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JOINT CROWDOUT: AN EMPIRICAL STUDY OF THE IMPACT OF FEDERAL GRANTS ON STATE GOVERNMENT EXPENDITURES AND CHARITABLE DONATIONS

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ABSTRACT

We estimate the effect of exogenous federal expenditure cutbacks on state social service expenditures and on charitable donations. In the process, we also estimate tax and income effects and explore the impact of community environment and "need" variables. Data consist of a unique three-year panel of aggregate itemized giving by state and income class and government expenditures by state. Our results confirm the 'flypaper effect' of federal grants on state spending and show statistically significant but partial crowdout of charitable donations. The flypaper effects appears to dominate the crowdout of donations, so that federal grants are especially productive of overall social service expenditures. Finally, we find that the state's poverty rate is a particularly strong and positive determinant of charitable giving.

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Many social services in the United States are provided by both governments and private nonprofit organizations. Direct federal provision of social services is relatively small, although indirect federal funding of social services is substantial. Instead of direct provision, federal grants are provided to states, and to a lesser extent to nonprofit organizations, to provide social services. State governments supplement this federal spending on social services with state tax revenues. Nonprofit organizations supplement those funds they receive through federal grants and governmental "contract" purchases of services by soliciting donations[1].

The interaction between federal, state, and nonprofit activities has not been empirically estimated. Researchers have examined two cases of "simple crowdout": the effects of federal grants on state government spending[2] and the effect of total government spending, federal and state, on donations[3]. Earlier attempts at estimating the complete set of interactions among these providers have faced severe data limitations[4].

The objective of this paper is to remedy this void. We merge data on individual giving, tax price, and income with data on state spending, federal aid, and demographic characteristics. We are therefore able to move beyond the existing work in this area and estimate a complete model of the interaction among the providers of social services. This permits us to estimate the likely effect of changes in federal grants on total social service provision.

Social service spending within a state comes from three sources: federal grants, state and local taxation,

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[1]See Musselwhite and Salamon (1987) for a statistical breakdown.

[2]See, for example, Craig and Inman (1986) and the references contained within.

[3]See Abrams and Schmitz (1978 and 1984), Reece (1979), Pacque (1982), Amos (1982), Jones (1983), Steinberg (1985a), Kingma (1989).

[4]In unpublished works, Steinberg (1983, 1984) attempted to estimate the full set of relationships using allocations to local United Ways as a proxy for local donations. The adequacy of this proxy remains unclear. However, preliminary estimates obtained from the National Survey of Philanthropy by Schiff and Steinberg (1988) show much more promise.

and private donations. The ultimate effect, including feedbacks, of an exogenous one-dollar increase in federal grants on spending by state and local governments and by private charities is known as "joint crowdout." The extent of joint crowdout is crucial for evaluating the efficacy of federal grants for social services.

Consider three possible ranges of values for the extent of joint crowdout: less than or equal to -1, between -1 and zero, and greater than zero. If joint crowdout is less than or equal to -1, then an additional dollar of federal grants reduces state, local, and nonprofit expenditures by at least one dollar. Additional federal grants are therefore ineffective at best (if joint crowdout is exactly -1), and may be counterproductive (if joint crowdout is less than -1).

On the other hand, if joint crowdout is greater than zero, then additional federal expenditures are especially effective at providing social services. In this case, the additional federal money will be matched by increased spending by states and donors. Each additional dollar of federal spending will produce more than one dollar of additional social services.

If the joint crowdout parameter is between zero and -1, then it can be said that "partial" crowdout occurs. That is, additional federal spending will reduce state and donor spending by some fraction of the added federal effort. In this case, additional federal spending will increase total social service provision, but by less than the amount of added expenditures. Thus, the effectiveness of federal grants is reduced, but not eliminated.

The empirical estimates of joint crowdout will also help resolve debates regarding other mechanisms for social service provision. Most notably, further light will be shed on the tax expenditure debate, recently joined by Roberts (1987) and Andreoni (1989), of whether it is more efficient for the government to subsidize donations through a tax deduction or to make equivalent expenditures directly. These results could also broaden the analysis of the effect of tax reform on charitable donations (Steinberg, 1986), beyond the direct impacts on price and income simulated by Lindsey (1986, 1987b), among others.

In order to estimate the joint crowdout effect, we rely on a three-year panel of giving and government spending in each of the 50 states. Most previous studies of the issue lacked information on the location of the donor, and so were unable to discern the effects of spatially-varying factors such as state government spending and community characteristics. Schiff (1985), Abrams and Schmitz (1984) and Hochman and Rodgers (1973) had locational data, but were restricted to a single cross-section. This reduced their ability (relative to a panel data set) to account for possible excluded variable bias. Wilson (1983) employed a panel of city-level data from

the consumer expenditure surveys. Unfortunately, these surveys combined charitable and personal gifts, such as Christmas presents, when reporting geographically disaggregated data.

Section II of this paper summarizes the existing literature on simple and joint crowdout. The data and empirical specifications are described in Section III. Section IV presents and discusses our estimates of the parameters of the model. Section V presents a summary.

II) The Theories of Crowdout

The empirical work on the interaction of federal, state, and private charitable provision of social services has been limited to the problem of simple crowdout. This crowdout takes two forms: the effect of aggregate government spending on aggregate donations (simple donative crowdout), and the effect of federal spending on state spending (simple government crowdout). We consider each in turn.

Simple Donative Crowdout. In many cases, social services are funded both by private donations and by government spending. These sources of funding are unlikely to be independent. Stated explicitly, simple crowdout is the derivative of aggregate donations with respect to government spending, after allowing for equilibrating adjustments (mutatis mutandis)[5]. A derivative between zero and negative one indicates partial crowdout. Total crowdout is indicated by a derivative of negative one with "super crowdout" occurring when the derivative is less then negative one. Negative crowdout, or "crowdin" occurs in the case of a positive derivative.

Donor motivation determines whether crowdout will be partial, total, super, or negative. Consider first the case of a pure public good in which the only motivation for private donors is to increase the aggregate level provided. In this model, government expenditures and the contributions of others are a perfect substitute for the individual's own giving. Warr (1982) and Roberts (1984) have shown that if the set of private donors is fixed,

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[5]By mutatis mutandus we mean to allow equilibrating adjustments between donors. Thus, in computing the response of each donor, we do not hold constant the giving of other donors. Rather, we compare changes in aggregate donations in Nash equilibrium. There is further ambiguity in the literature, as some authors examine the response of equilibrium to a balanced-budget change in governmental expenditures (appropriate for examining the effect of a change in the overall level of government expenditures), while others hold tax rates constant (appropriate for examining the effect of a change in the targeting of federal grants across communities). Our simple crowdout estimates in this study conform to the latter concept, as federal grants are regarded as retargeted. Our joint crowdout estimates blend both concepts: federal tax rates in a community are held constant with respect to grants to that community, but state taxes are assumed to vary in balanced-budget fashion with induced changes in state government expenditures.

there will be total simple crowdout for balanced-budget changes in government expenditures. This conclusion follows from the perfect substitutability, in the Hicksian or compensated sense, of government expenditure for private donations. Bergstrom, Blume, and Varian (1986) showed that if the set of donors is endogenous, crowdout may be partial, though even in this case, crowdout will be total if there is sufficient overlap between the sets of donors to competing causes (Bernheim, 1986). Much less can be said a priori for mixed public goods. These goods deliver utility from the act of giving which is distinct from the utility obtained from the level of provision of the good. For mixed public goods, donors would regard government expenditures and the giving of others as imperfect substitutes, or possibly even complements, for their own gift. Mixed public goods are commonly associated with donations because a private good is either jointly produced or consumed with the public good financed by the donation. Joint consumption goods include prestige, admiration, job advancement, the warm glow of doing right, or the lukewarm glow of following the crowd[6]. Joint production goods include newsletters or front-row seats provided by recipient organizations as a fundraising strategy (Posnett and Sandler, 1986).

