KILLED OR CAPTURED BY CONFIRMATION STATUS
(MONTHLY TREND: JANUARY 1971–NOVEMBER 1972)

The five equations do not provide enough information to derive unique solutions for our variables. Instead, we derive all possible solutions that are consistent with the definitions and inequalities (rather than admit only solutions with an odds ratio of exactly 26.14, we admit all solutions within a 1-unit band around this value). The mathematical details of how we obtained our solution set can be found in the appendix. We also conducted a robustness check, which shows our results hold even under weakened assumptions (see the appendix for details).

Table 2 exhibits the two extreme solutions of the system of equations, along with an approximately intermediate solution.[68] Although there are thousands of possible combinations of values consistent with the inequalities and definitions, all fall within a fairly narrow range. At one extreme, the model is consistent with all but one victim innocent. At the other extreme, only 689 out of a total 27,208 captured or killed are Vietcong. In the former case, the proportion of actual insurgents victimized approaches zero. In the latter case, the proportion of innocents victimized approaches unity. The intermediate solution, though far from unique, suggests more realistic values: 1 percent of actual Vietcong captured or killed and about 86 percent of innocents victimized.

Although we cannot pin down a precise solution, making some simple, conservative, and highly plausible assumptions, we estimate that the Phoenix Program victimized at least thirty-eight innocents for every one actual Vietcong agent (the intermediate solution is about seventy-eight innocents for every one Vietcong).

Two subsidiary observations are worth noting. First, our findings are consistent with the belief that the Phoenix Program was pretty good at identifying its enemies: over all solutions, 40–55 percent of the individuals selected into Phoenix are estimated to have been Vietcong. Of those subjected to the more rigorous process of "confirmation," we estimate that 92–96 percent were Vietcong. Identifying them was one thing, catching them another entirely. Second, for all solutions, more Vietcong in the unconfirmed category than in the confirmed category were victimized. These two observations suggest that, while Phoenix's results may have been inaccurate, the program was not entirely irrational, either in its selection procedures or in its targeting of "unconfirmed" persons.

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[68] By "extreme" we mean the two solutions with the largest and smallest number of innocents victimized. In this context there are various ways to conceive of an intermediate solution. We chose a solution in which the number of innocents versus Vietcong victimized is intermediate between the extremes.

TABLE 2

HOW MANY INNOCENTS WERE VICTIMIZED? EXTREME AND
INTERMEDIATE SOLUTIONS

	Minimum	Intermediate	Maximum
Confirmed Innocents (I_c)	310	443	624
Confirmed Vietcong (V_c)	7769	7636	7455
Unconfirmed Innocents (I_u)	26209	30875	35133
Unconfirmed Vietcong (V_u)	24680	20014	15756
Proportion of Innocents Victimized (p_{in})	0.999	0.856	0.761
Proportion of Vietcong Victimized (p_{vn})	0.021	0.012	0.000027
Total Innocents Victimized	26519	26864	27207
Total Vietcong Victimized	689	344	1
Total Victimized	27208	27208	27208

SOURCE: U.S. Department of Defense, NPIASS-II.

Table 3 repeats the analysis, this time using the figures for persons killed only. The mathematics are identical, with the exception that equations 3 and 4 are now

$$p_{vn}(V_c) + p_{in}(I_c) = 366 \qquad 0 < p_{vn}, p_{in} < 1 \qquad (3a)$$

$$p_{vn}(V_u) + p_{in}(I_u) = 10341 \quad 0 < p_{vn}, p_{in} < 1 \qquad (4a)$$

The solution set in this case is considerably larger. At one extreme, the data and assumptions are consistent with no actual Vietcong killed, despite having more than thirteen thousand real agents in the pool of suspects. At the other extreme, nearly 1,900 of the dead are actual Vietcong agents, while over 8,800 are innocent. In other words, the most optimistic (that is, most accurately selective) scenario is that about 4.7 innocent persons were killed for every Vietcong agent. In the intermediate case, we have about 10.3 innocents killed for every rebel participant.

Beyond these raw results, the data give us some reason to suppose that the pressure of indiscriminate or poorly selective violence fell disproportionately on certain sectors of the population. Table 4 shows multinomial logit estimates of the regression of Phoenix Program neutralization status (captured, killed, defected, at large) on confirmation of status, age, and sex.[69] Table 5 compares the predicted probabilities of each outcome for a "reference group" with several other possible com-

[69] Models 1 and 2 differ only in their operationalization of age. Model 1 collapses age into four categories (under 18, 18–31, 32–44, 45 and older). Model 2 uses a binary indicator for "military age," equal to 1 if a person was 18–44 years old, 0 for those both younger and older. We focus our discussion on model 2.

