AGRARIAN INEQUALITY AND THE THEORY OF PEASANT REBELLION*

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Insurgency has largely subsided in Central America, but the academic debate over the causes of the violence in the 1980s waxes hotter than ever. As scholars, we have an obligation to subject our theories to the acid test of reality. As individuals interested in the policy process, we must evaluate the outcomes of those policies, even though they are sometimes based on faulty readings of our theories. The increasingly rich body of literature and data available on Central America compels observers to move away from the speculation that dominated early scholarship on the region and toward serious empirical tests of our theories. In doing so, scholars will be in a position to evaluate the policies pursued by the United States and various Central American governments. My article in this journal on land tenure in El Salvador attempted to address both theory and policy with new data (Seligson 1995). Judging by the reactions of Martin Diskin and Jeffery Paige, the conclusions that I drew have succeeded in stimulating a rich debate.

Although I am being allowed the "last word" in this debate in this forum, it is unlikely to be the last word on the subject. My related piece written earlier with Edward Muller, "Insurgency and Inequality" (published in the *American Political Science Review*) has already generated several rounds of critical symposia and is now subjected to further scrutiny in Diskin's comments on my article in these pages (see Muller and Seligson 1987; Muller, Seligson, and Fu 1989; Brockett 1992; and Muller, Dixon, and Seligson 1993). My response is intended to advance the debate by first answering some of the questions raised by Diskin and Paige and then going beyond that discussion to challenge some of our basic theoretical assumptions on this topic. I suggest that we need to rethink our theories of peasant rebellion.

Response to Diskin

In many ways, Martin Diskin's critique is the easier one to address in that, rather than offering new and fruitful lines of investigation as does

*I would like to thank Ariel Armony, John Booth, Juliana Martínez, and Vadim Staklo for reading and commenting on a draft of this essay.

Paige's comment, its argument is based on a systematic (and rather polemical) misinterpretation of my article. The very title of Diskin's comment seems designed to mislead readers. He refers to "The Disappearance of the Agrarian Question in El Salvador," when no reading of my article could lead one to conclude that I claim the "disappearance" of the issue. What I suggest is that relative decline (not disappearance) of the agricultural sector has occurred and with it a decline in the proportion of the population that is landless and land-poor.

One motivation for my article was the desire to demonstrate that a considerable amount of the previous research on the agrarian question in El Salvador was not based on solid research. Among the works I critiqued is the Oxfam report to which Diskin contributed (see Simon and Stephen 1982). Given my critique, one would expect Diskin to have included in his comment a strong defense of the earlier Oxfam estimates of the land problem. Surprisingly, he does not, and one therefore must assume that he has accepted my critique of the Oxfam report's faulty efforts to provide a reasonable estimate of the magnitude of the problem. I referred in a single paragraph to the 1982 Oxfam *Impact Audit* that became a much-quoted source of information on El Salvador during the early part of the civil war (Seligson 1995, 46). The Oxfam report claimed, citing a published analysis of the census data, that "65 percent of the population is landless" (Simon and Stephens 1982). Diskin does not refute my rereading of the source Oxfam cites, showing that the figure cited in that source was only 29 percent of rural families, not the 65 percent that Oxfam claimed. Precisely these kinds of misleading claims about the land tenure picture in El Salvador motivated my reexamination of the data. Diskin could not defend the Oxfam report because his own estimate in commenting on my article is that the landless accounted for only 22.4 percent of the economically active population—or 33.5 percent if the tenant and land-poor populations are added—or about half of the Oxfam estimate (see Diskin's comment, p. 117 and his table 3). On this point, I can only conclude that Oxfam's 1982 work (with the supplement written by Diskin) was wrong and should no longer be taken seriously as an accurate source on this matter.

Diskin's central argument is that my data "explain away the persistent and grave social problems that continue to face El Salvador following a dozen years of bloody civil war" (p. 112). He asserts that my article deliberately underestimates the problem of landlessness in El Salvador and does so in order to provide a "conservative estimate" so as to make the case that "redistributive policies are a waste of money" (p. 122).

Dispassionate readers of my original article and Diskin's critique will wonder if he was reading the same paper that I wrote. How can Diskin make the case that I "explain away" El Salvador's serious agrarian-based social problems when the conclusion of my article states unequivocally, "The unavailability of sufficient land will prevent most [of the rural poor] from acquiring land and thus condemn them to absolute poverty and relative deprivation. . . . For most of the landless, land-poor, unemployed, and tenant farmers in El Salvador—the very ones in whose name the war was fought—neither the war nor the peace following it will lift them out of poverty" (Seligson 1995, 71). I go on to state that the land problem is not only serious but unlikely to disappear for the foreseeable future. The last sentence of my article reads, "in the final analysis, it may be that as peace comes to El Salvador, the legacy of human suffering caused by land scarcity and overpopulation will remain an enduring feature of the landscape for decades to come" (p. 71). I cannot understand how Diskin's critique squares with these statements.