An a priori case for the extent of crowdout is difficult to make. Cornes and Sandler (1984), Schiff (1985) and Steinberg (1987) showed that this is the case even when the giving of others is a substitute for one's own gift and all other goods are normal. The reasons for the ambiguity include the possibility of an anomalous income effect for some donors and possible feedback effects among all donors. Given a particular tax regime and level of donations by others, each donor has a most-preferred level of government spending on a particular activity. Beyond this point, an increase in government spending, accompanied by the reaction of other donors to this change, causes the donor's utility to fall. This is because the value of the increase in the provision level is outweighed by the value of foregone consumption caused by the accompanying tax increase. Thus, incremental government expenditures will lower the real income of donors. If donations are a normal good (as is likely), then added government expenditures will produce crowding out due to an income effect. This would be on top of the crowding out due to the substitution effect. Super crowdout becomes a possibility.

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[6]More detailed analyses of these motivations for giving can be found in Tullock (1966), Ireland and Johnson (1970), Arrow (1974), Long (1976), Margolis (1981), Rose-Ackerman (1982), Sugden (1984), and Andreoni (1989).

On the other hand, some donors may view the public good as underprovided. These donors obtain an increase in real income from added government expenditures. The income effect and substitution effects would work in opposite directions, leading to an ambiguous overall result for crowding out. Crowdout would be negative if the income effect outweighed the substitution effect; zero, if the two effects were equal; or partial if the substitution effect outweighed the income effect. In spite of this theoretical ambiguity, Steinberg (1987) argues that partial crowdout should be the most common.

Rose-Ackerman (1981) outlined other factors which would produce negative crowdout (crowdin). Government grants sometimes require private matching and so may stimulate giving. Alternatively, donors may view receipt of grants as a signal that the organization is meritorious or efficient. Finally, grants may be accompanied by strings which affect the ideology or output mix of the nonprofit organization. The changes wrought may make the nonprofit more attractive to potential donors. Rose-Ackerman (1987) pointed out the reverse possibility: that governmental grants can free an ideological nonprofit organization from the sorts of compromises necessary to attract donations.

Government Crowdout. The second type of simple crowdout, simple government crowdout, refers to the effects of exogenous changes in federal intergovernmental grants on state spending. The level of private activity in these studies is assumed to be constant, usually at a level of zero. The theoretical analysis of the sign and magnitude of the crowdout parameter depends both on the model of political decisionmaking used and on the form of the grant. We consider each in turn.

The dominant model of political decision making has been the median voter model[7], first proposed by Hotelling (1929) and formally developed by Bowen (1943). In this model, the collective decision reflects the preferences of the swing or median voter, so that half of the remaining voters want more and half less of the publicly-provided good. A change in the level of federal aid, or in the price of providing incremental state services will only affect the outcome if the budget set facing the median voter is changed, or if the ranking of voters is changed in such a way that the identity (and hence preferences) of the median voter is altered.

Controversy over the median voter model arises in two areas: doubt that equilibrium exists or can be

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[7]This is not the only model. Lindsey (1987a) looks at variations which incorporate the likelihood of voting. Romer and Rosenthal (1979), Filimon, Romer and Rosenthal (1982), and Craig and Inman (1986) employ various models of games between bureaucrats or politicians and voters. In a model specific to the crowdout problem, Roberts (1984) employs Stigler and Peltzman's model of vote-maximizing bureaucrats.

characterized as the most-preferred point of the median-preference voter, and questions about the empirical procedures commonly employed to identify the median voter and test the theory. There are many reasons to doubt whether representative governments will accurately reflect voter preferences. Rules controlling the election process, the setting of the legislative agenda, and the process of coalition formation, may well produce a different outcome than that preferred by a majority of the voters.

Some of these theoretical difficulties are less likely to arise in the present case of one-dimensional choice (that is, the choice of a single public-expenditure level from ${}^{+}\mathbf{R}^{1}$). If the preferences of voters are single-peaked, issues of agenda setting and coalition formation become irrelevant. Rules which limit voting may change the identity of the median voter, but do not change the characterization of equilibrium. On the other hand, although we model the problem as a one-dimensional choice, coalitions may form to trade off social service expenditures against some other dimension of political choice in the real world.

Even if a unique equilibrium exists at the most-preferred point of the median voter, there is some question whether this equilibrium will ever be implemented. Voters are commonly offered a limited range of choices in any one election, and the sequence of referenda may not include the equilibrium point. Those given control over the agenda can rig results in their interest. For example, Romer and Rosenthal (1978) showed that when a single alternative is offered to a low level of status-quo expenditure, voters who prefer a modestly lower amount of spending than that proposed would support the referendum. Thus, the outcome would support a higher level of spending than that desired by the median voter. On the other hand, vote-maximizing politicians have an incentive to select positions preferred by the median voter, either directly, or through their offerings of referenda.

The problem of identifying the median-preference voter can be quite difficult. In the absence of direct measures of preferences, it is common to identify the median-preference voter as the median-income citizen in the community. This identification is strictly valid only under very restrictive assumptions on the income distribution in the community and the price and income elasticities of demand for housing (if a property tax is used) (Bergstrom and Goodman, 1973). Moreover, the process of voting has different costs and benefits for different citizens, and some may systematically abstain. Rubinfeld (1980) reported differences between voters and non-voters in local school elections. An earlier paper by Rubinfeld (1977) found that renters were less likely to vote than were homeowners. Thus, the voter with the median income among <u>all citizens</u> in a community is

unlikely to be the median among voters.

Once the median voter is identified, there are problems in specifying his or her budget set. Ladd (1975) showed that the existence of a tax base other than residential real estate might produce a higher level of taxation. The existence of commercial and industrial property in the local tax base opens the possibility that the tax will be shifted forward in the form of higher prices, and not borne by the local residents.

Lindsey (1987a) investigated a number of possible alternatives to the median voter model. In one alternative, he considered a planning model in which the preferences of the median voter were neglected. Instead, planners, faced with an array of prices for raising revenues from different sources (due to differential incidence and federal deductibility), pick a least-cost (to the state) solution for each expenditure level. In this case, a complete set of prices of different options becomes a better description of the budget set faced by decision makers. Alternatively, plurality-maximizing politicians may care about non-median citizens as long as they are likely to vote. In this case, a weighted-average budget set of all of the voters in the state provides the best measure of the actual budget set faced by decision makers. The weighted-average approach may also provide a good approximation when the median-voter model fails for other reasons.

Lindsey's empirical tests found that the weighted average price model dominated both the traditional median-voter model and the cost-minimization model in explaining state decision making. In the present study, we employ both the weighted average price model and a new variant of the median price described below.

The Nature of Government Grants. In addition to the issue of political decisionmaking, the effect of government grants on state spending depends on the nature of the grant given. The simplest case to analyze is general, non-matching assistance such as revenue sharing which the state may spend as it chooses. This type of grant does not alter the price of incremental spending faced by any individual voter, but does provide increased purchasing power. Hence, regardless of the identity of the decisive voter, the effect on political equilibrium should be identical to the effect of an increase in this voter's income.

It would seem that non-matching grants which are targeted to specific programs should have much the same effect as general grants, for there is no way of determining what the state would have spent on the particular service in the absence of the grant. Federal requirements are met as long as post-grant state expenditure on the project exceeds the amount of the grant. Any funds which are freed may be spent on other projects or on tax relief. The targeted grant would have only income effects as long as pre-grant state

expenditures on the targeted program were at least as great as the grant. If pre-grant state spending were less than the amount of the grant, then the difference between the grant and the pre-grant level of spending would be committed to the targeted project, and the income effect would be limited to the pre-grant level of state spending.

Thus, theory implies that most of the effect of non-matching grants would involve a simple income effect. This income effect should, in theory, be widely diffused among the various projects supported by the state, and on tax reductions. One would expect a very small effect on incremental expenditures from nonmatching grants.

However, most empirical studies find this not to be the case. In these studies, estimated incremental spending on the targeted project is many times the level implied by the estimated income elasticity of spending. This has been dubbed the "flypaper effect" as money seems to stick where the targeted grant is placed. Moffit (1984) ascribed this anomaly to nonlinearities in the marginal voter's budget set. Hamilton (1983) suggested it was the result of the underlying technology of public goods production, for example, scale economies. Craig and Inman (1986) suggested that non-median voter models were the explanation, with benefitting interest groups lobbying against reductions in state spending following the receipt of federal aid.