TABLE 3
How Many Innocents Were Killed? Extreme and
Intermediate Solutions

	Minimum	Intermediate	Maximum
Confirmed Innocents (I_c)	63	366	1556
Confirmed Vietcong (V_c)	8016	7713	6523
Unconfirmed Innocents (I_u)	8749	28327	43969
Unconfirmed Vietcong (V_u)	42140	22562	6920
Proportion of Innocents Killed (p_{in})	0.999	0.340	0.235
Proportion of Vietcong Killed (p_{vn})	0.038	0.031	0.00000075
Total Innocents Killed	8811	9759	10707
Total Vietcong Killed	1896	948	0
Total Killed	10707	10707	10707

Source: U.S. Department of Defense, NPIASS-II.

binations of values. The reference group for model 1 is an unconfirmed man, under the age of eighteen. For model 2 it is an unconfirmed man of nonmilitary age. Each row of the table gives the probability predicted by the model that an individual with the given characteristics suffered the fate defined on the columns. Here we examine the comparative statics for unconfirmed persons only; the age and sex differences among confirmed persons are substantially similar. Note that the 95 percent confidence intervals for all of our predicted probabilities are very tight.

The data suggest an interesting story of "profiling" used in the infliction of violence. First, persons of military age were nearly twice as likely to be killed by the Phoenix Program as either older or younger people (0.22 versus 0.9). However, they were also more likely to escape completely (0.38 versus 0.26). Persons older or younger than military age had over twice the probability of capture, while the two groups defected at essentially the same rate. A similar pattern emerged for sex: men had a probability of being killed that was more than four times higher than that for women, yet they were also more likely to escape completely. Women were more likely than men to be captured and to defect.

In fact, the database shows that well over 50 percent of all the women in the database were captured, while slightly less than 5 percent were killed. These results are consistent with Jones's argument that young men are disproportionately targeted in episodes of mass violence, though with the caveat that women appear to have had a greater probability of suffering nonlethal victimization.[70] If the Phoenix Program was in fact

[70] Adam Jones, "Gendercide and Genocide," *Journal of Genocide Research* 2 (June 2000).

Table 4

Table 4
Some Correlates of Phoenix Program Status [a]
(1971–72)

Dependent Variable Category	Independent Variables	Model 1	Independent Variables	Model 2
Outcome = Captured	Confirmed (yes = 1)	−4.55	Confirmed (yes = 1)	−4.39
		(−44.42)		(−44.28)
	Age (18–31)	−2.60	Military Age (yes = 1)	−1.20
		(−40.62)		(−17.83)
	Age (32–44)	−2.39	Sex (male = 1)	−1.72
		(−37.80)		(−26.36)
	Age (45–)	−1.73	Sex * Military Age	−0.0001
		(−26.56)		(−0.00)
	Sex (male = 1)	−1.63	Constant	2.31
		(−50.92)		(38.13)
	Constant	3.56		
		(55.21)		
Outcome = Killed	Confirmed (yes = 1)	−2.29	Confirmed (yes = 1)	−2.25
		(−39.28)		(−38.75)
	Age (18–31)	−0.70	Military Age (yes = 1)	−0.073
		(−9.14)		(−0.57)
	Age (32–44)	−1.23	Sex (male = 1)	0.062
		(−15.94)		(0.50)
	Age (45–)	−1.91	Sex * Military Age	0.60
		(−22.09)		(4.49)
	Sex (male = 1)	0.78	Constant	−1.14
		(15.09)		(−9.77)
	Constant	−0.33		
		(−3.90)		
Outcome = Defected	Confirmed (yes = 1)	−5.58	Confirmed (yes = 1)	−5.50
		(−22.87)		(−22.61)
	Age (18–31)	−1.93	Military Age (yes = 1)	−1.00
		(−27.64)		(−12.64)
	Age (32–44)	−1.81	Sex (male = 1)	−1.19
		(−26.23)		(−15.72)
	Age (45–)	−1.70	Sex * Military Age	0.59
		(−23.42)		(6.82)
	Sex (male = 1)	−0.72	Constant	0.89
		(−18.88)		(12.75)
	Constant	1.84		
		(26.03)		
	N	48435		48435
	Pseduo-R2	0.166	Pseduo-R2	0.156

Source: U.S. Department of Defense, NPIASS-II.

