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How Free is “Free Riding” in Civil Wars?  
Violence, Insurgency, and the Collective  
Action Problem

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## Abstract

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*That rebels face a collective action problem is one of the most widely shared assumptions in the literature on civil wars. We argue that the collective action paradigm can be both descriptively inaccurate and analytically misleading as applied to civil wars—which are uniquely characterized by large-scale violence. First, while insurgent collective action may entail the expectation of future collective benefits, public costs predominate in the short-term. Second, the costs of nonparticipation and free-riding may equal or even exceed those of participation. We support these points by triangulating three different types of evidence: anecdotal evidence from counterinsurgency operations in a number of civil wars; the unique Phoenix dataset from the Vietnam War; and data from a regional study of the Greek Civil War. We conclude by drawing implications for the study of civil wars.*

## Resumen

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*El supuesto problema de acción colectiva al que se enfrentan los grupos rebeldes es una de las ideas más enraizadas en la literatura que estudia las guerras civiles. La visión de este trabajo es que este paradigma es descriptivamente impreciso y poco clarificador desde el punto de vista analítico en contextos como las guerras civiles, caracterizados por la existencia de una violencia generalizada. En primer lugar, incluso si la acción colectiva insurgente puede introducir expectativas de recibir beneficios colectivos en un futuro, en el corto plazo los costos públicos predominan. En segundo lugar, los costos asociados con el recibimiento de beneficios de alguna de las partes en conflicto sin participar a cambio (free riding), o el abstenerse totalmente de tomar parte en el conflicto, pueden igualar o incluso superar a los que se tendrían de tener una postura explícitamente beligerante. La evidencia que apoya estas afirmaciones se basa en tres fuentes diferentes de información: una parte anecdótica obtenida de operaciones de contrainsurgencia en guerras civiles, el uso de la base de datos Fénix (Phoenix), utilizada en la Guerra de Vietnam, e información obtenida de un estudio a nivel regional de la Guerra Civil Griega. El artículo concluye con la derivación de implicaciones de las hipótesis defendidas para los estudios de Guerras Civiles.*

## Introduction

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*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 or political organization, of arms, or merely of protection, a hiding place, a fire, a pair of shoes.*

Primo Levi  
*Survival in Auschwitz*

The social-scientific understanding of individual participation in large-scale mobilization is informed by theoretical arguments derived primarily from the collective action paradigm (Olson, 1965). Even though mobilization in the context of rebellion, insurgency, and civil war diverges from mobilization in demonstrations and mass protest, both are similarly informed by the collective action paradigm (Tullock, 1971). References to the collective action problem that must be overcome if rebellion is to take place are ubiquitous. According to a recent statement, “rebellion is a full-time commitment and it is dangerous” (Collier, 2001: 150). Indeed, such references constitute the foundation for one of the most influential theoretical arguments about civil war onset: since “the collective action problem for justice-seeking rebellion would usually be insuperable” it follows that insurgencies would tend to be about “greed” rather than “grievance” (Collier, 2001: 150).

In this paper, we revisit the collective action paradigm as specifically applied to the issue of insurgency.<sup>1</sup> This paradigm rests on two pillars: first, the “free-riding” incentive generated by the public good dimension of insurgency and second, the risks of individual participation in insurgent collective action. The paper’s contribution lies in its use of novel and systematic data on violence, suggesting that while it is true that rebels run serious personal risks in war zones, so do non-rebels. In other words, we argue that the collective action problem applies only if insurgent collective action is risky *relative* to nonparticipation (or free-riding is riskless *relative* to participation).

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 approach the question using three different bodies of evidence, each of which has shortcoming and advantages. By triangulating, we mitigate

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<sup>1</sup> In this paper, we use the terms rebellion, insurgency, and civil war inter-changeably.

the problems inherent in this type of exercise. We begin by providing anecdotal evidence from several instances of large-scale violence entailing harsh counterinsurgency operations of repressive regimes indiscriminately targeting entire areas. This evidence is highly suggestive, but also unsystematic. We then proceed to examine a uniquely detailed and systematic dataset from one of the most sophisticated programs of targeted violence, the Phoenix Program in Vietnam. 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 dataset lies in its detail, but its main limitation is its coverage of a particular group of people rather than the general population. Last, we supply systematic regional evidence from the Greek Civil War; the data cover all individuals in a single region, combatants and non-combatants alike -the main problem here being external validity. All three bodies of evidence converge in suggesting that the risks of participation in insurgent collective action have been overestimated relative to those of nonparticipation. In turn, this implies that nonparticipation and free-riding are often puzzling given the costs they entail.

### ***Violence and the Collective Action Problem***

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 Mancur Olson in the 1960s. Since the basic outlines of this framework are so widely known, we treat it only briefly. Individuals value many goods that can be produced only through collective action. Collective goods are non-rival and non-excludable. That I enjoy such a good does not in anyway 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, in short to "free-ride". If everyone reasons as they should, public goods will be systematically underprovided, even when everyone wants them. Olson's logic makes collective action for public goods a puzzle: under quite general conditions, it is irrational. Yet collective action is common, even where and when it is not enforced or promoted by the public power. 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 is overcome.

There has been much work on the conditions under which the collective action problem does or does not apply. The bulk of this work applies primarily to mass mobilization in the context of demonstrations, social protest, and nonviolent contentious actions and social movements (Tarrow, 1998), *i.e.*, in contexts short of mass violence.

Nevertheless, the collective action paradigm also dominates current understandings of insurgent collective action. Tullock's application (1971) was the first to challenge prevailing understandings in the revolutions literature of peasant grievances automatically translating into rebellion. This approach was further elaborated by Lichbach (1994), applied empirically by Popkin (1979), and critically extended by Wood (2003). 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 good and free-riding aspect of rebellion to the costs associated with insurgent participation.

Rebel groups typically claim public goods as their goals: secession, autonomy, democratization, and redistribution appear to be the most common. Even putting aside the question of how authentic and widely shared are these aims, they are subject to high levels of uncertainty and very long time-horizons.<sup>2</sup> The ills of civil war, death of self or family, economic collapse, and forced relocation, are immediate and affect non-rebels as well as rebels. Given that by 1999, the typical post-WWII civil war lasted 16 years (Fearon, 2004), and a great many concluded 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 unlike collective mobilization and protest, the study of insurgent mobilization has highlighted the costs of individual participation: in a demonstration is generally much less costly than in an armed insurgency (Collier, 2001). Thus, the central implication of the collective action paradigm for the study of insurgent action is obvious: rebel activists face tremendous obstacles in starting and sustaining insurgencies.

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—first movers or late joiners.<sup>3</sup> Our focus, in the context of this paper, is on the latter. It has been argued that first movers are high-risk political entrepreneurs with strong motivations for whom the collective action

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<sup>2</sup> Note, however, that it seems entirely logical, for example, that the order that exists at  $t-1$  (pre-insurgency) is a public good that individuals prefer not to sacrifice, and not joining the insurgency *de facto* contributes to the provision of the former good. Scholars have not considered order adequately.