Where did Diskin get the notion that I was trying to "explain away" El Salvador's serious rural problems rather than providing a reasoned estimate of the magnitude of the landless, land-poor, unemployed, and tenant populations of the country? After reading my estimate that in 1991, 295,130 Salvadorans (50.7 percent of the economically active adult agricultural population) were landless or land-poor, how can he conclude that I have attempted to minimize the magnitude of the problem? How can he claim that I was trying to provide reassurances to USAID when my article explicitly refutes the agency's own claims that Phase III of the 1981 land reform, the so-called land-to-the-tiller reform, caused an 80 percent reduction in tenancy? In fact, I show that tenancy has increased from 38 percent of farms in the 1971 census to 50 percent in 1991. And how could Diskin claim that I appear "opposed to conventional tenurial agrarian reform" (p. 115), when neither this article nor many other studies I have written could possibly be interpreted that way?

The article under discussion provides a review of El Salvador's land reform and concludes that partly because of that reform, the number of landless and land-poor have declined since 1971 and that the peace accords' land program will have a further beneficial impact (p. 65). I go on to lament the fact that given the size of El Salvador's population and the land available for agricultural purposes, some one million hectares of land (the equivalent of 45 percent of all land in El Salvador) would be needed to be able to provide an equal amount of land to the remaining landless, land-poor, and unemployed population of El Salvador of 1991.

Although I have not been blind to the many failings of land reform in Latin America, I have reported in other articles on the numerous positive results that I have found associated with it (see Seligson 1977, 1979, 1982, 1983, 1984). In a recent policy paper for USAID reviewing the successes and failures of land reform programs throughout the world, I argued for the importance of such reforms as part of a program to distribute assets to the agricultural poor (Lambert and Seligson 1994). Yet land reform is not a panacea, and when it is not accompanied by explicit efforts to redistribute income, its direct effect on mitigating insurgency induced by inequality can be minimal (Muller and Seligson 1987, 443).[1] One cannot dismiss the World Bank's recent conclusion that land reform can have a grave downside for the poorest of the poor: "A redistribution from large to small farmers can . . . reduce demand for hired labor, depressing wages or driving into poverty those who lose their jobs and do not gain their own land. . . . Land reform policies that restrict the use of tenant farmers have the paradoxical effect of closing off opportunities for agricultural laborers at the bottom of the heap" (World Bank 1995, 4).

Diskin made these inaccurate interpretations of my article because, I have sadly come to conclude, he was trying to set me up as a straw man. No clearer evidence of that emerges than in his reference to the USAID report on which my *LARR* article was partially based (Seligson et al. 1993). Diskin states, "some things not imported from that report are telling" and concludes that in that report I made a "startling statement" for which he accuses me of "telling USAID that redistributive policies are a waste of money" (p. 122). Readers probably do not have that report, but Diskin did when he wrote his critique. He is aware that the report focused on the 1991–1992 data rather than on comparing those data with the 1961 and 1971 censuses (as noted on the first page of my 1995 article in the acknowledgements). Indeed, it was only after submitting the report to USAID and thinking about developing an article out of the material that it struck me that the academic audience would be particularly interested in the long-term view of land tenure in El Salvador. The entire emphasis in the report to USAID was on impressing the agency with the existence of large numbers of landless and land-poor Salvadorans, most of whom have no hope of being assisted by the peace accords. Had I been trying to minimize that reality, I would not have written as the very first sentence of the executive summary (often the only portion of a consultant's report that policymakers have time to read): "It is both ironic and disturbing that El Salvador's 12-year civil war in which over 75,000 lives were lost and countless millions of dollars of damage was done to infrastructure, precious little progress has been made in resolving one of the central causes of that war, namely the problem of landlessness among the peasantry" (Seligson et al. 1993, 1). Few readers, I believe, would interpret those words as an effort to "reassure USAID" by minimizing the magnitude of the land tenure problems in El Salvador. That Diskin still seems to believe that additional major land reform is possible in El Salvador whereas I do not (for the reasons outlined in the report and the article) in no way implies that I was arguing that land reform is "a waste of money." Nor is

1. Estimating the impact of land reform on patterns of land tenure is not easy. Marc Edelman and I recently showed that our understanding of land tenure patterns has been inhibited by reliance on data sources that, we now contend, have provided systematically biased estimates (Edelman and Seligson 1994).

he justified in saying that "Seligson's research and his specific methodology were designed to show that destabilizing violence will not occur under present conditions" (p. 123). The research project for USAID was not designed a priori to prove any particular point regarding stability or instability but to provide an updated picture of land tenure in El Salvador from which individual scholars and policymakers could draw their own conclusions.