  [a] All models are multinomial logits. Z-scores are in parentheses. Outcome comparison group is "at large." Residual age category is < 18.

TABLE 5
COMPARATIVE STATICS ON THE STATISTICAL MODELS [a]

	Pr[captured]	Pr[killed]	Pr[defected]	Pr[at large]
Model 1: Reference Group	0.55	0.13	0.25	0.08
	(0.53 – 0.56)	(0.12 – 0.14)	(0.23 – 0.26)	(0.07 – 0.09)
Model 1: Age 18–31	0.19	0.28	0.16	0.37
	(0.18 – 0.19)	(0.27 – 0.29)	(0.16 – 0.17)	(0.36 – 0.37)
Model 1: Age 32–44	0.24	0.18	0.19	0.39
	(0.24 – 0.24)	(0.17 – 0.18)	(0.19 – 0.20)	(0.38 – 0.40)
Model 1: Age 45–	0.40	0.08	0.19	0.33
	(0.39 – 0.41)	(0.07 – 0.08)	(0.18 – 0.19)	(0.32 – 0.34)
Model 1: Sex = female Age 32–44	0.59	0.04	0.19	0.18
	(0.58 – 0.60)	(0.03 – 0.04)	(0.005)	(0.18 – 0.20)
Model 2: Reference Group	0.46	0.09	0.19	0.26
	(0.45 – 0.47)	(0.08 – 0.09)	(0.18 – 0.20)	(0.25 – 0.27)
Model 2: Military age†	0.21	0.22	0.19	0.38
	(0.20 – 0.21)	(0.22 – 0.23)	(0.18 – 0.19)	(0.38 – 0.39)
Model 2: Sex = female	0.73	0.02	0.18	0.07
	(0.71 – 0.74)	(0.02 – 0.03)	(0.16 – 19)	(0.06 – 0.08)
Model 2: Sex = female Military age	0.58	0.06	0.17	0.19
	(0.57 – 0.59)	(0.05 – 0.06)	(0.16 – 0.18)	(0.18 – 0.20)

SOURCE: U.S. Department of Defense, NPIASS-II. Model 1 reference group: unconfirmed, under 18 years, male. Model 2 reference group: unconfirmed, not military age, male.

[a]Simulations generated with *Clarify*. 95% confidence interval in parentheses. All differences from the reference group are noted explicitly. † interaction term set to one.

as inaccurate as we think, then the age and sex results suggest a particularly acute dilemma for Vietnamese men of military age during these years. In effect, we should assume that the denominator for calculating civilian risk is smaller than the general population. Old people, young children, and women may have been probabilistically "profiled out" in the process of selecting individuals for lethal violence.[71]

There are obvious limits to our ability to generalize these results for the Vietnam War and beyond it to other wars. The violence associated with the Phoenix Program represented a small fraction of the total carnage of Vietnam. Far more deaths resulted from conventional battlefield confrontations of the U.S. and South Vietnamese against the Vietcong and North Vietnamese armies. Barely discriminate bombing killed hundreds of thousands of combatants and civilians. Nevertheless, the core of the conflict for many years consisted of the sort of individualized

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[71] Note also that there is evidence of sex and age profiling for selection into the Phoenix list as a whole: over 70 percent of the persons on the list were of military age; over 75 percent were men.

capture and murder that the Phoenix Program documents. It is possible that the selective murder of Vietnamese was more accurate before, after, and outside of Phoenix, despite the level of resources and effort directed precisely toward improving selectivity. One reason to doubt this supposition is that forces directly tasked to Phoenix accounted for a relatively small share of the violence: 2 percent of neutralizations over a period of a year and a half analyzed by Thayer.[72] The vast majority of deaths were associated with the ongoing "military operations" of local paramilitary forces.[73] Thus, it is probable that Phoenix reflected more general processes of violence that had been ongoing in southern Vietnam for many years.