<sup>3</sup> This is not the same distinction between onset and duration of civil war. In the empirical literature, onset is computed on the basis of fatalities, a computation that assumes already large rebel organizations.

problem does not apply, Elster (1989) calls them “everyday Kantians”.<sup>4</sup> The world does not lack Che Guevaras ready to launch insurgencies and as likely to fail. What it does lack is followers willing to take the necessary risks; it is the success of entrepreneurs in recruiting followers that produces insurgencies. In Collier’s (2001: 143) formulation: “The Michigan Militia was unable to grow beyond a handful of part-time volunteers, whereas the FARC in Colombia has grown to employ around 12,000 people.”

The puzzle, then, is to explain extant participation given both highly costly sanctions and the posited public good nature of rebellion. There are two types of responses. A first body of research looks for private selective incentives powerful enough to overcome the cost of participation. Actual members of rebel groups enjoy several types of benefits that are both rival and excludable, such as “loot” –but also power and security. The enormous literature on “selective incentives” within the collective action tradition provides ample evidence that rebels usually receive such private material incentives for participation above and beyond any anticipated public goods.<sup>5</sup> The voluminous civil war literature on “greed” and looting of the past decade is a case in point. A second body of research strays out of the narrow rationalist paradigm by emphasizing non-rational behavior (Muller and Opp 1986), pointing to “opportunity structures” (Tilly 1978; Brockett 2005), stressing “in-process” benefits (Wood, 2003), and underscoring social ties (Gould, 1995; Petersen, 2001).

In this paper we take a look at the collective action paradigm by granting it its assumptions. Our analysis challenges its automatic applicability to cases of civil war, which are uniquely characterized by large-scale violence. In many, if not most, instances of large-scale violence, obstacles to collective action are much lower than assumed. This is so for two reasons. First, while insurgent collective action may entail the expectation of future collective benefits, in fact public costs predominate in the short-term. In other words, the classic collective action formulation of collective benefits vs. private costs misdescribes the nature of violent conflict. Second, the costs of nonparticipation and free-riding may equal or even exceed those of participation. In other words, it may be safer to join in insurgent collective action or the difference between participation and nonparticipation in terms of risk incurred may be too small to be relevant, thus priming decisions along dimensions other than risk. Hence, we argue, the collective action paradigm can be both descriptively inaccurate and analytically misleading as applied to civil wars.

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<sup>4</sup> According to Elster (1989: 192), “everyday Kantianism” states that “one should cooperate if and only if universal cooperation is better for everybody than universal defection” (192).

<sup>5</sup> Selective incentives is the term of art used for the individual side-payments that are dispensed by organizations to overcome the Olsonian logic of collective action. See Lichbach (1996; 1995: 215–238) for evidence of the widespread existence of the private benefits that accrue to rebels.

The literature has hinted at some of these points. Several scholars have attempted to understand how large-scale violence shapes individual incentives for participation (e.g. Lichbach, 1987; Mason, 1989; Mason and Krane, 1989; Maranto and Tuchman, 1992; Wood, 2003; Brocket, 2005). Because the focus has been on individuals choosing or not to join a rebel movement, these studies have focused on the role of government repression. The findings, however, are inconclusive and ambiguous. Lichbach and Gurr (1981) suggest a U-shaped relationship between repression and dissent, where higher levels of repression lead to more resentment and bring new groups into fold, increasing rebellion. In *Why Men Rebel*, in contrast, Gurr (1970:270) proposed a concave function, where anger over initial repression gives way to fear. Indeed, the effect repression will have on the organization and maintenance of an insurgent group could be to dampen the recruitment possibilities by raising the costs for individuals to join and the group to continue—as posited in most studies. For instance, Tullock (1971:90) assumes that, while the risk of injury or death is a constant cost for individuals, if the government increased the punishment of collaborators if revolution fails, then the net expected utility of the individual rebel decreases. Hence, repression only leads to a lower likelihood of rebellion. Lichbach (1987: 269), quoting Greene (1974: 112), considers the opposite outcome: 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”. As a result, Lichbach concludes, “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” and 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. Wood (2003) argues that the insurgents in El Salvador successfully framed the government violence as illegitimate and thus mobilized additional supporters.

Partly as a result of the absence of conclusive findings, the overwhelmingly shared assumption of the collective action literature is that participation in violent collective action is “risky”, and this risk is usually conceptualized as the principal individual (expected) cost paid by rebels: in Gould’s (1995: 204) 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.”

While it is undoubtedly true that rebels run serious personal risks in war zones, so do non-rebels. The relevant question, therefore, is whether violent collective action is risky relative to nonparticipation. Because of the obvious

obstacles in tackling this question directly, we formulate and supply evidence for the following claim: insurgent participation is much less dangerous, relative to non-participation, than posited by the collective action paradigm and generally thought. We suspect that, in actual fact, there is considerable variation across the landscape of violent cases, and perhaps even greater variation over space and time within cases. It could very well be, for instance, that in certain instances non-participation is, on average, more dangerous than participation.

A significant obstacle to the development of a research program about the effect on violence on collective action has been the dearth of systematic data—especially high quality disaggregated data. In fact, this question has not been examined systematically within the collective action literature, even though on it rests a central, vital assumption of the paradigm. Often, it is ignored completely.

Consider, for instance, two excellent recent additions to the literature on dissidence and collective violence, both of which use a micro-level research design and take their point of departure from the collective action problem. Wood (2003: 8-10) 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. Wood notes that: “The vast majority (more than 85%) of the serious acts of violence... were carried out by state agents or those acting under the direction of state agents against *alleged* supporters of opposition organizations (emphasis added).” 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 non-participants in their average risk.<sup>6</sup> Wood (2003: 12) further notes that “class differences among the *campesinos* of the case-study areas do not explain differences in their participation. The evidence presented here from the case-study areas show that participants in the insurgency came from a variety of poor rural class backgrounds. The many *campesinos* who joined government networks and civil patrols or served as government informants came from equally diverse economic backgrounds.” In other words, rural class position is not a reliable proxy for participation in dissident collective action. Hence, aggregated data cannot be used to reliably assess the relative risk of participation: knowing that many *campesinos* died is not informative regarding the risk of being a rebel.

Similarly, Petersen (2001) stipulates that participants in the anti-occupation violence of wartime and post-war Lithuania ran enormous risks.

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<sup>6</sup> Furthermore, Wood (2003: 151-3) reports that in the regions of El Salvador where she conducted ethnographic research, a stalemate had emerged and there was generally low violence against civilians exercised by both sides, with the insurgents having an advantage in the exercise of local control. During this period, she estimates that about one third of the peasants in the area she studied collaborated with the insurgents.

Much like Wood, he cites only aggregated figures on victimization: “Hundreds of thousands deported, tens of thousands killed, 200,000 Jews murdered, and so on” (2001: 302). Were Soviet counterinsurgents effective in identifying and selecting guerrillas and their supporters from the Lithuanian population? Petersen’s own evidence suggests that Soviet counterinsurgent violence was indiscriminate in the extreme.