What seems to have irked Diskin the most is that in my article, I opt to provide what I call a "conservative estimate" of the landlessness problem. He concludes from this approach that I was trying to minimize the problem. Nothing could be further from the truth. To demonstrate this point, it is necessary to review briefly the circumstances under which the research was undertaken for USAID. As chief of party of the land tenure team contracted by USAID, I was told that we were to develop a study that could assist the government of El Salvador in developing postwar plans for the agricultural sector and that we should do so based on two key assumptions: first, because the FMLN had agreed to the peace accords and they contained specific and wide-ranging programs for land distribution, the problem of landlessness had been essentially resolved; and second, because no agricultural census had been taken since 1971, no current database existed on which to draw conclusions about the current land tenure picture in the country.

Shortly after beginning work on the USAID study I came across the EHPM database analyzed in my article in *LARR*. It did not take long to determine from that database that USAID's working assumption that landlessness would disappear with full implementation of the peace accords was fundamentally erroneous. As a consultant, I was faced with the task of demonstrating that reliable data on the subject existed and that the persisting tenure problem was likely to remain long after all those who had been promised land by the peace accords were granted their parcels. I therefore decided that in order to make the study credible, I would carefully avoid any appearance of overestimating the magnitude of the landlessness problem. Other studies (including the Oxfam report) had made these kinds of overestimates and lost their credibility. My study therefore took great pains to develop a careful, defensible typology of land tenure types and patterns. Hence, the "conservative estimates" contained in the study were intended by no means to minimize the problem but rather to convince the skeptical that a serious land tenure problem remained in the country. It is for this reason, for example, that the population between ten and fifteen years old was excluded from my analysis, given that many of those young people will eventually inherit their parents' land. Diskin agrees with the rationale for that exclusion but immediately contradicts himself by arguing that I should have devised a separate estimate with this category included in order to avoid "disguising the truly desperate rural situation" (p. 114). Including these youngsters would be the equivalent of counting high school students in the United States as poor because the allowance they receive from their parents is below the minimum wage.

It is true nonetheless that I do not end up recommending land reform as a solution to the problem, and that outcome bothers Diskin as well. But he is unable to explain where the land is to come from. My article examines the possibilities and finds that a million hectares (45 percent of the size of the entire country) would be needed to provide 3.5 hectares of land to satisfy the 1991 population of landless, land-poor, and unemployed agricultural workers (1995, 65). Short of building dikes in the Pacific Ocean or invading Guatemala or Honduras, such land is not available.[2] Moreover, the cost to the economy would be enormous and the prospects for recovering the investment, dim. As has been pointed out in the report to USAID by the analysis conducted by William Thiesenhusen, even though some of the country's best farmland was turned over to cooperatives during the 1980 land reform, many are operating at a loss and only 3.3 percent of the original mortgage debt acquired in 1980 has been paid off (Seligson et al. 1993, xiii). Perhaps most important, the political will to carry out such a reform is absent. The incumbent ARENA government would strongly oppose any efforts to widen the reforms of the peace accords, nor is it clear that support for another major agrarian

2. Paige's critique speculates about a new land reform that would confiscate the coffee farmlands. He admits that such speculation is "purely academic. Seligson is right. As a practical matter, there is no more land to distribute because it is politically impossible to redistribute coffee lands that constitute the core source of wealth and the backbone of the ARENA party" (p. 137). But Paige's speculative figures vastly overstate the amount of land that would become available under such a reform. He calculates that the average size of the coffee estates is 70 hectares, which means that the average coffee farm is smaller than the original land-reform lower limit of 100 hectares and far below the 1982 constitutional limit of 243 hectares. In order to expropriate the average coffee farm legally, the constitution would have to be amended to allow redistributing farms that are smaller than one-third of the current constitutional limit. Under revolutionary circumstances, such a change could be effectuated. But even if the constitution were to undergo such a radical modification, Paige's figures still do not add up. He calculates that the land available could provide farms for 75,000 families, but to make this calculation, he has to redistribute all the land in the coffee estates, leaving the former owners without a single hectare of land. Virtually all land reforms leave former owners with some land. Even if such a massive redistribution program were politically possible (and Paige and I agree that it is not), the problem of insufficient land would still not be solved. First, what would be done with the owners of the coffee plantations? If they were to be compensated for their land, the cost would be overwhelming. If the land were to be confiscated outright, then several thousand formerly gainfully employed coffee growers without assets or employment would be likely to take up arms against the usurpers of their land and livelihood. Second, even if one can imagine a radical constitutional amendment and confiscation of all coffee estate land, Paige's figures resolve only the problem of 75,000 out of the nearly 300,000 thousand landless and land-poor economically active Salvadoran adults. Thus even if the political and economic impossibility of such a reform is disregarded, one must conclude that there is not enough land to meet the needs of the landless and land-poor.

reform could be generated across a wide spectrum of opinion within the FMLN.

