

DEMOCRATIZATION AND EXTERNAL CONFLICT: (DIS)ENTANGLING RELATIONSHIPS

I. Introduction

The literature on the war-making effects of political regimes undergoing democratization (Mansfield and Snyder 1995a, 1995b, 1976) has added a new touch to the democratic peace thesis, in itself still much debated (cf. Elman 1997 and Mearsheimer 1994). The democratic peace deals with consolidated regimes, a goal that regimes undergoing the democratization process want to arrive at. Important as this distinction is, it does not imply that either branch of research is irrelevant to the other.

At present, it is too early to decide whether adherents of the democratic peace thesis are right. Critics raise serious arguments such as definitional issues, ad hoc adjustments in the operationalization, flaws in the causal logic, and more realistic and more parsimonious alternative explanations (Elman 1997, 1997a). Equally serious, perhaps even more serious is the set of theoretical, empirical and statistical arguments raised against the Mansfield and Snyder-thesis. Many more caveats are to be reckoned with before the issues of the democratization → external war thesis as well as the democratic peace thesis might be considered settled, if ever.

This paper has modest aims (and is written under great time pressure): There are at least six different literatures and streams of research that need to be linked when disentangling relationships between democratization and war. Researchers take some of the data and operationalizations used in the "other" literatures for granted and employ them, sometimes without great or substantial reflection, as instruments in their own research. These separate but overlapping literatures are: (1) the studies on *international war*, the dependent variable in most of the studies. Militarized disputes or other variants of external conflicts are also employed as dependent variables to scrutinize the wider dimensions of international conflict. (2) The *democratization* literature, or broader, research on *regime change in general*. Here many more dimensions are involved than tapped

in the most often used quantitative data source, the Polity data set of Gurr and his associates (Gurr 1990, Jaggars and Gurr 1995). (3) Literature on the *linkages between internal and external conflicts*. The diversionary thesis, i.e. using external conflicts to defuse internal conflicts, is the most prominent thesis here. Yet, many other theoretical and empirical linkages need to be considered (Gelpi 1997, Enterline and Gleditsch 1999). (4) The *trade-conflict* literature. Does international trade reduce (e.g., Oneal and Russett 1999) or increase (e.g., Barbieri 1996) foreign conflict and under what conditions, or is it "irrelevant" (cf., e.g., the overview in Barbieri and Schneider 1999)? (5) The *democratic peace* studies are not directly relevant to the Mansfield and Snyder thesis. The former deal with a fixed regime status, the latter with a changing one. Indirectly, however, there is much to be learned from the democratic peace research. Also democracies could experience regime change and then go to war. (6) Looming in the background to all these theories is the question whether the optimism of the (normatively- and structurally-grounded) democratic peace thesis (cf. Maoz and Russett 1993) can successfully be defended against *realist or neo-realist scepticism* that nothing in international relations is predictable but anarchy and self-interest of states, or rulers and their support groups to substitute actors (e.g., elites) for large compounds (states, regimes).

Our aims thus are threefold: First, we want to raise some critical arguments against the research of Mansfield and Snyder and other recent work in this domain. In particular, the operationalization of the independent variables and the data used deserve much more scrutiny. In my opinion, the danger of producing a large number of artificial results that have little to do with reality or substantive research is clearly increasing. Second, we shall try to point to some of the linkages in the six domains mentioned above. This occurs throughout the paper. Third, some of the consequences of the present arguments are summarized in a very rudimentary causal diagram and a set of typologies that might prove helpful in setting up more adequate designs that test multivariate relationships in a more appropriate way. Since external wars, and even milder forms of external military conflict, are relatively rare occurrences amongst an increasing number of states, the issues of overdetermination and multicollinearity will never be solvable, given the large number of war determinants (e.g., Bremer 1992). The permanent debate between adherents of the democratic peace and neo-realist critics speaks to this (see the chapters in Elman 1997 and the introduction and summary by Elman 1997a, Elman 1997b for an excellent summary). What can be learned in

particular from this debate (Elman 1997a) is selecting contrasting cases in a careful manner so that the likelihood of refuting hypotheses is maximized in view of the strength and weakness of rivaling explanations.