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 other violent contexts. In riots or violent protests, we reasonably suppose that staying home dramatically attenuates individual risk.<sup>7</sup> When war passes through an area, 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 non-combatants 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.

By contrast, wars fought in irregular fashion tend to implicate civilians in a way that is much more direct and consequential. The reason is that rebel fighters, as well as the spies and agents of both the state and the rebels, hide among the civilian population. This feature of irregular war, also known as the “identification problem” (Kalyvas, 2006), is present in the following remark of John Kerry from his experience in Vietnam (in Brinkley, 2003: 50): “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 [*i.e.* 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?”

A consequence of the identification problem is that armed actors may target the civilian population indiscriminately, *i.e.* using a series of very rough “profiles”, such as a person’s ethnicity, locality, sex, or age (Kalyvas, 2006).<sup>8</sup>

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<sup>7</sup> Quite obviously, this claim holds only for those who choose whether or not to participate, not for those who may be targeted by rioters during ethnic riots and pogroms.

<sup>8</sup> The difference between selective or targeted and indiscriminate violence lies in the level of target selection: individualized selection makes for selective violence whereas aggregation (village, region, ethnic group, etc.) produces indiscriminate violence.

If this is the case, then shunning participation in the rebellion and free-riding may actually prove deadlier than joining it, since the rebels may be in a position to offer a degree of protection.<sup>9</sup> Adding the benefit of risking one's life with a modicum of dignity and honor, as well as a sense of choice in one's fate (as opposed to being killed in an undignified way without even the possibility of acting), makes the difference even starker.

The danger that truly indiscriminate violence may "push the population into the arms of the enemy" has been widely appreciated in the literature on insurgency and civil war (see the following section). Yet even violence that is intended to discriminate between combatants and civilians may be sufficiently inaccurate as to create similar pressures. We explore this possibility in a later section by focusing on a "hard case", an instance in which there is substantial evidence of an intent to discriminate, yet violence fell disproportionately on the innocent.

Whether violence is indiscriminate or merely poorly selective in intent, there are good reasons to suppose that it is perversely selective in effect. The reason is that rebel combatants have access to skills, resources, and networks that should promote their survival relative to non-combatants. 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 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.

If large-scale violence entails public ills and private benefits, then the theoretical and empirical puzzle is precisely the converse of the one raised by the collective action problem: if participation is rational for everyone, why don't we see more of it? 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. Below we present some evidence to that effect.

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<sup>9</sup> In Gross's formulation (1979: 212) about the German occupation of Poland: "One would expect that noncompliance with German demands carried such drastic penalties that scarcely anyone would dare to defy them. But full compliance was impossible; terror continued and even intensified with time. The population quickly recognized the new logic of the situation: whether one tried to meet German demands or not, one was equally exposed to violence... It makes no sense, in the context of random punishment, to style one's life according to the possibility of being victimized, any more than it makes sense to orient all of one's everyday acts to the possibility of an accident."

## ***Large-scale Violence in Civil War and Military Occupation***

The counterinsurgency campaigns waged by the German and Japanese armies in occupied territories during World War II have been widely described as indiscriminate instances of mass violence. A combination of factors accounted for this practice, including the fact that these armies were fighting a total war, were racist, and lacked the resources for less violent forms of pacification.

Few observations enjoy more currency among historians than the futility of the German and Japanese anti-partisan 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” (Condit, 1961: 268). 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” (Browning, 1990: 68). Nazi reprisals produced a similar effect all over occupied Europe (Mazower, 1998: 179),<sup>10</sup> while Japanese reprisals had similar effects in Asia.<sup>11</sup> Kalyvas (2006) identifies several mechanisms underlying the 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.

An example of how such violence inadvertently produces private goods to be used by the rebels is the case of rebel protection. Such protection emerges as a private good only because of indiscriminate violence. As this violence escalates, so does the value of protection against it. Survival-maximizing civilians will be likely to join and/or collaborate with an organization that credibly offers them protection against an indiscriminate rival. In El Salvador the power of the revolutionary organization often was its ability to provide security for its members. When asked why he joined, a Salvadoran insurgent answered that he “had no choice... It was a matter of survival. Those were the days when *not* to go meant getting killed” (Anderson 2004: 222). A former Muslim rebel in the Southern Philippines remarked that

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<sup>10</sup>In the Soviet Union (Cooper, 1979), Poland (Lotnik, 1999: 87), Bosnia (Gumz, 2001: 1037), Italy (Klinkhammer 1997: 83), and France (Kedward, 1993:190).

<sup>11</sup>In China (Li, 1975:209-10; 231), the Philippines (McCoy, 1980:215), Malaya (Kheng, 1983), Burma (Tucker, 2001), and Vietnam (Herrington, 1997: 21).

he “joined because of the violence created by the *Ilaga* (Christian fighters); because there was no place safe during the trouble at that time” (in McKenna, 1998: 183). In occupied France, “when the acts of reprisals 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” (Kedward, 1993: 190). Historians of the French resistance link its development to German forced labor dragnets: rather than being sent to Germany as industrial laborers, many Frenchmen opted to join the resistance movement – an option they had not considered before the German initiative. As a result, participation in the French *maquis* took off only after the Germans began to recruit laborers (Kedward, 1993).

Under such circumstances, non-participation and free-riding can be extremely costly.<sup>12</sup> An implication is that many rebel organizations often welcome and even provoke state reprisals.<sup>13</sup> What is more, rebel organizations with the capacity to provide civilians with protection from state violence can decide whether to turn it into a public good available to all or, preferably, a private good available only to those individuals or communities who opt to collaborate with them.<sup>14</sup> The latter option turns indiscriminate violence into an extremely counterproductive weapon: the decision by insurgents not to protect a village that is unfriendly to them amounts to exposing it to state violence: in other words, using one’s enemies as one’s own enforcers.<sup>15</sup>

### *The Phoenix Program*

Although the evidence is very suggestive it remains highly unsystematic. This is an exceptionally difficult problem, because civil war tends to degrade data collection capabilities, and because it usually involves systematic attempts to conceal the identities of insurgent (and sometimes incumbent) members. Even systematic knowledge of the identities of the victims of large scale violence, which is now available for some civil wars, is not sufficient for these purposes. Short of extremely detailed ethnographic/historical work it can be very

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<sup>12</sup> On this point see Tone (1994: 78), Stoll (1993: 20), Mason and Krane (1989), and Davis (1988: 23).

<sup>13</sup> See Aussaresses (2001: 62) and Hayden (1999: 39; 57). International sympathy caused by atrocities adds an additional benefit for insurgents.

<sup>14</sup>In Japanese-occupied China, the communists were able to teach peasants how to face Japanese raids following the “run for shelter under enemy attack” *paofan* method. By inducing collective discipline and eliminating free-riding, they were able to turn peasants into a disciplined group; in turn, the peasants won safety, which they could not have achieved on their own (Wou 1994: 231). Similar tactics have been used in many places, including such methods as in-site hiding through the building of underground community tunnels (Vietnam), bunkers (Lithuania), or foxholes and caves (Latin America) (Wickham-Crowley 1991: 43; Lansdale 1964: 85).