In order to determine who was safer overall, we would need to know considerably more than we do about other sources of violence beyond the Phoenix Program, as well as the rate of Vietcong membership in the overall population. However, it is important to contextualize Phoenix, which represented the best efforts of a highly sophisticated military and intelligence bureaucracy to *improve* on past performance. The creators of the Phoenix Program understood and to a certain extent regretted the low level of discrimination of earlier U.S. efforts in the Vietnam War. Phoenix was the fruit of what they learned. Thus, we have some reason to suppose that it was more accurate than what went before. And though we have ample reason to question the accuracy of American efforts at discrimination, it is not clear that we should question their competence relative to other counterinsurgency campaigns waged by poorer and less sophisticated governments.

In comparative perspective, the Phoenix Program is probably closer in nature to typical civil war violence than the battles and aerial bombardments that tend to be associated in the public mind with the Vietnam War. It is possible that American counterinsurgent forces in Vietnam were simply far less competent than is typically the case in civil wars. If so, this specifically American problem seems to have persisted in Iraq. According to a February 2004 confidential report by the International Committee of the Red Cross, American military officers estimated that 70–90 percent of the prisoners held at Abu Ghraib at the time were unconnected to the insurgency.[74]

[72] Thayer (fn. 56).
[73] Elliott (fn. 48), 1137.
[74] "Certain CF military intelligence officers told the ICRC that in their estimate between 70% and 90% of the persons deprived of their liberty in Iraq had been arrested by mistake. They also attributed the brutality of some arrests to the lack of proper supervision of battle group units." See International Committee of the Red Cross (ICRC), *Report of the International Committee of the Red Cross (ICRC) on the Treatment by the Coalition Forces of Prisoners of War and Other Protected Persons by the Geneva Conven-*

Yet indiscriminate and inaccurate violence is generally associated with the poorly trained and poorly disciplined soldiers of developing country armies, rather than with the highly professionalized first world militaries. If military discipline or overall resources have any bearing on the capacity of an organization to be reliably selective, then we should expect the Phoenix Program to have been more accurate than typical counterinsurgent efforts. If this supposition holds up, then our data would reflect one of the more optimistic scenarios for defending the assumptions of the collective action problem; in other words, this is a hard case for us.

It is worth asking here why violence that is selective in intent may end up being so inaccurate in practice. The reason is that civil war stretches the bureaucratic capacity of states: it demands a lot of information while straining existing resources. States (as well as challengers) rely on individual informants who have incentives to denounce their personal or local enemies. Phoenix was no exception.[75] According to one account:

> The people who identified members of the [Vietcong] shadow government often had many types of non-Communist enemies in their area of operation, particularly if they worked in their native areas. Like most people, they had personal enemies: the men who had insulted their sisters, the men who had stolen their sweethearts, the farmers who had borrowed money from their families and failed to repay it, and even the GVN [South Vietnam Government] officials who had beaten their cousins. Family members of these enemies also could be fair game, especially when previous offenses had involved relatives.[76]

A South Vietnamese colonel had this to say about the men of the Provincial Reconnaissance Units who often relied on Phoenix information in their raids: "If they saw a beautiful girl, they tried to be her boyfriend. If they got turned down, then they accused her of being a VCI [a member of the Vietcong Infrastructure]."[77] A U.S. adviser confirmed this ten-

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dency by recalling an example: "One guy who was a source of information about the VC relieved his family of three generations of debt. He turned in phony reports fingering as Viet Cong people his family owed money to."[78] The same tendency can be observed in many civil wars. For example, in El Salvador many false denunciations were "enough to seal one's fate, since government forces seldom sought to investigate the charges and 'innocent until proven guilty' was not a principle recognized by the military, security forces, or ORDEN civilian irregulars."[79]

Obviously, armed actors are aware of this trend. As a CIA adviser in Vietnam recalls: "There were times when I questioned a name on the blacklist of VCI. 'Is this guy actually VC infrastructure, or is he a political enemy or a business enemy of the province chief or district chief of somebody else?'"[80] However, given extremely stretched resources and the need to act, they tend to err in the direction of false positives: "Better to kill mistakenly than release mistakenly" went a Vietnamese slogan popular among some insurgents; for them, "justice was not an abstract ideal, but a tool in the political struggle"; "if it came down to a conflict between the revolution's prestige and abstract notions of justice, it was clear which would prevail."[81] A U.S. commander in Iraq remarked about Iraqi counterinsurgents that "if they shoot somebody, I don't think they would have remorse, even if they killed someone who was innocent."[82]