Regardless, the received wisdom suggests that general non-matching grants will decrease state spending from its own resources (partial simple government crowdout) while increasing total spending. Received wisdom is less clear about the effect of targeted non-matching grants, which may produce simple government crowdin.

Matching grants, if open-ended, should have substitution effects as well as income effects as they will alter the relative prices faced by the voters. However, many matching grants have caps on the amount which will be matched. If the cap is not binding, then both income and substitution effects result. If the cap is binding, then the marginal price is unaltered and there should be no substitution effect. Nonetheless, inframarginal matching will result in increases in "virtual income" (Burtless and Hausman, 1978) and hence cause income effects.

Joint Crowdout. The combined effects of exogenous federal changes on state spending and donor spending, allowing for feedbacks between the latter two, is known as joint crowdout. The existence of these feedbacks indicates that joint crowdout is not simply the sum of simple crowdout and simple government crowdout. The only theoretical treatment of joint crowdout is Steinberg (1987), who relied on a decisive (not necessarily median) voter model in which the voters are cognizant of private donations and of simple crowdout,

and vote accordingly. In this model, each individual regards the donations of others, state spending, and federal spending as perfect substitutes for one another. However, the individual's donations may involve private benefits, and so is regarded as different from the other sources of spending. Furthermore, the individual gift is considered as atomistic in the total level of spending. The individual takes these factors into account both in voting for additional state spending, and in contributing.

The effect of these assumptions is to make the price of state spending on a particular good proportional to unity divided by unity plus the simple crowdout parameter. If there is total crowdout, the price of buying additional goods with state spending is viewed as infinite, as an additional dollar of state spending will not produce any additional net spending on the good[8]. Thus, such goods would only be provided by state governments when donations are zero. On the other hand, goods with crowdin effects represent very good buys from the view of the voter, so state level provision of these goods would be much higher, ceteris paribus.

Steinberg (1987) showed that joint crowdout can be partial, zero, or negative in political-economic equilibrium. For joint crowdout to be partial, it is sufficient that giving by all individuals be a normal good, voter choice sets are convex and have interior equilibria, and simple crowdout is non-negative. As these conditions seem not only plausible, but likely, partial joint crowdout would be the most common result. As noted above, if simple crowdout is total, then either government or donor activity must be zero, and joint crowdout is undefined. A similar result would occur if simple crowdout falls in the "super crowdout" range.

Finally, if simple crowdout is negative, joint crowdout may be partial, zero, or negative. State government spending will fall in response to a non-matching grant, though total spending, including the grant, will rise. If the added donor spending exceeds the decline in state spending, then negative joint crowdout exists. Partial or zero crowdout would exist if the response of individual donors is small relative to simple governmental crowdout.

III) Empirical Specification and Data

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[8]The marginal price would fall to the Bergstrom-Goodman level when governmental expenditures increase to the point where they drive donations to zero. This implies that the voter choice set is not convex, with the usual complications. Fortunately, important nonconvexities are less likely when simple donative crowdout is partial.

Specification. The preceding theoretical analysis provides the basis for specifying the empirical model. We assume that federal intergovernmental grants to each state are exogenous[9], and determine the likely ultimate impact of these grants on donations and on state governmental expenditures. We estimate several variants of the following reduced form:

$$\left.\begin{matrix} CHAR_{ijt} \\ S_{jt} \end{matrix}\right\} = f(PCHAR_{ijt}, PCHAR_{jt}, PS_{jt}, Y_{ijt}, Y_{jt}, TARG_{jt}, GEN_{jt}, DEM_j)$$

where:

i indicates an income group
j indicates state of residence
t indicates year
CHAR is donations per itemizer
S measures state social service expenditures per capita
PCHAR is the price of donations; if subscript i is omitted, this is the weighted price across income groups.
PS is the price of state spending
Y measures donor income; if subscript i is omitted, this is the average per-capita income across income groups.
TARG is per-capita federal grants targeted for social services
GEN is per-capita nontargeted federal grants
DEM is a vector of demographic and taste variables

The level of aggregation varies among variables and equations, and, indeed, we have varied the number of equations across specifications. Many variables are aggregates for states in particular years, such as per-capita federal targeted transfers to Ohio in 1980. Others are further subdivided by income class, such as per-itemizer gifts by donors in Ohio with Adjusted Gross Income between $40,000 and $50,000 in 1980. The latter disaggregation permits calculation of price and income elasticities which are the most commonly studied determinants of giving.

[9]More precisely, nonmatching grants are assumed to be exogenous. The level of matching grants is endogenous, but the matching rate is assumed to be exogenous.

However, every income group in a particular state responds to the same federal transfer payment and state demographic characteristics, so these variables are not disaggregated[10]. Though these variables do not vary with income class, variation across time (for the federal grant variables) and states (for both demographic and grant variables) enables us to estimate their importance. The state government spending equation is entirely aggregated across income classes as the dependent variable, state spending, does not vary with income class.

All our estimates utilize the log-log functional form, resulting in elasticity estimates for all continuous variables. Our basic model (henceforth referred to as the two-equation model) estimates the two equations above by ordinary least squares (OLS). Estimation by the seemingly unrelated regression technique (SUR) is preferred, for better estimates of standard errors can be obtained from integrating the information contained in the cross-equation correlation of errors, but SUR is problematic in this case. The CHAR equation employs 900 observations (6 income groups times 50 states times 3 years), while the S equation employs only 150 (50 states times 3 years). This problem suggested our second basic specification, the seven-equation model. In this specification, we estimate six separate equations for the dependent variable CHAR, one for each income class. Each equation employs 150 observations, and the six CHAR equations are estimated simultaneously with the S equation by SUR.

Theory implies that the giving of each person depends on the giving of others in the state and on state spending. In reduced-form estimation, this implies that the exogenous determinants of giving by others and state spending belong in the donations equations. Consequently, we include a weighted sum[11] of prices facing different income groups as well as the price facing the donor's income group. Similarly, we include per-capita income in

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[10]Indeed, the demographic variables are not even disaggregated by year, though this reflects data limitations rather than proper estimation technique. To the extent this induces measurement error, the coefficients on the demographic variables will be biased (generally toward zero). Note also that because these variables vary across states but not across time or income group, their coefficients will be confounded with any state-specific intercepts, complicating the interpretation. See the discussion below under 'data'.

[11]Prices for itemizers in each of the six income groups are weighted by the estimated number of itemizers in that category in the state population. Nonitemizers face a price of unity (see below), and they are added in. The result is divided by the estimated number of taxpaying units in the state.

We also tried including the entire vector of prices and incomes across income groups to explain giving in each income group. Although this procedure produced interesting estimates for the state spending equation, it produced apparent nonsense for the giving equations. Presumably this is because the functional form forced the elasticity of giving with respect to the price of, say, the average donor in the first income group to be the same for donors in the first and second income groups, thus losing the distinction between own- and cross-price. Detailed results are available on request from the authors.

the donor's state as well as the average per-capita income in the donor's income class.

In other variations, we vary the definition of the dependent state-spending variable, and modify our measure of PS, the price appropriate for determining state spending. These variations are described in greater detail in the section on data below.

Data. In order to estimate the model, three sets of data are required: on state and local spending, demographic conditions, and tax and charitable giving status. Each set of data required a different source and the resulting estimates were performed on a merged data base containing information on all sources.

Income, giving, and tax variables were obtained using the National Bureau of Economic Research TAXSIM model. This computerized model of the U.S. personal income tax is designed to process the Individual Tax Model File Public Use Samples prepared by the Internal Revenue Service. Each Public Use Sample contains detailed information from a stratified random sample of tax returns. In 1979, this sample contained 173,359 returns. The sample sizes for 1980 and 1981 were 171,391 and 144,205 respectively. TAXSIM converts the raw data on each taxpayer in the Public Use Sample into information on the income, tax liability, and marginal tax rates of the taxpayer population as a whole.