Case studies will never supplant wide cross-national quantitative analyses. Yet, given the present state of knowledge they definitely can point, first, to weaknesses in the causal chain of arguments in the more rigid quantitative studies, second, to much more leeway in politics both internally and externally than assumed by the quantitatively-oriented scholars, and thus, third, lead to potentially fruitful new hypotheses. The range of options on the various variables and their interlinkages can seriously be increased by such case studies or paired cases. At this point we shall concentrate on the critical points rather than on documenting or explicating the broad literature in greater detail.

II. The Mansfield and Snyder-study: Attacks and Vindications

Democratization typically creates a syndrome of weak central authority, unstable domestic coalitions, and high-energy mass politics. It brings new social groups and classes onto the political stage. Political leaders, finding no way to reconcile incompatible interests, resort to shortsighted bargains or reckless gambles in order to maintain their governing coalitions. Elites need to gain mass allies to defend their weakened positions. Both the newly ambitious elites and the embattled old ruling groups often use appeals to nationalism to stay astride their unmanageable political coalitions (Mansfield and Snyder 1995b:88).

Supplementing - *not* substituting - the notion of democratic peace, Mansfield and Snyder argue that political regimes en route to democracy are more likely involved in external war. Their dependent variable is war (Correlates of War-data with at least 1000 combatants involved per state and at least 100 deads per state, according to Small and Singer (1982), and some variant thereof).

Regime type is the independent variable and measured with the *Polity II*- data of Gurr (1990). In

further analyses, the impact of the preceding regime type on the subsequent one is included in the analysis, though nowhere coming close to the suggestions of Linz and Stepan (1996) in the regime transition literature.

The Gurr data in general are much less reliable than the Small-Singer data (both used for the period of 1811-1980). The unit of analysis of Mansfield and Snyder is not dyads but rather the "monadic" political characteristics of states at a particular point of time. Lags of one year, five and ten years in the dependent variable are employed thus excluding a confounding causal link from war to regime change. It is not surprising that results with a ten-years lag provide the strongest results, since simply more external conflicts can happen the longer the period is. Also other war-increasing factors may play a role during such a period, e.g., military armament programs and economic power shifts (cf., e.g., Bremer 1992). All this underlines the need for more detailed multivariate analyses addressed below.

The quality of the design and the data is fundamental for the quality of a scientific study. Here several strong caveats arise as to the independent variables, *regime type* and *regime changes*. Gurr (1990) distinguishes between three types of political regimes: democracy, autocracy and anocracy. *Democratic* regimes are positively characterized by openness in the recruitment of government, by competitive political participation and by the control of the executive. *Autocratic* regimes, conversely, are typically mostly closed political regimes with a lack of control of the rulers, whereas the neologism of *anocracy* is to stand for a mixed typed of both (or the absence of both?). Anocracy is not to be confused with political anarchy. Yet, it is totally unclear whether this type of political regime is a distinct one, or rather a hybrid, or defined as a residual of the other two regime types. Which types of hybrids stand for which form of anocracy and which degree of anocracy? Simply coining a new word with unclear meaning is not enough for comparative political regime analysis.

Russett (1993) and many other authors in this scientific community follow the operational outline of Gurr and then simply deduct the autocratic index score (i.e. the sum of autocratic political regime characteristics) from the democratic index score (the sum of democratic political regime

characteristics) to arrive at anocratic regimes (which are given the margins of -25 to +29, with pure democracies rating at +100 and pure autocracies rating at -100). There is no adequate and substantial assessment of the *one-dimensionality* of political regime characteristics thus subscribed to in the underlying continuum. Anybody familiar with comparative regime analysis would highly doubt such a degree of reductionism with the simple goal to arrive at some - highly questionable - scaling of political regime characteristics. Classic (Finer 1998) and modern (e.g., Linz and Stepan 1996) analyses of political regime changes imply *multidimensionality* of regime characteristics. This is a more reasonable assumption to start from than the nonreflected assumption of onedimensionality. As holds for earlier empirical work of Gurr on relative deprivation (Gurr 1968, cf. the criticism in Zimmermann 1983), too many divergent indicators are lumped together to reduce the data space with neither sufficiently substantial nor empirical arguments.