THE GREEK CIVIL WAR

While extremely detailed and systematic, the Phoenix data cover only a subset of the civilian population while providing no information about actual insurgent combatants. We therefore conclude our triangulation effort with regional data from the Greek Civil War. Like the Vietnam War, this was a complex war fought in different phases between 1943 and 1949. Using extensive archival sources, we were able to calculate the exact toll of violence in one region, the Argolid, located in southern Greece. The civil war in that area was fought during a single year, 1943–44, and coincided with the German occupation of Greece. The conflict pitted members of the procommunist resistance army ELAS (Greek Popular National Army) against right-wing collaborationist militias supported by the German occupation troops (the "state"). Both

[78] Ibid., 293.
[79] Binford (fn. 52), 107.
[80] Moyar (fn. 76), 122.
[81] Elliott (fn. 48), 947.
[82] Quoted in Peter Maass, "The Way of the Commandos," *New York Times Magazine*, May 1, 2005, 47.

resisters and collaborationists recruited locally and the war had a strong "neighbor against neighbor" aspect.

Between September 1943, when the conflict began, and September 1944, when the Germans left, the militiamen and Germans killed 353 individuals, slightly less than 1 percent of the rural population of that area (0.78 percent). At the same time, civilians were also targeted by the rebels, who killed 372 individuals. Although some women and children were killed during these operations, about 90 percent of the victims were men of military age (18–45 years old)—318 and 353, respectively. Assuming that men of military age made up about 30 percent of the total population, we estimate that 2.35 percent of all military-age men inhabiting the rural Argolid were killed by the counterinsurgents, while 2.47 percent of military-age men were killed by the rebels. The total percentage of victims across the entire population of military-age men is, therefore, 4.82 percent. Note that this tally excludes nonlethal violence, which was considerable. For example, the Germans drove hundreds of civilians to prisons and concentration camps in Greece and to slave labor camps in Germany.[83]

How about the victimization of the (overwhelmingly male) combatants? Twenty local rebel fighters (members of the 6th Regiment of ELAS, which operated in the wider area) were killed in action during the same period (about 4 percent of all Argolid rebels), while fourteen local collaborationist militiamen also lost their lives (4.67 percent) (see Table 6).[84] This distribution is consistent with the unsystematic yet common observation that civil wars tend to victimize civilians at higher rates than combatants.

[83] The rosters of the local prison in the town of Nafplio show over one thousand individuals held there during the same period. Hundreds more were sent to a concentration camp in the neighboring town of Korinthos, while a smaller but unspecified number were sent to slave labor camps in Germany. However, the Germans did not take rebel prisoners. Indeed, a significant proportion of the rebels killed in action were shot after being captured. A common practice of German occupation troops was to list most civilian victims as rebel fatalities. See H. F. Meyer, *Von Wien nach Kalavryta: Die blutige Spur der 117: Jäger–Division durch Serbien und Griechenland* [From Vienna to Kalavryta. The Bloody Trail of the 117 Jaeger Division through Serbia and Greece] (Moehnesee: Bibliopolis, 2002). Careful disaggregation based on extensive archival and field research confirms the intuition of many historians that most of these "partisan fatalities" were in fact civilians fleeing the German advance.

[84] On top of the twenty fatalities suffered during the occupation, the regiment lost fifteen more local fighters during the battle of Athens (December 1944), when the communists attempted to seize power. They are not included in the analysis since violence against civilians subsided after the end of the occupation. Our source on rebel fatalities is Emmanouil Vazeos, *Ta agnosta paraskinia tis Ethnikis Antistaseos eis tin Peloponnison* [The Unknown Backstage of the National Resistance in the Peloponnese] (Korinthos: Self-published, 1961). The 6th Regiment of ELAS, which was active in the Argolid and Korinthia areas and recruited primarily from these two regions, reached thirty-five hundred men in October 1944, after the occupation's end; many of these men were recruited or conscripted after the German evacuation. The regiment's full force prior to this was closer to five hundred men. Data on militiamen were collected from archival sources and civil registries.

