*European Sociological Review*, Vol. 3 No. 2, September 1987



# *Some new evidence on correlates of political violence: income inequality, regime repressiveness, and economic development*

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ABSTRACT Sociological research on political protest and violence either attempts to explain violence by absolute or relative deprivation or by rational action, resource mobilisation and political power (or by some combination of these factors). Although the connection between these theoretical approaches and cross-national studies rests on little more than considerations of plausibility, monotonic linkages between the level of economic development or income inequality and violence seem to fit with deprivation approaches, whereas a curvilinear relationship between regime repressiveness and violence fits easily with a rational action or political power explanations. Taking the disagreement between previous cross-national studies as a starting point, but analysing a new compilation of data, which covers up to 47 nations and the mid-1960s, the following conclusions are reached. First, inequality does not contribute to violence. Second, the level of economic development does not affect violence. Third, there is a rather weak curvilinear relationship between regime repressiveness and violence. Thus while deprivation explanations of violence receive no support at all in this study, there is at least *some* weak support for rational action or political power explanations.

## INTRODUCTION

Karl Marx suggested that capitalism cannot avoid imposing hardship upon the working class, which therefore has no choice but to rebel and to overthrow capitalism. Some may dispute whether Marx had absolute or relative deprivation in mind, but he certainly belongs to a sociological tradition which explains political violence by deprivation. Influenced by psychological work on frustration and aggression (Dollard *et al.*, 1939), more recent writers (Davies, 1962; Feierabend and Feierabend, 1966; Gurr, 1968, 1970; Huntington, 1968), focus on discontent, frustration, or *relative* deprivation in order to explain political protest and violence. From a macrosociological and comparative perspective, one would like to know whether there are objective and measurable background characteristics linked to relative deprivation and ultimately to violence. If so, we could arrive at general statements linking broad societal characteristics on the one hand and protest, violence, or even revolution on the other hand—as Marx sought to do in the nineteenth century.

Contemporary *macro*sociological research on political conflict and deprivation falls into two categories. It is possible to argue that imbalances in socio-economic development lead to relative deprivation which, in turn, leads to protest and violence (Deutsch, 1961; Feierabend and Feierabend, 1966; Gurr, 1968; Gurr and Duvall, 1973; Huntington, 1968). Excessive social mobilisation, creating wants which cannot be satisfied because of rather slow economic development, provides an example of such imbalances. Then the task is to construct indices of relative deprivation or social frustration from a variety of background variables and to relate these indices to protest or violence. In my view, these efforts have not proved to be cumulative (for more or less critical appraisals see Eckstein,

1980; Snyder, 1978; Weede, 1975, 1986a; Zimmermann, 1980, 1983). Moreover, there seems to be less interest in this enterprise in the 1980s than there was in the late 1960s or early 1970s.

Alternatively, one may argue that inequality is the most obvious background condition of relative deprivation and political violence. Disregarding studies which focus on some particular aspect of inequality such as land distribution (Russett, 1964; Nagel, 1974, 1976) or which rely on sectoral inequality data (Parvin, 1973), there are only five cross-national studies which investigate the relationship between (personal or household) income inequality and political violence (Sigelman and Simpson, 1977; Hardy, 1979; Weede, 1981; Powell, 1982; Muller, 1985, challenged by Weede, 1986b, and defended by Muller, 1986). In their pioneering study Sigelman and Simpson (1977: 124) conclude 'that the overall level of societal well-being is a more critical determinant of political violence than is income inequality, a measure of relative well-being'. While Sigelman and Simpson still admitted some marginal contribution of income inequality to political violence, Hardy (1979) and Weede (1981) even denied the existence of a relationship between inequality and violence once economic development is controlled. Analysing the relationships between *per capita* income, income inequality and deaths from political violence across democracies only, Powell (1982: 51) arrived at the very same conclusion. Despite some major disagreements about research design and the proper way of analysing the data, results from Sigelman and Simpson (1977), Hardy (1979), Weede (1981) and Powell (1982) were cumulative in casting ever-growing doubts upon the plausible proposition that income inequality strongly affects political violence.