Donations. Special tabulations using the NBER TAXSIM program provided data on charitable giving for each state by income class. We limited our analysis to the 50 states, omitting taxpayers who filed in the District of Columbia or who lived abroad. Within each state, taxpayers were divided into six income classes based on their Adjusted Gross Income (AGI): under $10,000, $10,000-$20,000, $20,000-$30,000, $30,000-$40,000 $40,000-$50,000 and over $50,000. In order to protect the confidentiality of taxpayers, the Internal Revenue Service does not provide the state of residence for taxpayers earning over $200,000. These taxpayers comprise less than 0.2 percent of the taxpayer population, and were omitted from the sample. Though these taxpayers were rare in the period studied, they contributed a disproportionate amount of all gifts, 10.5 percent, 10.8 percent, and 11.3 percent in the three sample years. Thus, we are likely to underestimate the amount of crowdout.

The tax model file contains information on the charitable giving of each taxpayer who elected to itemize deductions rather than use the zero bracket amount. Our measure of giving, CHAR, includes claimed gifts of both cash and property. This property is usually in the form of securities such as stocks and bonds, real estate, or works of art. The actual deduction taken by the taxpayer is limited to 50 percent of adjusted gross income

(AGI) in the case of cash gifts and 30 percent of AGI for gifts of property. Donations in excess of these limitations are usually deducted in future years in the form of a carryforward deduction.

Government Spending. Data on state and local spending and federal support for that spending were gathered from the Commerce Department's Government Finances annual series. In order to test for possible differences in the importance of various types of spending on donor behavior, two state spending variables were considered. S1 represents per-capita state and local spending on elementary and secondary education, higher education, welfare, health and hospitals, housing and urban renewal, and natural resources, parks, and recreation. This broadest measure of state spending includes virtually all facets of state activity which may complement, or supplement, private donor activity. S2 represents per-capita state and local spending on all of the above except for housing and urban renewal and natural resources, parks, and recreation.

Federal support for state and local spending was divided into two categories - grants targeted to social programs likely to be related to donations and to state social service expenditures (TARG), and general (GEN) grants, including both nontargeted grants and grants which are targeted for purposes other than social programs. Our measure of TARG is "federal support for education and welfare expenditures," as reported in the Government Finances series, expressed in per-capita terms[12]. Open-ended matching grants were removed from TARG in order to maintain the exogeneity of this measure[13]. The effect of federal matching provisions is incorporated in our measure of the voter-price of state spending. GEN consists of "total federal transfers to states" (per-capita) minus TARG and minus open-ended matching grants.

Prices. Federal tax deductibility lowers the net-of-tax cost of contributing to charity, as itemizers receive a tax refund which is proportional to their donations. In effect, this net-of-tax cost of providing a dollar to the charity is the price of donating (PCHAR). State and local taxes are also deductible, lowering the effective price of state spending for a typical voter. These considerations require calculating four prices for each taxpayer.

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[12]In order to obtain per-capita figures, we used Census data on state populations on July 1, 1979 and July 1, 1981. The figure for July 1, 1980 was obtained by averaging the 1979 and 1981 figures because official figures for the 1980 Census are based on an April 1 enumeration.

[13]The two federal open-ended matching programs during this period were Aid to Families with Dependent Children (AFDC) and Medicaid. States could elect to receive AFDC grants under one of two formulae - a close-ended one, or the open-ended formula used for Medicaid. In 1979, all but four states chose the latter formula (Missouri, South Carolina, Arizona, and Texas were the exceptions). In 1980 and 1981, only Arizona and Texas remained in the close-ended program. For these states and years, AFDC was not subtracted out in generating TARG.

PCASH represents the price of making a gift of money. This price depends upon the taxpayer's federal and state tax rate, as well as on the eligibility of the taxpayer to deduct the charitable contribution at the federal and state level. As a general rule, PCASH can be represented as:

$$PCASH = 1 - FRATE \quad FDED - SRATE \quad SDED + FRATE \quad SRATE \quad SDED \quad FDED$$

where:

FRATE is the taxpayer's federal marginal tax rate
FDED represents federal itemizer status, and is unity for itemizers, zero for others
SRATE is the taxpayer's marginal state rate
SDED represents the deductibility of charitable gifts at the state level. This figure would be unity only if the taxpayer itemized on the state tax return and if charitable giving were an itemizable deduction in that state. Otherwise, the value of SDED would be zero.

The final term in the above equation represents the deductibility of state taxes at the federal level. If a charitable gift is deductible at the state and federal level, some of the combined effect of lower state and federal taxes is offset because the lower state tax figure will mean a lower level of itemized deductions at the federal level, thus increasing federal taxes.

In some states, the interaction between charitable giving, state taxes and federal taxes is more complex. For example, Minnesota allows federal income taxes to be deducted at the state level. TAXSIM computes the actual price for each taxpayer by evaluating the effect of charitable giving on both federal and state taxes, accounting fully for such idiosyncrasies in state tax codes.

There are two generally recognized and interrelated problems in calculating the price of donations - exact sample dependence of price on income, and endogeneity of price. The first problem is common in cross-section data sets that lack information on state taxes. Because all taxpayers in a cross-section face the same federal tax code, price can only vary when taxable income varies sufficiently to push taxpayers into different tax brackets. In turn, taxable income can only vary if gross income varies or if deductions and credits vary.

If the cause is of such variation is variation in deductible charitable donations, then our price measure is endogenous. If the cause is variation in income, then there is no sample variation in price after controlling for income (which we must do to disentangle the price from the income elasticity). If the cause is variation in other deductions or credits, then any variation in the price of giving is due to variation in those taste and

demographic variables which lead taxpayers to select differing levels of other deductions. In this case there is no independent sample variation in price after controlling for taste differences (which we must do to estimate donative demand curves properly). Thus, price elasticity cannot be estimated.

That problem does not infect the present data set, for variation in state tax schedules over space and variation in state and/or federal tax schedules over time provide exogenous and independent variation in the price of giving. However, the second problem, the endogeneity of price, remains. Several solutions to this problem have been implemented in the literature. The most common solution, following Feldstein (1975), is to employ an exogenous proxy for price denoted the "first-dollar price". The first-dollar price uses the marginal tax rate the donor would face if he or she contributed only one dollar, rather than the price they actually face (which can be made endogenously larger by a sufficiently large donation). This is the solution we adopt here, although we take it one step further and calculate the marginal tax rate appropriate when both donations and state tax payments are zero. We take this extra step because, in our expanded model, deductible state tax payments are also an endogenous source of variation in the price of giving[14].

Feenberg (1987) proposed a related solution. He employed data on state tax rates to break the sample dependence of price on income, and used a first-dollar measure of price. Because this proxy is correlated with the "true" marginal price relevant for donor decisionmaking, but not with the error term, he employed first-dollar price as an instrumental variable, not as a regular control variable (as had been the practice before Feenberg)[15]. The ideal solution was implemented by Reece and Zieschang (1985). They developed a maximum likelihood routine that takes account of all of the nonlinearities in the donor budget set directly[16].

PASSET is the price of making a gift of property. Most of the value of such gifts is in the form of

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[14]We should note, however, that choice of the price variable made little qualitative difference to our results (see the discussion in the section on price elasticities below). Evidently, the sort of endogeneity bias resulting from use of last-dollar price is much less important with the aggregate data we employ than with the microdata employed by some other authors.

[15]In results not reported here, we tried the same strategy with less success. Evidently, the quality of the instrument is such that you need many more observations than we had available to obtain estimates with small standard errors. Feenberg employed a much larger data set containing observations on individual donors, and still had relatively fuzzy estimates. Also, the instrument has a different mean than true price, as discussed below in the section on prices.

[16]This maximum likelihood routine is difficult and expensive to estimate, and the advantage over first-dollar price, while important for microdata, is unlikely to be large for the aggregate data employed here. Thus, we have eschewed this option.

appreciated property -- property which has increased in value since the time the taxpayer purchased it. By giving property to a charitable organization, the taxpayer not only receives a deduction, but also avoids the capital gains tax on the appreciated portion of the gift. Thus, PASSET depends not only on the federal and state ordinary rates and deductibility status, but also on the federal and state capital gains tax rates[17]. As a general rule, PASSET is:

$$PASSET = PCASH - APPC \quad (FCGRATE + SCGRATE) + APPC \quad SCGRATE \quad FDED \quad FRATE$$

where:

APPC is the appreciated portion of a gift of property
FCGRATE and SCGRATE are the federal and state capital gains tax rates, respectively
all other variables are previously defined.