TABLE 6

CIVILIAN AND COMBATANT VICTIMIZATION, THE ARGOLID
(1943–44)

Total rural civilian population	45,140
Estimate of military-age men	13,542
Male victims of Germans/militiamen	318
Male victims of the Germans/militiamen as a percentage of military-age men	2.35%
Civilian victims of rebel violence	353
Male victims of the rebels as a percentage of military-age men	2.47%
Total male victims as a percentage of military-age men	4.82%
Estimate of local rebel combatants	500
Local rebels killed in action	20
Percentage of local rebels killed in action	4.00%
Estimate of local militia combatants	300
Local militiamen killed in action	14
Percentage of local militiamen killed in action	4.67%
Local combatants killed as a percentage of all local combatants	4.25%
Local combatants killed as a percentage of military-age men	0.25%

In short, a civilian man of military age was more likely to be killed by one of the two sides than was an actual (male) combatant. This contrast is starker if one compares civilian and combatant deaths with both expressed as a percentage of all military-age men: 4.82 percent for civilians and 0.25 percent for combatants. Clearly, it was safer to be a combatant than a civilian in the Argolid in 1943–44.

Indeed, civilians in the Argolid faced a classic problem of noncombatants in civil wars: they could be targeted by both sides simultaneously, a predicament known as being "between two fires."[85] Overall, about half the violence against civilians was indiscriminate and the other half was selective in intent—but not infrequently indiscriminate in practice—as the two sides relied on information gathered through denunciations motivated by a variety of private and local grudges. Of all the homicides attributed to the occupation army, 68.5 percent resulted from indiscriminate violence as compared with 31.4 percent of those attributed to the rebels—which is consistent with the differential access the two sides had to local information. The rebels were also more successful than their opponents in recruiting locally.

Detailed analysis of the rosters of those killed, along with qualitative evidence from over 150 interviews, indicates that the great majority of civilians killed during the war had little or no active participation

[85] Stoll (fn. 49).

in either camp besides standard acts of compliance with the side that wielded power in their village; that is, civilian collaborators of the rebels did not face a graver danger than noncollaborators. In fact, most victims of counterinsurgent violence were mowed down while attempting to flee their villages and many families were victimized by both sides.[86]

Clearly, this body of regional data from the Greek Civil War fails to support either side of the core assumption of the collective action paradigm: the benefits of nonparticipation (or free riding) did not exceed those of participation, and the risk of joining the insurgency (or the collaborationist militia) was lower than the risk of not joining.

Conclusion

The evidence presented in this article challenges the assumption that rebellion is necessarily subject to the collective action problem: that is, free riding in civil wars is not the kind of attractive option that the term (and the theory) imply. If the collective action paradigm has been so dominant, it is because scholars have tended to overestimate the risks to rebel fighters or to underestimate the risks faced by nonparticipants—a result of limited attention to the dynamics of violence and of the tendency to impute preferences rather than investigate them empirically. In fact, Tullock's critique of the revolutions literature for imputing motivations to individuals from the observed macro "public good" post hoc was right on target.[87] But in the years that followed his pathbreaking analysis, many social scientists have tended to infer the calculations that supposedly lead people to join an insurgency from equally arbitrary assumptions about comparative risk.

Typical of such arbitrary assumptions is the oft-repeated claim that it is always worse, in a civil war, to be a fighter than a civilian. William McNeill, a young American envoy in Greece who would later become a renowed historian, knew otherwise. He described the process of joining the rebels with remarkable insight:

> [A] soldier in ELAS lived a good deal better than did the ordinary peasant, and did not have to work with the same drudging toil. He further had the psycho-

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[86] In light of this analysis it should not comes as a surprise that mass displacement is so common in civil wars. Nevertheless, it is rarely the first choice of civilian populations: rural populations depend on land for their livelihood and abandon their villages only under tremendous pressure. Flight is not a form of free riding. It carries substantial costs and is often not an option for military-age men who may be shot attempting to flee (hence the predominance of women, children, and the old in refugee camps).

[87] Tullock (fn. 2), 93.

logical exhilaration of believing himself a hero and the true descendant of the robber *klefti* who had fought in the War of Independence and were enshrined in the Greek national tradition. Under the circumstances, many a peasant's son found himself irresistibly attracted to the guerrilla life; and an over abundant peasant population made recruitment easy.[88]

McNeill's description combines with our analyis to suggest that sometimes the real puzzle in civil wars is nonparticipation rather than collective action. Some simple mechanisms suggest themselves as explanations for why rebel armies do not grow without limit.

