## 1AC

### Advantage

#### CONTENTION ONE – CUBA

#### ECONOMIC SANCTIONS HAVE A STATISTICALLY SIGNIFICANT, NEGATIVE EFFECT ON DEMOCRATIC FREEDOMS

Drury & Peksen 10

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Amid Manuel Noriega’s rise to power in the 1980s, the Central American leader made friendly overtures toward the Soviet Union and some of its allies. The Reagan administration consequently moved on a pending set of comprehensive economic sanctions against Panama because of Cold War security concerns and drug trafficking to the U.S. Within months of the sanctions, Noriega began solidifying his hold on power. He created paramilitary forces to intimidate and suppress opposition groups, many of which were initially emboldened by the ideas that the U.S. would support their effort and that the sanctions would weaken Noriega. Through this progressively more authoritarian rule, Noriega held on to power until the American-led invasion removed him in late-1989. While the sanctions caused significant damage to the Panamanian economy, they also created an exter-nality: the erosion of democratic freedoms and governance in the target. The possibility of a negative association between sanctions and democracy as in the Panama case coupled with the increasingly common use of sanctions warrants that we assess more closely what impact these foreign policies have on democratic freedoms within the targeted country.1 In this paper, we offer a closer examination and show that economic sanctions have a significant, negative influence on democratic freedoms .2

We structure our argument around the political repercussions of both the economic damage caused by sanctions and the signals sent by the sanc-tioning state. More specifically, we assert that as leaders attempt to stay in power they will use the economic hardship caused by sanctions as a strategic tool to enhance their political support and as a consequence, we argue that the leaders curtail domestic groups calling for political pluralism and participation. Economic coercion, as an external threat to the targeted leadership’s political survival, also creates new incentives for the regime to restrict democratic freedoms of citizens so as to undermine any challenge to its authority. The empirical findings—based on analysis of time-series cross-national data over a 28-year (1972–2000) period—support the assertion that the presence of sanctions reduces political liberties in target countries. Furthermore, the analysis demonstrates that the longer economic sanctions are in place, the greater cumulative negative effect they inflict on democracy. The findings also show that comprehensive economic sanctions —those that entirely cut the economic ties between the sender and target—have a greater corrosive impact than selective sanctions that partially restrict the economic transactions.

ECONOMIC SANCTIONS AND DEMOCRACY

A significant portion of systematic research on economic statecraft focuses on the issues of sanction effectiveness and the conditions under which they will likely achieve their intended policy objectives (Drury 1998; Galtung 1967; Hufbauer et al. 1990; Kirshner 1997; Martin 1992; Pape 1997). Scholars

argue that sanctions fail between 65% and 95% of the time they are imposed (Hufbauer et al. 1990; Pape 1997). Another strand of the literature on sanctions shows that sanctions are not only ineffective but also coun-terproductive; scholars argue that economic coercion generates negative externalities. The scholarship on the consequences of economic sanctions especially stresses the unintended humanitarian consequences of sanctions, suggesting that economic coercion inadvertently worsens public health, economic conditions, the development of civil society, and education in sanctioned countries due to the disproportionate economic impact on cit¬izens rather than the leadership (Drury and Li 2006; Cortright, Millar, and Lopez 2001; Galtung 1967; Lopez and Cortright 1997; Weiss 1999; Weiss et al. 1997).

2Throughout the paper, we refer to democracy as the extent of respect for civil liberties (e.g., freedoms of expression and belief, associational rights, and personal autonomy) and political rights (e.g., political pluralism and participation, free and fair electoral process, and freedom of elected officials on policy decisions) in a political regime.

Others suggest that economic sanctions also lead to political instabil¬ity in sanctioned countries by destabilizing the target leadership (Marinov 2005) and inciting more violence in the forms of protests and riots against the established regimes (Allen 2008). Furthermore, other studies demon¬strate that economic coercion worsens human rights conditions (i.e., the level of government respect for physical integrity rights) in those countries facing economic coercion as a result of failing to harm the political and mili¬tary power of targeted elites, while socioeconomically hurting civilians’ lives (Drury and Li 2006; Lopez and Cortright 1997; Peksen 2009; Wood 2008).

Research on how international factors influence democratization also overlooks the connection between sanctions and democratic liberties. In this literature, among others, scholars address the effect of various factors including financial and institutional assistance (Carothers 1999; Carothers and Ottoway 2005; Knack 2004); economic globalization—that is, inter¬national economic and financial crises, trade openness, foreign direct investment, and financial market development (Gasiorowski 1995; Haggard and Kaufman 1992; Rudra 2005); military intervention (Meernik 1996; Peceny 1999; Pickering and Kisangani 2006); membership in international organiza-tions (Pevehouse 2002); and geographic diffusion of democracies across the world (Gleditsch and Ward 2006; Huntington 1991). Yet, scholars overlook whether economic coercion, a widely used policy tool, significantly affects democracy.

The paucity of research on the consequences sanctions have on demo-cratic freedoms leaves a gap in our understanding of economic coercion. Turning next to our theoretical framework, we argue that sanctions cre¬ate new incentives and opportunities that cause leaders to reduce political freedoms, making the sanctions harmful to democracy.

SURVIVING ECONOMIC SANCTIONS

States initially apply economic coercion against other countries anticipating that sanctions will cause economic hardship, which in turn, will harm both the legitimacy and capacity of the political leadership. As a consequence, economic sanctions should force the regime to concede to the sender’s demands (Galtung 1967; Kirshner 1997). This line of reasoning asserts that economic coercion will also signal opposition groups that another state or states (normally large, powerful ones) seek policy change from the target and may support a more active opposition to the current leadership. Further, foreign economic pressure disrupts the regime’s support, especially from key political and social groups, by reducing resources available to the leadership to pay groups for their loyalty. Economic coercion is also expected to harm the regime’s coercive capability by denying them essential military and other scarce resources. Limited wealth within the state should reduce what assets the leadership has at its disposal to maintain power; for example, the police and military will have fewer resources to repress the citizenry. Consequently, once targeted regimes feel the destabilizing impact of economic pressure, they should give in to foreign demands for political reform.

This view of sanctions is problematic, however. A growing body of literature shows that economic coercion hardly harms the coercive capac¬ity of the targeted regimes. On the contrary, sanctions generally harm the socioeconomic and political status of average civilians, while political elites remain insulated from the coercion (Andreas 2005; Gibbons 1999; Peksen 2009; Weiss 1999; Weiss et al. 1997). Deriving insight from this research, we argue that sanctions consolidate the regime’s power and create incentives within the target that lead the regime to restrict the democratic freedoms of citizens in order to preserve its hold on power. First, the target leadership can use the economic disruption (caused by sanctions) as a strategic tool to manipulate access to and redistribute resources made scarce by sanctions to enhance its authority and subsequently to weaken opposition groups.3 Second, sanctions generate new incentives for the state to restrict demo¬cratic freedoms by (1) creating audience costs if the leadership concedes to the sender’s demands and (2) providing encouraging signals to domestic opposition groups to be more active. Below, we discuss how each aspect of this argument thwarts political freedoms.4

#### WE USE A LAGGED TIME SERIES MODEL OF 102 COUNTRIES OVER 28 YEARS CONTROLLING FOR TYPE OF SANCTION, GDP, ECONOMIC GROWTH, FDI, POPULATION, CIVIL WAR, OIL, AND ARAB COUNTRIES

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ASSESSING THE IMPACT OF SANCTIONS ON DEMOCRACY

To empirically examine the hypothesized, negative impact of sanctions on democracy, we utilize time-series cross-section data delineated by years and countries, respectively. Before introducing the operationalization of variables, the question of appropriate sample domain requires attention. In order to avoid any bias resulting from case selection, we exclude all liberal/consolidated democracies from the analysis since these regimes are rarely sanctioned and score at the upper limit of democracy scores (Cox and Drury 2006; Lektzian and Souva 2003).13 Their inclusion would bias our results toward finding support for our hypotheses. Specifically, the absence of sanctions and extremely high democracy scores creates a strong contrast to any country that has been sanctioned. Therefore, based on the findings of the earlier research, we limit our data to less democratic and nondemo¬cratic countries. 14 Overall, the sample size includes 102 countries from 1972 to 2000, inclusive.

Dependent Variables

Since the focus of this paper is on the extent to which sanctions improve political rights and civil liberties, it is important that we deploy a continu¬ous measure of democracy that specifically accounts for the extent of such democratic freedoms. The Freedom House Index (2004) offers the best con-ceptual fit for our analysis by offering a maximalist definition of democracy that includes various civil liberties and political freedoms. The Index aggre-gates political rights and civil liberties on a 13-point scale that ranges from 2 to 14, where 2 represents the highest degree of political freedom, and 14 the lowest level.15 The variable measures aspects of civil liberties (freedom of expression and belief, associational and organization rights, rule of law, and personal autonomy and individual rights) and political rights (fairness of electoral processes, extent of political pluralism and participation, and the freedom of elected officials to have a decisive vote on public policies).16 To make the interpretation easier, we recoded the Freedom House index so that higher values indicate a higher level of civil liberties and political rights.

12 We do not expect the effect of sanctions to be constant through time simply because a state can only become so autocratic. The sanctions will therefore have a negative impact that decreases with time.

13As a common practice in the literature, we use Polity2 scores higher than 6 to represent liberal democ¬racies. It is worthwhile noting that when we use different Polity2 scores for the liberal democracy cut point (5 or 7), there was no significant change in our findings reported below.

14When we use a global sample of all countries, the results from the data analysis largely remained the same across different model specifications. None of the significance levels dropped in the global models. The results of the global sample analysis appear in Appendix I. Additionally, we repeated the analysis using a sample excluding all the major developed countries (Australia, Canada, Japan, the US, New Zealand, and Western European countries). Because developed countries have greater economic wealth and stability, some might claim that they are unlikely to suffer from economic sanctions as much as developing countries that have lesser economic capacity to cope with foreign economic pressures. In the models excluding the developed countries, there was also no significant change in our findings.

Independent Variables

Economic sanctions refer to government-led trade and financial coercion such as export restrictions, investment bans, asset freezes, reduction or sus-pension of military aid, restrictions on limited dual-use technologies, and travel bans on target countries’ officials (Hufbauer et al. 2008). The com¬piled data include both imposed unilateral sanctions initiated by individual countries as well as multilateral sanctions imposed by multiple countries under the auspices of the United Nations (UN) or regional intergovernmen¬tal organizations such as the European Union (EU) and the Organization of African Unity (OAU). The data on imposed economic sanctions are com¬piled from Hufbauer et al. (1997), Hufbauer and Oegg (2003), Hufbauer et al. (2008), and the TIES (Morgan, Krustev, and Bapat 2006).17 Our major goal of using these two major sanction datasets is to incorporate as many sanction episodes as possible for the empirical analysis.

In operationalizing economic sanctions, our first independent variable is Economic Sanctions, which is a dichotomous variable that takes the value of 1 if a country is under economic coercion in a given year and takes the value of 0 otherwise. This variable examines the impact of economic sanctions on political liberalization in general. In addition to the dummy variable that accounts for any type of economic sanctions, we also code two variables (Extensive Sanctions and Limited Sanctions) that account for the severity of the coercion. The Extensive Sanctions variable takes the value of 1 if a country is under comprehensive sanctions and 0 otherwise. The com¬prehensive or extensive sanctions refer to imposed sanctions that restrict all major trade and financial economic exchanges between the target and imposer countries (Hufbauer et al. 1997; Hufbauer and Oegg 2003; Yang et al. 2004).18 Similarly, Limited Sanctions variable is coded as 1 where a coun¬try is facing partial economic sanctions and 0 otherwise.19 The selective or limited sanctions include all limited economic restrictions such as investment bans or asset freezes and partial export restrictions, reduction or suspension of arms exports or limited dual-use technologies, constraints on military and other sorts of aid, and travel restrictions (Hufbauer et al. 2008).