Recently, the emerging consensus has been challenged. Muller (1985) claims that income inequality does indeed contribute to political violence. While the contradictory findings of Muller (1985) on the one hand and earlier researchers, on the other, necessitate a renewed effort at analysing the inequality-violence linkage, it is import also to note Muller's (1985: 58) strongest finding: that the death rate from political violence is a non-monotonic function of regime repressiveness, where deaths are maximised at intermediate levels of regime repressiveness. This latter result easily fits with a rational action, resource mobilisation or political process explanation of political violence (Coleman, 1978; Oberschall, 1973; Tilly, 1975, 1978; Tullock, 1974; Weede, 1985, 1986a): If there is a lot of consistently applied repression, then resource mobilisation by dissidents is more or less impossible, and violent as well as non-violent opposition is deterred. If there is almost no repression, then dissidents may organise themselves, but low cost and non-violent means of exerting influence are available and preferred. At intermediate levels of repressiveness, however, peaceful opposition seems ineffective, while violent opposition is not effectively deterred. Muller's (1985) findings on the inequality-violence linkage, however, seem to favour a discontent or relative deprivation approach (e.g. Davies, 1962; Feierabend and Feierabend, 1966; Gurr, 1968, 1970). Some may find Muller's even-handedness in his evaluation of contending theoretical approaches and his note of caution, concerning the rather weak correspondence between micro-explanations of why men rebel and macro-relationships between inequality and violence, reassuring. Others may deplore the fact that we make so little progress in choosing rationally between contending paradigms or research programs.

Of course, it is possible to argue that relative deprivation matters in producing political violence without claiming that income inequality matters. Relative deprivation may result from different characteristics of nations or even be a purely psychic state largely independent of objective or measurable macro-characteristics of nations. Or, the impact of the size distribution of income on violence may depend on the presence or absence of some widely accepted ideological justification of inequality. Moreover, from *micro*sociological research (Muller, 1979, 1980), we know that utilitarian and normative justifications of violence itself matter. Unfortunately, there are no measures for these variables at the cross-national level of analysis. It is also possible to reconcile an inequality-violence linkage with a resource mobilisation, political process, or

rational action approach.

A robust and strong linkage between inequality and political violence *in an additive model* implies that more inequality *always* contributes to more political violence. This is a relative deprivation view. From a rational action perspective, action by the underprivileged *depends* on their chances of success which are, to say the least, not improved by being underprivileged. From a resource mobilisation perspective, the linkage between being underprivileged and political violence should *depend* on the connectedness of underprivileged people, on their organisation for collective action—none of which is a necessary corollary of lacking privileges or suffering from exploitation. If the inequality-violence linkage is conditional on other variables, then *unconditional* effects of inequality on violence *in additive models* should be fairly weak or absent.

In this paper I shall not investigate those conditional effects of inequality on violence which are compatible with a rational action, resource mobilisation and political process perspective. I shall restrict myself, rather, to an investigation of the additive and general effects of inequality on violence which are to be expected according to a straightforward application of the relative deprivation approach. I limit my research agenda, because it is almost impossible to find cross-nationally valid measures for many variables which play a central role in rational action, resource mobilisation, or political process explanations of violence. Moreover, it makes methodological sense to investigate the simpler additive models first, before moving to a more complicated explanatory approach which includes some interaction effects (Allison, 1977).

While there are no final verdicts in empirical research, while there always remain anomalies (Kuhn, 1962; Lakatos, 1968/69), it is conceivable to strengthen the case against relative deprivation and to promote resource mobilisation, political process, and rational action approaches to political violence. This can be done by calling into question Muller's (1985) finding on the relationship between inequality and violence and by thus reaffirming the earlier consensus (Sigelman and Simpson, 1977; Hardy, 1979; Weede, 1981) that inequality makes little, if any, difference, *and* by preserving Muller's valuable and welcome[1] finding on the non-monotonic relationship between regime repressiveness and political violence. This is the agenda of the present paper.

## DIFFERENCES BETWEEN PREVIOUS STUDIES

There is little need to discuss the differences between the Sigelman and Simpson (1977), Hardy (1979), and Weede (1981) studies in detail. While Hardy (1979) accused Sigelman and Simpson (1977) of a defective research design, in particular of not consistently avoiding ratio variables, Weede (1981) could demonstrate that it does not really matter whether one applies Sigelman and Simpson's or Hardy's procedure. Moreover, substantial conclusions in all three studies are fairly similar to each other. Therefore, my attempt to uncover the reasons for Muller's deviant results will contrast his work on the one hand and the three pieces of previous work on the other hand.[2]

While Sigelman and Simpson, Hardy and, to a lesser degree, Weede relied on Paukert's (1973) inequality data, Muller thinks that we can and should do better. That is why he collected upper-quintile income shares *ca.* 1960 and *ca.* 1970 from a number of previous compilations. Previously, Weede (1986b) accepted the idea that Muller's (1985) data are somewhat better than those used by other researchers but still questioned his conclusions. The purpose of this paper is to move beyond Muller's data-base and to find out whether other inequality data-sets support Muller or not.