First, rebel organizations do not always maximize recruitment. It is often forgotten that military success is not simply a function of manpower. With limited logistic means, few weapons, and low capacity to support troops, many rebel organizations prefer to recruit and train a small number of full-time fighters and channel their sympathizers into large civilian networks of support. This is why the absence of a collective action problem on the rebel side does not automatically translate into larger (or more successful) rebel armies. Second, if civil war works as we suggest, then membership in a rebel organization may be a "club good." That is, members of the club receive special benefits, both material and nonmaterial, from which nonmembers are excluded. If this assumption holds, we should expect rebel groups to screen out opportunistic or unfit joiners who seek to benefit from the protection the rebels can offer but without pulling their weight.[89] The relative scarcity of rebel club goods leads us to expect that the optimal size of rebel organizations is smaller than otherwise assumed, even in the absence of logistical problems. Hence, an important implication for empirical research is that the size of a rebel army is a misleading indicator of this army's actual recruitment capacity (and, more generally, of its overall military quality).

Rethinking the application of the collective action problem to civil war contexts is not merely a question of getting the description right, though that is a benefit in and of itself; it also suggests previously unexplored answers to key theoretical problems in the study of mass violence. For instance, stalemated civil wars of long duration are puzzling

[88] William H. McNeill, *The Greek Dilemma: War and Aftermath* (Philadelphia: J. B. Lippincott, 1947), 80–81.

[89] An implication is that club goods should be common across all types of armed groups. Jeremy Weinstein has suggested instead that only well-funded rebel organizations recruit via club goods (mainly loot). Consequently, they attract opportunistic and undisciplined individuals who abuse the civilian population. Aside from the assumption that individuals joining an armed group to acquire club goods cannot be socialized to become motivated and disciplined combatants later, this argument requires a key condition: that the state facing the rebels must be exceedingly weak or even nonexistent. Otherwise, an army with these characteristics has little chance of surviving. See Jeremy M. Weinstein, *Inside Rebellion: The Politics of Insurgent Violence* (New York: Cambridge University Press, 2007).

if we assume that combatant risk significantly exceeds civilian risk. If participation improves or has no effect on individuals' short-run survival prospects, the puzzle is easily resolved.[90] Our approach is also a promising way to account for the counterintuitive (yet common) phenomenon of people who fight for the "wrong" side in civil wars (for example, Chechens in pro-Russian militias, Kurds in the Turkish army, and indigenous Guatemalans in government militias), as participation in any militant organization may improve the odds of survival. Moreover, we point to private goods provided by rebel organizations, such as protection, that have been eclipsed by the recent focus of the literature on insurgent looting. Likewise, our analysis calls for renewed attention to the endogenous relation characterizing rebellion and repression. Last, we show the importance of conceptualizing civil wars as processes that generate incentives and constraints, in contrast to the view (best summarized by Tullock) that payoffs to participation derive almost exclusively from expectations about outcomes.

In comparative terms, our analysis is consistent with the expectation that rebel recruitment is likely to display a convex function, subject to the conditions outlined above. Recruitment should increase when the state is totally absent, a situation that eliminates the risk of sanctions against rebels while allowing rebels to sanction nonparticipants. Rebel recruitment may also increase under conditions of extreme violence by the state, provided that the rebels can survive as an organization and seek to enlarge their army. Other things equal, in particular the perceived accuracy of targeting, better discrimination in state violence should depress rebel recruitment, both because of the disruption to rebel operations and because of the increase in the relative risk of participation. Our analysis also points to some important areas of theoretical and empirical investigation. How do rebels respond to indiscriminate state violence? When do they have the capacity to protect the civilian populations and when do they fail? When do they choose to expand their armies? And what impact do humanitarian action and cross-border refugee camps have on their choices?

Overall, our analysis shows why we need to pay closer attention to temporal and spatial variation in violence: not all civilians or combatants face similar risks and incentives all the time. Additionally, the perception of risk may diverge from real risk—and violence may be a factor in generating such misperception. Furthermore, civilian preferences are heterogeneous, as is civilian behavior during civil war: the

[90] Why, then, would individuals join "go nowhere" insurgencies? An answer is that they are attracted by access to club goods.

behavioral range from rebel fighter to nonparticipant is very wide. An enhanced understanding of civil wars requires a double correction: an emphasis on systematic and disaggregated evidence from a wide cross-section of individuals and an awareness of the dynamics of violence. More complex models of participation require disaggregated and fine-grained data and a corresponding shift from the current emphasis on macrolevel models. Once we reach a better theoretical and empirical understanding of the microdynamics of violence, we will be able to bring greater sophistication to our analysis as we revisit questions such as the causes and effects of violence at the aggregate level.