15In the original dataset, the 13-point scale ranges from 1 to 7.

16We also ran models using the individual components of the Freedom House Index (Political Rights and Civil Liberties Indices). The results showed that economic sanctions cause a significant negative impact on both political rights and civil liberties in target countries.

17We exclude the sanction cases over environmental policy and minor trade practice disputes, which account for a significant portion of the TIES dataset, because those cases do not lead to any substantial political and economic consequences for target countries. In addition, we were unable to include some of the sanctions cases with a threat stage from the TIES since the actual imposition data for those cases were not indicated in the dataset.

Accounting for the severity of economic coercion is crucial to exam¬ine the extent of economic disruption and political hardship sanctions will inflict (Cortright and Lopez 2000; Weiss 1999; Weiss et al. 1997). Specifically, numerous studies emphasize that comprehensive economic sanctions cut most of the economic ties between the sender and target countries (e.g., Cuba, Iraq, and Yugoslavia) result in more economic damage, humanitarian suffering, and domestic instabilities compared to more selective sanctions (Cortright and Lopez 2000; Gibbons 1999; Hufbauer et al. 1997; Weiss 1999; Weiss et al. 1997). Hence, we expect that extensive sanctions will have a greater impact in consolidating the coercive power of the target regime by making the remaining resources more valuable and restricting economic, political, and cultural exchanges with the outside world. Although the selec¬tive sanctions that impose partial financial or trade-related limitations will also result in economic disruption, we expected that their humanitarian and political impact to be moderate. Thus, the suggested corrosive impact of sanctions on political liberalization will likely be greater in the coun¬tries under extensive economic coercion than the ones under selective (i.e., limited) sanctions (Gibbons 1999; Lopez and Cortright 1997).20

In addition to the immediate impact of sanctions, we also consider the effect of sanctions in the long/intermediate term. The duration variable is the log of Sanction Duration, which is simply a counter variable that accounts for how long an imposed sanction has been in place in a given year. The log form of the variable is used to account for the data’s positive time trend and our expectation that the negative effect of sanctions will decrease over time in a curvilinear fashion. The purpose of examining the impact of sanc-tions using a counter variable is to investigate the expected negative effect of sanctions over time. Sanctions will continue to cause a negative impact over their duration. In order to observe the difference between extensive and limited sanctions in the long run, we created two additional duration variables, log of Extensive Sanction Duration and log of Limited Sanction Duration, that account for the duration of extensive and limited sanctions in a country respectively.

18It is worth noting here that identification of the severity of sanctions is a challenging task since there is no consistent categorization in the literature. Some scholars, for instance, prefer dividing the list of sanctions into the categories of limited, moderate, and extensive sanctions to determine the severity of sanctions (Hufbauer and Oegg 2003; Wood 2008). Yet, the data for limited and moderate sanctions are not available in the existing data sources for the sanction episodes not involving the U.S. or the UN (see for example Wood 2008). Therefore, due to data limitations, we choose to divide sanctions into two categories (for a similar operationalization, see Caruso 2003; Peksen 2009; Yang et al. 2004).

19Some target countries face multiple economic sanctions in a given year. In those circumstances, the sanction episode with more severe consequences was considered as the basis to code the sanction variables. In order to ensure that those incidences do not bias the results, we ran additional models dropping those countries from the analysis; there was no major difference in the results.

20 Some sanctions are imposed by sender countries for domestic or international symbolic purposes that are not explicitly designed to induce compliance from target countries. We expect that such “symbolic” sanctions will have a minimal impact on democratic freedoms since they are unlikely to inflict any major damage on the economic and political stability of the target.

Control Variables

To control for the effects of other independent factors, a battery of con¬trol variables is included in the analysis. The literature on the determinants of democratization has significantly improved our understanding of what domestic and international factors might improve political liberalization. Yet, the direction and significance of many of these independent factors on democratization remain highly contested (Geddes 1999; Przeworski and Limongi 1997). Our purpose here is to control for the major independent factors explaining democratization by briefly justifying the inclusion of those factors and bringing in the competing views on the same variables .21

The natural log of Gross Domestic Product (GDP) per Capita (in 1995 constant US dollars) and Economic (GDP) Growth rate (annual percent-age) are used to control for the effects of developmental differences across countries.22 Advocates of the modernization theory suggest that increasing economic wealth is essential for the emergence of a strong, well-educated middle class that is essential to create more demand for responsive govern-ment and thus increases the probability of transition to democracy (Lipset 1959; Barro 1999; Epstein et al. 2006). However, some others claim that the suggested theoretical link appears to fail in empirical analyses (Przeworski et al. 2000; Przeworski and Limongi 1997). The latter view finds empirical evidence suggesting that democracies may occur in poor as well as rich countries.

21The research on political liberalization is one of the well-studied areas in political science. While we do not report them here to keep our models parsimonious, based on the literature on democratization we also added several other control variables to check the robustness of our findings to the different model specifications. These additional control variables include Inflation rate, Trade (percent of GDP), Unemployment rate, Population Growth, percent of Democratic Polities in the world, British and French Colonial heritage, dummy variables for different regions, and a year variable to control for time trend. In these alternative model specifications, there was no major change in our findings on the effect of economic coercion on democratization.

22 As mentioned above, we employed trade as a percentage of GDP as a control variable. This variable provides an excellent control for the target’s level of trade dependence and thus vulnerability to sanctions. There were no significant or substantive changes to the model when we used this control. It does not appear in the final models because it does create a degree of multicolinearity. Since there is no effect on our variables of interest, we chose not to include it.

In order to account for the level of economic openness, we include Foreign Direct Investment, which accounts for the amount of total for-eign direct investment flows as a percentage of GDP in a given year. Economic openness is associated with more democracy by promoting eco-nomic wealth, which is expected to increase the size of the middle class, reduce income distribution (equality), and promote education (Lipset 1994). Furthermore, some others also point out that economic globalization makes pro-democracy movements in less democratic states stronger by enhanc¬ing their ties and communication with transnational pro-democracy groups (Brunn and Leinback 1991; Keck and Sikkink 1998). The natural log of Population controls for the size of total population in each country. It is suggested that large population size may lead to scarcity of economic resources and economic grievance, which will make the use of repression by the government more likely (Henderson 1993; Poe et al. 1999). The data for economic development, growth, foreign direct investment, and pop¬ulation are from the World Bank’s World Development Indicators dataset (2004).

Previous research also points out that the presence of a Civil War in a country will harm political liberalization. As the government copes with antigovernment armed forces, it employs more restrictions over basic rights and freedoms of citizens to maintain control over the society (Poe et al. 1999). The civil war variable is coded as 1 if a country is experiencing a civil war with at least 25 battle-related deaths per year and 0 otherwise. The data for civil wars are from the Armed Conflict Dataset (Strand et al. 2005) of the International Peace Research Institute (PRIO).

We also add the Oil variable, which takes the value of 1 for countries whose oil export exceeds one-third of export revenues (Fearon and Laitin 2003). Scholars suggest that because the government in oil-rich states derives most of its revenue from oil, it tends to face little pressure from the public for accountability and representation. Furthermore, in the” rentier” states, the resource wealth helps the government invest heavily in military and policy forces to use against citizens demanding greater freedom and accountability (Ross 2001). Finally, to control for the notoriously low level of democracy in Arab countries, we add the Arab Country variable, which takes the value of 1 for the 21 countries that are members of the Arab League and domi¬nated by Arab populations (Donno and Russett 2004; Stepan and Robinson 2003).

Methodological Issues

There are several methodological issues that require attention at this point. First, in each of the models where we estimate the level of democratization, we use a one-year lag (t−1) of all the independent and control variables to mitigate any simultaneity issues between the dependent and independent variables. In other words, lagged-IVs allow us to make sure that our inde-pendent variables temporally precede the dependent variables to lessen any incorrect direction of inference.

Second, diagnostic tests indicate that the Freedom House democracy scores do not have a unit root (Im, Pesaran, and Shin 1997; Levin and Lin 1993; Wooldridge 2006) and are therefore stationary. However, there is a significant, strong autoregressive process. We correct for this tempo¬ral dependence in the data with two different strategies. First, we create a dynamic model by lagging the dependent variable. The institutional inertia present in a regime, whether democratic or autocratic, makes past values of the democracy score related to the present values. The lagged dependent variable both models this process and accounts for the serial correlation.23 In addition to the lagged dependent variable, we also first-difference the data (Keele and Kelly 2006; Wooldridge 2006).24 While this approach is a strong solution to the temporal dependence, it completely removes all issues with serial correlation in our data .25 Hence, we apply both of these specifica¬tions for consistency and as a robustness check. We also control for the heterosdeskasticity present in the model by using the fixed-effects estimator. Because the model has a time-invariant variable (the Arab country variable), we use Plümper and Troeger’s (2007) fixed-effects vector decomposition method to estimate all of the models.26

Finally, but perhaps most importantly, we address the endogeneity issue. It is possible that instead of sanctions causing a deterioration of democratic freedom, antidemocratic actions by the regime cause economic sanctions to be initiated. Certainly, democratic states have used sanctions in reaction to authoritarian coups in the past. While we tried to make a strong argument that economic coercion changes the conditions and incentives within the target that lead to suppression of democratic freedoms, there is also considerable empirical evidence that sanctions are the cause rather than the effect. First, if we change the lag structure of the model so that the sanc¬tions variable is lagged more than one year, the results remain significant. We lagged the sanction variable one, two, three, and four years, and while the size of the coefficient grew somewhat smaller, it remained negative and very significant. It seems hard, to say the least, to argue that a reduction indemocratic freedoms in one year would cause sanctions to be levied four years earlier.

23 Scholars have debated what method is best for correcting for an autoregressive process in the depen¬dent variable—AR(1) models or lagged dependent variables (Achen 2000; Keele and Kelly 2006). We feel that our results are extremely robust because in addition to the lagged dependent variable and first differenced models, we also estimate the models using an AR(1) estimator—the strategy used in Rudra’s (2005) study of globalization and political freedoms. The impact of our main independent variables in the AR(1) model was substantively identical to all of the other models.

24First differencing subtracts the past value of the variables from their current values (i.e., Xt−Xt-1).

25 The first-differenced dependent variable also captures the extent of political liberalization or de-liberalization by allowing us to examine how much change the presence of economic coercion causes in democratic freedoms over time.

26 Diagnostic tests revealed that there was no issue with multicolinearity in any of the estimations.

Second, while the four-lag specification provides strong evidence that slips in democracy are not causing sanctions, we also ran a two-stage simultaneous model where the level of democracy was the first depen-dent variable and the presence of sanctions was the second dependent variable (Amemiya 1978; Maddala 1983). We used the same independent variables for the first model as in all of the democracy models. In the second equation (predicting sanctions), we used economic, financial, and political control variables as well as the appropriate temporal controls. The results showed that sanctions still had a significant, negative effect on democracy, but the reverse was not true. The level of democracy was not associated with sanctions.

The third empirical finding, which suggests sanctions cause democratic rights to decrease (rather than vice versa), is that sanctions that had nothing to do with regime-reform (i.e., nondemocratic sanctions) have the same sig-nificant, negative impact on democratic governance. That is, sanctions that sought goals other than democratic reform—such as nonproliferation, drug trafficking, and terrorism—still lead to a reduction in the level of democratic rights. These nondemocratically motivated sanctions account for 57% of all sanctions, while those aimed at democracy or human rights account for 43%. Further, the impact of these sanctions was also robust across different lag structures. We hold that the robust nature of the lag structure, the lack of a significant effect in the simultaneous models, and the different coding of the sanction variable offer compelling evidence that there is not an endogeneity problem with our argument that sanctions lead to reductions in democracy.