Muller's work, however, differs from *all* previous studies in other respects as well. His dependent variable is the natural logarithm of deaths from political violence per million inhabitants (plus one).[3] In choosing deaths from political violence as the only indicator of political violence, Muller (1985: 51) accepted Weede's (1981) previous warnings of reporting bias in armed attacks which might be transferred to a composite index. In insisting on the standardisation of deaths by population *before* the regression, he rejects Weede's (1981) and Sigelman and Simpson's (1977) or, for that

matter, Hibbs'[4] (1973) procedure of implicitly standardising violence *within the regression* by entering population as another independent variable. Muller's procedure implies that we regard a nation that is twice as populous as another one as equally violent as the smaller one only if we observe exactly twice as many deaths from political violence. Standardising violence within the regression avoids *a priori* commitments of this kind. Since it is difficult to argue persuasively in favour of either strategy, we should find out whether the difference in standardisation for population-size effects really matters. As previously noted (Weede, 1986b), it does not. Therefore, I can safely accept Muller's procedure.[5]

In so far as the time-spread of available inequality measures permitted, the previous work by Sigelman and Simpson, Hardy, and Weede was cross-sectional. By contrast, Muller argues that inequality today affects violence about five years later. That is why he specifies the death rate to be a time-lagged function of income inequality. While the difference between Muller's and previous practice is easy to grasp, I find it difficult to accept either the idea that time-lagging is a substantial improvement or that it should affect results. Neither Muller nor any previous researcher made a theoretical defense of lagging or not lagging. In general, income inequality is fairly stable over short periods of time, such as a mere five years.[6] If we had access to time-series data, we could rarely attribute observable short-run changes in income shares to true change rather than to measurement error.[7] Moreover, Muller is as incapable of sticking to five year lags as earlier researchers were of using truly simultaneous measurements. Since income inequality data refer to *different years* between either 1958–62 or 1968–72, Muller's lags differ from nation to nation.

Where inequality data refer to 1958 (or 1968), lags are longer than where they refer to 1962 (or 1972). In itself, the time spread of inequality measures makes it impossible to take a strong position on the lagging issue—whether in favor of lags or against lags.[8] That is why I feel that we—not only participants in the published debate but the social science community at large—are in trouble if lagging or not lagging matters. As earlier demonstrated (Weede, 1986b) we are indeed in trouble: without lagging, Muller's relationship between *ca.* 1970 income inequality and violence is reduced to insignificance.

In my view, the simple fact that inequality *ca.* 1970 seems related to violence in the mid-seventies, but *un*related to violence *ca.* 1970 should be sufficient reason for doubts and concern. Nevertheless, I shall accept Muller's five-year-lag and thereby bias my study in favor of reproducing his results. If I do not replicate them, then there are very strong reasons to doubt his positive link between inequality and violence.

In multiple regression analysis the relationship between inequality and political violence is affected by control variables. Again, Muller applies a different and somewhat longer list of control variables than previous researchers. In one respect, Muller's list is obviously superior to the earlier ones. He alone included regime repressiveness in his equations. This variable turned out to have a stronger impact on political violence than any other one. Therefore, one might reject earlier research and accept Muller's results as the best available.

In my view, this would be a premature response. Muller (1985: 58) also applies ethnolinguistic fractionalisation and government sanctions per million as control variables. Since the former control variable does not even come close to significance, we should omit it, as Muller himself recognised for the mid-1970s. It is plausible to stipulate that government sanctions affect political violence. But does it make sense to standardise sanctions—for example: press censorship, curfew, or martial law—by population? Does a government ruling twice as populous a nation as another one require twice as many such sanctions in order to be considered equally coercive? In my view, the imposition of press censorship or martial law (if it ever happened) would be as coercive in Canada as in the US rather than about ten times as coercive because of the different population size. Moreover, it is hard to understand not only why Muller standardises sanctions by population, but also why he keeps this insignificant predictor in

his equation. Admittedly, it is unlikely that the elimination of insignificant and poorly operationalised independent variables makes much difference. It is also easy to find out by focusing on the major and less dubious control variables only.