Diskin is correct in using World Bank data to demonstrate that most Salvadorans are poor and that poverty levels are even higher among the agricultural population. Far too many Salvadorans are poor, whether measured by the poverty line approach or the basic needs approach. That is the sad reality to which I referred in my article. But this reality will not go away by ignoring the empirical data that define the agrarian structure or by operating under the delusion that more land reform in one of the most densely populated countries on earth will end poverty. For that reason, my article compares relative levels of income in El Salvador in order to explore the alternatives to agricultural employment and their implications for generating income.

Diskin is not correct in claiming (based on a review of attitudinal data presented in the report to USAID but not mentioned in my *LARR* article), "The very high levels of rural poverty, land invasions, political militancy, and alienation all denote great potential for instability" (p. 122). He is wrong on two counts. First, he misrepresents the data that I presented in that report. Second, other data have shown that his basic assumption of high levels of alienation in El Salvador is fundamentally wrong. Diskin claims that the data I presented in that report (Seligson et al. 1993, 2-25) show "high degrees of political alienation. . . . These measures of dissatisfaction and alienation show that the countryside is anything but stable" (p. 122). Examination of the data presented in that report will verify that political alienation, as measured by the negative end of the continuum on a three-point scale, generally ranged between 10 and 20 percent.[3] Only among the "*tenedor*" population (peasants in the former conflict zones assumed to be supportive of the FMLN) was alienation expressed by more than half of the respondents, and then only on the items measuring support for the military (69 percent negative) and support for the institutions of government (57 percent negative). Thus Diskin's claims regarding the data in my report are not supported by the report itself.

The second error made with respect to the attitudinal data gets to the heart of Diskin's view of El Salvador. Although we both agree that the country and much of its agricultural population are poor, it does not necessarily follow that in the period after the civil war, the population is now highly alienated and ripe for future instability. Evidence to the contrary was found in a survey taken in February 1995, the results of which were presented in a monograph published jointly by the Fundación Dr.

3. The items were coded "1) almost always, 2) at times, and 3) almost never." Category 1 is considered the positive end of the continuum, category 2 the midrange, and category 3 the negative end.

Guillermo Manuel Ungo and the University of Pittsburgh (Seligson and Córdova 1995). The sample was national in scope, consisting of 1,609 Salvadorans across all fourteen departments (for details on the methodology, see Seligson and Córdova 1995). Table 1 shows the results of key items gauging political support (a measure of citizen perception of the legitimacy of the political system), based on an expanded political support scale utilized in the report critiqued by Diskin. The results show that renters and landless agricultural workers do not differ greatly from their counterparts in other occupations in terms of alienation. In a number of cases, they expressed lower levels of alienation than most other sectors. For purposes of comparison, a subset of Salvadorans defined by their vote for the FMLN presidential candidate in the 1994 election is included in table 1. As would be expected, FMLN supporters express consistently lower support for the system than any group defined by occupation and are particularly negative toward the armed forces. Renters and landless workers as groups are far less alienated than FMLN supporters. Thus it is not the occupational category of landlessness and renting that produces high levels of political alienation, as Diskin claims, but affiliation with the FMLN, the political party that fought in the civil war. As the recent elections show, however, FMLN supporters account for a minority of the population, even in most rural areas.

The results presented in table 1 demonstrate that compared with other Salvadorans, landless workers and renters are not an especially alienated sector of the Salvadoran population, as Diskin claims. These data may be misleading, however, and may mask an overall high level of political alienation in the country, in which the landless and renters are no more alienated than those in other occupations. One might also suspect that these items do not really tap into the deep levels of political alienation that Diskin cites. This is not the case, however, as can be demonstrated by examining another item from the 1995 survey. We asked a question that has been used worldwide by the University of Michigan's World Values Survey (Inglehart 1988). That item asks the respondent to choose one of three alternatives to describe their own feelings about their society: "The entire way our society is organized must be radically changed by revolutionary action"; or "Our society must be gradually improved by reforms"; or "Our present society must be valiantly defended against all subversive forces" (Inglehart 1988, 1214). When we asked this question in El Salvador, only 3.8 percent of the population selected the path of revolutionary action, while 78.5 percent supported gradual improvement via reforms. I analyzed the data in comparative perspective and found that El Salvador's results exhibit little of the alienation of which Diskin speaks (see Muller and Seligson 1994, 648). For example, while El Salvador's level of support for revolutionary change is substantially higher than that found in Costa Rica (0.5 percent), it is lower than in Nicaragua in 1991 (10.1

TABLE 1 *Support for the System in El Salvador, February 1995, according to Occupation*