PCHAR represents a weighted-average price of giving both cash and assets. The weights of cash and assets in total giving reflect the shares of cash and assets in total giving for the taxpayer's income group. For each of these prices, the price assigned to an income group within a state represented the average price of all taxpayers in that income group in that state, rather than the price that would be faced by a taxpayer with the average characteristics in that income group.

A final measure of price, PS, was constructed to represent the relevant price of state spending facing voters in the state. Three factors determine PS: the federal tax consequences of state spending, open-ended matching provisions in federal grants, and the political model underlying state decision-making[18]. The first factor

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[17]Since the tax file does not contain data on the amount of appreciation in the value of contributions, the appreciated portion of a gift of property was assigned using a Monte Carlo process and data from a survey of taxpayers regarding their gifts of appreciated property. The survey of donors to a major charitable organization gathered income and gift information from a total of 14,000 donors. That data formed the underlying distribution to which taxpayers in the Individual Tax Model File were matched by TAXSIM. Regrettably, results of the survey remain confidential.

[18]A fourth factor, the extent to which the decisive voter is able to shift tax consequences of his decision onto others (including other personal taxpayers, corporate owners facing corporate taxes, and citizens of other states paying excise taxes on exports) is neglected here. If states vary in the extent of tax shifting, our price coefficient will suffer from measurement error bias. It would be quite difficult to improve our measure here, as even in simpler settings tax incidence remains controversial. Further, the way in which tax shifting affects equilibrium depends upon the political model underlying decisionmaking.

A fifth factor, the effect of simple donative crowdout, is also neglected. As noted earlier, donative crowdout increases the price to the decisive voter of obtaining an increase in social service expenditures via increased state spending. However, if the crowdout parameter is identical across states and time (and, indeed, our estimating procedure assumes that it is), this omission should bias only the size of the price coefficient, and

is straightforward: PS generally is unity reduced by the decisive voter's marginal federal tax rate if an itemizer, unity otherwise. Again, the calculation is more complex for some states owing to deductibility of federal from state taxes.

Federal open-ended matching programs reduce this price fractionally. In effect, matching allows the decisive taxpayer to shift a fraction of the costs onto the residents of other states. We incorporate matching by taking the weighted average of the price above and that price divided by unity plus the matching rate[19]. The weights reflect the share of the state's matched expenditures in the measure of overall state spending, and were appropriately different for our two measures of state spending (S1 and S2). Close-ended matching grants have no effect on the marginal price of state spending as long as the cap is binding (which it always is), and so are neglected[20].

Because we remain agnostic on the political model underlying state decisionmaking, we calculate the relevant marginal tax rate two different ways, resulting in two different measures of price. The first way produces the variable PSMEAN by employing a weighted average of tax rates in the state. The second way produces the variable PSMED by employing a weighted median of tax rates in the state. In both cases, we employ our modified first-dollar tax rate - the marginal tax rate applying if the taxpayer contributed only one dollar and paid only one dollar in state taxes.

For both these measures, the weights are the same. First, we weight married couples filing jointly as representing two potential voters, not one. Second, we weight each taxpaying unit to reflect the probability that members of that unit will vote. Statistics on voter participation rates by age and income class were obtained from exit poll information following the 1984 election. These statistics were applied to the tax returns to compute a new sample weight for each taxpayer based on the estimated likelihood that the filer will vote[21]. Regardless of the true political model, abstaining voters are likely to have less importance in determining political outcomes,

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not its statistical significance.

[19]See footnote 13 for details of the calculation of matching rates.

[20]As noted earlier, close-ended grants lower the inframarginal price, resulting in an increase in "virtual income." Because we do not incorporate this effect, our measure of income (not price) is flawed.

[21]The exact procedure follows that of Lindsey (1987a), who developed the PSMEAN measure but did not apply his weights to calculate a PSMED measure of voter price.

but the appropriateness of PSMEAN versus PSMED depends upon the true political model.

Income. As in the calculation of prices, disposable income should be regarded as endogenous in our formulation. The reason is that the endogenous decisions on state spending and personal donation affect state and federal taxes, hence after-tax income. Our solution is the same as for prices. We calculate federal taxes owed if donations and state spending were zero, and subtract this from adjusted gross income. This measure avoids endogeneity bias but introduces measurement bias, as before. However, in results not reported here, we obtained very similar estimates using other income measures (including after-federal-tax income, after-federal-and-state-tax income, and a traditional first-dollar after-tax income measure in the absence of donations but not state spending).

In reduced form, income of others should matter (as determinants of state spending and of aggregate donations) as well as income-class specific income. Following the pattern for prices, we include a variable measuring others' income, but unlike before, we do not need to construct a weighted average of our income variables. Instead, we simply used state per-capita income as reported in Statistical Abstract of the United States.

Demographic Variables. To isolate the effect of crowdout, we must remove the influence of confounding taste differences between states. Control for interstate variations can be accomplished either by including separate dummy variables for the states in the regression equations or by using demographic variables which are likely to explain the variation of tastes across states. Part of what makes Vermont's tastes different from Georgia's may be the greenness of the mountains or the redness of the clay soil, while most is probably due to demographic differences. Because we obtained only a single cross-section of demographic variables, the coefficients on these variables reflect state-specific latent variables as well as demographic effects. The use of demographic variables therefore incorporates the effects of state dummies, but places more structure on the variation in state-specific intercepts and utilizes more information to estimate this structure[22].

We therefore use a series of variables designed to measure the demographic conditions of each state. Data on these demographic factors were obtained from the 1980 Census. The number of pupils per 100 inhabitants, denoted PUPILS, was used to control for demographic differences in the need for state and local

[22]We also explored the traditional dummy-variable specification for individual effects. We found this specification less compelling for the reasons specified in the text. Nonetheless, we discuss price elasticities from this procedure below. Full results are available from the authors.

education spending. Higher levels of population of school age should raise demand for education spending[23]. MILEAGE, or road mileage per capita, was included to control for the possible effects of population dispersion on state spending. To the extent scale economies exist in social service provision, higher values of mileage are likely to raise the level of social service spending as any given quantity of provision is likely to cost more.

NONWHITE measures the percent of the population which was classified as nonwhite in the Census. This variable could determine both the taste of donors and taxpayers (as nonwhites may have a different propensity to give for cultural reasons) and the perceived need for social services (as nonwhites may be perceived as having special affirmative action needs beyond those associated with poverty, or, conversely, if the electorate is less willing to support social services when many of the recipients are nonwhite). We cannot identify these separate effects in our reduced form, but can remove the confounding effects of both upon crowdout estimates by including this variable.

URBAN measures the percent of population living in urban areas, again using the Census definition. POVERTY reflects the percent of the population living in poverty. Because these variables reflect characteristics of both the donor and recipient population, interpretation of their coefficients is as problematic as for the variable NONWHITE.

HOMEOWN, which measures the fraction of the state's households that own their own homes, is likely to be negatively related to the demand for social services. Homeowners have been shown to be more sensitive to increases in property taxes than have renters.

While any of the demographic variables may be correlated with the error term in a time series, this study employs only a single cross-section for these variables. While this introduces the possibility of measurement error, it allows us to take these variables as predetermined when explaining donations or state spending. Finally, one might object that some of these variables seem likely to explain state expenditures but not donations. This is irrelevant in reduced-form estimation, for, as state spending partly determines donations, any determinants of state spending can have impact on donations.

In results not reported here, we also included dummy variables to represent the year in which the observation was collected. These two dummy variables were never significant (singly or in combination) in any

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[23]To the extent that endogenous state expenditures determine the dropout rate, the coefficient on this variable may suffer from endogeneity bias.

equation, so we do not report any regressions containing these variables.