Since Muller's inclusion of the regime repressiveness variable looks more important than his inclusion of the dubious sanctions per million variable, one may still be inclined to accept his rather than earlier results. But there is another specification difference. None of the earlier researchers included a lagged violence term in his equations. Muller did. Since the lagged violence term is highly significant and contributes more to the explanation of later violence than inequality or most other independent variables do, this issue matters. Weede (1986b) demonstrated that the effects of inequality on later violence become insignificant once the lagged violence term has been eliminated. Is inclusion or exclusion of the lagged violence variable preferable? On the one hand, it is plausible to argue that violence begets violence. Therefore, the lagged term should be included. On the other hand, it is almost impossible to make theoretical sense out of the contradictory findings resulting from exclusion or inclusion of previous violence. The regression equation including a lagged violence term may be so rearranged that the left side of it contains the difference between current violence and (weighted) previous violence. Income inequality affects this difference between current and previous violence.[9] But it does not significantly increase current violence in an equation without previous violence. Why does inequality seem to affect the persistence of violence differently from new violence? In analysing a new and different data set, we might be lucky and get similar results whether we include or exclude a lagged violence term. (Or, we might be stuck with the problem.)

Weede's (1986b) criticism of Muller's (1985) paper claimed that Muller's positive link between inequality *ca.* 1970 and violence is more tenuous and less robust than Muller admits. First, it depends on relating inequality to later rather than simultaneous violence. Second, it depends on regressing *later* violence on inequality *and earlier violence* (and some other control variables like regime repressiveness). Third, it depends very much on Zimbabwe (formerly Rhodesia), which is the worst outlier on violence in Muller's sample. Muller's (1986) response to Weede very much focused on the outlier problem and argues that Panama is an even more influential case than Zimbabwe, and that one should not delete Zimbabwe only, but either Panama only or Panama and Zimbabwe or Panama, Zimbabwe, and Argentina in exploring the robustness of the inequality-violence linkage. I shall give special attention to these cases below.

## DATA AND ANALYSES

Muller's (1985: 50) major source of upper quintile income shares *ca.* 1970 is the World Bank (1979, 1982). Muller also took some data from Ahluwalia (1976) and Jain (1975) who have previously been affiliated with the World Bank and published their data and analyses under World Bank auspices. Presumably, the World Bank team compiling income inequality data for the World Development Reports knew about Ahluwalia's (1976) and Jain's (1975) in-house efforts and probably about other available inequality data, but decided not to use all of them any more. In my view, non-publication of inequality data within the World Development Reports does not reflect a preference for missing values but a hesitation to include data of dubious quality.

Certainly, Muller was more critical about inequality data than previous authors including myself (Weede, 1981). But the World Bank (1977–1985) seems to be even more cautious and critical. In looking through all these World Development Reports, I could find top 20 per cent income shares in the 1968–72 period for only 25 nations. By contrast, Muller found 51 estimates from a wider variety of sources. These 25 nations constitute my *small* sample. Such a small number of cases is unfortunate. Therefore, I decided to run different risks in different analyses. It is dangerous to accept too wide a time spread of inequality data which are analysed later, *as if* they referred to a single year (Menard, 1986). But it is also dangerous to

TABLE 1 *Regressions of ln (Deaths from Political Violence per Million +1) on Income Inequality, Economic Development and Regime Repressiveness (small sample)*[a]

| | | | Argentina and Panama deleted | |
|---|---|---|---|---|
| | (b) | (c) | (b) | (c) |
| Top 20% income share | −0·024<br>0·59<br>−0·12 | −0·014<br>0·74<br>−0·07 | −0·0051<br>0·91<br>−0·03 | 0·024<br>0·55<br>0·13 |
| ln energy consumption per capita 1970 | 0·26<br>0·34<br>0·26 | 0·54<br>0·06<br>0·55 | 0·19<br>0·48<br>0·24 | 0·61<br>0·03<br>0·75 |
| Death rate, 68–72 | — | 0·46<br>0·04<br>0·46 | — | 0·58<br>0·01<br>0·68 |
| Regime repressiveness 1973–77 | 2·81<br>0·00 | 2·41<br>0·01 | 1·82<br>0·07 | 1·00<br>0·23 |
| Regime repressiveness squared | −0·34<br>0·01 | −0·28<br>0·02 | −0·20<br>0·13 | −0·078<br>0·49 |
| Beta of b1 regime repressiveness +b2 regime repressiveness squared | 0·81 | 0·76 | 0·73 | 0·72 |
| Constant | −3·57 | −6·15 | −2·90 | −7·05 |
| N | 25 | 25 | 23 | 23 |
| Adjusted $R^2$ | 0·26 | 0·38 | 0·16 | 0·44 |