Findings

Our findings, appearing in Table 1, show that the immediate impact of sanctions on political liberalization is negative and, we think, rather dra¬matic. The results in the first and second columns show support for our first hypothesis that economic sanctions worsen the level of democratic freedoms in sanctioned countries. Economic coercion has a significant and relatively strong negative impact on democracy. The results in columns three and four show support for our second hypothesis that states more extensive sanctions will have a greater impact on democracy than limited sanctions due to their greater economic cost and destabilizing impact on the regular functioning of domestic politics of the targeted countries. The impact from extensive sanctions is three to four times greater than limited sanctions in both specifications.27 Both sets of results hold in the presence of a battery of control variables under both statistical specifications (i.e., the lagged dependent variable and first-differenced models). We interpret these results as strong evidence that economic coercion creates externalities that are quite corrosive to democratic governance.

To put these results in perspective, we use the first differenced model (column 2 in Table 1) to show how large the immediate impact economic sanctions have on the target country. Using the Freedom House regime measure, our model predicts a 7% reduction in the average political liber¬ties score the year after the sender state initiated sanctions. The substantive effects of extensive and limited sanctions illustrate the much greater nega¬tive impact on political liberalization from extensive sanctions. Specifically, limited sanctions cause only a 6% decline in democracy in target coun¬tries, while the democracy score drops by more than 16% when extensive sanctions are employed.

Table 2 shows our estimation of the continuing impact that economic coercion has on democracy. In columns one and two, the sanction duration variable—the logged, count of years the sanctions are in place—is statisti¬cally significant and in the expected direction. This result lends support to our third hypothesis that economic coercion damages democratic freedoms throughout their imposition, although the negative effect decreases over time. This result suggests that the longer sanctions are in place, the greater cumulative negative impact they will inflict on the sanctioned country. That impact will vary with the severity of the sanction. Both extensive and limited sanction duration variables—seen in columns three and four—are statisti¬cally significant. The dramatic differences between extensive and limited show how much more change is induced by the more severe sanctions.

To illustrate the substantive impacts of sanctions on democracy, Figures 1 and 2 display the estimated effects of sanctions on democracy over a 15-year period of economic coercion.28 The first graph (Figure 1) shows the predicted impact that economic sanctions will have on a regime. The second graph (Figure 2) reports the substantive impact of extensive and limited sanction duration in the long term, respectively. The imposition and maintenance of sanctions—particularly extensive ones—erodes democratic governance quickly at first and then more gradually with the passage of time. Given that the average sanction lasts only seven years, clearly, the predicted impact of economic coercion is lasting and negative. While their negative effect on democracy diminishes over time, it stays consistently harmful to democratic freedoms, with extensive sanctions having the greatest corrosive impact.29

**TABLE 1** The Immediate Impact of Sanctions on Democracy: Fixed-Effects Vector

|  |  |  |  |  |
| --- | --- | --- | --- | --- |
| Decomposition Model |  |  |  |  |
|  | Dynamicmodel | Firstdifferenced | Dynamicmodel | Firstdifferenced |
| Lagged Democracy | 0.647\*\*\* |  | 0.648\*\*\* |  |
|  | (0.016) |  | (0.016) |  |
| Economic Sanctions (all) | -0.266\*\*\* | -0.727\*\*\* |  |  |
|  | (0.070) | (0.139) |  |  |
| Extensive Sanctions |  |  | -0.368\*\* | -1.516\*\*\* |
|  |  |  | (0.145) | (0.437) |
| Limited Sanctions |  |  | -0.160\*\* | -0.672\*\*\* |
|  |  |  | (0.072) | (0.138) |
| GDP per capita† | -0.492\*\*\* | 0.282 | -0.476\*\*\* | 0.317 |
|  | (0.050) | (0.438) | (0.045) | (0.436) |
| Economic Growth | 0.000 | 0.003 | 0.001 | 0.003 |
|  | (0.003) | (0.004) | (0.003) | (0.004) |
| Foreign Direct Investment | -0.004 | 0.006 | -0.004 | 0.006 |
|  | (0.006) | (0.005) | (0.006) | (0.005) |
| Population† | 0.710\*\*\* | 5.244\*\*\* | 0.689\*\*\* | 5.227\*\*\* |
|  | (0.042) | (1.734) | (0.041) | (1.729) |
| Civil War | -0.190\*\*\* | -0.092 | -0.180\*\*\* | -0.073 |
|  | (0.066) | (0.095) | (0.067) | (0.095) |
| Oil | 0.021 | 0.032 | 0.0258 | 0.008 |
|  | (0.069) | (0.202) | (0.069) | (0.202) |
| Arab Country | 0.229\*\*\* | -0.022 | 0.222\*\*\* | -0.022 |
|  | (0.076) | (0.075) | (0.076) | (0.075) |
| FEVD Error Term | 1.000\*\*\* | 1.000\*\*\* | 1.000\*\*\* | 1.000\*\*\* |
|  | (0.058) | (0.117) | (0.058) | (0.116) |
| Constant | -6.093\*\*\* | -0.119\*\* | -5.877\*\*\* | -0.117\*\* |
|  | (0.466) | (0.049) | (0.461) | (0.049) |
| Observations | 1626 | 1625 | 1626 | 1625 |
| R-squared | 0.81 | 0.07 | 0.81 | 0.08 |

Standard errors in parentheses. All independent variables are lagged one year. \*Significant at 10%; \*\*significant at 5%; \*\*\*significant at 1%. †Logged variables.

**TABLE 2** The Medium and Long-term Impact of Sanctions on Democracy: Fixed-Effects

|  |  |  |  |  |
| --- | --- | --- | --- | --- |
| Vector Decomposition Model |  |  |  |  |
|  | Dynamicmodel | Firstdifferenced | Dynamicmodel | Firstdifferenced |
| Lagged Democracy | 0.640\*\*\* |  | 0.648\*\*\* |  |
|  | (0.016) |  | (0.016) |  |
| Economic Sanctions (all) | -0.241\*\*\* | -0.452\*\*\* |  |  |
|  | (0.036) | (0.108) |  |  |
| Extensive Sanctions |  |  | -0.225\*\*\* | -1.019\*\*\* |
|  |  |  | (0.069) | (0.314) |
| Limited Sanctions |  |  | -0.019\*\* | -0.061\* |
|  |  |  | (0.008) | (0.033) |
| GDP per capita† | -0.496\*\*\* | 0.302 | -0.449\*\*\* | 0.315 |
|  | (0.045) | (0.439) | (0.044) | (0.440) |
| Economic Growth | 0.001 | 0.004 | 0.001 | 0.003 |
|  | (0.003) | (0.004) | (0.003) | (0.004) |
| Foreign Direct Investment | -0.003 | 0.006 | -0.004 | 0.006 |
|  | (0.006) | (0.005) | (0.006) | (0.005) |
| Population† | 0.788\*\*\* | 5.109\*\*\* | 0.690\*\*\* | 5.092\*\*\* |
|  | (0.046) | (1.739) | (0.041) | (1.743) |
| Civil War | -0.190\*\*\* | -0.117 | -0.183\*\*\* | -0.097 |
|  | (0.066) | (0.096) | (0.067) | (0.096) |
| Oil | 0.026 | 0.0335 | 0.0214 | 0.017 |
|  | (0.069) | (0.203) | (0.069) | (0.203) |
| Arab Country | 0.222\*\*\* | -0.020 | 0.207\*\*\* | -0.018 |
|  | (0.076) | (0.075) | (0.076) | (0.075) |
| FEVD Error Term | 1.000\*\*\* | 1.000\*\*\* | 1.000\*\*\* | 1.000\*\*\* |
|  | (0.057) | (0.117) | (0.058) | (0.116) |
| Constant | -7.230\*\*\* | -0.113\*\* | -6.055\*\*\* | -0.111\*\* |
|  | (0.505) | (0.050) | (0.465) | (0.050) |
| Observations | 1626 | 1625 | 1626 | 1625 |
| R-squared | 0.81 | 0.07 | 0.81 | 0.06 |

Standard errors in parentheses. All independent variables are lagged one year. \*significant at 10%; \*\*significant at 5%; \*\*\*significant at 1%. †Logged variables.

#### DEMOCRATIC FREEDOM DIRECTLY IMPACTS ONE’S QUALITY OF LIFE

Orviska et al 12

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There is now a substantial literature on “happiness”. Reviews can be found in Frey and Stutzer (2002a and b), Kahneman, Diener and Schwarz (1999), Layard (2006) and Dolan, Peasgood and White (2008). Much of the literature has focused on both countries (e.g. Deaton, 2008) and individuals within cross section analyses, aimed at exploring the socio-economic determinants of “happiness”. It is an important area of research in which a substantial number of economists and others are now working. The research has focused on linking wellbeing to socio-economic variables such as income, age, gender, marital status and level of education. There has also been increased research into the impact of factors external to the individual. Included in this have been aspects of governance.

Governance will be the specific focus of this paper, concentrating on the impact of democracy on well-being. There have been some studies which have analysed this, although there is some disagreement on the nature, if any, of the relationship. Theoretically it seems plausible that there should be such a relationship. Dorn et al. (2007) argue that democracy facilitates outcomes closer to citizen preferences. They also argue that the act of participating in the democratic process may in itself increase well-being. However, empirical problems may arise in detecting such a relationship because of the impacts governance may have on other variables, such as prosperity, and also because of the potential impact of variables such as culture on both democracy and well-being. There are also potential endogeneity problems when we use individual perceptions of democracy to explain individual wellbeing. In our analysis we focus on the impact of satisfaction with democracy on individual wellbeing. Endogeneity problems are overcome by using the average democratic satisfaction of others in the individual’s region as an explanatory variable, rather than individual satisfaction. This focus on regional impacts is almost unique in the literature. Pittau et al (2010) are one of the few papers that analyse life satisfaction at the regional level, in their case within the EU. They find wide variability within countries, particularly for Belgium, Germany, Spain, Italy and Portugal.

The main findings are that regional democratic satisfaction impacts on happiness. This is the case even when account is taken account of life satisfaction. Such views we argue reflect the actual state of democracy in the region. This impact is however less significant for women and rich people. It is also not in evidence for richer countries as a whole. The fact that the analysis is based on regional differences in democratic satisfaction reminds us that governance and indeed happiness often differs within countries as well as between them. We also find that many of the factors which have been found to impact on wellbeing in other studies mainly based in developed countries, also apply more generally and in particular to less developed countries.

The paper will proceed as follows. In the next section we will review the literature, first focusing on the specific impact of governance and then the more general determinants of well-being. The theoretical analysis follows after which the data will be described and the empirical results reported. Finally we will conclude the paper.

#### WE USE A REGIONAL AVERAGE MODEL OF WORLD VALUES SURVEY DATA FROM OVER 22,000 PEOPLE IN 2005 FROM 29 COUNTRIES CONTROLLING FOR LIFE SATISFACTION, GENDER, AGE, FAMILY LIFE, EDUCATION, INCOME, SAVINGS, HEALTH, EMPLOYMENT, LOCATION, RELIGION, AND MINORITY STATUS

Orviska et al 12

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3.1 The Impact of Democracy on Happiness

The traditional approach of the economist to the individual’s problem is to assume utility maximization subject to constraints. The constraints generally include income and sometimes time. Income is largely endogenous to the individual’s maximization problem being dependent upon the number of hours people choose to work. Skills and education become relevant in determining the average hourly pay. Skills, education and age can also impact on the maximization problem in potentially impacting on the household production function. Within this context gender may be relevant if there is a gender pay gap. Gender may also impact on the time constraint, as may age. Of course happiness is also the consequence of social relations and socio-economic variables such as marital status, gender and age can also impact on these in quite different ways to productivity in the labor market or household production.