	White-collar	Business Owner	Student	House-wife	Retired Owner	Farm-worker	Renter	Land-less	FMLN Voters
Institutions									
(Valid N)	(135)	(94)	(99)	(710)	(26)	(47)	(55)	(122)	(202)
Mean	5.0	5.3	5.2	5.1	5.5	5.7	5.1	5.1	4.5
Rights									
(Valid N)	(137)	(82)	(98)	(675)	(27)	(41)	(54)	(114)	(205)
Mean	3.2	3.5	3.7	3.7	3.9	3.8	3.8	4.1	2.6
Pride									
(Valid N)	(136)	(86)	(98)	(693)	(27)	(45)	(50)	(122)	(207)
Mean	3.9	4.2	4.0	4.1	4.6	4.4	4.2	4.4	3.0
Support									
(Valid N)	(132)	(84)	(97)	(673)	(26)	(45)	(51)	(113)	(207)
Mean	4.2	4.4	4.9	4.4	5.7	4.3	4.8	4.6	3.8
Armed forces									
(Valid N)	(134)	(96)	(92)	(744)	(26)	(46)	(57)	(121)	(209)
Mean	3.7	4.0	3.8	4.0	4.3	4.8	4.0	4.3	2.1
Legislature									
(Valid N)	(132)	(84)	(87)	(622)	(23)	(42)	(43)	(95)	(188)
Mean	3.8	3.9	4.1	3.9	4.0	4.5	4.3	4.3	3.4

Source: Seligson and Córdova (1995). Sample N = 1,609. Means based on a scale of 1 to 7, with 1 indicating low system support (high alienation) and 7 indicating high system support.

percent). Indeed, support for radical change in El Salvador in 1995 is far lower than that measured in France (9 percent) or that in Argentina (13 percent) or that in South Africa prior to the election of a black government (25 percent).

These data demonstrate clearly something that Diskin is unprepared to admit: the civil war is over, and very few Salvadorans want to do anything that would return the country to that violent period. Radical change is shunned by virtually the entire population. Only among those who voted for the FMLN in the last election does support for radical change increase substantially, and even among that group, 79 percent reject radical change.

Response to Paige

Jeffery Paige accepts the utility of the data analysis I provide in my article but seeks to reinterpret the data in a manner that he considers more consistent with Roy Prosterman's index of rural instability (IRI). While I am sympathetic to the intent, Paige errs in applying the IRI and thus reaches the erroneous conclusion that the index did not decline between 1971 and 1991.

Paige's fundamental mistake lies in conflating the categories of "rural" and "agricultural." He correctly quotes Prosterman as defining his index as the percentage of "all families in the society who work the land but do not own it." The focus here is on the relationship of the individual to the land, in this case, the agricultural occupation of the individual. But in Paige's next sentence, in which he operationalizes Prosterman's index, he slips away from that occupation-based definition and focuses instead on rural families rather than agricultural families: "The [Prosterman] index is calculated by dividing the number of rural families without access to land by the total number of families in the society as a whole" (p. 128). Although Prosterman called his measure an index of rural instability, he did so to locate the instability, not to claim that rurality has something directly to do with causing the instability.

Two kinds of errors result from Paige's conflation of *rural* with *agricultural*, one theoretical and the other practical. In terms of the error in theory, the title of Paige's classic work, *Agrarian Revolution: Export Agriculture and Social Movements* (1975), suggests the problem. Paige did not seek to explain rural revolution but agrarian revolution. He chose to focus on agriculture because his theory, like so many others, centers on transformations in agriculture (typically commodification and export agriculture) that give rise to revolutions in the countryside. Barrington Moore (1966) and Eric Wolf (1969) similarly centered their work on agrarian transformations. Wolf defined peasants as those who are "existentially involved in cultivation" (1969, xiv). Moore, Wolf, and Paige do not focus on those who dwell in rural areas except to the extent that those individuals are engaged in agriculture. Not all those who live in rural areas work in agriculture, and not all who work in agriculture live in rural areas. It is for that reason that my article focused on the population that is economically active in agriculture.

The second error produced by Paige's focusing on the rural population involves vagaries in the practical problem of defining the term *rural*. In El Salvador (and Central America generally), census bureaus make the distinction between *rural* and *urban* based largely on population criteria. For example, if a given municipality is inhabited by more than three thousand people, it is labeled as "urban" even though the area may be one in which most workers pursue agricultural occupations. But this definition is not always consistent. In many cases, a given municipality will be subdivided into its urban and rural components. If a county seat exists and is surrounded by a cluster of houses, that immediate area is labeled "urban" and the remainder of the municipality "rural." El Salvador contains 262 municipalities, 203 of which have fewer than 20,000 inhabitants. Any foreign visitor to these areas would clearly view them as rural, yet the census bureau often categorizes their town centers as urban. Within these town centers, some individuals engage in agricultural pursuits and some do not, while outside the town centers, many residents are farmers but a substantial number work as shopkeepers, teachers, truckers, and other occupations. In short, the term *rural* is very imprecise and misleading if one wishes to focus on agriculture and its connection to insurrection.