Notes: (a) In the first five panels, first cell entries are unstandardised regression coefficients, second cell entries are significance levels in two-tailed tests, and third cell entries are standardised regression coefficients. For computation and interpretation of the standardised regression coefficient in the sixth panel, which summarises the curvilinear effect of repressiveness, see Jagodzinski and Weede (1981). The significance of the curvilinear effect should be judged according to the performance of simple and squared terms in the first regression run (panels 4 and 5).
(b) Lagged violence excluded.
(c) Lagged violence included.

restrict one's sample size too much. In the small sample I tried to minimise the former danger, in the *large* sample I try to minimise the latter danger by accepting any inequality data in-between 1965 and 1977 published by the World Bank (1979–1985). Of course, where there is a choice the value closest to 1970 is taken. Still this yields top 20 per cent income shares for only 47 nations in the 'large' sample.

The basic idea of the research design is to run cross-sectional regressions of violence on income inequality, the level of economic development, regime repressiveness, and earlier violence in both data-sets. The dependent variable is deaths from political violence 1973–77, taken from Taylor and Jodice (1983, 2nd vol.: 48–51). Following Muller's example, deaths are standardised by population *before* the regression and logarithmically transformed. Operationally, violence is ln (deaths per million +1). Of course, the lagged endogenous variable derives from the same source and is defined the same way.[10]

Again following Muller's example, energy consumption *per capita* in 1970 is the indicator of the level of economic development. The data source is Taylor and Jodice (1983, Vol. 1 and the corresponding tape).[11] Regime repressiveness refers to the average score of a nation on civil liberties and political liberties indices ranging from one to seven in the 1973–1977 period. Although originally collected by Gastil and

Freedom House, the data are easily available in Taylor and Jodice (1983, Vol. 1, and the corresponding tape). According to Muller, the relationship between regime repressiveness and violence is curvilinear and non-monotonic. That is why we have to add a squared term of repressiveness, too. Since the two standardised coefficients in a polynomial regression (one for the simple and one for the squared repressiveness term) do not permit a meaningful interpretation, we should use the *un*standardised coefficients from a first regression run as weights for the simple and squared terms respectively, and enter the weighted sum of simple and squared repressiveness instead of both terms in a second regression run. While the sign of the single standardised regression coefficient in the second run is necessarily positive and not meaningful, its absolute magnitude may be compared with other standardised coefficients in the usual manner (see Jagodzinski and Weede, 1981, for technical details).

Table 1 refers to the small sample. It defines violence in Muller's way. It accepts his insistence on relating *ca.* 1970 inequality to violence about five years later. It includes the independent variables which had the strongest effects in Muller's analysis or in other previous research. While lagged violence is excluded in the first (and third) column, it is included in the second (and fourth) column, as Muller insists on doing. But to no avail. Income inequality is not significantly related to violence. Worse still, even the sign of the relationship contradicts Muller's expectations. In the small sample at least, more inequality seems (weakly and insignificantly) related to *less* violence. It is difficult to imagine results which more strongly contradict Muller's previous findings than the ones in Table 1.

Previously, I argued (Weede, 1986b) that there are three ways to call Muller's findings into question. One can relate inequality to simultaneous violence instead of later violence: this has not been done in Table 1, and could not have been done because there are no regime repressiveness data available for the 1968–72 period. Or, one can eliminate the lagged violence term: in Table 1 this does not make much difference. Or one can eliminate the case of Zimbabwe. This has been done in Table 1, for the World Bank did not publish the estimate which Muller took from Jain (1975)—i.e., from an earlier World Bank publication than later World Development Reports. The Bank seems to have had some second thoughts. So, there might be two good reasons to eliminate Zimbabwe from the calculations: its outlier status *and* the dubious validity of its income inequality data.

In other respects, Table 1 reaffirms Muller's previous findings. There is a curvilinear relationship between regime repressiveness and violence. While either the absence of repression or the ruthless pursuit of repression minimises violence, intermediate levels of repression look dangerous, and there might be a positive relationship between a higher level of economic development (and higher average incomes) on the one hand and more violence on the other hand. While the strength and significance of this relationship very much depends on the inclusion of a lagged violence variable, the positive sign of the relationship fits well with Muller's findings and contradicts strongly those of Sigelman and Simpson (1977), Hardy (1979), or Weede (1981).