One important factor impacting on both the labor market and the household production function are public goods, and more generally services facilitated by regional and national government, such as education, health, law and order and transport infrastructure. This is supported by Wagner, Schneider and Halla (2009) who conclude that higher-quality institutions increase satisfaction with democracy. For this reason good governance can also impact on happiness in a meaningful manner. Bohnke (2008) uses perceptions of governance in analysing well-being. This is a perception based measure and hence subjective rather than objective, but the assumption is that such perceptions are based on reality. Better rule of law, lower corruption, less regulation of political participation are all associated with higher degrees of satisfaction with democracy. It has also been suggested that participation in the democratic process yields utility to the citizen (Dorn et al., 2007). For example, Fiorina (1976) argues that the utility from voting depends upon the act of expressing a preference similar to applauding a fine symphony performance (Aldrich, 1997). However democracy is not without its flaws. Fosu, Bates, and Hoeffler (2006) are sceptical of the value of democracy within the context of Africa arguing that politically accountable governments are associated with a greater risk of political disorder, which may also impact adversely on well-being.

3.2 Dealing with Endogeneity

There are obvious problems of endogeneity in using individual perceptions of democracy to explain well-being. Satisfied or happy people may be more likely to voice approval of institutions and governance, i.e. causality could plausibly run from well-being to satisfaction with governance as well as vice versa (Frey and Stutzer, 2002b and Graham and Pettinatio, 2002). The argument is that happy or satisfied people are more likely to be benign in their judgments of both people and institutions. However, there are also problems at the aggregate level, for example, a with a potentially simultaneous relationship between well-being and democracy and democratic institutions (Frey and Stutzer, 2002b).

We will go some way to meeting these issues by (i) focusing on individuals rather than countries and (ii) in explaining individual well-being using regional measures of the state of democracy rather than individual measures. This regional variable will for the i’th individual, represent the average response of others in the region on democratic satisfaction. This is based on 388 regions. Its use allows us to capture the impact of governance on individual attitudes to democracy provided governance differs between regions of the country. If it does not, if governance and attitudes to democracy are the same within each country, then the country fixed effects will pick that up and this variable will neither vary greatly across regions within a country, nor be significant. But much that impacts on individuals, particularly in federal systems, is done at the regional level and differs between regions. This includes the police, possibly the courts, local offices of national bodies and regional governance. Even in non-federal systems much is decentralised to both the region and the municipality (Work, 2002). In addition in the absence of decentralization spatially distributed heterogeneity of citizen preferences can in itself lead to different levels of democratic satisfaction.

This deals with part of the endogeneity problem, but not fully with the issue raised by Frey and Stutzer (2002b) that a satisfied population may foster democracy and democratic institutions. Our approach to this rests on the assumptions that at the regional level (i) life satisfaction impacts on both democracy and happiness and (ii) life satisfaction also impacts on democracy and democratic satisfaction. Hence in the regressions, democratic satisfaction may be picking up the impact of life satisfaction on happiness. We thus include a second regional variable, regional life satisfaction. Any impact of regional democratic satisfaction will then be in addition to that of regional life satisfaction. This is thus a strong test for the impact of democracy on happiness.

The literature tends to support the validity of these assumptions. Most research has recognized that life satisfaction and happiness are different but then proceeded to analyze both as being representative of subjective well-being. An exception is Tsou and Liu (2001) who argue that with respect to Taiwan the effects of individual characteristics on happiness and satisfaction with different aspects of life are fundamentally different. Selim (2008) and Gitmex and Morcol (1994) also argue that although there is some correlation between the two they are nonetheless distinct concepts. Selim also argues that satisfaction is a cognitive evaluation that, apart from comparisons with others, is dependent upon an evaluation with respect to an individual’s desires, expectations and hopes. In contrast happiness is defined as ‘an emotional state’ produced by positive and negative events and experiences in the life of an individual. Tsou and Liu, building on the work of Vermunt et al (1989), also define happiness as an emotional state which is subject to sudden mood changes whilst life satisfaction is a cognitive state which refers to an assessment of life as whole. Pittau et al (2010) argue that life satisfaction and happiness: are broadly consistent measures of subjective well-being, but are different. But that happiness is a more volatile concept of current emotional state, while life satisfaction is closer to the concept of an overall and more stable concept. Given this we would expect happiness to be impacted on by the more stable individual life satisfaction and thus positively linked to regional satisfaction. We might also reasonably expect the more stable life satisfaction to facilitate democracy more than happiness per se. This is consistent with Inglehart’s (1999) argument that that high satisfaction with life in a population increases the legitimacy of the political regime in power and may thus foster democracy.

Using regional averages also helps with a second problem with using individual responses. These may give substantially different measures with respect to democracy for individuals in the same region. But, if it is the impact of democracy on well-being we are seeking to analyse rather than individual perceptions, the averaged view of all other individuals in the region is preferable. This is also why we choose not to endogenize individual democratic satisfaction within an instrumental variable framework. In any case the instruments would be the regional based measures and hence in practice the two approaches are largely similar.

Hence, the equation we will estimate will include the standard socio-economic and cultural variables the literature suggest are important, plus the regional variables and country fixed effects. We will (i) be estimating regressions for both happiness and life satisfaction with just regional democratic satisfaction and not regional life satisfaction and (ii) for happiness based on all the explanatory variables including regional life satisfaction. Finally we will do separate regressions for different socio-economic groupings and countries in seeking to inform us on whether the impact of democratic satisfaction is homogenous across these groups.

4. The Data and Empirical Formulation

The World Values Survey data has become increasingly well-known in recent years, and, in addition to the research already referred to, have been utilised in hundreds of publications. Recent examples, many linked with research into well-being, include Guiso et al (2008), Bonini (2008), Bruni and Stanca (2008), Snoep (2008), Tesch-Romer et al (2008) and Sanfey and Teksoz (2007). It is a worldwide investigation of socio-cultural and political change conducted by a network of social scientists at leading universities all around the world. Interviews are carried out with nationally representative samples of the publics of more than 80 countries covering 85% of the World’s population. Five waves of surveys have been carried out in 1981, 1990-1991, 1995-1996 and 1999-2001 and 2005. Each sample contains at least 1,000 respondents. In more recent years greater emphasis has been given to obtaining better coverage of non-Western societies and analysing the development of a democratic political culture in the emerging democracies. The results in this paper are based on the fourth wave3.

The dependent variables relate to standard questions on happiness and life satisfaction. Because of the discrete nature of the data, we use ordered probit regressions to estimate the equations. Happiness is measured on a four point one. The governance variable relates to satisfaction with the way democracy is developing in the respondent’s country. It will thus reflect (i) the extent to which the country and region is democratic, (ii) any movement in that situation and (iii) the extent to which the democratic system is delivering satisfactory outcomes. Figure 1 shows a clear relationship between average regional happiness and satisfaction with democracy. Regions where people tend to be satisfied with democracy are also regions where people are happy. But is this picking up anything more than variations between countries? Is there also a relationship within countries? Figure 2 suggests that there is. It plots the deviations of regional well-being and democratic satisfaction from the country averages. The figure shows that regions where people are more satisfied with democracy than the average for the country also tend to be happier regions than the average. Of course this could be picking up spatial factors, such as population density. But the regression analysis which follows will allow for such possibilities.

Insert Figure 1 about here

Insert Figure 2 about here.

Included in this regression analysis, apart from the regional variables relating to democratic and life satisfaction, we will include control variables as suggested by the literature. These will include gender, age, education, relative income, savings, marital status, health, whether unemployed, whether the individual has children, locality, religion, and religious and linguistic minority variables. With respect to savings, which has not been previously used in this type of analysis, the expectation is that people who have had to borrow money in the previous year are more likely to be struggling and hence less likely to be happy or satisfied. It thus helps augment the data on income, All variables are defined in a data appendix where there is also a list of countries.

Table 1 shows the correlation matrix between the variables. Partly because of the large sample sizes most correlations are significant, even though in many cases the correlations are quite small. Focusing on the impact of regional democratic satisfaction, we note the positive correlation with happiness and life satisfaction, i.e. people in regions with high levels of democratic satisfaction tend to be happierand more satisfied.

Table 2 shows the regression results. The first two columns show the results for life satisfaction and happiness without including the regional well-being variable. In both regressions, regional democratic satisfaction is significant at the 1% level. Surprisingly perhaps the impact is greater in the happiness equation. The remaining columns show the regressions with happiness as the dependent variable and including regional life satisfaction. Regional democratic satisfaction is positively significant at the 1% level in the regression for the full sample of countries. In other regressions however, it is less significant for women and rich people. It is also not in evidence at all for richer countries as a whole. The significance of democratic satisfaction despite the inclusion of regional life satisfaction is a strong result. This both addresses the problem that the impact of democratic satisfaction may be picking up the possibility that satisfied regions tend to be more democratic ones, due to a greater ease of governance. It also largely addresses the problem that regional democratic satisfaction may be proxying other regional variables which impact on happiness. This significance is robust to specification and remains if we correct the standard errors allowing for intra-country correlation of the error term. In these regressions, regional life satisfaction has a non linear impact on happiness, there were no such nonlinearities present for regional democratic satisfaction.

Insert Table 2 about here

With respect to the control variables, the impact of the socio-economic variables is largely as in other studies and thus we will focus on the differences. Being married increases happiness for everyone, but being a widower has no significant impact on men, richer people and richer countries. Having children reduces happiness, possibly because of the impact on the time and income constraints. The linguistic minority variables are generally not significant, but become so if we omit regional life satisfaction. The religious minority variable is only weakly significant although Protestants, and to an extent Catholics, tend to be happier than other people4.

Data Appendix: Variable Definitions

Attitudinal Variables

Happiness A dependent variable; coded 1 if the individual responded that taking

all things together they were not at all happy to 4 very happy

Life A second dependent variable; scaled from dissatisfied (1) to satisfied

satisfaction (10)

Democratic Coded 1 if the respondent is very dissatisfied satisfied with the way

satisfaction democracy is developing in their country to 4 (very satisfied).

Socio economic variables

Male Binary variable, coded 1 if the respondent is a man

Age Age of the respondent in years

Married Binary variable, coded 1 if the respondent is married.

Widow Binary variable, coded 1 if the respondent is widowed.

Children Binary variable, coded 1 if the respondent has children.

Education Coded from 1 (no formal education) to 9 (university level education

with degree)

Income Coded from 1 to 10 reflecting increasing levels of household income -the exact classification varies from country to country. In effect this is

a relative income measure.

Savings Coded 1 if the respondent’s family saved money in the previous year

to 4 if they spent savings and borrowed money.

Poor health The self-perceived state of the individual’s health. A Binary variable,

coded 1 if in poor or very poor health.

Unemployed Binary variable, coded 1 if the respondent is unemployed.

Location Coded 1 to 8 (large city) reflecting the size of the settlement in which

the individual lives.

Religious Binary variables, coded 1 if the individual identified themselves as

Group members of a religious group (Catholic, Protestant, Orthodox, Jew,

Muslim, respectively).

Linguistic The proportion of the country’s population having as their first

language

Minority the same as the respondent, where first language is defined as the one

normally spoken at home.

Religious The proportion of the country’s population having the same religion

Minority as the respondent.