The impact of Paige's confusing *rural* with *agricultural* on his efforts to calculate Prosterman's index for El Salvador is dramatic. Although he claims that his table 1 is "estimated from Seligson's data," in fact he deviates from my original data in two ways. First, in calculating the landless for 1971, he refers not to the census data that I used, which yielded a figure of 38.1 percent (see my table 5) but to Liisa North's (1985) interpretation of the census data, which produced a figure of 29 percent. Second, and far more importantly, his table 1 uses the "percent rural" in the population rather than the percent in agricultural occupations (as I do in my article). Paige, citing my 1995 article (based on World Bank data), uses the figure of 61 percent of the Salvadoran population as rural in 1971, and 56 percent in 1991–1992. Given this slight decline in the rural population, it is easy to see that unless the landless and renters declined markedly, Prosterman's index would not decline by much, and given the fact that renters have increased, the index increases.[4] The flaw in Paige's calculation is that he uses the indirectly relevant rural population rather than the directly relevant agricultural population. When the agricultural population is substituted for the rural population (as I did in my article, as shown in Figure 1), a marked decline becomes apparent in the agricultural population, far steeper than the decline in the rural population. According to Paige, the rural decline from 61 percent to 56 percent represents an absolute decline of only 5 percent in twenty years, or a decline of 8 percent relative to the 1971 base year. The decline in the agricultural population was far greater, dropping from 47 percent in 1971 to 33 percent in 1991 (an absolute decline of 14 percent), nearly three times the decline in the rural population. Relative to the 1971 base, the agricultural population declined by 30 percent, a decline 3.75 times as great as that in the rural population.

4. Paige's table 1 lists renters as 27 percent of rural families in 1971 and 38 percent in 1991–1992. As noted in the text of my 1995 article, I base my calculations not on rural families but on the agricultural population. By that calculation, 33 percent of all farms in 1971 were rented, to which I added the colono population for a total of 38 percent (see Seligson 1995, 57). For 1991–1992, I show that renting had increased to 44.2 percent, plus an additional 4.9 percent of sharecroppers and 0.9 percent of colonos, for a total of 50 percent. Paige shows a 40.7 percent relative increase in renting from the 1971 base, while my figures show a relative increase of 31.6. Using my figures rather than Paige's would reveal an even greater decline in the Prosterman index.

Many have doubts about the validity of census data, but the "eye-ball test" would support my view, not Paige's. His figures would strike most Salvadorans as out of kilter with reality. No one who has observed the evolution of the country since the early 1970s would accept the image rendered by Paige's figure of a 5 percent decline in rurality. El Salvador is no longer a country dominated by agricultural pursuits. The World Bank reported that by 1970, only 28 per cent of the gross domestic product was coming from agriculture; by 1991, that proportion had declined to 10 percent (World Bank 1993, 242).

Paige raises one other issue that I wish to deal with briefly. He says that I claim that agrarian reform "has eliminated one cause of the Salvadoran revolution" (p. 128). In fact, I make no such claim. Rather, agrarian reform is one of three factors enumerated in my article as being responsible for reducing the magnitude of the agrarian problem (1995, 63–64). Land reform is the third one I mention, the others being urbanization and out-migration.

Who Rebels? Reconsideration of Theories of Peasant Uprisings

The two critiques of my article, especially Paige's comment, raise a fundamental question that needs to be addressed: Who rebels? A great deal of social science theory is based on the premise that peasant rebellion is a direct response to one or more conditions that affect peasants. Roy Prosterman and Jeffrey Reidinger (1987) focused on landlessness, Barrington Moore (1966) on exploitation, Eric Wolf (1969) on agrarian capitalism, and John Womack (1969) on proletarianization, among various studies. In Paige's (1975) earlier work, his cross-national investigation led him to conclude that particular combinations of income sources of cultivators and noncultivators yield insurrectionary outcomes, with situations where sharecropping and migratory labor predominate leading to agrarian revolution while commercial haciendas are particularly susceptible to agrarian revolts.