Muller (1986) suggested that if Zimbabwe is deleted, we should better delete Panama and, may be, Argentina, too. In the third and fourth column of Table 1 both of these nations are deleted. Eliminating Panama only affects results to a lesser degree. The elimination of these two nations does *not* lead to the significant and positive relationship between income inequality and violence which Muller (1985) supported in his study. In particular if the elimination of Argentina and Panama is combined with the inclusion of a lagged violence term as in the fourth column of Table 1, then the *positive* relationship between the level of economic development and violence becomes very strong indeed, while the curvilinear relationship between regime repressiveness and violence loses significance. Certainly, some results in Table 1 constitute problems. The relationships between energy consumption *per capita* and regime repressiveness on the one hand and violence on the other are rather strongly affected by the inclusion or exclusion of two influential cases. But there is a single unambigu-

TABLE 2 *Regressions of ln (Deaths from Political Violence per Million +1) on Income Inequality, Economic Development and Regime Repressiveness (large sample)*[a]

| | | | Argentina and Panama deleted | |
|---|---|---|---|---|
| | (b) | (c) | (b) | (c) |
| Top 20% income share | F-level insufficient | 0·016<br>0·55<br>0·10 | 0·0088<br>0·74<br>0·06 | 0·034<br>0·21<br>0·23 |
| ln energy consumption per capita 1970 | 0·069<br>0·68<br>0·09 | 0·30<br>0·11<br>0·36 | F-level insufficient | 0·31<br>0·09<br>0·41 |
| Death rate, 68–72 | — | 0·46<br>0·00<br>0·45 | — | 0·49<br>0·00<br>0·54 |
| Regime repressiveness 1973–77 | 1·72<br>0·00 | 1·62<br>0·01 | 1·12<br>0·06 | 0·88<br>0·12 |
| Regime repressiveness squared | −0·22<br>0·01 | −0·20<br>0·01 | −0·14<br>0·09 | −0·095<br>0·23 |
| Beta of b1 regime repressiveness +b2 regime repressiveness squared | 0·51 | 0·50 | 0·38 | 0·42 |
| Constant | −1·75 | −4·58 | −0·95 | −4·71 |
| N | 47 | 46 | 45 | 44 |
| Adjusted $R^2$ | 0·16 | 0·29 | 0·11 | 0·30 |

Notes: (a) For explanation of table layout see note (a) to Table 1.
(b) Lagged violence excluded.
(c) Lagged violence included.

ous result in Table 1: top 20 per cent income shares and violence are *un*related to each other.

The difference between Tables 1 and 2 derives from sample size. Moreover, the lag between inequality and violence, although close to five years in many cases, varies quite a bit from nation to nation in Table 2 because inequality data refer to different years in the 1965–1977 period. The larger sample has been achieved at a price in other respects. Fortunately, results in Tables 1 and 2 are fairly similar to each other. Income inequality is still not significantly related to more violence. And this result is robust over the inclusion or exclusion of influential cases and over the inclusion or exclusion of a lagged violence term. Replacing Muller's data by those in which the World Bank puts some confidence unambiguously destroys the inequality-violence link, even if Muller's (1985, 1986) prescriptions on lagging the inequality-violence relationship, on inclusion of a lagged violence term, and on the influential cases of Argentina and Panama are accepted.

Unfortunately, those results in Table 2 which concern the linkages between the level of economic development or repressiveness and violence seem less clear. Exclusion of the two influential cases and inclusion of a lagged violence term weakens the curvilinear relationship between regime repressiveness and violence, while strengthening the relationship between energy consumption *per capita* and violence. But note that the larger sample in Table 2 does not produce so strong a positive link between the level of economic development and violence as the smaller sample did in Table 1 above.

In analysing the relationship between

TABLE 3 *Regressions of ln (Deaths from Political Violence per Million +1) on Regime Repressiveness and Economic Development (very large sample)*[a]

| | | | Argentina and Panama deleted | |
|---|---|---|---|---|
| | (b) | (c) | (b) | (c) |
| ln energy consumption per capita 1970 | −0·081<br>0·50<br>−0·07 | 0·094<br>0·40<br>0·08 | −0·095<br>0·43<br>−0·08 | 0·082<br>0·46<br>0·07 |
| Death rate, 68–72 | — | 0·40<br>0·00<br>0·47 | — | 0·40<br>0·00<br>0·48 |
| Regime repressiveness 1973–77 | 1·35<br>0·01 | 1·11<br>0·01 | 1·23<br>0·01 | 1·00<br>0·02 |
| Regime repressiveness squared | −0·17<br>0·01 | −0·14<br>0·01 | −0·15<br>0·01 | −0·12<br>0·02 |
| Beta of b1 regime repressiveness +b2 regime repressiveness squared | 0·26 | 0·22 | 0·24 | 0·20 |
| Constant | −0·14 | −1·55 | 0·079 | −1·34 |
| N | 126 | 126 | 124 | 124 |
| Adjusted $R^2$ | 0·06 | 0·26 | 0·06 | 0·26 |