GD PPC The level of GDP per capita in the individual’s country in the year

2000 in US$ purchasing power parity (Source: World Bank data set). Countries included: Albania, Argentina, Bangladesh, Bosnia, Canada, Chile, Egypt, India, Indonesia, Iran, Japan, Jordan, Kyrgyz, Macedonia, Mexico, Moldova, Montenegro, Morocco, Peru, Philippines, Puerto Rico, Serbia, South Africa, Spain, Tanzania, Uganda, USA, Vietnam

#### CUBANS WANT DEMOCRATIC FREEDOMS

Moreno & Calingaert 11

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The Reforms Cubans Would Like to See

Freedom House researchers recorded a long list of aspirations when respondents were asked what reforms they would like to see in Cuba. The most common responses were reforms that increased freedom of expression and the freedom to travel (28 percent). Others said they want better salaries (11 percent) or economic improvement (8 percent). Some wanted a change in government (5 percent), more private economic activity (5 percent), the elimination of the double currency (4 percent), and more jobs (3 percent). Many other responses obtained fewer mentions. It is remarkable, however, that the main response had to do with the lack of freedom and not with economic adversity.

This is a noticeable change from the previous study conducted by Freedom House, where economic changes were the top pick. In December 2010, for example, the most common single response to what reforms Cubans would like in their country was “the economy”, with 14 percent; 10 percent mentioned freedom of expression and freedom to travel.

A young woman who works in a pharmacy in Camagüey said that she dreams of getting out of Cuba, traveling to Spain and working there: “In Cuba there are no possibilities for young people to dream and have the things that others have; we cannot go out of the country.” A 60-year old woman who talked about her spiritual beliefs during most of the interview expressed a similar view: “I wish we had more freedom to travel, I wish people could go out of Cuba for vacations.”

Some respondents favor fundamental change in Cuba. A medical doctor interviewed near Santa Clara said: “I would like to see a total collapse of the system, to bring capitalism to the country. We are always told that capitalism is bad, that it creates inequality. Maybe so, but it is better to have a few rich people than have everyone poor.” The doctor was well informed about the reforms, but in his opinion, they are “only cosmetic; none of these are really important.” He argued that “most Cubans are living in the same situation or even worse than before. I don’t think there will be big changes in the country as long as the Castros are in power.”

Other respondents, in contrast, sounded nostalgic for the rule of Fidel Castro. A 20- year old student who lives from the money sent by her family in the United States, for example, expressed the view that “Cuba is moving backwards. We were better off with Fidel; we received a lot of things with him. We still have many things he gave us before the periodo especial, when the state gave us a refrigerator, television, a stove, and machines for the kitchen... I think that if Fidel had stayed longer in power he would have given cell phones to everybody in only two years.”

Change Comes to Cuba: Citizens’ Views on Reform after the Sixth Party Congress

Conclusion

The findings of this survey stand in stark contrast to the findings of previous Freedom House field research conducted before the Sixth Communist Party Congress. In the December 2010 survey, announced reforms had yet to take effect. Cubans generally were skeptical that change would come, and many of them expected any changes to make matters worse. By June 2011, by contrast, change was visible, notably in the appearance of cuentapropistas “on every corner.” Change has generated optimism among a significant segment of the population in Cuba, where previously optimism was almost completely absent. This segment of the population is doing better and expects economic conditions to improve further.

The opening of a private sector, while still limited, is driving genuine change in Cuba. This is the most significant positive change to have taken place in Cuba since communism was introduced half a century ago. Cubans are moving from the state to the private sector, becoming entrepreneurs in growing numbers, taking the initiative to earn their own living, and in many cases succeeding to do better than Cubans in government jobs.

The changes are causing a sense of insecurity and resentment among some Cubans, as might be expected in a country where citizens were almost entirely dependent on government for their material needs and had no experience of market competition. Such insecurity and resentment accompanied the shift from communism to market economies in Eastern Europe and the former Soviet Union. While the insecurity and resentment presents a challenge for reform in Cuba, it is also a reflection of how profound are the changes that are currently underway.

The increased desire for civil liberties is a striking finding of this survey. Perhaps the improvement in economic conditions makes Cubans more secure to seek individual freedoms, rather than focus on their poor living standards, or the independence that comes with self-employment motivates more Cubans to value free expression and personal autonomy. Either way, economic reform is accompanied by a growing interest in civil liberties and thus appears to spur aspirations for broader freedom in Cuba.

#### WE USE SIMPLE STATISTICS FROM 190 INFORMAL INTERVIEWS IN CUBA

Moreno & Calingaert 11

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In June 2011, Freedom House sent five field researchers to Cuba to conduct in-person interviews about the reforms announced during the Sixth Communist Party Congress, people’s perceptions and expectations about real change, and their exposure to information. A total of 190 face-to-face interviews were completed following an informal, semi-structured format. A standard questionnaire of 31 items was used to guide the conversation-style interviewing. Some questions were open-ended, while others required a simple yes or no answer. All questions were coded to quantify responses, and most of them were also recorded in a qualitative form.

The respondents were selected in an informal way, avoiding Cubans in the surroundings of tourist areas or hotels. The sample of respondents is not a probability sample, which is difficult to obtain in Cuba, but it covers a wide range of geographical and social variation, making this an interesting cross-sectional study of Cuban society. The selection of respondents followed criteria based on age, sex, and urban-rural residence. Race was not considered in the selection groups, but there is a diversity of ethnicities in the interviews, as described in the paragraphs below. Researchers were sent to six of Cuba’s 14 provinces to conduct their interviews: Ciudad de la Habana, Camagüey, Holguín, Pinar del Río, Santiago de Cuba, and Villa Clara. Interviews were conducted in different towns and locations in these provinces.

Because of the nature of the interviews, this special report analyzes two types of data: quantitative results that derived from the coding of responses; and qualitative analysis based on the more conversational style followed by researchers, who recorded verbal reactions and phrases, witnessed living conditions and experiences, and observed contextual surroundings.

**Appendix 3** - **Quantitative Results**

|  |  |  |
| --- | --- | --- |
| Provinces | Interviews | Percent |
| City of Havana | 61 | 32.1 |
| Camagüey | 33 | 17.4 |
| Villa Clara | 33 | 17.4 |
| Pinar del Río | 35 | 18.4 |
| Santiago | 22 | 11.6 |
| Holguín | 6 | 3.2 |
| Total | 190 | 100.0 |
| Sex of the respondent | Interviews | Percent |
| Male | 101 | 53.2 |
| Female | 89 | 46.8 |
| Total | 190 | 100.0 |
| Age of the respondent | Interviews | Percent |
| 18-29 | 44 | 30.1 |
| 30-49 | 59 | 40.4 |
| 50 or more | 43 | 29.5 |
| Total | 146 | 100.0 |

Note: 44 respondents were not asked or did not answer what age they are.

|  |  |  |
| --- | --- | --- |
| Ethnic group | Interviews | Percent |
| European/white | 105 | 55.3 |
| Afro-Cuban | 38 | 20.0 |
| Mulato | 47 | 24.7 |
| Total | 190 | 100.0 |
| Residence | Interviews | Percent |
| Urban | 148 | 77.9 |
| Rural | 40 | 21.1 |
| Not recorded | 2 | 1.1 |
| Total | 190 | 100.0 |

Page | 32

*Change Comes to Cuba: Citizens’ Views on Reform after the Sixth Party Congress* Religion Interviews Percent

Page | 33

Catholic 76 47.2

Protestant 14 8.7

Atheist 28 17.4

*Santerfa* 14 8.7

Believer 15 9.3

Other 14 8.7

Total 161 100.0

Note: 39 respondents were not asked or did not answer what religion they profess.

|  |  |  |
| --- | --- | --- |
| Occupation | Interviews | Percent |
| Professional | 23 | 13.7 |
| Non-manual work / supervisor | 8 | 4.2 |
| Non-manual work / under supervision | 12 | 6.3 |
| Specialized manual work | 20 | 10.5 |
| Non-specialized manual work | 35 | 18.4 |
| Agricultural worker | 6 | 3.2 |
| Housewife | 5 | 2.6 |
| Student | 23 | 12.1 |
| Retired | 8 | 4-2 |
| Other | 47 | 24.7 |
| Total | 190 | 100.0 |

Interviews Percent

What reforms would you like to see

Freedom of expresión / Freedom to travel 41 28

Better salaries 17 11

Economic 12 8

Change of government 7 5

More private business, less government 7 5

*Cuenta propia* 6 4

No more double currency 6 4

More jobs for the youth 5 3

Start own business 3 2

Support for the countryside 3 2

None 9 6

Other 32 22

Total 148 100

Note: 42 respondents were not asked or did not answer this question.

### Plan

#### Plan:

#### The United States federal government should remove its economic sanctions on Cuba

### Framework

#### CONTENTION 2 - FRAMEWORK

#### PREDICTIONS MUST HAVE A CLAIM, A WARRANT, AND STRONG DATA TO GET A DEFAULT 100% PROBABILITY, ANYTHING LESS GETS 0%.

#### STRONG DATA REQUIRES (1) EXPLICIT DISCLOSURE OF (2) QUANTIFIABLE RESEARCH METHODS

OUR INTERPRETATION IS BEST FOR DEBATE

SUBPOINT A - TRUTH – REGRESSION ANALYSIS ALLOWS US TO MAKE ACCURATE PREDICTIONS

Braumoeller & Sartori 02

[Bear F. Braumoeller, Associate Professor of Political Science at Ohio State University and Anne E. Sartori , Associate Professor of Political Science at Northwestern University 6 Empirical-Quantitative Approaches to the Study of International Relations in Cases, Numbers, Models: International Relations Research Methods edited by Detlef F. Sprinz and Yael Wolinsky REVISED, November 2002]

Advantages of the Statistical Method

One advantage of the statistical method is that it permits political scientists to aggregate information from a tremendous number of cases. This advantage is perhaps so obvious that its importance is often overlooked. To comprehend its magnitude we need only imagine trying to make sense of a thousand surveys of individual attitudes, beliefs, voting behavior, etc., without the aid of statistics. The ability to extract even basic summary statistics from such a mass of data is immensely valuable: even something as unsophisticated as a sample mean—say, per capita GNP—conveys a wealth of information in compact and understandable form.

The ability to aggregate information is a potent stimulus for theorizing. Theory development often begins when a researcher uncovers an empirical puzzle that remains unexplained by prior theory (Lave and March 1993). Such a puzzle leads to a search for an explanation, and eventually to new or better-developed theory. A puzzle can emerge from a single case, but the researcher often would like to know whether or not it indicates a prevalent pattern of behavior. Only statistics can provide the answer to this question.2

For example, statistical analyses indicate that a number of pairs of states (e.g., India and Pakistan) engage in a disproportionate number of wars (Goertz and Diehl 1992). The empirical discovery of this phenomenon, which the literature terms “enduring rivalry,” has led to a number of attempts to explain the behavior of this set of dyads (e.g. Vasquez 1995; Bennett 1998; Diehl and Goertz 2000): what is it that makes states become rivals; why do rivals fight so often; and how do rivalries end?

The use of statistics also makes the terms of a given debate more explicit. Inference requires assumptions, whether implicit or explicit; statistics force scholars to be quite explicit about the nature of at least some assumptions. Transparency is valuable both because assumptions should be as clear as possible and because one can compensate for violated assumptions if they are understood.3

 In addition to standards of inference, the use of statistics necessarily entails standards of evidence. Even the most scrupulous researcher can be hard-pressed to avoid selectively evidence that would contradict his or her theory. Here, too, standardization is an asset; the need for coding procedures forces the researcher to be explicit about criteria for measurement and mitigates the human tendency to notice only trends that are consistent with the theory under investigation. Quantification can be a considerable boon both to reliability and validity: in the former case, explicit tests of reliability can flag unacceptably “noisy” measures, while in the latter details of the coding process make it clear what is, and is not, being measured.4For example, the Polity democracy index is an aid to scholars because the coding rules are quite specific and reliability can be calculated.