<sup>15</sup>An interesting twist is this: as a sanction for tax evasion, the Vietcong sent offenders for “reeducation” in hamlets which were shelled by the government army (Elliott 2003: 873).

difficult to distinguish civilians from combatants based on the facts of individual cases.

In this section we examine a unique data source from the Vietnam War to try to improve our grasp on the rate of victimization of participants in violent collective action relative to the larger population for at least one civil war. By analyzing the individualized targeting data left behind by the notorious US Phoenix Program (and making some fairly cautious assumptions), we derive an estimate of the proportions of Vietcong guerrillas 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 US and South Vietnamese governments to target the Vietcong selectively and 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. This 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” (Herrington 1997: 69).

In essence, the Phoenix Program was a clearinghouse for information gathered by numerous military and police organizations operating as part of the US alliance. The intelligence gathered by Phoenix could then be used to more accurately target individuals for capture or assassination. 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. In practice, very few individuals were killed or captured by forces directly tasked to Phoenix. The system mostly just kept track of what happened to identified persons at the hands of other organizations. In theory, Phoenix was oriented toward identifying and disposing of the most important Vietcong agents, “executive cadre at all levels of the communist apparatus” (Thayer, 1985: 208).

At the end of the war, Phoenix left behind a database, the National Police Infrastructure Analysis Subsystem II - NPIASS-II (NARA: n.d.), 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.” The 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% of those selected, had been killed. We use this and other information in the database to attempt an estimate of the percentage of Phoenix’s victims that was innocent.

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 identified by three or more independent sources, or by an “irrefutable source”, as a Vietcong agent 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. Just about 11% of all the persons identified in the Phoenix list met this standard of confirmation (Table 1).

TABLE 1: PHOENIX PROGRAM RESULTS BY CONFIRMATION STATUS FROM JANUARY 1971

STATUS	CONFIRMED VCI	UNCONFIRMED VCI	
KILLED	<b>4.53%</b> (366)	<b>20.32%</b> (10,341)	18.16% (10,707)
CAPTURED	<b>1.35%</b> (109)	<b>32.21%</b> (16,392)	27.98% (16,501)
DEFECTED	<b>0.21%</b> (17)	<b>15.33%</b> (7,801)	13.26% (7,818)
AT LARGE	<b>93.91%</b> (7,587)	<b>32.14%</b> (16,355)	40.60% (23,942)
KILLED + CAPTURED	<b>5.88%</b> (475)	<b>52.53%</b> (26,733)	46.14% (27,208)
TOTAL	100.00% (8,079)	100.00% (50,889)	100.00% (58,968)

$PR(\chi^2) = 0.000$

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% of those in the confirmed category had been killed, while roughly 94% remained at large. By contrast, 20% of the unconfirmed had been killed, additional large percentages had been captured or had defected to the government – roughly 34% and 18% respectively – while only 25% 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 94 out of every 100

highly suspicious individuals escaped the US and South Vietnamese net entirely. Of those killed, over 96% 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 31.88, while the same odds ratio for killed + captured is about 26.14. That is, an individual had close to 32 times greater odds of being killed, and 26 times greater odds of being captured or killed, in the unconfirmed category.

The simplest and most plausible explanation for these data is that the confirmation process was at least 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 and the second mechanism divided those on the list into confirmed and unconfirmed. It seems unreasonable to assume that the US/GVN directed *more* effort toward locating and “neutralizing” unconfirmed persons than confirmed ones.<sup>16</sup> 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, while 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 favor of this view 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.

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

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<sup>16</sup> According to NPIASS-II, Phoenix was aware of the “current address” of nearly 64% of confirmed persons, but less than 1% of unconfirmed persons. Likewise, nearly 23% of confirmed persons, but less than 1% of unconfirmed persons, were the subjects of individualized arrest warrants. These data strongly suggests that significantly greater effort was oriented toward those under higher suspicion.

be partitioned into Vietcong and innocents, resulting in four integer-valued variables and two equations as follows:

$$\begin{aligned} V_u &\equiv \text{number of Vietcong in unconfirmed category} \\ I_u &\equiv \text{number innocents (civilians) in unconfirmed category} \\ V_c &\equiv \text{number of Vietcong in confirmed category} \\ I_c &\equiv \text{number innocents (civilians) in confirmed category} \end{aligned}$$

$$(1) \quad V_u + I_u = 50889$$

$$(2) \quad V_c + I_c = 8079$$

The equations follow analytically from the first assumption and the totals of confirmed and unconfirmed persons in the data. The first assumption is the most controversial, and we return to 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.<sup>17</sup> In other words, we stipulate that nothing about the process of confirmation itself affected rates of victimization, but rather that these were determined by the characteristics of the actors. Note that this is a conservative assumption, since we would expect more effort to be directed toward those under greater suspicion. The assumption is operationalized in equations (3) and (4):

$$(3) \quad p_{vn}(V_c) + p_{in}(I_c) = 475 / 366 \quad 0 < p_{vn}, p_{in} < 1$$

$$(4) \quad p_{vn}(V_u) + p_{in}(I_u) = 26733 / 10341 \quad 0 < p_{vn}, p_{in} < 1$$

Here we give two totals (once again derived from the data) for each equation, the first for captured + killed, the second only for killed. Below we give estimates based on both sets of totals. The inequalities follow from the definition of  $p_{vn}$  and  $p_{in}$  as proportions.

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. This assumption is summarized in equation (5):

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<sup>17</sup>  $p_{vn}$  stands for “proportion of vietcong neutralized” and  $p_{in}$  stands for “proportion of innocents neutralized”.

$$(5) \quad \frac{\frac{I_U / 50889}{V_U / 50889}}{\frac{I_C / 8079}{V_C / 8079}} = \frac{I_U V_C}{I_C V_U} = 26.1400$$

Once again, the four denominators are derived from the data, while the odds ratio compares captured + killed/at large across the confirmed and unconfirmed categories.<sup>18</sup>

Equation (5) is easier to grasp intuitively if we rearrange the terms as follows:

$$(5^*) \quad \frac{I_U}{V_U} = 26.14 \frac{I_C}{V_C}$$

What this equation says is that the ratio of innocents to Vietcong in the unconfirmed category is more than 26 times greater than the same ratio for the confirmed category. On its face, it makes considerable sense to assume a higher ratio of innocents to Vietcong in the unconfirmed category. Since "confirmation" was a more rigorous selection process, we should not be surprised to see that it resulted in more "hits", that is more correctly identified Vietcong. The question is: how much higher? Equation (5) implies that the entire difference in the rate of victimization between the confirmed and unconfirmed categories is accounted for by the composition of these categories. This assumption exploits and operationalizes the argument we defended above, namely that the reason "confirmed" persons were much less likely to be victimized than unconfirmed persons is precisely because they were much more likely to be real Vietcong agents.<sup>19</sup>

Returning to the first assumption, that membership and non-membership 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 *important* Vietcong agents were

<sup>18</sup> Why should we use killed + captured, instead of just killed? Both capture and assassination required that an individual be physically located and identified by forces sufficient to take action against him. In fact, 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.