IV) Results

In general, we present results for eight specifications. Models one through four explain the broader measure of state spending, S1 whereas models four through eight explain S2. Models one, two, five, and six employ two equations (one explaining total giving and one explaining state spending), and models three, four, seven, and eight employ seven equations (six explaining giving in each income class and one explaining state spending). Finally, models one, three, five, and seven utilize a weighted median measure for the price of state spending (PSMED), while the remaining models use a weighted mean measure (PSMEAN).

The models are all reduced forms, and so include the same explanatory variables (PCHAR, PS, Y, TARG, GEN, PUPILS, MILEAGE, NONWHITE, URBAN, POVERTY, AND HOMEOWN), although two details vary. Most obviously, the price of state spending depends upon the model, and varies (across PS1MED, PS1MEAN, PS2MED, AND PS2MEAN) as indicated in the previous paragraph, with the same measure employed in all equations within each model. A second disparity results from the differing levels of aggregation of the dependent variables. The distinction between average price of donations within an income group and average price of donations across all income groups is meaningless for the state spending equation, as the dependent variable is not income-class specific. The situation is similar for donor income. Thus, only the aggregate price of donations and income variables are included in the state spending equations.

It is difficult to say which is the best model. Of course, no meaningful statistical comparison can be made between the S1 and S2 models, which simply explain different dependent variables. One can test for the superiority of the seven-equation over two-equation specifications using a slight variation in the specification to nest the latter in the former[24]. F-tests strongly reject the nesting constraints, arguing for the superiority of the seven-equation specifications. On the other hand, the coefficients on own-price of donations are far more

[24]The variation is necessary because of the differing levels of aggregation and the inclusion of the variables reflecting average price of donations in other income groups and average income in other income groups in each donation equation. If we exclude these variables from both the two- and seven-equation models, then the two-equation model is the special case where the coefficients of all right-hand variables in the six donations equations are identical across equations. One can reject this constraint at better than .001.

plausible in the two-equation models (see the discussion below), with significant wrong signs in all equations. Thus, we report both types of models. Evidence is also mixed for the comparison of the two measures of the price of state spending. In the seven-equation models, the median price outperformed the mean price as measured by system R^2 (.5874 versus .5680 for S1, .5632 versus .5478 for S2). In the two-equation models, the median price again did better as measured by corrected R^2 for the state spending equations (.8401 versus .8232 for S1, .8156 versus .8002 for S2), although the mean price of state spending had a slight edge in the contributions equations (.7050 versus .7025 for both PS1 and PS2). Overall, models three and seven are slightly preferred over the alternatives.

Crowdout. In table I, we report our estimates for joint crowdout of donations and state spending by targeted federal grants. We report corresponding estimates for general federal grants in table II. These estimates are derivatives[25], not elasticities, and indicate the dollar change in the indicated category of spending caused by a one-dollar increase in federal grants. Equivalently, the estimates indicate the decrease in the indicated expenditure category caused by a one-dollar decrease in federal grants, as our estimating technique imposes symmetry of response to increases and cutbacks.

Both the sign and magnitude of joint crowdout of donations by targeted grants varied across the specifications, with the preferred specifications indicating that a one-dollar federal grant increase ultimately results in a 4.6 cent donative decrease (significant at the 6% level using a two-tailed test). This sort of partial crowdout is evidenced in all four seven-equation models, but crowdout appears numerically and statistically less significant when the mean-price measures are employed. In contrast, the two-equation models imply partial crowdin, with federal grants resulting in donative increases of up to 6.5 cents (also significant at the 6% level for the median-price model, insignificant for mean-price).

In common with other studies, state spending seems to increase by more than the federal targeted grant. Because targeted grants are administratively spent by the states, we have subtracted one from the derivative of state spending with respect to targeted grants to obtain the change in "own-financed S" reported in table I. Thus, our preferred estimates indicate that a targeted federal grant of one dollar will increase S1 by about $1.28 above

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[25]To convert our coefficients to derivatives, we multiplied by the average ratio of donations or state spending to the indicated type of grant. In so doing, we first converted donations per itemizer into compatible terms with federal spending per capita. Standard errors were computed the same way, and should be regarded as conditional on the sample.

Table I: Joint Crowdout Estimates for Targeted Federal Grants

Model Number	1	2	3	4	5	6	7	8
Number of Equations	2	2	7	7	2	2	7	7
Measure of S	S1	S1	S1	S1	S2	S2	S2	S2
Measure of PS	med	mean	med	mean	med	mean	med	mean
change in:								
C1			-.0017 (.0038)	-.0009 (.0042)			-.0019 (.0038)	-.0009 (.0042)
C2			-.0317 (.0091)	-.0291 (.0098)			-.0317 (.0091)	-.0291 (.0096)
C3			.0045 (.0144)	.0119 (.0147)			.0044 (.0144)	.0120 (.0147)
C4			-.0201 (.0098)	-.0169 (.0102)			-.0201 (.0098)	-.0169 (.0102)
C5			.0054 (.0081)	.0117 (.0086)			.0054 (.0081)	.0117 (.0086)
C6			-.0020 (.0117)	-.0018 (.0126)			-.0019 (.0117)	-.0017 (.0126)
Total C	.0652 (.0353)	.0241 (.0383)	-.0456 (.0246)	-.0251 (.0258)	.0652 (.0354)	.0241 (.0383)	-.0458 (.0246)	-.0250 (.0258)
Own-Financed S	1.2766 (.2361)	1.3413 (.2731)	1.2766 (.2361)	1.3413 (.2731)	1.1908 (.2229)	1.1829 (.2552)	1.1908 (.2229)	1.1829 (.2552)
Total Spending	2.3418 (.2387)	2.3654 (.2758)	2.2310 (.2374)	2.3162 (.2743)	2.2560 (.2257)	2.2070 (.2581)	2.1450 (.2243)	2.1579 (.2565)

Standard errors are in parentheses below parameter estimates.
Ci represents total contributions in income group i.

and beyond the grant ($1.19 for S2). This pattern holds across all eight models.

Overall, targeted federal grants appear to be especially productive in increasing social service expenditures, with the total of joint crowdouts (adding back the 1 to include federal-financed S) leading to a multiplier greater than 2 in all cases. In the preferred specification for S1, the $1.28 increase in own-financed state expenditures more than makes up for the 4.6 cent decrease in donations, leading to an overall increase of $2.23 per targeted federal dollar.

A similar pattern emerges for general federal grants. The preferred specifications indicate numerically and statistically smaller partial crowdout of donations of about 2.1 cents per dollar. Unlike before, the sign of the donative crowdout derivative does not vary across specifications, and most specifications indicate numerically and statistically greater crowdout. For example, model 1 estimates 6.2 cents of crowdout per dollar (significant at better than 5%).

The distinction between own-financed and federal-financed state spending is not meaningful for general grants, which, by construction, are not targeted towards the appropriate state spending measures. Our preferred estimate for S1 indicates that a one-dollar federal general grant increase results in an increase in state social-service expenditures of about $1.46, so that total spending increases by about $1.44. This general pattern is repeated in all specifications, with the state spending derivative ranging from $1.15 to $1.61. Thus, even nontargeted federal grants appear to be especially productive in generating social-service expenditures, despite the partial crowdout of donations. General federal grants appear less productive than targeted grants, but both are surprisingly productive.

Joint crowdout is directly revealed by reduced-form estimation (since we do not include any variables to hold state spending constant in the donations equation or to hold donations constant in the state spending equation), but simple crowdout can be inferred from reduced-form estimates[26]. We estimate the simple crowdout of donations by targeted federal grants at -.0139 for the preferred S1 specification. That is, we infer that if state spending had not been allowed to respond to the change in federal grants, donations would

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[26]In effect, we estimate the parameters of the structural model $S = a + b(D + F)$, $D = c + d(S + F)$ by indirect least squares. The parameters b and d are estimates of the simple crowdout of state spending and donations, respectively, and can be identified as quotients of reduced-form parameters. Because the structural coefficients are a nonlinear function of reduced-form estimates, the usual problem complicates calculation of standard errors for the structural model.