Paige's critique of my article suggests yet another possible source for rebellion: the predominance of a "semi-proletariat" or what he terms a "*pobretariado*" of impoverished peasants. Although I think that this newest thesis for explaining peasant revolt also misses the mark, Paige's suggestion raises an even more significant possibility: we may be looking in the wrong place for the explanation. Paige notes correctly that the areas in El Salvador with the strongest FMLN control did not have high degrees of landlessness. Citing Wickham-Crowley's (1992) study, Paige correctly concludes that the areas of El Salvador with the highest proportion of landless laborers relative to the agricultural population were almost entirely quiescent during the civil war, particularly the coffee-growing region around Sonsonate in the west. Yet the astonishing contradiction noted by Paige is that these areas were precisely the centers of the 1932 socialist uprising and the subsequent brutal military repression known as "*la matanza*." Paige tries to explain this away by arguing that the memory of the repression may have "insulated this region against further political mobilization" (p. 132). But countless illustrations could be cited of rebellious activity breeding further rebellion, as has been the tradition in southern Mexico. Furthermore, virtually all the guerrilla fighters in the rebel armies were young, many in their teens, whereas all those old enough to remember the 1932 massacre would have been in their sixties or older by the time the civil war broke out.

These contradictory findings in the Salvadoran case shine a bright light on our efforts at theorizing to account for the causes of peasant rebellion. When we find that an identical condition (such as the predominance of landless laborers in western El Salvador) is associated in one instance with a major uprising (in the 1932 rebellion) and in another with quiescence (in the 1980s), we must be led to conclude that the alleged causal factor is entirely spurious.[5]

We need to look beyond our current models of peasant rebellion and move away from the notion that a particular type of peasant is the most revolutionary. Skocpol (1981) has pushed scholars in this direction, the same one that Leslie Anderson and I have recently endorsed in an empirical test of the Paige thesis (Anderson and Seligson 1994). It is also the direction in which two recent brilliant books ought to encourage us: Mark Danner's *The Massacre at El Mozote* (1994) on El Salvador and David Stoll's *Between Two Armies in the Ixil Towns of Guatemala* (1993).

Danner has provided a detailed account of the massacre at El Mozote in December 1981, a remote area in the province of Morazán. Paige describes this region as having a sizable "pobretariado." Morazán was one of the strongest centers of guerrilla control for much of the war. I saw no other area as heavily damaged by the war, and the town of Perquín in this province was chosen by the FMLN for its war museum, replete with remains of downed helicopters and unexploded bombs. The standard sociological approach to explaining the guerrilla strength in this region would be to examine the conditions of the peasantry and to report (as does Paige) that a certain type of peasant predominated and then to conclude that the prevalence of this kind of peasant explains the guerrillas' strength.

Danner's *Massacre at El Mozote* is not a work of theory, and his main emphasis is to demonstrate what the U.S. government knew about the massacre and when. But this highly detailed account of the massacre also reveals that El Mozote was not a stronghold of guerrilla strength before the army attacked. Indeed, it appears that the village and the

5. I wish to thank Vadim Staklo for noting this contrast between 1932 and the 1980s.

https://doi.org/10.1017/S0023879100017982 Published online by Cambridge University Press

surrounding hamlets were opposed to the guerrillas. As Danner explains, "El Mozote had been uniquely unreceptive to such blandishments [of liberation theology], for the hamlet was known throughout the zone as a stronghold of the Protestant evangelical movement. People had begun to convert as early as the mid-sixties, and by 1980 it is likely that half or more of the people in El Mozote considered themselves born-again Christians. . . . So, unlike many other hamlets of Morazán, El Mozote was a place where the guerrillas had learned not to look for recruits" (Danner 1994, 19).

Danner describes the massacre, various official denials in San Salvador and Washington, efforts made by the U.S. Department of State and the *Wall Street Journal* to discredit reporting on the massacre by Raymond Bonner of *The New York Times*, and finally the work of Argentine forensic pathologists in October 1992, which unearthed unmistakable proof of a massacre of nearly a thousand people, many of them women and children. While all these details raise fundamental questions about human rights and foreign policy, Danner's account reveals a great deal as well from the perspective of theory of peasant rebellion. The most important lesson is that those who were killed at El Mozote were not those who rebelled. Therefore, whatever the socioeconomic and tenurial characteristics of villagers of El Mozote and the surrounding hamlets, it was not fertile ground for the emergence of a guerrilla army, and therefore those conditions cannot be held responsible for the ensuing violence. In sum, this area was not a guerrilla stronghold before the army moved in to do its killing. The massacre erupted from a government army frustrated by its own inability to capture guerrillas and wanting to teach the people of Morazán a lesson, led by a military commander who wanted to make his mark. The army and its local commander succeeded in this goal, but the lesson learned by the populace was that even the innocent could be murdered by the Salvadoran Army and it therefore made sense to seek protection from the guerrillas. My guess is that large numbers of previously neutral peasants joined the guerrillas as a direct result of the El Mozote massacre.