Appendix

To obtain the solution set D for the system of equations generated by our assumptions about the Phoenix Program, we created the set of all possible integer-valued 4–tuples
$D \in \{(I_u, I_c, V_i, V_c)\}$ such that:

$$V_u + I_u = 50889 \tag{1}$$

$$V_c + I_c = 8079 \tag{2}$$

(that is, such that Vietcong and innocents in the categories "confirmed" and "unconfirmed" sum to the total for each category in the data).

Next, assuming that p_{in} and p_{vn} are proportions (see equations 3, 3a, 4, and 4a in the text), we dropped all 4-tuples for which these variables fell outside the unit interval. We considered two cases, the first for killed + captured (equations 3.1 and 4.1), the second for killed only (equations 3a.1 and 4a.1). We solved equations 3 and 3a for p_{vn}, simplified, substituted the result into equations 4 and 4a, respectively, and solved for p_{in}, resulting in equations 4.1 and 4a.1.

$$p_{vn} = \frac{475}{V_c} - p_{in}\left(\frac{I_c}{V_c}\right) \tag{3.1}$$

$$p_{in} = \frac{26733 V_c - 475 V_u}{I_u V_c - I_c V_u} \tag{4.1}$$

$$p_{vn} = \frac{366}{V_c} - p_{in}\left(\frac{I_c}{V_c}\right) \tag{3a.1}$$

$$p_{in} = \frac{10341 V_c - 366 V_u}{I_u V_c - I_c V_u} \qquad (4a.1)$$

Finally, we calculated the odds ratio (equation 5) for all 4-tuples, and dropped those cases outside a 1-unit band around the odds ratio derived from the data:

$$25.64 < \frac{I_u V_c}{I_c V_u} < 26.64 \qquad (5.1)$$

Taken together, D subject to the conditions 1, 2, 3.1, 4.1, 5.1 or 1, 2, 3a.1, 4a.1, 5.1 is an exhaustive specification of our solution set.

We also subjected our results to a robustness check, by modifying condition 5.1 for a ten-unit interval around the odds ratio taken from the data:

$$21.14 < \frac{I_u V_c}{I_c V_u} < 31.14 \qquad (5.2)$$

Using this larger band for the odds ratio resulted in the solutions outlined in Tables 7 and 8. We can see by comparing these figures to the ones drawn from Tables 2 and 3 that the results do not differ greatly. This alternative specification of our model permits solutions with somewhat smaller proportions of innocents victimized. Nevertheless, the overall findings do not change: under the new assumption, innocents were still far more likely to suffer from the Phoenix Program than were Vietcong agents.

TABLE 7
How Many Innocents Were Victimized? Extreme and
Intermediate Solutions[a]

	Minimum	Intermediate	Maximum
Confirmed Innocents (I_c)	214	398	666
Confirmed Vietcong (V_c)	7815	7681	7413
Unconfirmed Innocents (I_u)	26063	29202	37488
Unconfirmed Vietcong (V_u)	24826	21687	13401
Proportion of Innocents Victimized (p_{in})	0.999	0.904	0.713
Proportion of Vietcong Victimized (p_{vm})	0.027	0.015	0.0000097
Total Innocents Victimized	26327	26768	27208
Total Vietcong Victimized	881	440	0
Total Victimized	27208	27208	27208

SOURCE: U.S. Department of Defense, NPIASS-II.
[a]Robustness check with 21.14 < odds ratio < 31.14.

TABLE 8
How Many Innocents Were Killed? Extreme and
Intermediate Solutions[a]

	Minimum	Intermediate	Maximum
Confirmed Innocents (I_c)	54	350	1592
Confirmed Vietcong (V_c)	8025	7729	6487
Unconfirmed Innocents (I_u)	8701	27864	45000
Unconfirmed Vietcong (V_u)	42188	23025	5889
Proportion of Innocents Killed (p_{in})	0.999	0.345	0.230
Proportion of Vietcong Killed (p_{vn})	0.039	0.032	0.000025
Total Innocents Killed	8755	9731	10707
Total Vietcong Killed	1952	976	0
Total Killed	10707	10707	10707

SOURCE: U.S. Department of Defense, NPIASS-II.

[a]Robustness check with 21.14 < odds ratio < 31.14.