Notes: (a) For explanation of table layout, see note (a) to Table 1.
(b) Lagged violence excluded.
(c) Lagged violence included.

inequality and violence, we have no choice but to restrict our analysis to a sample where information on inequality is available. Once it has been established that inequality does not affect violence, as has been done above, there is no reason to investigate the linkages between energy consumption *per capita* or regime repressiveness and violence in so small a sample. Using the same data sources as above, we may easily expand the larger sample by a factor of three. Such an expansion should reduce the weight of influential cases. If we are lucky, even inclusion or exclusion of a lagged violence variable no longer affects the very existence of a significant relationship between the level of economic development or repressiveness and violence.

The sample in Table 3 refers to all nations for which the World Handbook (Taylor and Jodice, 1983) provides data. Thus, some small, underdeveloped societies with authoritarian governments had to be neglected. But there are no major omissions in the data-set. Whereas the effects of lagged energy consumption *per capita* and regime repressiveness in Table 2 above very much depended on the inclusion or exclusion of influential cases, i.e., of Argentina and Panama, and on the inclusion or exclusion of a lagged violence term, Table 3 permits a simple and straightforward evaluation of the effects of the level of economic development and regime repressiveness. The level of economic development does not significantly affect (logged) deaths from political violence (per million). There is a weak to moderate curvilinear relationship between regime repressiveness and violence, i.e., intermediate repressiveness evokes more violence than either more or less repression. But smaller samples restricted by data availability on income inequality (Tables 1 and 2 above; Muller, 1985) seem to exaggerate the strength of the relationship somewhat. Fortunately, previously influential cases (Argentina and Panama) are no longer influential.

And the inclusion or exclusion of a lagged death rate affects only the percentage of variance explained, as was to be expected.

## CONCLUSION

For good reasons Muller (1985: 52) underlined that macro-hypotheses about the relationship between income inequality and violence do not *necessarily* correspond to any particular micro-theory about individual behavior. This caveat notwithstanding, it seems most straightforward to link macro-relationships between the level of economic development or inequality and violence with a deprivation perspective and to link a macro-relationship between regime repressiveness and violence with a rational action, resource mobilisation or political power approach.

Except for Muller's contribution, most previous researchers seemed to discover a negative relationship between the level of economic development and violence and a negligible, although occasionally significant relationship between inequality and violence. According to Muller, the relationship between the level of economic development and violence might be positive rather than negative and there is a moderately strong relationship between inequality and violence. As Weede (1986b) has outlined in his criticism of Muller's (1985) paper, Muller's results depend heavily on relating inequality or development in t to violence in t+5, on explaining later violence by earlier violence among other causes, and on influential cases like Zimbabwe. Muller's (1986) response largely focused on the influential cases issue, on whether Argentina and Panama or Zimbabwe or all of them should be deleted in order to reduce the weight of outliers. Whether they prefer Muller's or Weede's arguments and conclusions, most readers of the debate are likely to be rather unhappy with the weight of technical considerations.

That is why this paper has attempted a new approach by moving beyond Muller's inequality data set and analysing World Bank data on income inequality only. Muller's divergent results should be replicable with other data, too. Moreover, I hoped that the effects of inequality on violence would not depend as much on the inclusion or exclusion of a lagged violence term and on influential cases in the new data set as in Muller's. Fortunately, this hope has been largely fulfilled. So, we get rather unambiguous results from the new analyses.

First, income inequality does *not* increase violence in the data set compiled from World Development Reports. This finding rests on analyses which accepted Muller's five year lag between inequality and violence. It still holds if one accepts Muller's suggestion to include earlier violence as a determinant of later violence. It holds whether you include or exclude Argentina or Panama. If *all* the technical problems disputed between Muller (1986) and Weede (1986b) are resolved in Muller's favour, but another compilation of data is analysed, then we reproduce the close-to-zero relationship between inequality and violence reported by earlier researchers (Sigelman and Simpson, 1977; Hardy, 1979; Weede, 1981; Powell, 1982) and not Muller's (1985, 1986) moderately strong link between inequality and violence. As Weede (1986b) has demonstrated before, acceptance of Muller's data in itself is insufficient to produce his results. You have to accept all of his technical recommendations on lags, earlier violence and outliers, too.[12] Summarising earlier research and controversy, I cannot see convincing and easily replicable evidence in favor of a theoretically or statistically significant relationship between inequality and violence.