Statistical techniques also permit us to assess the claim that observed associations among variables are due to chance. Such assessments are critical to the testing of theory, and they are often very difficult to make. The statistical method can make the task almost trivially easy. For example, the extent to which any given Third World country votes with the United States in the U.N. will naturally vary from year to year; as a result, it can be difficult to determine whether an increase or decrease following a change in domestic political regime is an indicator of realignment or simply the product of random fluctuation. Absent the ability to assess the odds that such fluctuations are due to chance, analysts could argue endlessly over their substantive significance.5 Hagan (1989) addresses this question by testing to determine whether mean voting scores under a given regime differ significantly from mean voting scores under its successor; in about half of the 87 cases he examines, he finds that random fluctuation is a highly improbable (p<0.05) explanation for the difference in voting patterns across regimes. Although statistical testing does not answer the question with perfect certainty, it gives far more precise answers than could otherwise be obtained. In so doing it dramatically narrows potential areas of disagreement.

By answering the question of whether observed associations are the plausible result of chance, the statistical method also permits us to draw causal inferences. Using statistics, one can investigate ancillary associations implied by a posited causal process and assess the probability that these associations are due to chance.6 Because international relations scholars constantly seek to understand why actors behave as they do, this ability is perhaps the method’s greatest contribution to the discipline. To continue the above example, one might wonder not just whether a given country’s U.N. votes coincide to a greater or lesser degree with those of the United States but why. One obvious possibility would be that American foreign aid, to put it crudely, buys votes: American leaders use foreign assistance to induce cooperation. If this is the case, increases in American aid should be followed by an increased coincidence of votes in the U.N. on issues considered to be important by the U.S. Wang (1999) tests this hypothesis by examining the voting records of sixty-five developing countries from 1984 to 1993 and finds that an increase in American foreign aid generally precedes an increase in voting alignment; moreover, the positive relationship between the two is very unlikely (again, p<0.05) to be the result of chance. Absent statistical techniques, the effects of American aid could be debated one anecdote at a time without any conclusion in sight. Even the most meticulous case selection and comparison could never produce such precise results.

A final strength of the statistical method is the fact that it conveys the ability to test two explanations against one another with remarkable precision. For example, while tests of realist and of domestic-political explanations of conflict typically limit themselves to ruling out chance associations, Clarke (2001) tests realism against two domestic-political explanations. He finds that realism “either does as well as the rival or better than the rival” theory (28).7

#### SPECIFICALLY, EXPLICIT DISCLOSURE IS REQUIRED – WE USE A SIMPLE REGRESSION MODEL OF SHARED DATA FROM 49 PAPERS CONTAINING OVER 1148 TEST STATISTICS

Wicherts et al 2011

[Jelte M. Wicherts\*, Marjan Bakker, Dylan Molenaar Psychology Department, Faculty of Social and Behavioral Sciences, University of Amsterdam, Amsterdam, The Netherlands "Willingness to Share Research Data Is Related to the Strength of the Evidence and the Quality of Reporting of Statistical Results" PLoS ONE 6(11)] [http://www.plosone.org/article/info%3Adoi %2F10. 1371%2Fjournal.pone.0026828]

In the current study, we related the willingness to share data from 49 papers published in Journal of Personality and Social Psychology or Journal of Experimental Psychology: Learning,Memory, and Cognition to two relevant characteristics of the statistical outcomes reported in the papers, namely the internal consistency of the statistical results and the distribution of significantly reported (p,.05) p-values. We restricted the attention to JPSP and JEP:LMC, because (1) authors in these journals were more willing to share data than authors in the other journals from which Wicherts et al. requested data, (2) no corresponding authors in these two journals declined to share data, because they were part of an ongoing project or because of propriety rightsor ethical considerations, and (3) studies in these two journals were fairly homogeneous in terms of analysis and design (mostly lab experiments).

CARD CONTINUES

Errors in the Reporting of Statistical Results The 49 papers contained a total of 1148 test statistics that were presented as significant at p,.05 . Table 1 presents for each paper the number of significantly reported test results, the number of misreporting errors, and the median and average of all genuinely significant p-values (as based on the recalculated values). Forty-nine of these statistics (4.3%) were inconsistent with the reported (range of) pvalues. In forty-seven of the inconsistent results (95.9%), the reported p-value (range) was smaller than the recalculated p-value. Figure 1 gives the origin of three types reporting errors. Although 51.1% (587) of the tests statistics from papers from which no data were shared, most incorrectly reported p-values (36 out of 49; 73.5%) originated from these papers. These errors include quite small ones (e.g., p=.0002 reported as p,.0001). Twenty-eight of the 32 p-values (87.5%) were incorrectly reported at the level of the 2nd decimal (e.g., =.02 reported as p,.01) were from papers from which no data shared. Negative binomial regressions (Table 2) that accounted for the number of test statistics and the average p- values in each paper (see below) showed that reluctance to share data was predictive of the prevalence of both types of reporting errors

CARD CONTINUES

In this sample of psychology papers, the authors ’ reluctance to share data was associated with more errors in reporting of statistical results and with relatively weaker evidence (against the null hypothesis) . The documented errors are arguably the tip of the iceberg of potential errors and biases in statistical analyses and the reporting of statistical results. It is rather disconcerting that roughly 50% of published papers in psychology contain reporting errors [33] and that the unwillingness to share data was most pronounced when the errors concerned statistical significance .

Although our results are consistent with the notion that the reluctance to share data is generated by the author’s fear that reanalysis will expose errors and lead to opposing views on the results, our results are correlational in nature and so they are open to alternative interpretations. Although the two groups of papers are similar in terms of research fields and designs, it is possible that they differ in other regards. Notably, statistically rigorous researchers may archive their data better and may be more attentive towards statistical power than less statistically rigorous researchers. If so, more statistically rigorous researchers will more promptly share their data, conduct more powerful tests, and so report lower p-values. However, a check of the cell sizes in both categories of papers (see Text S2) did not suggest that statistical power was systematically higher in studies from which data were shared.

The association between reporting errors and sharing of data after results are published may also reflect differences in the rigor with which researchers manage their data. Rigorously working researchers may simply commit fewer reporting errors because they manage and archive their data more diligently. A recent survey among 192 Dutch psychological researchers highlighted a rather poor practice of data archiving in psychology [36]. When asked whether they archived their research data, only a third of the psychologists responded positively. This is remarkable in light of guidelines of the APA [11] that stipulate that data should be retained a minimum of five years after publication of the study. Even among those psychologists who indicated that they “archive” their data, most did not follow proper archiving standards (e.g., by keeping code books and writing meta-data [37]), but simply stored data on their own (current) computer (32%), on CDs/DVDs (18%), or on the shelf (20%). Haphazard data management is documented in a number of scientific fields [37,38,39], may result in errors in analyzing and reporting of results, and obviously impedes the sharing of data after results are published. Regardless of the underlying processes, the results on the basis of the current papers imply that it is most difficult to verify published statistical results when these are contentious. We focused here on NHST within two psychology journals and so it isdesirable to replicate our results in other fields and in the context of alternative statistical approaches. However, it is likely that similar problems play a role in the widespread reluctance to share data in other scientific fields [13,14,15,16,17,18,19,20]. Because existing guidelines on data sharing offer little promise for improvement [40], progress in psychological science and related fields would benefit from having research data itself be part of the process of replication [15,16], notably by the establishment by journals, professional organizations, and granting bodies of mandatory data archiving policies. More stringent policies concerning data archiving will not only facilitate verification of analyses and corrections of the scientific record, but also improve the quality of reporting of statistical results. Changing policies require better educational training in data management and data archiving, which is currently suboptimal in many fields [36,37,38,39]. On the other hand,technical capabilities for storage are already available. For instance, several trial registers in the medical sciences (like clinicaltrials.gov) enable storage of research data. Rigorous archiving of data involves documentation of variables, meta-data, saving data files in formats that are robust (e.g., ASCII files), and submitting files to repositories that already require these standards. Best practices in conducting analyses and reporting statistical results involve, for instance, that all co-authors hold copies of the data, and that at least two of the authors independently run all the analyses (as we did in this study). Such double-checks and the possibility for others to independently verify results later should go a long way in dealing with human factors in the conduct of statistical analyses and the reporting of results.

Table 2 –Negative Binomial Regressions





#### SUBPOINT B IS POLICYMAKING EDUCATION – STRONG DATA IS KEY

Saks 86

[Michael J. Prof of Law at Arizona State University, cited in a Supreme Court opinion and thousands of articles. B.A., B.S., Pennsylvania State University, 1969; M.A., 1972; Ph.D., Ohio State University, 1975; M.S.L., Yale Law School, 1983. \*63 IF THERE BE A CRISIS, HOW SHALL WE KNOW IT? 46 Md. L. Rev. 63 Fall, 1986]

I. EMPIRICAL EVIDENCE OF THE PROBLEM AND ITS CAUSES

One of the most important aspects of this as well as related earlier articles by Professor Galanter [FN2] and his colleagues [FN3] is that they inquire into the degree to which relevant empirical evidence supports the claims made concerning a litigation explosion, and they share with us the findings of that inquiry. The explosion appears to be more rhetorical than real.

Those offering wholesale condemnation of our civil justice system, and counseling a variety of reforms ranging from tinkering to \*64 radical alteration, are confident they know a serious problem exists and, what is more, they know its causes . [FN4] Their language is so strong and so clear that one hesitates to doubt the accuracy of their vision. But in support of their views, they generally offer little more than unsupported assertions or anecdotes , examples of which Professor Galanter has cited. Mere assertion is simply that, and repeating something often or enlarging the chorus does not make it any more true.

As I have noted elsewhere, [FN5] government by anecdote is a bad idea not because the anecdotes are untrue or are not evidence (though sometimes they are untrue and therefore are not evidence), [FN6] but because they contribute so little to developing a clear picture of the situation we are concerned about. It makes a difference if for every ten anecdotes in which an undeserving plaintiff bankrupts an innocent defendant, zero, ten, one hundred, one thousand, or ten thousand equal and opposite injustices were done to deserving and innocent plaintiffs . [FN7] The proportion of cases that results in some sort of error , [FN8] and the ratio of one kind of error to the other, ought to be of greater interest to a serious policy-maker than a handful of anecdotes on either side of an issue. After all, the reforms to be adopted are intended to change that ratio and the tens of thousands of anecdotes it summarizes.

This brings us, then, to the kind of information that should form the core of the debate: data . If the explosion is real and the \*65 crisis serious, it should not be difficult to find data confirming those fears. In this regard, Professor Galanter makes two important contributions to the liability crisis debate. He summarizes some important data, and he helps us to think about what they mean. Conscientious policy-makers will be interested to learn that 98% of civil litigation goes on in state courts, that those filings have declined in the past several years, and that even tort filings have increased only 1 % more than population growth. [FN9] Those urging reform, when they do point to data, usually point to the 2% of litigation that is handled by federal courts. Professor Galanter helps us to interpret the meaning of those federal data, [FN10] which show a 123% increase in filings over the past decade.

First of all, he notes that an increase in filings is not necessarily a reflection of an increase in plaintiffs' “litigiousness.” Changes in filing rates are equally a reflection of defendants' resistance to resolving disputes short of litigation. The filing rate reflects, as well, the volume of transactions, the number of actionable injuries resulting from those transactions, lawyers' case-screening practices, and, no doubt, numerous othervariables. [FN 1 1] Any real understanding of what is going on requires knowing what lies behind and gives rise to any change (or stability) in filing rates.