<sup>19</sup> It is worth pointing out in this connection that were we to assume an odds ratio of 1, that is were we to assume that the confirmed and unconfirmed categories had approximately the same composition, there are no possible solutions that satisfy the remainder of our system of equations. An odds ratio slightly higher than 5 is the lowest possible value consistent with the remaining equations.

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 (1985).<sup>20</sup> 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, it would require more fine-grained data than we have available to evaluate this supposition rigorously. 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. Also, Phoenix includes agents' supposed "echelon," or the scale of operations in which they were involved (e.g. 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 non-members, 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 confusing. Full party members were less likely to be captured *or* killed than non-party members, but more likely to be killed. Probationary party members were more likely to be killed or captured than full members, non-members, or those whose membership status was unknown. Similarly, controlling for echelon does not affect the 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 either. The data suggest at best a very weak relationship between perceived importance and the rates at which individuals were captured or killed.

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<sup>20</sup> As an important Defense Department analyst during the Vietnam War, Thayer had full access to all the data we use here, as well as 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 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.

Returning to summarize our model, we have the following 5 equations in 6 variables:

$V_u$	$\equiv$	<i>number of Vietcong in unconfirmed category</i>
$I_u$	$\equiv$	<i>number innocents (civilians) in unconfirmed category</i>
$V_c$	$\equiv$	<i>number of Vietcong in confirmed category</i>
$I_c$	$\equiv$	<i>number innocents (civilians) in confirmed category</i>
$p_{in}$	$\equiv$	<i>proportion of innocents victimized</i>
$p_{vn}$	$\equiv$	<i>proportion of Vietcong victimized</i>

$$(1) \quad V_u + I_u = 50889$$

$$(2) \quad V_c + I_c = 8079$$

$$(3) \quad p_{vn}(V_c) + p_{in}(I_c) = 475 / 366 \quad 0 < p_{vn}, p_{in} < 1$$

$$(4) \quad p_{vn}(V_u) + p_{in}(I_u) = 26733 / 10341 \quad 0 < p_{vn}, p_{in} < 1$$

$$(5) \quad \frac{\frac{I_u / 50889}{I_c / 8079}}{\frac{V_u / 50889}{V_c / 8079}} = \frac{I_u V_c}{I_c V_u} = 26.1400$$

Obviously, these equations cannot be solved for unique solutions. Instead, we graph 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 subject our results to a “robustness check,” that shows our results hold even under weakened assumptions.

Figures 1 and 2 contain all solutions based on the number of individuals victimized (killed + captured). Figure 1 graphs the proportion of innocents victimized ( $p_{in}$ ) against the difference  $I_u - V_u$  (the number of innocents in the unconfirmed category minus the number of Vietcong in the unconfirmed category, which must sum to a constant) and against the difference  $V_c - I_c$  (Vietcong in the confirmed category minus innocents in the confirmed category, which once against must sum to a constant). In Figure 2 we graph the proportion of Vietcong victimized ( $p_{vn}$ ) against the same two quantities. In Figures 3 and 4, we repeat the same graphs, but in this case using only the totals for persons killed.

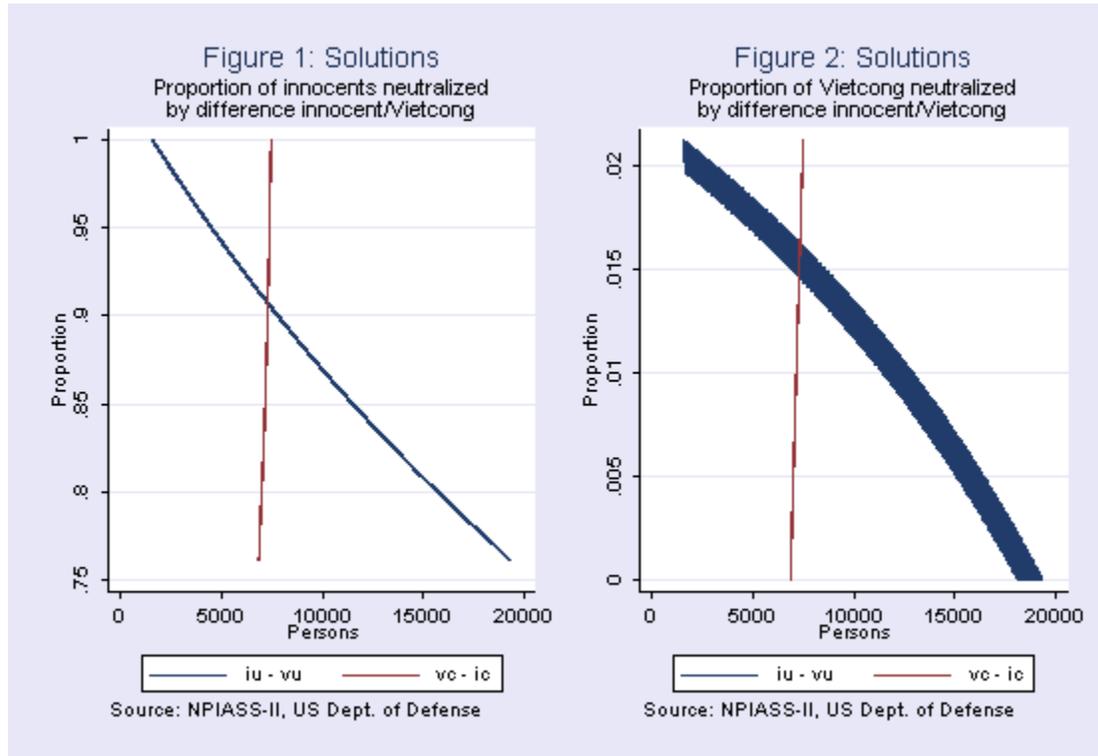


Table 2 exhibits the two extreme solutions of Figures 1 and 2, along with an approximately intermediate solution.<sup>21</sup> Although there are thousands of possible combination of values consistent with the inequalities and definitions, all fall within a fairly narrow range of possibility. On one extreme, the model is consistent with all but one victim innocent. At the other extreme, only 689 out of a total 27208 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% of actual Vietcong captured or killed, and about 86% 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* 38 innocents for every 1 actual Vietcong agent (the intermediate solution is about 78 innocents for every 1 Vietcong).