Second, the effects of the level of economic development, as operationalised by logged energy consumption per capita, and regime repressiveness on violence seem affected by influential cases like Argentina and Panama and by the inclusion or exclusion of earlier violence as a determinant of later violence, but only if analyses are based on a sample restricted by data availability on income inequality. But since income inequality does not robustly and significantly affect violence, there is no reason to accept a major limitation of the sample size where the purpose is to evaluate the effects of development and/or regime repressiveness on violence.

Third, in large global samples there is no significant relationship between the level of

economic development and violence. All contrary reports from much smaller samples (Sigelman and Simpson, 1977; Hardy, 1979; Weede, 1981; Muller, 1985) should be viewed with equal suspicion, whether they report a negative link or come close to suggesting that it might be positive.

Fourth, in large global samples there still is a significant curvilinear relationship between regime repressiveness and violence where intermediate repressiveness maximises violence and either extreme reduces it. While the relationship is not as strong as Muller's (1985) much smaller sample suggested, it at least survives.

Taken together the macro-relationships give little comfort to deprivation theorists. Neither the level of development which is, of course, closely related to average incomes and standards of living, nor income inequality seems to affect violence. Neither average nor relative deprivation seems to matter. But regime repressiveness influences the incidence of violence, as it should if people behave rationally, consider the likely consequences of their actions, and take the realities of power into account. While the relationship between regime repressiveness and violence is rather weak in a large sample, it is the only robust and significant cross-national result and it is compatible with a rational action, resource mobilisation or political power view of social conflict.

NOTES

1. 'Welcome', that is, from a rational action, resource mobilisation or political process approach to political violence.
2. Since Powell's (1982) analysis refers to democracies only, while all other studies concern all political regimes where data are available, I shall not investigate the reasons why Powell (1982) and Muller (1985) arrive at different conclusions on the inequality-violence relationship. By restricting his analysis to democracies Powell (1982) implicitly controlled for regime repressiveness, i.e., for the dominant explanatory variable in Muller's (1985) study. By definition, regime repressiveness in democracies is low.
3. Since some nations score zero deaths from political violence and since the log of zero is not defined, the increment has to be added before logging.
4. Sigelman and Simpson (1977) and Weede (1981) borrowed the procedure from Hibbs (1973).
5. I also did it the other way, following the example of Hibbs (1973), Sigelman and Simpson (1977), or Weede (1981) and obtained more or less the same results.
6. The correlation between Muller's inequality data *ca.* 1960 and *ca.* 1970 is 0·85. For a five year difference it should be still higher. But see Menard (1986) for a dissenting view on the stability of income inequality.
7. The reliability of difference scores approaches zero if the average reliability of cross-sectional measures is of the same order of magnitude as is stability over time. See Guilford (1954: 394) for the relevant formula.
8. Of course, the aggregation of deaths from political violence over five years also adds to the problem of lags differing from nation to nation. Most of the violence might occur in the first year of the aggregation period and thereby effectively shorten the lag. Or, most of the violence might occur in the last year of the period and thereby lengthen the lag.
9. The difference between current violence and previous violence is almost certainly much less reliable than raw violence scores are. The reliability of difference scores depends on the reliability of both sets of raw scores and on their stability over time (see Guilford 1954: 394). The correlation of violence 1968–72 with violence 1973–77 is 0·51 in Muller's sample. If the reliability of raw scores were 0·9, the reliability of difference scores would be only 0·8. If the reliability of raw scores were 0·8, the reliability of difference scores would be only 0·59.
10. Since earlier violence data are missing for Bangladesh, this case has to be deleted wherever there is a lagged violence term.
11. Since Taylor and Jodice (1983) do not report energy consumption *per capita* for Bangladesh in 1970, I took the 1973 estimate from Ballmer-Cao and Scheidegger (1979: 185).
12. In particular (if you want to reproduce Muller's findings), you must not eliminate only Zimbabwe from the analysis, as I did. But the World Bank did not reprint information about Zimbabwe's income distribution from an earlier World Bank source on which Muller relied. This fact in itself makes the Zimbabwe data suspect. Moreover, the meaning of Zimbabwe's income inequality (whatever it was *ca.* 1970 when the country still was named Rhodesia) is different from elsewhere. Except for South Africa there are few nations where race and income are as closely correlated as they probably were in Rhodesia. The racial stigmata of the poor in southern Africa affect mobility opportunities and thereby an obvious alternative to collective action.

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Manuscript received: February 1987.