Moreover, Professor Galanter shows us that the 123% increase \*66 means something other than appears at first blush. By disaggregating those cases into the categories supplied by the Administrative Office of the United States Courts, Professor Galanter finds that it is the federal government itself that has added by far the largest fraction of the increase in litigation, having increased its filings (of over-payment recoveries) by 6,683%! [FN12] Except for products liability (of which one-fourth were asbestos claims, now waning), the federal caseload for tort cases has been fairly stable.

These are but a few illustrations of the way Professor Galanter, and other empirically oriented legal scholars, force us to deal with the evidence of the world we propose to reshape through law reform. We need not limit policy debates to a mutually uninformative swapping of anecdotes or a heated exchange of quotations. In my view, it is enormously helpful to inform ourselves about our world empirically , and to think intelligently about the alternative interpretations of the relevant empirical data. [FN13]

That the topographic map of vociferous reformers is not consistent with the most fundamental features of the landscape over which they presume to reign should give us all pause. If their assessment of our condition — the easiest part of problem-solving — can be so inconsistent with the evidence, we might well be hesitant to accept their diagnosis of causes and their prescribed treatment.

#### **SUBPOINT C IS WEAK DATA GETS ZERO PERCENT**

#### ASSUME THE NEG DISADS, CRITIQUES, AND CP SOLVENCY HAVE ZERO PROBABILITY BECAUSE THEY ARE WITHIN THE STATISTICAL “MARGIN FOR ERROR”

Zellner 07

[Arnold Graduate School of Business, University of Chicago Philosophy and objectives of econometrics Journal of Econometrics Volume 136, Issue 2, February 2007, Pages 331-339]

On the relation of science and econometrics, I have for long emphasized the unity of science principle, which Karl Pearson put forward as follows: the unity of science is a unity of methods employed in analyzing and learning from experience and data. The subject matter discipline may be economics, history, physics, or the like, but the methods employed in analyzing and learning from data are basically the same. As (Jeffreys, 1957) and (Jeffreys, 1967) expresses the idea, “There must be a uniform standard of validity for all hypotheses, irrespective of the subject . Different laws may hold in different subjects, but they must be tested by the same criteria ; otherwise we have no guarantee that our decisions will be those warranted by the data and not merely of inadequate analysis or of believing what we want to believe . ” Thus the unity of science principle sets the same standards for work in the natural and social sciences. For example, this range of considerations is particularly relevant for those in economics who cross-correlate variables and assert causation on the basis of such correlations alone (See Zellner (1979a) for consideration of such tests and of alternative definitions of causality) or those who carelessly test all hypotheses in the “5% accept–reject syndrome.” Also, we must emphasize the importance of a general unified set of methods for use in science and the undesirability of unnecessary jargon and ad hoc methods.

Given that we take the unity of science principle seriously, we may next ask what are the main objectives of science. As Karl Pearson, Harold Jeffreys, and others state, one of the main objectives of science , and I add of econometrics, is that of learning from our experience and data. Knowledge so obtained may be sought for its own sake, for example, to satisfy our curiosity about economic phenomena and/or for practical policy and other decision purposes. One part of our knowledge is merely description of what we have observed; the more important part is generalization or induction, that is that part which “... consists of making inferences from past experience to predict future [or as yet unobserved] experience,” as Jeffreys puts it.

Thus there are at least two components to our knowledge, description and generalization or induction. While generalization or induction is usually considered to be more important, description plays a significant role in science, including economics. For example, Burns and Mitchell's monumental NBER study Measuring Business Cycles is mainly descriptive but valuable in providing general features of business cycles about which others can generalize . While some have damned this work as “measurement without theory ,” the opposite sin of “ theory without measurement” seems much more serious. In fact there are too many mathematical economic theories which explain no past data and which are incapable of making predictions about future or as yet unobserved experience. Such economic theories are mathematical denk-spielen and not inductive generalizations to which I referred above. Further, I shall later mention another important role for description in connection with reductive inference.

In learning from our experience and data, it is critical that we understand the roles and nature of three kinds of inference, namely, deductive inference, inductive inference, and reductive inference.

As regards deductive inference, Reichenbach (1958) explains, “Logical proof is called deduction; the conclusion is obtained by deducing it from other statements, called the premises of the argument. The argument is so constructed that if the premises are true the conclusions must also be true. ... It unwraps, so to speak, the conclusion that was wrapped up in the premises.” Clearly, much economic theory is an exercise in deductive inference. However, the inadequacies of deductive inference for scientific work must be noted. First, traditional deductive inference leads just to the extreme attitudes of proof, disproof, or ignorance with respect to propositions. There is no provision for a statement like “A proposition is probably true” in deductive inference or logic. This is a deficiency of deduction for scientific work wherein such statements are very widely employed and found to be useful. Second, deduction or deductive inference alone provides no guide for choice among logically correct alternative explanations or theories. As is well known, for any given set of data, there is an infinity of models which fit the data exactly. Deduction provides no guide for selection among this infinity of models.

Thus, there is a need for a type of inference which is broader than deductive inference and which yields statements less extreme than deductive inference . This type of inference is called inductive inference by Jeffreys. It enables us to associate probabilities with propositions and to manipulate them in a consistent, logical way to take account of new information. Deductive statements of proof and disproof are then viewed as limiting cases of inductive logic wherein probabilities approach one or zero, respectively.

Jeffreys (1967), who has made major contributions to the development of inductive logic in his book Theory of Probability states that inductive inference involves “ making inferences from past experience to predict future experience ” by use of inductive generalizations or laws . And given actual outcomes, the procedures of inductive inference allow us to revise probabilities associated with inductive generalizations or laws to reflect the information contained in new data .

Note that for Jeffreys induction is not an economical description of past data, as Mach suggested since Mach omitted the all-important predictive aspect of induction. Further, predictive inductive inferences have an unavoidable uncertainty associated with them, as Hume pointed out many years ago. For example, it is impossible to prove, deductively or inductively that generalizations or laws, even the Chicago quantity theory of money , are absolutely true . Even Newton's laws, which were considered “ absolutely true ” by many physicists in the nineteenth century, have been replaced by Einstein's laws. Thus there is an unavoidable uncertainty associated with laws in all areas of science, including economics. Inductive logic provides a quantification of this uncertainty by associating probabilities with laws and providing logically consistent procedures for changing these probabilities as new evidence arises . In this regard, probability is viewed as representing a degree of reasonable belief with the limiting values of zero being complete disbelief or disproof and of one being complete belief of proof.

For Jeffreys, Bayesian statistics is implied by his theory of scientific method. Thus, Bayesian statistics is the technology of inductive inference. The operations of Bayesian statistics enable us to make probability statements about parameters ’ values and future values of variables . Also, optimal point estimates and point predictions can be readily obtained by Bayesian methods. Probabilities and/or odds ratios relating to competing hypotheses or models can be evaluated which reflect initial information and sample information. Thus, many inference problems encountered in induction can be solved by Bayesian methods and these solutions are compatible with Jeffreys's theory of scientific method. See, e.g., Berry et al. (1996), Box and Tiao (1973), DeGroot (1970), Fienberg and Zellner (1975) and (Zellner, 1971) and (Zellner, 1979b) for presentations, discussions and applications of Bayesian methods.

To illustrate inductive inference in econometrics, consider Milton Friedman's Theory of the Consumption Function . In his book Friedman set forth a bold inductive generalization which, he showed, explained variation in much past data, a fact that increased most individuals ’ degree of reasonable belief in his theory. Further, Friedman proposed a number of additional tests of his model and predicted their outcomes, an example of what we referred to above as inductive inference . Many of these tests have been performed with results compatible with Friedman's predictions. Such results enhance the degree of reasonable belief that we have in Friedman's theory. This is the kind of research in economics and econometrics , which illustrates well the nature of inductive inference and is, in my opinion, most productive .

As regards inductive generalizations, there are a few points, which deserve to be emphasized. First, a useful starting point for inductive generalization in many instances is the proposition that all variation is considered random or nonsystematic unless shown otherwise . A good example of the fruitfulness of such a starting point is given by the random walk hypothesis for stock prices in stock market research. Many researchers have put forward models to forecast stock prices by use of variables such as auto sales, changes in money, and the like only to find that their forecasts are no better than those yielded by a random walk model. In other areas, when a researcher proposes a new effect, the burden is on him to show that data support the new effect . The initial hypothesis is thus, “ No effect unless shown otherwise . ”

#### ASSIGNING A NON-ZERO VALUE TO WEAK DATA ONLY PREVENTS FUTURE DEBATES OVER STRONG DATA BY REWARDING THE LAZY

Sterba 06

[Sonya K. Department of Psychology University of North Carolina at Chapel Hill Misconduct in the Analysis and Reporting of Data: Bridging Methodological and Ethical Agendas for Change ETHICS & BEHAVIOR, 16(4), 305–318 2006]

In conclusion, ethical and methodological specialists ’ gatekeeping efforts in the area of data analysis and reporting have remained strikingly disparate and insular to date. They neither coordinate with each other nor involve the research community in outreach efforts aimed at engendering self-monitoring. Their independent efforts have led to insufficient examination of the prevalence of overt and covert misconduct, and to inconsistent standards that are unreliably enforced. Yet the quality control of data analyses and reporting practices is of prime importance . Thus, I propose three tactics to improve the prevention, detection, and deterrence of analysis and reporting misconduct that each involve melding of the methodological and ethical arenas. First, psychologists need to better coordinate ethical and methodological standards pertaining to data analysis and reporting. Published methodological standards can lack the ethical imperative to motivate change, and published ethical standards can lack the specificity to direct that change. One first step toward coordinating standard setting across ethical and methodological specialties is offered here. Methodologists could be included on the committees of psychologists who create and revise research ethics codes and who respond to allegations of research ethics misconduct. In turn, committees disseminating methodological guidance, such as the APA Task Force on Statistical Inference, could include psychologists with research ethics expertise to aid in integrating an ethical perspective. Second, we need to increase applied researchers’ access to coordinated training in quantitative methods and research ethics. This will afford them the detailed methodological knowledge and the ethical imperative to better selfmonitor their own analysis and reporting. Specifically, a cross-fertilization of ethics and methods instruction needs to take place throughout undergraduate and graduate training , and also at the faculty level. Currently, statistical and methodological courses are typically devoid of research ethics discussions, and vice versa. In fact, these ethics courses and methods courses are typically offered in different departments, by faculty members who rarely interact. Faculty guest lectures from the companion discipline can begin to bridge these fields. In addition, short quantitative workshops (such as those offered by the Interuniversity Consortium for Political and Social Research) and ethics workshops (such as those sponsored by the APA Ethics Committee) are outlets for reaching researchers who may not have access to methodological or ethical specialists at their home institutions. (Neither the Interuniversity Consortium for Political and Social Research nor the Ethics Committee currently lists ethics in data analysis and reporting as a topic area covered in their educational outreach efforts.) It is essential that undergraduate and graduate psychology students be made mindful of the intersection of their methodological practices with ethical imperatives as they begin to conduct their own investigations—before poor habits become ingrained. We cannot expect students to completely autonomously make the connections between ethical and methodological imperatives; we need to scaffold them in this endeavor. This type of blended educational effort would increase the pool of journal and grant reviewers qualified to detect and enforce standards for analysis and reporting conduct. This, in turn, would render the field ’ s examination of data analysis and reporting practices more pervasive and more reliable . Third, psychologists need to more consistently implement strategies for preventing and deterring data analysis and reporting misconduct. Random auditing of analyses in articles submitted for peer review, and perhaps also systematic surveying of peer reviews themselves, are potential preventative deterrents (Kimmel, 1996). These deterrents would essentially be an expansion of the Code’s mandate to keep data available for potential reanalysis. If an audit of a given analysis reveals errors or discrepancies, the response would not be to try to determine whether this error was intentional or accidental. Instead, journal editors and reviewers would take it as their responsibility to inform authors of the ethical or methodological standards that were violated and issue a penalty—such as a request for reanalysis or replication—regardless of intent. This removes some of the professional hesitancy, fear of reprisals, and time involved in trying to prove intentional misconduct. This suggestion is in line with Snow’s (1959) argument that “if we do not penalize false statements made in error, we open up the way, don ’ t you see, for false statements by intention ” (quoted in Kimmel, 1996, p. 273).