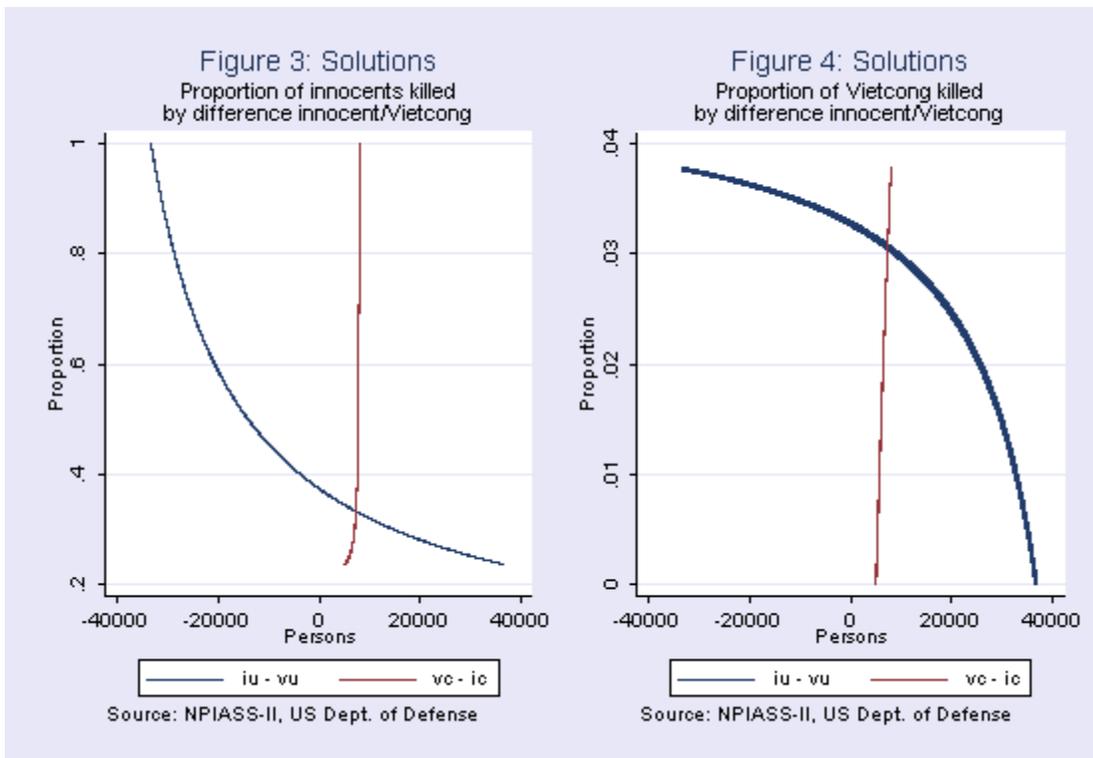
Two interesting subsidiary observations are worth noting. First, our findings are consistent with the belief that the Phoenix Program was pretty good at identifying its enemies: over all solutions, 40 – 55% of the individuals selected into Phoenix are estimated to have been Vietcong. Of those subjected to the

<sup>21</sup> In this context, there are various ways to conceive of an intermediate solution. We chose a solution in which the number of innocents vs. Vietcong victimized is intermediate between the extremes.

more rigorous process of “confirmation,” we estimate that 92 – 96% were Vietcong. Identifying them was one thing, catching them another entirely. Second, for *all* solutions, more Vietcong were victimized in the unconfirmed category than in the confirmed category. These two observations suggest that, while Phoenix’s results may have been inaccurate, the program was by no means irrational, either in its selection procedures or in its targeting of “unconfirmed” persons.

TABLE 2: HOW MANY INNOCENTS WERE VICTIMIZED? EXTREME AND INTERMEDIATE SOLUTIONS

	MAXIMUM	INTERMEDIATE	MINIMUM
CONFIRMED INNOCENTS ( $I_c$ )	624	443	310
CONFIRMED VIETCONG ( $V_c$ )	7455	7636	7769
UNCONFIRMED INNOCENTS ( $I_u$ )	35133	30875	26209
UNCONFIRMED VIETCONG ( $V_u$ )	15756	20014	24680
PROPORTION OF INNOCENTS VICTIMIZED ( $P_{VN}$ )	0.761	0.856	0.999
PROPORTION OF VIETCONG VICTIMIZED ( $P_{IN}$ )	0.000027	0.012	0.021
TOTAL INNOCENTS VICTIMIZED	27207	26864	26519
TOTAL VIETCONG VICTIMIZED	1	344	689
TOTAL VICTIMIZED	27208	27208	27208



In Table 3, we repeat the exercise using figures for killed only, showing the extreme and intermediate solutions from Figures 3 and 4. 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 13,000 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 (i.e. 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 logistic regression estimates of killed/captured and killed on confirmation status, age, and sex.

TABLE 3: HOW MANY INNOCENTS WERE KILLED? EXTREME AND INTERMEDIATE SOLUTIONS

	MAXIMUM	INTERMEDIATE	MINIMUM
CONFIRMED INNOCENTS ( $I_c$ )	1556	366	63
CONFIRMED VIETCONG ( $V_c$ )	6523	7713	8016
UNCONFIRMED INNOCENTS ( $I_u$ )	43969	28327	8749
UNCONFIRMED VIETCONG ( $V_u$ )	6920	22562	42140
PROPORTION OF INNOCENTS KILLED ( $P_{IN}$ )	0.235	0.340	0.999
PROPORTION OF VIETCONG KILLED ( $P_{VN}$ )	0.00000075	0.031	0.038
TOTAL INNOCENTS KILLED	10707	9759	8811
TOTAL VIETCONG KILLED	0	948	1896
TOTAL KILLED	10707	10707	10707

The data suggest an interesting story of “profiling” used in the infliction of violence. First, age has a strong association with both dependent variables. The odds of being killed decreased by about 2.7% for every year of age, while the odds of being captured *or* killed decreased by about .8% for every additional year. The association between sex and violence is more complicated: women had slightly higher than twice the odds of being captured *or* killed once identified by Phoenix, but men had nearly 5.5 times greater odds of being killed. In fact, the database shows that well over 50% of all the women in the database were captured, while slightly less than 5% were killed. These results are consistent with Jones’s (2000) 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 non-violent victimization. If the Phoenix Program was in fact as inaccurate as we think, then the age and sex results suggest a particularly acute dilemma for military age Vietnamese men during these years. In effect, we should assume

that the denominator for calculating civilian risk is much smaller than the general population. Old folks, young children, and women may have been probabilistically “profiled out” in the process of selecting individuals for lethal violence.

TABLE 4: LOGISTIC REGRESSION ESTIMATES ON KILLED/CAPTURED AND KILLED<sup>22</sup>

	DV = KILLED/CAPTURED	DV = KILLED
CONFIRMATION	0.064 (-54.91)	0.267 (-22.86)
SEX (MALE = 1)	0.460 (-33.54)	5.445 (36.08)
AGE IN YEARS	0.992 (-10.29)	0.970 (-25.24)
PSEUDO-R <sup>2</sup>	0.11	0.07
N	48435	48435

SOURCE: US DEPARTMENT OF DEFENSE, NPIASS-II. ALL COEFFICIENTS IN ODDS RATIO FORM. Z-STATISTICS IN PARENTHESES.

How should we contextualize these results? There are obvious limits to our ability to generalize, 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 larger numbers of deaths resulted from conventional battlefield confrontations of the US 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 selective and individualized 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% of neutralizations over a period of a year and a half analyzed by Thayer (1985). The vast majority of deaths were associated with the ongoing “military operations” of local paramilitary forces (Elliott, 2003: 1137), though what these operations consisted of is difficult to say. Thus, it is probable that Phoenix reflected more general processes of violence that had been ongoing in southern Vietnam for many years.