Apart from these theoretical issues when using the Polity data set (whether Polity I, II, or III), the data collection and coding of indicators and variables in the Gurr data sets needs much more scrutiny. Too many hidden assumptions are made about the composition of government (or even broader about the real power-holders in a political regime) and its functioning to take both, the variables selected and their raw figures, at face value. Area specialists would probably make many important differentiations amongst states, e.g., in Black Africa or Latin America, whereas the Gurr figures come up with the very same values. Consequently we strongly warn against both, the data quality and the way the data are treated both by Gurr and his associates as well as researchers in the democratic peace community in a broad sense. In short, researchers like Russett (1993) do not seem to adequately reflect on the *assumed one-dimensionality* of their political regime scale.

To give an illustration of the questions raised by Gurr's codings, *hereditary succession* is listed as a trait of anocracy. I fail to see any reason for this. Equally well it could be listed under "autocracy." Following these criteria of Gurr, it is only consequent that the Victorian Regime is coded as partially democratic and partially anocratic (Mansfield and Snyder 1995a:9). Even though anocracy is not equivalent to anarchy, one will have great difficulties to find traits for the absence of authority in the Victorian Empire.

Mansfield and Snyder perform several quantitative tests of their hypothesis that democratizing political regimes are more likely to be involved in external war. Some of their fundamental results are:

[..] on average, the percentage increase in the probability of war was smallest for countries making transitions from autocracy to anocracy and greatest for countries making the dramatic leap from autocracy to democracy. More dramatic transitions toward democracy therefore seem more likely to promote wars than do less profound changes of this sort (Mansfield and Snyder 1995a:17).

Also dramatic changes toward autocratization also seem to strongly increase the probability of regime aggression against non-state opponents. The authors employ a chi-square test to assess the zero hypothesis that political regime types do not differ in their frequency of external war involvement. Though the authors deal with a complete universe, they speak of "significance" tests as do their critics (e.g., Weede 1996a who shows that less than half of the cases are "significant"). In other tests more "robust" results show up (Mansfield and Snyder 1995a:15; 1996; in general cf. the many, often very technical, issues raised by Enterline 1998 and Thompson and Tucker 1997).

The authors speak of clear evidence for their thesis that the democratization of political regimes is accompanied by greater involvement of these regimes in war and other forms of warfare. Political regimes transforming into autocracies also demonstrate a higher proclivity to war, but this effect is even stronger in democratizing regimes. Yet, there is also evidence that authoritarian regimes preceded by a democratic regime display the greatest inclination towards war amongst all regimes, at least according to the summary regime indices (Mansfield and Snyder 1995a:18).

There are several further grave arguments against the design of Mansfield and Snyder. They compare, e.g., autocratic and democratic regimes, democratizing or autocratizing regimes with persistent regimes and also inquire into the different patterns of those regime changes. In all these instances, the assumption is that other important characteristics of those states (e.g., whether poor or not, whether multicultural or not, whether marred by *irredenta* or not, whether with a history of

war or not) as well as those of the international environment (e.g., war alliances, international economy) are constant. Even the substitution of a cross-sectional design with a diachronic one, i.e. studying countries *during* their various phases as autocratic, democratizing, democratic, or autocratizing regimes, will not solve these problems given the unlikely case that other major conditions will remain constant. Misspecification will remain even with a diachronical design. There might be a trade off between some factors (e.g., cultural ones) that could be better controlled in such instances and others that change as the form of political regimes changes.