#### **JUDGES CANNOT ACCURATELY ACCESS HIGH MAGNITUDE / LOW PROBABILITY EVENTS – WE USE A PROBIT MODEL USING SURVEY DATA FROM OF 95 STATE COURT JUDGES**

Viscusi 99

[W. Kip Harvard Law School “How Do Judges Think about Risk?” American Law and Economics Review VI N1/2 1999 (26- 62)]

The standard economic prescription for determining an efficient level of safety is to assess whether the benefits of the safety improvement exceed the cost. For continuous changes in safety, the question is whether safety levels have been increased until the marginal benefits just equal the marginal costs. These same kinds of principles form the foundation for law and economics interpretation of negligence rules as well.8

Judges considered one of three survey questions designed to test the degree to which they would apply the principles embodied in this standard negligence test. The cost of the safety improvement in every instance was $2,000 . In addition, the expected benefits of the safety improvement, which equal the reduction in the risk probability multiplied by the size of the loss, equaled $ 1,500 in every instance . Thus, applying the negligence rule as cast in law and economics terms would suggest that the safety measure was not efficient and that the firm should not be held liable for the repair.

The three experimental manipulations varied the probability of the accident and the size of the loss but held constant the expected value of the loss that would be prevented by undertaking the $2,000 repair. In the first instance, judges considered a property damage loss of $15,000 cou¬pled with a risk probability of 1/10 that would be eliminated through the safety repair. The expected loss is consequently $1,500, which is less than the repair cost. The second variant increased the size of the property dam¬age by a factor of 100 to $1.5 million, reducing the probability of loss by a factor of 100 to equal 1/1,000, leaving the expected loss unchanged at $1,500. The third variant increased the size of the loss to $1.5 billion, which included the value of personal injury losses, and accompanied it with a probability of the loss of 1/1,000,000. Thus, this change scaled losses up by a factor of 1,000 and scaled the probability down by a fac¬tor of 1,000, leaving the expected loss unchanged. For the personal injury question, the lives lost were valued at $5 million per life, and respondents were told that this amount would reflect the full social value of the loss. In every instance, the survey indicated that the company had sufficient resources to pay the damages.9

An example of one of these questions (the intermediate case) is the following:

You are CEO of Rocky Mountain Airline. The cargo door on the plane does not operate properly. Fixing it costs $2,000. If it is not fixed, there is absolutely no safety risk. Very reliable engineering estimates indicate that there is only a 1/1,000 chance over the expected life of the plane that there will be a total loss to your company of $1.5 million due to property damage caused by this problem. Thus, there is a 999/1,000 chance that there will be no damage whatsoever. Your com¬pany has no insurance but does have sufficient resources to pay these damages. 1°

Respondents were then asked to circle whether the firm should under¬take the repair and second, if the repair is not undertaken and there was $1.5 million in property damages, to indicate whether punitive damages should be awarded.

How one views the scenario depends in part on the test being applied. The chief executive officer (CEO) of the company should presumably be concerned with profit maximization. The safety measures described involved financial effects that would all be internalized by the firm. Since safety improvements fail a benefit-cost test, they would not enhance firm profitability. Judges responding as CEOs might, however, impute a loss in the value of the company's reputation in the event of an accident involving personal injury, making them more likely to advocate safety improvements in this instance.

Application of legal rules should not be affected by broadly based reputational effects. If a safety measure does not pass a benefit-cost test, the company should not be found negligent for failing to adopt it. Punitive damages pertain to situations of reckless behavior. To be reckless, not only must the foregone safety measure pass a benefit-cost test but presumably there should be a wide spread between benefits and costs, a repeated failure by the company to adopt safe practices, or other considerations that make the company truly reckless and not simply negligent. In none of the three scenarios is there any basis for awarding punitive damages. Indeed, by construction the company will never be negligent for failing to adopt the safety improvement. Table 5 summarizes the responses to the two questions for each of the risk scenarios. In the case of the low property damage amount, 68% of the judges would not undertake the repair, which is consistent with economic efficiency principles. Almost a third of the sample would undertake the repair even though the cost of the repair was below the expected benefits.

The attitude toward punitive damages in this low loss case shown in Panel A of Table 5 differs moderately, depending on whether repairing the plane to prevent a $15,000 loss is attractive. In each case, a minor¬ity of the judges believe that punitive damages would apply if the repair was not undertaken and a loss occurred, where the fraction favoring puni¬tive damages is greater for those who chose to repair the plane. What is perhaps most striking is that three of the judges who did not believe that the plane should be repaired nevertheless would have awarded punitive damages had the plane not been repaired and a loss was suffered. For the entire group, 18% of the judges would award punitive damages, which is not in line with economic efficiency principles, since not only are punitive damages not warranted but based on a negligence test the repairs should not even be undertaken.

Panel B of Table 5 indicates how the responses change if the stakes are increased by a factor of 100 and the probability of damages is reduced by a factor of 100. Judges in this instance are almost evenly divided as to whether the plane should be repaired. Respondents who did not indi¬cate that repairing the plane was worthwhile almost unanimously opposed punitive damages, whereas for the respondents who favored repairing the plane there was an equal division between those who supported punitive damages and those who did not.

The final variation in Panel C increases the loss to $1.5 billion, which includes the value of personal injuries, where the survey indicated that this damages amount is intended to reflect the full social cost of the acci¬dent. As before, the expected loss is $1,500, but the responses differ quite starkly from those in the previous scenarios. Respondents are now unanimous that the plane should be repaired . Moreover, more than two-thirds of the respondents supported punitive damages in this instance. What appears to be most consequential is that, in situations involving personal injury, there is a much greater willingness to undertake repairs and impose punitive damages than in situations involving property damage even though the expected economic losses are the same in each instance. The results in Panel C for both the award of punitive damages and repair¬ing the plane differ to a

statistically significant degree from the results in Panels

Table 6 refines this analysis using probit regressions for the determinant of the probability that the respondent will indicate that the cargo door should be repaired and that punitive damages should apply. The level of damages does not have a significant effect on the cargo door repair decision. What does matter is the nonmonetary character of the loss, which was sufficiently influential that these respondents could not be included in the repair equation. There was no variation in this scenario group, as all respondents in the personal injury variant favored repairing the cargo door. The implicit value of life measures and the risk perception measures are not statistically significant, except for one instance. Respondents who had higher values of the perception equations slope coefficient Si were less likely to undertake the cargo door repair. Increased values of 13, indicate that the respondents' assessed probabilities were closer to the 45° line and thus tended to reflect the actual risk level more accurately. Thus, accurate risk beliefs and lower biases in risk perceptions are associated with judges being more willing to act according to efficiency norms with respect to the cargo repair decision. A priori the role of this variable is not clear, since higher values of Si could indicate more alarmist responses to risk in that perceived risks respond more quickly to changes in actual risks. Since all Si values were below 1.0, however, in this case the variable seems to better reflect the accuracy of risk judgments.

This variable is not, however, directly influential in the punitive damages decision, as the only statistically significant variables here are the level of expected damages and whether the judge believes that repairing the cargo door was worthwhile. Thus, to the extent that the risk perception slope variable matters, it is indirectly in that it increases the probability that the respondent will want to repair the cargo door, which in turn increases the probability that the respondent believes that punitive damages should apply. Overall, however, it seems that perceptional biases and the respondent's own implicit values of life do not play a central role in how they would address the negligence issue or the punitive damages issue in this instance. Attitudes toward the underlying repair decision and the size of the accident loss are the primary factors of consequence . An attractive aspect of this finding is that personal preferences and perceptional biases do not greatly affect negligence judgments. However, the size of the stakes ideally should not matter, since the expected losses (i.e., probability x damage) is the same in every instance .

Although personal risk perception biases and risk valuations do not appear to be instrumental, the results are not entirely favorable with respect to the soundness of judicial decisions. In terms of the overall responses to the scenarios, judges were evenly divided between repairing and not repairing the plane, even though strict application of economic negligence rules would indicate that not repairing the plane was desirable. Moreover, even though the firm was not negligent in these examples, many judges believe that punitive damages were applicable, particularly when nonmonetary losses are high. Awarding punitive damages when a firm meets a negligence standard is certainly inappropriate, as it indicates a failure to reflect on the underlying benefit-cost tradeoffs, particularly when there are large nonmonetary stakes.

This result is a sobering message for companies faced with risk-cost calculations . If these companies follow the urgings of judicial scholars such as Judge Frank Easterbrook and attempt to think systematically about the risks and costs of their action, then even if they make the correct economic decision it is possible that they will risk punitive damages, par¬ticularly when nonmonetary consequences are involved. 11 In the General Motors (GM) truck side impact case, GM had calculated the cost of the safety improvement and concluded that these costs were not outweighed by the expected safety benefits. 12 This analysis paralleled the approach taken for the Ford Pinto. These analyses undervalued the personal injury loss by considering only the prospective court awards and not also the implicit value of life and health. Even if the calculations had been done correctly and had generated the result that the safety improvements were not worthwhile on an economic basis, however, then it is quite possible that the company would nevertheless have been found liable for puni¬tive damages. The company had confronted the risk decision with explicit probabilities of risk, clear potential for adverse health effects, and a level of costs that would not have jeopardized the solvency of the company. If companies cannot rely on economic efficiency prescriptions or negligence rules for determining the level of safety after such an analysis, then there may be no safe harbor other than the zero-risk level, which is infeasible.

CARD CONTINUES

The two key questions explored in this article were whether these aspects of individual preferences in valuation affected attitudes in judi¬cial contexts and whether decisions in these contexts exhibited forms of irrationality that have been identified in the literature. Judges' application of negligence rules became much more out of line with standard law and economic prescriptions once substantial nonpecuniary damages were involved . Large stakes — small probability catastrophic events seemed to pose greater problems for judicial decision making than did higher probability — lower loss events . The potential for such errors and the large costs of error in terms of incorrect major penalties highlight the potential benefits of judicial review for such large stakes cases.

CARD CONTINUES

This article will examine the responses by a sample of 95 state judges to a written survey about risk decisions. Although reliance on the results of a questionnaire may not capture the particular biases that are most influential in actual judicial decisions, it does provide a structured frame¬work for exploring a wider range of issues than can be examined using case data. The judges in the sample were participants in the law and econ omics program s offered by the University of Kansas Law and Orga¬nizational Economics Center. The judges were sent these written surveys before the program began and returned the surveys before participating in the program, where the survey formed the basis for class discussion. The response rate was close to 100 %. The sample consisted of program participants in two different sessions, both of which took place in 1997 . Although the meetings were in Copper Mountain, Colorado, and Sanibel, Florida, participants in the program were from state courts throughout the country. The participants included many judges from state courts of appeals, state superior courts, and state supreme courts. The experience base of the sample consequently is likely to be greater than that of the average state court judge.