We would need to know considerably more than we do about the rate of Vietcong membership in the overall population in order to determine who was safer overall. 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 US efforts in the Vietnam War. Phoenix was the fruit of what they learned. Thus, we have some reason to suppose that it was more

<sup>22</sup> These results should be treated with caution due to large numbers of missing values on the age variable. Note also that the inclusion of the age and sex variables has virtually no effect on the size of the coefficient for confirmation.

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% of the prisoners held at Abu Ghraib at the time were unconnected to the insurgency (ICRC 2004).<sup>23</sup>

Yet, indiscriminate and inaccurate violence is generally associated with the poorly trained and disciplined soldiers of developing country armies, rather than with 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 much 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, it is a hard case for us).

It is worth asking here why violence that is selective in intent may end up being so inaccurate. 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. 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" (Moyar, 1997: 114). A South

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<sup>23</sup> "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" (ICRC, 2004: 8). Even the highly selective process by which individuals were shipped to Guantanamo appears to have suffered from similar problems: it turns out that 92% of the 517 Guantanamo detainees had not been al-Qaeda fighters, while 95% of them were not captured by the Americans themselves; some 86% were handed over in Afghanistan and Pakistan after a widespread campaign in which big financial bounties were offered in exchange for anyone suspected of links to al-Qaeda and the Taliban (Simpson 2006).

Vietnamese colonel told Moyar (1997: 116) about the men of the Provincial Reconnaissance Units, among those relying 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)." A U.S. adviser confirmed this tendency 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" (quoted in Moyar, 1997: 293). The same tendency can be observed in many civil wars (Kalyvas, 2006). For example, in El Salvador, many false denunciations were "enough to seal one's fate, since government forces seldom ought to investigate the charges and 'innocent until proven guilty' was not a principle recognized by the military, security forces, or ORDEN civilian irregulars" (Binford, 1996: 107).

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?'" (quoted in Moyar, 1997: 122). 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" (Elliott, 2003: 91, 947). 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" (in Maass, 2005: 47).

### *The Greek Civil War*

While extremely detailed and systematic, the Phoenix data covers only a subset of individuals<sup>24</sup> and provides little information about actual insurgent combatants, focusing rather on alleged civilian agents and collaborators. We conclude with data from a regional study on the Greek Civil War. Like the Vietnam War, this was a complex war fought in different phases between 1943 and 1949 and entailing extensive foreign presence.

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<sup>24</sup> In other words, there may be a selection bias in the Phoenix database. The data give us no way to evaluate this possibility systematically. Yet, the motivational mechanism for denunciation established in Kalyvas (2006) leads us to believe any such bias is likely to be small. That is, we believe as a general matter that denunciation in civil war is at best weakly correlated with guilt.

Using extensive archival sources, we were able to calculate the exact toll of violence in one region, the Argolid, located in the northwest tip of the Peloponnese, Greece's southern peninsula. The civil war in that area was fought during a single year, in 1943-44, coinciding with the German occupation of Greece. The conflict pitted members of the pro-communist resistance army ELAS (Greek Popular National Army) against right-wing collaborationist militias supported by the German occupation troops. Both resisters and collaborationists recruited locally and the war had a strong "neighbor against neighbor" aspect.

TABLE 5: REBEL COMBATANT AND CIVILIAN VICTIMIZATION RATES, ARGOLID, 1943-1944

TOTAL RURAL CIVILIAN POPULATION	45140
MILITARY AGE MEN	13542
VICTIMS OF GERMAN/COLLABORATIONIST VIOLENCE	353
PROBABILITY OF CIVILIAN VICTIMIZATION	.0261
REBEL COMBATANTS	500
REBELS KILLED IN ACTION	20
PROBABILITY OF REBEL VICTIMIZATION	.04

Between September 1943, when the conflict began, and September 1944, when the Germans left, the collaborationists and Germans killed 353 individuals, slightly less than one percent of the rural population of that area (.78%). Although a few women and children were killed during these operations, about 90% of the victims were men of military age (18-45 years old). Assuming that men of military age made up about 30% of the total population, we estimate that the likelihood that a man of military age inhabiting the rural Argolid was killed by the occupation army to be 2.61%. On the other hand, 20 local rebel fighters (members of the 6<sup>th</sup> Regiment of ELAS-- which operated in the area) were killed in action during the same period-- about 4% of the Argolid rebels (Table 5).<sup>25</sup>

The difference between 2.61% and 4% is rather negligible and falls within the range of measurement error. The assumption that joining the rebels was a highly risky choice relative to not joining, is unsupported by the evidence. In fact, this slight difference vanishes when one takes into account two additional aspects of the violence. First, the Germans and their local auxiliaries did not restrict themselves to killing civilians. They drove hundreds

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<sup>25</sup> The data on rebels killed in action is from Vazeos (1961: 131-4). The 6<sup>th</sup> regiment of ELAS which was active in the Argolid and Korinthia areas and recruited primarily from these two regions reached a force of 3,500 men in October 1944, after the occupation's end (Vazeos, 1961: 96). However, many men were recruited after the Germans left. An estimate of recruits from the Argolid prior to the Germans' departure is 500. Another 15 rebel fighters were killed during the battle of Athens, in December 1944, when the Communist resistance lost its bid for power against the Greek government and the British. We exclude them from the analysis because after the departure of the Germans the violence against civilians subsided and the Communists, in total control of the Greek countryside, drafted thousands of peasants into their army.

more to prisons and concentration camps in Greece, and slave labor camps in Germany.<sup>26</sup> In contrast, the Germans did not take rebel prisoners. In fact, a significant number of the rebels killed in action were actually shot after being captured. The problem from the Germans' perspective was that the rebels avoided contact and were able to hide in the mountains and escape the dragnets. Second, violence was not uniformly distributed; some villages had comparatively many more victims. The worst-hit villages exhibited rates of civilian victimization approximating 10%. In these villages, it was clearly *safer* for military-aged men to join an armed faction rather than stay home.

To summarize, the data from the Argolid contradict both core assumptions of the collective action paradigm (the benefits of free-riding and the risk of joining an insurgency). In conjunction with the anecdotal evidence from a wide range of civil wars and the Phoenix data, these data challenge the collective action paradigm.

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<sup>26</sup> The rosters of the local prison in the town of Nafplio show over 1,000 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.

## *Conclusion*

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Our data question the assumption that insurgency is automatically subject to the collective action problem. “Free riding” is not as free in civil wars as currently thought. If the collective action paradigm has been so dominant it is because scholars have tended to overestimate the risks faced by rebel fighters and/or underestimate the risks faced by nonparticipants—a result of limited attention to the dynamics of violence and of the tendency to impute preferences over investigating them empirically. Tullock’s (1971: 93) critique of historians and social scientists of the revolutions literature for imputing individual motivations for participation from the observed macro ‘public good’ post-hoc, was right on target. But, in the years that followed his pathbreaking analysis, many social scientists have likewise inferred the calculations that supposedly lead people to join an insurgency from empirically unsupported assumptions about comparative risk.