Second, many cases in the universe are not independent of each other (e.g., the Krim wars or the two Worlds Wars in this century). This is a standard counter-argument against quantitative war studies. Further, adding yearly scores of political regimes is not a sufficient control for the independence of cases (Ray 1998:37; cf. also the control procedures of Oneal and Russett 1997). Important factors such as international alliances, contiguity, boundary problems and ideological distances need to be explicitly controlled in multivariate analyses. In this respect, the history of research on the democratic peace (cf., e.g., Oneal and Russett 1999) is instructive for the Mansfield and Snyder thesis, namely bringing in more and more control variables only to corroborate the basic tenets of the democratic peace. Mansfield and Snyder have begun to control for period effects and for regime history. Yet, for a sound theoretical evaluation of the democratization \rightarrow war thesis this is not enough.

Third, as indicated, the typology of regime forms is crude. Linz and Stepan (1996) summarize a wide consensus in the literature when they speak of five types of political regimes: *totalitarian*, *post-totalitarian*, *autocratic*, *sultanistic* and *democratic*. (The post-totalitarian type is a relatively modern one.) Each of these types is characterized by a *syndrome* of *specific* characteristics. The legacy of different political regimes becomes apparent in that certain options are likely to be closed at political regime change. According to Linz and Stepan, a pluralist party system and pluralist political institutionalization at the most can occur in autocratic regimes that have undergone political liberalization and - much less so and much more unlikely - under post-totalitarian regimes also en route to democratization. The classification of Linz and Stepan also makes the awkward type of anocracy unnecessary. The sultanistic regime (according to Weber), however, is not to replace

anocratic rule. (Russett 1993 presents such - questionable - cases of anocracy as the political regimes of Iran under Mossadegh, of Indonesia after the first election of Sukarno and of Chile under Allende).

Fourth, as implicitly done in the studies of Mansfield and Snyder, the democratization \rightarrow war hypothesis calls for a broader universe on both sides of the equation, on the side of the dependent variable(s) and the independent one(s) as well. If war is to be explained, democratization might provide an additional explanatory factor (besides the classic ones, cf. Bremer 1992). This in itself would be an enrichment of comparative war analyses and a warning against generalizing the benefits of democracy. The latter is a state of regime that according to the results of Mansfield and Snyder gravely differs from the paths towards it, democratization (see the classic arguments in Huntington 1968). Thus, when studying the democratization \rightarrow war nexus, researchers automatically enter the comparison and competition with other explanatory factors accounting for war.

The scope of the independent variable needs to be enlarged, too. Other forms of political regime change, more precisely, *all* forms of political regime change must be included for an adequate testing of the original Mansfield and Snyder thesis (cf. Thompson and Tucker 1997a, 1997b, Enterline 1998). Just as in studies of revolutions which are outcomes where you will have to study both, revolutionary and non-revolutionary situations and revolutionary and non-revolutionary outcomes. Otherwise, sampling on the dependent variable becomes an inherent bias due to an underspecified design.

Critics have either attacked the analyses of Mansfield and Snyder (Wolf 1996; Weede 1996a - issues between the universe of discourse and the statistical tests) or they have called the analysis into question by replication and extension (Enterline 1998; Thompson and Tucker 1997a, 1997b).

At least four consequences spring from this debate: first, use an appropriate design (regime changes); second, employ multi-method measures with variations in the dependent variable (war, militarized interstate disputes, covert external warfare) and also capturing degrees of regime change;

third, employ more adequate statistical testing (Thompson und Tucker 1997a, 1997b looking into more gradual regime changes as well), and, fourth, look for other important variables in an explicitly multivariate causal model.

We shall extend the criticism against the Mansfield and Snyder model of democratization → external war with respect to other important theoretical variables. These stem from other links between domestic and international conflicts (Gelpi 1997, Walt 1996), from the system collapse and regime transition literature and, of course, also from analyses of war in general. Sometimes these variables have been shown to be empirically relevant as well.