While conducting research for the article on land tenure in El Salvador, my team members and I had the opportunity to talk at length with one young man who had fought with the guerrillas for nearly the entire war and was being trained in a camp set up by the United Nations for demobilized guerrillas.[6] He explained that in 1980, when he was ten years old, he was traveling with his mother on a bus in the countryside. The vehicle was stopped by the army, he and his mother were taken off

6. Present at the interview were William Thiesenhusen and Malcolm Childress, both at the University of Wisconsin and coauthors of the USAID report cited by Diskin (see Seligson et al. 1993).

the bus, and she was shot dead and acid poured on her face. The young boy was then taken to an orphanage in San Salvador. In the orphanage, he was recruited by a rebel group, which brought him back to the region where he had been raised and trained him in military tactics. He spent the remainder of the war there, where he lost an arm while setting up a land mine. Countless other cases could be related of individuals who joined the guerrillas for similar reasons.

David Stoll's *Between Two Armies* refutes our conventional theories more explicitly. He focused on the Ixil triangle in Guatemala, an area of great conflict at the height of the guerrilla war of the 1970s. Stoll demonstrates that the areas in which the military engaged in the severest repression were not areas where the population was under extreme pressure from land tenure conflicts or economic decline. Stoll convincingly shows that, if anything, conditions were improving in the Ixil region when the violence exploded. But once the army, in searching for small guerrilla bands that were infiltrating the region, began torturing and killing villagers, the peasants sought protection from the guerrillas. Stoll explains the reaction of the villagers in this way:

> Judging from their stories, the main reason the Ixils cast their lot with the guerrillas were the coercive pressures created by the blows and counterblows of two military forces, a dilemma Nebajeños typically describe as being *entre dos fuegos* ("between two fires"). . . . Once an armed conflict is under way, the violence exercised by both sides can easily become the most important factor in recruitment. People may join the revolutionary movement less because they share its ideal than to save their lives, because of a set of coercive pressures emanating from both sides that I will refer to as "dual violence." (Stoll 1993, 20)

In both El Mozote and the Ixil triangle, social scientists would argue, the conditions in the zone—a particular constellation of land tenure, cropping patterns, proletarianization, and related factors—caused these areas to become centers of violence. Yet the accounts of Danner and Stoll suggest that these variables, while important in setting the backdrop for rebellion, may not be central. It may well be that massive repression launched by the state to root out what are initially small groups of guerrillas (who may be urban dwellers, disaffected university intellectuals, or students who themselves have been victims of repression) initiates a cycle of violence that eventually brings others into the fray. In my interviews in Morazán and elsewhere in El Salvador, I was told repeatedly by those who had joined the guerrillas that they did so after members of their family had been "disappeared" or killed by the army.

I am not saying that conditions in Morazán or Ixil country were good or that no objective economic reasons existed for discontent, nor am I saying that these were regions of abundant land, high levels of employment, and excellent schools, hospitals, and roads. No observer of these regions could make that claim. I am saying that these very conditions extended far beyond the regions in which the violence escalated but did not produce major armed conflicts everywhere. I therefore conclude that the particular conditions of the regions themselves are not responsible for the violence and that government repression of real or imagined guerrilla bands caused major escalations of the violence and forced otherwise neutral bystanders to chose between "two armies." Other observers have perceived repression as an important element in the Central American uprisings (Booth 1991, 60–61), but I am suggesting that it may be far more significant as a central causal factor than has previously been acknowledged. Terrible social conditions may provide the necessary condition for rebellion, but when extreme repression is present, we may have a "necessary and sufficient" combination.

Paige cites a detailed study of the rise of the guerrillas in El Salvador that he believes supports his "pobretariado" thesis (see Cabarrús 1983). The study shows that semi-proletarian peasants were more likely to join activist opposition peasant groups than were other types of peasants. Paige also mentions but does not comment on the significance of additional data presented in the same source showing that these same presumably proto-revolutionary semi-proletarian peasants were as a group more likely to be apolitical or to join an army-supported organization than they were to join opposition groups. Thus each one of Cabarrús's three groups of peasants (middle, semi-proletarian and wage-laborer) was almost equally likely to join the army-supported group. My conclusion from this data is that knowing the type of peasant one is studying tells a researcher virtually nothing about the likelihood of that peasant's joining one side or the other or staying neutral.

Conclusions

When I wrote my article on land tenure in El Salvador, I wondered whether—in light of the end of the cold war, the achievement of peace in El Salvador, and the end of the Contra war in Nicaragua—the social science community had lost interest in studying land tenure in Central America. The reactions of Diskin and Paige make it clear that even though these wars are over, social scientists still want to find out why they occurred in the first place. Moreover, the scores of military conflicts continuing to rage over the globe make it sadly clear that interest in the subject is far more than academic and historical. Perhaps this debate has demonstrated that we need to continue to examine issues of land tenure, proletarianization, and poverty but that we ought to be more careful when we seek to link cause and effect.

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https://doi.org/10.1017/S0023879100017982 Published online by Cambridge University Press