Table II: Joint Crowdout Estimates for General Federal Grants

Model Number	1	2	3	4	5	6	7	8
Number of Equations	2	2	7	7	2	2	7	7
Measure of S	S1	S1	S1	S1	S2	S2	S2	S2
Measure of PS	med	mean	med	mean	med	mean	med	mean
change in:								
C1			-.0000 (.0029)	-.0002 (.0030)			-.0000 (.0029)	-.0002 (.0030)
C2			.0011 (.0068)	-.0025 (.0069)			.0011 (.0068)	-.0025 (.0069)
C3			-.0034 (.0110)	-.0110 (.0107)			-.0033 (.0110)	-.0110 (.0107)
C4			-.0079 (.0076)	-.0097 (.0076)			-.0079 (.0075)	-.0097 (.0076)
C5			-.0052 (.0066)	-.0078 (.0066)			-.0053 (.0066)	-.0078 (.0066)
C6			-.0055 (.0089)	-.0089 (.0093)			-.0055 (.0089)	-.0089 (.0093)
Total C	-.0619 (.0281)	-.0379 (.0286)	-.0209 (.0183)	-.0402 (.0190)	-.0619 (.0281)	-.0379 (.0286)	-.0208 (.0188)	-.0402 (.0190)
S	1.4621 (.1882)	1.6097 (.2028)	1.4621 (.1882)	1.6097 (.2028)	1.1512 (.1783)	1.3103 (.1901)	1.1512 (.1783)	1.3103 (.1901)
Total Spending	1.4002 (.1903)	1.5718 (.2048)	1.4412 (.1891)	1.5695 (.2037)	1.0893 (.1805)	1.2724 (.1922)	1.1304 (.1793)	1.2701 (.1910)

Standard errors are in parentheses beneath parameter estimates.
Ci represents total contributions in income group i

have fallen by only 1.4 cents instead of 4.6 cents. Joint crowdout is so much larger than simple crowdout because state spending moves in the same direction as federal grants, so that donations are responding to a $2.28 increase in government spending when there is a $1 increase in federal targeted grants. In contrast, simple crowdout of state spending is larger than joint crowdout. We estimate that if donations had not been allowed to fall by 4.6 cents, then own-financed state spending (S1) would have risen by $1.34 instead of $1.28. The same patterns apply for the S2 specifications and for general federal grants. For example, simple crowdout of donations by general federal grants is estimated at -0.6 cents and simple crowdout of state spending at $1.49 in model 3.

Is Charity a Giffengood? Clearly, the answer one expects is "no," and yet our preferred specification yields paradoxical results. When we estimate any of the two-equation models, the coefficient on the log of own-price is negative, statistically significant, and numerically similar to the bulk of published estimates[27]. Surprisingly, when we estimate any of the seven-equation models, we obtain coefficients on the log of price which are always positive and statistically significant in five of the six giving equations[28], indicating a significant "wrong sign." In table III, we report these estimated price elasticities from the eight main specifications and several additional alternatives.

The basic difference between the two- and seven-equation specifications is that the former exploit between-income-group price variation as well as within-group, while the latter employs only within-group variation. If within-group variation were small relative to between-group, we would be less able to detect the influence of price in the seven-equation models and would expect statistical insignificance. However, this would

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[27]There appears to be a rough consensus that giving is price elastic, with most point estimates of elasticity around -1.2 (Clotfelter, 1985). However, the evidence from panel data sets seems to call the consensus into question. Clotfelter himself obtained a much lower elasticity in his (1980) panel study when he employed a first-difference specification to remove the confounding influences of latent explanatory variables. His cross-section elasticity estimates of -1.40 fell to -0.33 and were not statistically significantly different from zero. Although he attributed much of this decrease to lagged effects, he did not estimate lags in a first-difference framework, so his estimates may be biased. Broman (1986) estimated a similar model from a different panel, and found that slow adjustment accounted for only a small fraction of the decline in estimated elasticity when employing first differences. In preliminary results, Daniel (1988) estimated a fixed-effects covariance model (basically a multi-year extension of first-differencing) and obtained an estimated price elasticity of -0.03, insignificantly different from zero. In summary, recent evidence leads one to question whether donations respond at all to the tax-price of giving, but there is no other evidence that donations are a giffengood. See the discussion in Slemrod and Shobe (1988).

[28]One wag suggested that we really ought to be doing a one-tailed test on the price coefficient, so that our perverse results indicate that price is insignificant, rather than significant with the wrong sign. While correct from both a technical and marketing standpoint, that suggestion doesn't make our price coefficients any less of a puzzle.

Table III: Estimated Own-Price Elasticities for Donations

		Equation for:					
	C	C1	C2	C3	C4	C5	C6
Model #							
Two-Equation Models							
1	-2.72*** (0.20)						
2	-2.85*** (0.20)						
5	-2.72*** (0.20)						
6	-2.85*** (0.20)						
1D	-3.61*** (0.30)						
Seven-Equation Models							
3		2.91** (1.29)	2.84*** (0.76)	1.40** (0.70)	1.51*** (0.40)	0.83** (0.38)	0.29 (0.31)
4		2.90** (1.29)	2.41*** (0.87)	2.10*** (0.71)	1.47*** (0.49)	1.04** (0.40)	0.07 (0.33)
7		2.91** (1.29)	2.84*** (0.76)	1.44** (0.69)	1.51*** (0.40)	0.83** (0.39)	0.29 (0.31)
8		2.90** (1.29)	2.41*** (0.87)	2.12*** (0.71)	1.48*** (0.49)	1.05** (0.40)	0.07 (0.33)
9		1.75 (1.35)	-1.34 (0.85)	-0.22 (0.52)	-0.09 (0.42)	0.62* (0.37)	0.41 (0.30)
10		2.36 (1.43)	-1.28 (0.82)	-0.19 (0.57)	0.20 (0.53)	0.69 (0.44)	0.62** (0.26)
3A		0.44 (0.38)	0.05 (0.29)	0.43 (0.41)	0.13 (0.51)	2.21*** (0.75)	0.46 (0.36)
3B		1.95 (1.41)	3.09*** (0.99)	2.35** (0.90)	0.98 (0.62)	1.77*** (0.49)	-1.36*** (0.46)
3C		2.72** (1.28)	1.69** (0.67)	0.48 (0.54)	0.99*** (0.35)	0.83** (0.36)	0.41 (0.28)
3D		4.97** (2.44)	2.69** (1.26)	0.78 (1.37)	1.34* (0.69)	0.41 (0.65)	0.72 (0.49)
3E	0.94*** (0.21)						

Key: *significant at 10%; **significant at 5%; ***significant at 1%; all using a two-tailed test.
Standard errors are in parentheses beneath parameter estimates.
Ci indicates that the elasticity is for giving in income group i. C without a subscript indicates the parameter was constrained to be equal across i.

not explain significant wrong signs. Thus, we tried several variations. Models 9, 10, and 3C report experiments with variable deletion. The only right-hand variables in 9 are own-price and the intercept; the only right-hand variables in 10 are own-price, own-income, and the intercept. Model 3C is identical to model 3 except for the exclusion of the variable representing average price of giving (across income groups) in the state. All three were estimated by OLS. Most of the estimates from model 9 are statistically insignificant, but there are still three wrong signs (one significant). Similarly, there was only one statistically significant wrong sign in model 10, but three other estimates had the wrong sign. All of the elasticities had the wrong sign (four significantly) in model 3C.

In model 3B, we substituted dummy variables for each state and year for the time-invariant demographic variables and estimated by SUR. We could not include both sets of variables in the same equation because, as discussed previously, the time-invariant demographic controls are equivalent to a structure imposed on state-specific dummy variables. The elasticity estimate for the richest income group looks "nice" for this specification, but all the other estimates had the wrong sign (three significantly).

Model 3D was identical to 3, but we replaced our observations on giving in each state, year, and income group with observations on the average giving across time in each state and income group. We tried this specification because variation in price within income groups may be more sensitive to transitory fluctuations than price across income groups. For contrast, we estimated model 1D, the equivalent two-equation specification. Once again, the elasticity estimate looked good (if a little high) in the two-equation case, but bad in the seven-equation case (with all signs wrong, three significantly).

Model 3E was identical to 3, but a restriction was placed forcing the coefficient on own-price to be identical across the six giving equations. This restriction was rejected (at nearly the .01 level), but nonetheless did little to explain the wrong sign, which remains highly significant for all equations.