Once more, however, a strong warning must be expressed against using the Gurr-data Polity III set uncritically. Gurr uses several criteria to evaluate the authority characteristics of a polity. Yet, beyond Eckstein and Gurr (1975) one finds no further theoretical or empirical validation for this theoretical selection. Consequently, the numbers give to the state of democracy or autocracy remain unclear or at least highly debatable - most prominently of that totally unknown baby in comparative regime analysis "anocracy" which has passed away in the Polity III dataset. The literature is full with awkward coding examples. Any country specialist easily can come up with a nightmare of dubious classifications. The even bigger issue is, *what do these dimensions stand for, why are they chosen, what is their interrelation, theoretically and in reality?* Underlying assumptions as to the functioning of a state or regime, as to the relative importance of dimensions, as to their interrelationship (whether these are additive or substitutive components, whether there are threshold affects, whether they are necessary or sufficient characteristics) - all these are untested assumptions. Sometimes these issues are not even raised.

In short, my advice is: stay away from those data, until much more theoretical reflection, theoretical and empirical groundwork has been done. The meaning of those data is anything but clear. They lack theoretical and discriminant validity. Those scholars who attribute construct validity to these data, in my opinion, carry the burden of proof for a substantive theoretical validation of the numerous figures employed in extensive cross-national testing.

Subtracting autocracy scores from democracy scores makes things even worse not better. Summing a whole political system in one number (philosophers of science call this hypostatization!) and even

subtracting such a figure from another similar one reminds me of utter hegelianism. To me this is the weakest spot in much of the research on democratization and the democratic peace alike wherever those data are used. Unfortunately, no other data are available for the period from 1800 till now .

Anything that is built on such shaky fundamentals will not make for comfortable living of political scientists. In short, the intention of Gurr and his co-workers to fill an important research gap is to be lauded but not the results they have come up with so far. Even stronger warnings consequently are to be expressed against the usage of this data. As documented, researchers are aware of the shortcomings of these data, but how grave these shortcomings are apparently is not reflected.

III. Theoretical Extensions: Some Explorations

Comparative regime analysis and research on regime collapse and regime transition definitely need to be tied to the literature on international war. Mansfield and Snyder have made one such effort and partly relied on Huntington's (1968) conceptualization of political institutionalization (though not explicated by them) and game-theoretical notions (bargaining by and between old and new elites).

Yet, the regime analysis in comparative politics has many more suggestions to offer how to study regimes and regime changes. Linz and Stepan (1996) make five general points presented here in a drastically reduced form: first, the five regimes of totalitarianism, post-totalitarianism, autocracy, sultanism and democracy are differently constructed and have evolved in a distinct manner. Second, the regime differences make for different forms of regime collapse and transition. Third, the legacy of both, the ancient regime and the transient one differs amongst these political regime types. Fourth, the ancient regime allows for some options and precludes others. Fifth, as a summary of these four causal effects, each new regime type is markedly different from the others, notwithstanding some common features or only limited variations amongst some elements across the set of regimes. These are reasonable hypotheses filled with a lot of comparative case materials by Linz and Stepan. It does not mean, however, that more succinct causal testing and regime comparisons will shatter some of these propositions. Altogether, at present, however, they make

much more sense and are much more in line with reality than the abstract and insufficiently reflected dimensions and codings of Gurr and his associates.

Major *variables from the collapse-and-transition literature* are: counter-elites from within, mass demonstrations, violence, ethnic conflict, irredenta claims; double economic and political regime transition or single one; prior experience with market economy or not; international environment, law and property rights development (initial development level and/or international development). Applying the background notions of Linz and Stepan, it becomes apparent that only some of these variables are relevant (and only to some degree) in some of the political systems. They in turn have a strong impact on subsequent developments. A counter-elite from within is unlikely in all political systems except democracy and autocracy. This closes forms of peaceful regime change for the other political regime types. If mass demonstrations arise, usually the process becomes more complicated and the costs of transition costs become enlarged. A full set of substantial hypotheses can be derived with the basic theoretical framework of Linz and Stepan and be enriched through other substantial theoretical work in the field (cf. the discussion in Zimmermann 2001).