Our data suggest that taking the collective action problem seriously implies that sometimes the real puzzle in civil wars is nonparticipation rather than collective action. Some simple mechanisms suggest themselves. First, if civil war works as we suggest, then membership in a rebel organization can be a “club good”. That is, members of the club receive special benefits, both material and nonmaterial, from which non-members are excluded. Consider William McNeill’s (1947: 80-1) description of the process of joining ELAS:

In actual fact, 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 psychological 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 peasant’s son found himself irresistibly attracted to the guerrilla life; and an over abundant peasant population made recruitment easy. Fewer came from towns; life was relatively comfortable there, and EAM had other work for townsmen, organizing strikes or serving as propagandists among the more illiterate peasants. From the very beginning the chief factor that limited the number of the guerrillas was lack of weapons.

The classic club goods problems from economics are adverse selection and moral hazard. A rebel organization provides security and resources to its members, but it also seeks tough, ruthless, and determined combatants who share its agenda and goals. Much like insurance companies seeking to screen out individuals with serious pre-existing conditions, rebel groups will set up mechanisms to restrict entry, and they will reject many volunteers, something that we should not observe if the collective action problem holds. By the logic of moral hazard, rebels should accept the security benefits of being in the

organization, while trying to minimize their effort and risk. Furthermore, as suggested by McNeill, rebel organizations do not always maximize recruitment. Faced with limited logistic means, few weapons, and a limited capacity to support thousands of troops, many organizations prefer to recruit relatively few active full-time fighters.

We have argued in this paper that the assumption of a collective action problem automatically emerging in situations of civil war is unsupported by available systematic evidence. Rethinking the application of the collective action problem to contexts of large scale violence 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 violence. For instance, stalemated civil wars of long duration are puzzling if we assume that combatant risk significantly exceeds civilian risk. If, on the other hand, participation improves individuals' short-run survival prospects, the puzzle is easily resolved. Our approach is also a promising way to account for the highly counterintuitive (yet common) phenomenon of people who act as combatants for the "wrong" side in civil wars (e.g. Chechens in pro-Russian militias, Kurds in the Turkish army, Mayan Guatemalans in government militia, etc.). Last, we point to private goods provided by rebel organizations, such as protection, which have been eclipsed by the recent focus on looting; likewise, our analysis shows 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 are linked almost exclusively to expectations about outcomes.

In terms of comparative patterns, we would expect rebel recruitment to display a convex function: On the one hand, recruitment should go up under conditions of complete absence of the state; such absence would eliminate the risk of state sanctions against rebels, consistent with Fearon and Laitin (2003), while it would make possible the implementation of rebel sanctions targeting free-riding. On the other hand, rebel recruitment should also increase under conditions of extreme violence by the state, provided the rebels are able to survive. Significant but lower levels of state violence should, everything else, depress rebel recruitment, both because of the disruption of rebel operations and the increase in participants' risk. Likewise our analysis suggests the importance of paying close attention to the temporal and spatial variation of violence: not all civilians 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 behavioral range between the opposite stances of rebel fighter and nonparticipant is very large and includes all types of collaboration. Clearly, more complex models of participation and recruitment call for fine-grained micro-data.

In short, an enhanced understanding of civil wars requires a double correction: an emphasis on systematic evidence from a wide cross-section of individuals and an awareness of the dynamics of violence. Both require a much more systematic micro-empirical approach than currently practiced.

## Appendix

To obtain the solution set  $D$  for the system of equations generated by our assumptions about Phoenix, we created the set of all possible integer-valued 4-tuples  $D \in \{(I_U, I_C, V_U, V_C)\}$  such that:

$$(1) \quad V_U + I_U = 50889$$

$$(2) \quad V_C + I_C = 8079$$

(i.e. 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) and (4) 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 (3.2) and (4.2)]. In each case, we solved equation (3) for  $p_{vn}$ , simplified [equations (3.1) and (3.2)], substituted the result into equation (4), and simplified again, resulting in equations (4.1) and (4.2).

$$(3.1) \quad 0 < \frac{475}{V_C} + p_{in} \left( \frac{I_C}{V_C} \right) < 1$$

$$(4.1) \quad 0 < \frac{26733V_C - 475V_U}{I_U V_C - I_C V_U} < 1$$

$$(3.2) \quad 0 < \frac{366}{V_C} + p_{in} \left( \frac{I_C}{V_C} \right) < 1$$

$$(4.2) \quad 0 < \frac{10341V_C - 366V_U}{I_U V_C - I_C V_U} < 1$$

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:

$$(5.1) \quad 25.64 < \frac{I_U V_C}{I_C V_U} < 26.64$$

Taken together,  $D$  subject to the conditions (1), (2), (3.1), (4.1), (5.1) or (1), (2), (3.2), (4.2), (5.1) is an exhaustive specification of our solutions.

We also subjected our results to a “robustness” check, by modifying condition (5.1) for a 10-unit interval around the odds ratio taken from the data:

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

Using this larger band for the odds ratio resulted in the solutions outlined in Tables 5 and 6, below. 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 6: HOW MANY INNOCENTS WERE VICTIMIZED? EXTREME AND INTERMEDIATE SOLUTIONS  
(ROBUSTNESS CHECK WITH  $21.14 < \text{ODDS RATIO} < 31.14$ )

	MAXIMUM	INTERMEDIATE	MINIMUM
CONFIRMED INNOCENTS ( $I_c$ )	666	398	214
CONFIRMED VIETCONG ( $V_c$ )	7413	7681	7815
UNCONFIRMED INNOCENTS ( $I_u$ )	37488	29202	26063
UNCONFIRMED VIETCONG ( $V_u$ )	13401	21687	24826
PROPORTION OF INNOCENTS VICTIMIZED ( $P_{IN}$ )	0.713	0.904	0.999
PROPORTION OF VIETCONG VICTIMIZED ( $P_{VN}$ )	0.0000097	0.015	0.027
TOTAL INNOCENTS VICTIMIZED	27208	26768	26327
TOTAL VIETCONG VICTIMIZED	0	440	881
TOTAL VICTIMIZED	27208	27208	27208

TABLE 7: HOW MANY INNOCENTS WERE KILLED? EXTREME AND INTERMEDIATE SOLUTIONS  
(ROBUSTNESS CHECK WITH  $21.14 < \text{ODDS RATIO} < 31.14$ )

	MAXIMUM	INTERMEDIATE	MINIMUM
CONFIRMED INNOCENTS ( $I_c$ )	1592	350	54
CONFIRMED VIETCONG ( $V_c$ )	6487	7729	8025
UNCONFIRMED INNOCENTS ( $I_u$ )	45000	27864	8701
UNCONFIRMED VIETCONG ( $V_u$ )	5889	23025	42188
PROPORTION OF INNOCENTS KILLED ( $P_{IN}$ )	0.230	0.345	0.999
PROPORTION OF VIETCONG KILLED ( $P_{VN}$ )	0.000025	0.032	0.039
TOTAL INNOCENTS KILLED	10707	9731	8755
TOTAL VIETCONG KILLED	0	976	1952
TOTAL KILLED	10707	10707	10707

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