In the theoretical model of Mansfield and Snyder, the syndrome of a weak central authority, unstable domestic political coalitions and strong mass mobilization is reduced to three explanatory factors, leaving out minor ad hoc arguments here. *First*, the *elites* of the ancient regime often have *economic interests* which are *non-compatible* with those of ascending new elites or those of the masses calling for political participation. At least these interests do not *appear* to be compatible. The British agrarian upper classes were an exception in consenting to the reduction of corn tariffs which had protected their position against foreign competition. Since they were already owners of capital they had other options to enhance their economic position. The Prussian junkers by contrast were not in command of such a resource. At the end of the 1870s, they perceived the only solution for bolstering their economic and political position in a protective tariff coalition between the great agrarians and heavy industry. Domestically this implied a strong antiliberal component and internationally "hostile" competition with Britain in the West and Russia in the East.

Second, these incompatible and therefore non-bargainable interests (zero-sum conflict structure) meet *non-existent* or only *rudimentary political institutions*. Such institutions do not allow the integration by means of a differentiated, flexible and adaptive party system, neither the integration of the political masses and their demands for political participation nor that of the new political elites. Mansfield and Snyder draw implicitly on the approach of Huntington (1968). He postulated that, with increasing social mobilization, the ratio of increased demands for political participation and available political institutionalization (mainly in terms of the party system) decides over the political stability of a polity (cf. Zimmermann 1983:108-18 for a more detailed critique).

Third, under such circumstances, in particular in times of economic crisis, old and new political elites are said to be inclined to *feed the masses with maximal political demands*. *Internal conflicts* arising therefrom and from lacking options for political coalitions are then *projected externally*. The basic explanation here is the topos of tension reduction in the membership group by externalizing conflicts onto the outward group, both to clear boundaries and to enhance the own social identity. This *diversion hypothesis* (cf. Levy 1998:151-7) provides a classic theoretical explanation in the social sciences since Simmel and Sumner. Also under such conditions, the demands of political elites remain uncontrolled by responsible masses, as the positive counter-example of the British two-party system has demonstrated (Mansfield and Snyder 1995a:30). Unrealistic demands and programs are created in domestic politics. The necessary internal political compromise, internal coalition-building does not occur and can not occur. Rather the tension is externalized to bolster the own authority vis-à-vis domestic rivals and the masses. Mansfield and Snyder provide several illustrative examples, mostly from the 19th century and the first half of the 20th century. Dramatic cases are France under Napoleon III, the United Kingdom under Palmerston during the Krim conflicts, the fleet policies of the Wilhelminian Reich and its imperial demands in general, and Japan with the autonomy of political leaders at the begin of the 1930s.

The three decisive causal factors of Mansfield and Snyder can be graphically explicated as follows:

Figure 1 about here

The short time horizon under which the main actors perceive their interests increases the importance of political ideologies. Both internally and externally, a lack in the capability of compromising becomes evident. Established democracies employ the domestically proven virtues of moderation and compromise also in their foreign policies, at least towards similar democratic polities. In the case of democratizing countries this crucial mechanism may be reversed. In short, *democracy* presupposes *trust* and creates it again and again, *democratization*, however, initially creates *distrust*, depending, of course, also on the international political environment.

The three intervening variables worked out by Mansfield and Snyder substantially can be enriched by several other important factors. First, in multicultural empires that are breaking into parts, there is a tendency of new "democratic" elites now in the position of an ethnic majority to forget about their former minority position and to create new cultural minorities through repression. Each former Eastern European state, with the exception of culturally relatively homogeneous Poland (and perhaps Hungary) provides vivid examples. The split of the CSSR into a Czech and a Slovak Republic, being the work of political elites operating independently of the will of the masses, probably has prevented this conflict to go off. These ethno-culturally induced tensions almost automatically increase conflicts in foreign policy (*ethnonationalism*) in particular where *irredenta* are involved. This also holds when the own cultural group has been residing only relatively recently in contested territories, as is the case with the Russian minorities in the Baltic Republics or in the new central Asian states. The permanent forced and voluntary migrations on the Balkans show these mechanisms even more dramatically.

Second, drawing on Deutsch (1966, and Huntington 1968 as well), political democratization also furthers social mobilization. The same holds for economic development. Is the underlying population *culturally heterogeneous* before the onset of mobilization, one has to reckon with more, and not less, conflicts at a higher level of social mobilization and consequently with less rather than more societal integration. The development of ethnic tensions and of ethnic terrorism underlines this causal mechanism, from Northern Ireland via the Basque territories to Québec, Palestine and

South Africa (cf. Hibbs 1973). In Huntington's terminology, both factors increase the burdens on and demands from the political system without increasing the system capacities.

The *internal-external conflict nexus* needs to be further specified (cf., e.g., Gelpi 1997, Enterline and Gleditsch 1999). Here, e.g., the following arguments seem to be central: who carries which costs in which scenario? Is the principal-agent approach (Gelpi 1997) a fruitful general theoretical device here to understand possible internal-external linkages? Enterline and Gleditsch find that there are tradeoffs between using repression both internally and externally, which depends on both, the regime type and the foreign environment. Thus, there are several *additional response patterns of rulers to internal conflict* than diverting it onto external conflict.

Having suggested several important theoretical extensions, what is the relevancy of the *democratic peace thesis*? Here democracies are the units of analysis, namely stabilized modern political regimes with open channels of mass participation. Since such a goal is not (yet) reached in democratizing regimes, the democratic peace research is only in parts relevant. It is mainly the foreign trade nexus and the alleged pacifying role of international institutions that deserves attention. On the other hand, employing *differential diagnosis* here might provide further theoretical payoff when comparing democratic regimes to democratizing ones.

Concerning *international trade*, the evidence is mixed (cf. the summary in Barbieri and Schneider 1999), again partly due to variations in time, space, operationalizations and theoretical variables. On the other hand, the more conflictual correlates of international trade before World War I (Barbieri 1996) might be specific to the period. Likewise the more beneficial effects of international trade after the Second World War in part can also be attributed to other factors (e.g., bipolarity, nuclear deterrence, gains from alliance membership) and thus suffer from overdetermination, or at least multicausality.

Reversing the causality from war to trade or discussing "counter-intuitive" issues like trading with the enemy (Barbieri and Levy 1999) provides additional insights, but is beyond the dependent variable under study here: external war as linked to internal and international factors.

Do *international institutions* in democratizing or changing political regimes act or have influence as to moderating external war? Clearly this issue is understudied. Challenging the democratic peace thesis from a realist (or neorealist) perspective, Mearsheimer (1994) marshalls an arsenal of critical attacks and raises serious questions, i.e., as to flaws in causal order, as to non-addressed issues and as to more parsimonious explanations. *A fortiori* his caveats should apply to the allegedly moderating impact of international institutions in the case of (democratizing) regime changes.

In democratizing regimes the linkages making for beneficial effects of trade and international institutions are often absent. All this implies that weak democracies or instable democratizing states could be marred by their instability and by temptations of external war.

Trade and international institutions point to (in the broadest sense) *environmental or international factors*. This also holds for some of the explanatory factors for war, e.g. contiguity, alliance, type of neighborhood polity, military and economic preponderance (cf., e.g., Bremer 1992). More important, however, seem to be the dyadic analyses and findings of the democratic peace camp (cf. Oneal and Russett 1999 for a recent example). Also many other correlates and consequences of democratic international regimes have been brought up in the literature, going beyond the mere dyadic relationship. Yet, it remains true that the democratic dyad may be a sufficient condition for external peace, but there are others causes well (cf. the discussion in Fenman 1997).

As with the democratic peace argument, also the Mansfield and Snyder argument needs to be specified at *three different levels*: the monadic one (as pursued by Mansfield and Snyder) and at the dyadic level stressing in particular the aggressive consequences of autocratic neighbours vis-à-vis a democratizing regime. The latter example is already part of a systemic analysis.

As to the latter, Gleditsch and Hegre (1997) point to a curvilinear relations between a rising number of democracies in the respective international environment and external conflicts.

IV. Some Consequences

What follows from our brief debate? First, there is need for a multivariate analysis. Figure 2 presents a rough explanation sketch that links major bloc variables between regime change and war.

Figure 2 about here

Within each variable category, many causes and effects might be distinguished with a more detailed focus. Also the box linking internal and external politics is written across to indicate the many linkages that exist beyond the externalization mechanism (cf. Gelpi 1997). Also no arrows have been attached from here to two of the subsequent variables. The other causal links and respective paths have been mentioned *passim* in the paper. More specific research might start within some of the boxes, but should be aware of the basic causal patterns postulated here.

Second, more and better data in particular for the dimension of regime functioning and regime change are needed. (There may be equivalences, and functional non-alternatives as to regime functioning.) Elman (1997) is full of interesting linkages between domestic and foreign politics in the various combinations of authoritarian and democratic countries (cf. also Gelpi 1997).

The following table summarizes some of their suggestions:

Table 1 about here

Following the contrasting case analyses in Elman (1997), formal regime type (democracy and autocracy) is crossed with *de facto* foreign policy relations (accommodative or conflictive).

A few perhaps helpful suggestions immediately spring from such an exercise. There may be mixtures in foreign policies, cooperation in some areas, e.g., trade, and conflict in others, e.g. as to territorial claims.

One could break down this table further in specifying the neighborhood quality of other regimes (alike or different). Another dimension is to distinguish within such a table between initiator and recipient of conflict, just as much one will have to distinguish between onset and escalation or

endurance of conflict. It is unlikely that the full set of the explanatory variables holds in each instance.

Such typologies along the lines suggested here (which follow distinctions in the literature) may be helpful in locating adequate pairs or groups of countries for more in-depth studies as to the causal variables linking (democratizing) regime change to external war. Thus, Table 1 could carry just as much the heading: *democratizing* instead of democracies.

Such a typological procedure strongly suggested by the contributions to Elman (1997) and taken up here is not to substitute for more explicit and more rigorous multivariate analyses. It is to complement them and to bring out the special functions of such a strategy of carefully selected paired comparisons (in-depth scrutiny of causal mechanism, enlargement of the variation space of concepts, extreme and contradictory cases).

The relevancy of the Linz-Stepan set of hypotheses explicated here could also be challenged by such a strategy of looking for unlikely, counter-cases where a quiet dog did bark. In particular in broadening the regime change aspect beyond the mere democratizing variant Mansfield and Snyder focus on, such a strategy might prove fruitful. In any case, the regime collapse and transition literature together with the comparative regime literature needs to be definitely incorporated in the Mansfield and Snyder type of analysis.

V. Conclusion

Our main points are in brief:

First, future research will have to broaden the design for both, dependent and even more so, independent variables in studying the democratization → war nexus. Second, multitrait-multimethod strategies in tracing variants of regime change are indicated. Employing different measures and spending more time on reflecting on regime dimensions, their interrelationship and the quality of measurement is one of the tasks ahead. Third, much better data than those of Gurr and his associates are desperately needed. Fourth, there is the call for multivariate causal models incorporating bloc variables like transition modes and regime legacies, response options to internal

political change, additional internal-external linkages, international trade and international organizations, and the full arsenal of well-established explanatory factors for war. Fifth, given the present state of knowledge, a case-oriented typological approach with theoretically carefully selected contrasting cases might prove helpful, just as it does when dealing with criticism of the democratic peace thesis (Elman 1997).

In sum, research on the democratization → war nexus has described dangerous scenarios, raised disturbing questions and provided much scepticism against the enthusiasm of the democratic peace community. Yet, together with the research on the democratic peace, and even more so, it shares the fundamental weaknesses of using unreliable and most likely invalid data. In the Polity dataset the alleged fundamental regime structures are not sufficiently corroborated, the respective components and in particular their causal interrelationship remain unvalidated. Building on such a shallow theoretical baseline, the aggregation procedures of individual political regime characteristics both within and across regimes become even more questionable. We have suggested some other routes to follow in classifying political regimes and their impact while undergoing regime change.

Not only the future of the democratization and external conflict nexus is open and shaky, but also the status of much of existent research, in my opinion, calls for the same verdict.

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