As indicated earlier, we employ a first-dollar price of giving to eliminate possible endogeneity bias. However, this proxy for the true marginal price is biased. Under a progressive tax system, the marginal price is always greater than or equal to the first-dollar price, with the size of the gap between the two proportional to the size of one's donation. This imparts a bias on estimated price elasticity when the true elasticity is negative which operates in the right direction to explain our current paradoxical results. That is, the estimated slope of the donation demand curve will be greater than the true slope (in a numeric, not absolute-

value sense), and a sign flip is logically possible. However, the size of this bias is likely to be small for the current tax code and range of observed giving, as few donors push themselves into very different tax brackets solely because of the size of their donations, and our data aggregates away some of this bias. Nonetheless, to test this explanation, we estimated model 3A, which is like model 3 but substitutes a marginal own-price for the first-dollar price of giving. Unfortunately, all signs remained wrong in model 3A, with one significantly wrong. Although we are trading off one bias (due to the systematically different mean of the proxy) for another (due to endogeneity of price), the results from model 3A do not encourage us to believe we have found the full explanation for the perverse signs.

Cross-price Elasticities. We estimate two cross-price elasticities in the giving equations - one for the gifts of others in the state, and one for the price of state spending. We estimate one cross-price elasticity in the state spending equations, measuring the effect on state expenditures of the average price of donations in the state.

The cross-price of others' giving had a positive sign in all of the two-equation specifications, and was statistically significant at the .05 level in models 2 and 6. This result suggests that giving by others is, in some sense, a substitute for one's own giving. However, this cross-price had a generally negative sign in the seven-equation specifications. For example, in model 3, all cross-price elasticities were negative, significantly so for the lower four income groups. Since specification 3 is mildly preferred, the evidence is slightly stronger for a 'bandwagon effect,' in which giving by others is a complement to one's own gift.

The cross-price of state spending on giving was negative for all two-equation models (significantly so for the mean-price measure), and generally positive in the seven-equation models. For example, this cross-price elasticity was positive for five out of six income groups, significantly so for three groups in model 3. It was positive for all income groups (significantly for two) in model four. Again, the evidence is mixed, with slightly stronger evidence favoring the substitutability of state spending for giving in donor preferences. This cross-price result is also consistent with the generally negative estimates of the simple crowdout parameter.

The cross-price of donations in the state spending equation was negative and significant at the .01 level in all eight specifications, with point estimates clustered around -2.7. Thus, although the cross-price elasticity of state spending in the donations equations suggest (mildly) that the goods are substitutes, the cross-price elasticity of donations in the state spending equation suggests that the goods are complements.

The asymmetry in estimates here is not too troubling, as there are quantity constraints on donor/voters which imply complex income effects which are not fully removed by inclusion of our income variables.

Income Elasticities. Two income elasticities are estimated for each giving equation - one for own income, and one for average income in the state. Only the latter elasticity was estimated for the state spending equations.

Giving emerges as a normal but inelastic good. Point estimates for the own-income elasticity of giving ranged from 0.21 to 0.24 in the two equation models, with standard errors of 0.04. The pattern is similar (if more chaotic) for the seven-equation models, with generally larger point estimates and a couple of insignificant wrong signs.

The cross-income elasticity provides further evidence on the relation between own gifts and gifts of others, for a higher level of others' income results in a higher level of others' giving. However, this effect is confounded by considerations of need. A higher level of others' income may indicate less poverty, reducing the need for contributions of any sort. To some extent, the latter effect is removed by inclusion of the poverty rate as a control variable, but the number of 'near-poor' may be correlated with per capita state income. Our estimates for this parameter are almost always negative, but only rarely significant at the 10% level. The sign is consistent with both explanations if giving of others is a substitute for own giving.

State spending also emerges as a normal but inelastic good. Point estimates for the income elasticity of state spending cluster tightly around 0.25 for the median-price equations (significant at .01), and 0.13 for the mean-price equations (significant at .10). Again, the income elasticity may be confounded with need-based state social service expenditures, but inclusion of the poverty rate as a control variable reduces this difficulty.

Demographic Variables. One striking pattern crosses all specifications - the poverty rate is a positive determinant of donations, and a negative determinant of state spending. A similar relation between poverty and giving has been seen only inconsistently in previous studies (see Clotfelter (1985)). The semi-logarithmic formulation for this variable implies that a one percentage-point increase in the poverty rate causes a three percent increase in giving (under the two-equation specifications) and between a one and six percent increase in giving by various income groups (with most estimates between four and six percent) under the seven-equation specifications. The coefficient is statistically significant at the .01 level for all but the poorest

income group. In contrast, a one percentage-point increase in poverty appears to cause a decrease in state social service spending of about one tenth of one percent in all specifications (also significant at .01). The latter effect is consistent with a fear of in-migration of the poor if the state is too generous with its social service expenditures (Smith, 1988), but that fear does not appear to inhibit private donations.

Further evidence supporting need-related giving is provided by the coefficient on PUPILS. Gifts are positively related to PUPILS in all estimates, and this effect is significant for all two-equation specifications and for most income groups in the seven-equation models. This variable also has a significant positive coefficient in the state spending equations.

Giving also appears to respond positively (at the .01 level) to the percent of population living in urban areas, another plausible measure of need. In contrast, the positive coefficient on this variable in the state spending equations was never statistically significant.

The coefficient on NONWHITE was negative and significant for aggregate giving. In the seven-equation models, this variable was only significant for the wealthiest income class. As indicated earlier, it is unclear whether the coefficient on NONWHITE reveals more about the effect of the recipient's race or the effect of the donor's race on giving, as we have no variables directly measuring the donor's race. NONWHITE was insignificant in the state spending equations.

MILEAGE had a generally insignificant effect on giving, and a strong positive effect on state spending. Finally, HOMEOWN had a positive effect on both giving and state spending, generally significant. The sign in the state spending equation is contrary to expectations, as homeowners have been found to be more sensitive to increases in property taxes than renters. The result for both equations may partly be the result of a permanent income effect, or may reflect taste differences or a different environment.

V) Conclusions

The efficacy of federal grants depends, in large part, upon the reactions of voters and donors. While expenditure levels do not tell the whole story, federal grants would not appear useful if each federal increase was matched by a decrease in the combination of state government and private nonprofit support for social services. In this paper, we provide pioneering estimates of the parameters of this "joint crowdout".

Combining information from IRS Public Use Samples of Individual Income Tax Returns with information on government spending and demography, we develop a three-year panel of average giving in each of six income groups and fifty states and state spending and federal grants in each state. We divide federal grants into those grants specifically designed to be spent on social services (targeted grants) and those not so designed (general grants). We then estimate a reduced form in which contributions and state spending each depend upon federal grants, income, prices, and demographic variables.

Our results indicate that federal grants of either sort are quite productive of social service expenditures. Despite crowding out an estimated 4.6 cents of donations, a marginal dollar of targeted grants results in an estimated $1.28 increase in state spending from its own resources, so that total expenditures increase by $2.23. A general grant is less productive, but still results in impressive expenditure increases. We find that a marginal dollar of general federal grants results in a 2 cent decrease in donations, a $1.46 increase in state spending, for a total expenditure increase of $1.44. Although these state expenditure increases appear implausibly large, they are consistent with the results of other studies which have found the so-called flypaper effect (federal money sticks where it hits).

These induced expenditure changes are in part a reaction to the federal grant and in part a feedback reaction between state spending and donations. We find that if state spending were somehow held constant, a marginal targeted federal grant would only result in a 1.4 cent decrease in donations. Similarly, if donations were somehow held constant, we estimate that own-financed state government spending would rise by $1.34.

We employ two measures of the price of incremental state spending. One represents the mean, and the other the median after-federal-tax cost of a dollar of state income taxes to the average voter in each state. In both cases, we take account of the number of voters in each taxpaying unit and the likelihood that each unit will vote, based on exit poll data. The median likely-voter price measure appears to be superior on statistical grounds, though evidence on this point is equivocal.

Giving appears to be quite sensitive to measures of "need" such as the poverty rate, the number of pupils per 100 inhabitants, and the percent of population living in urban areas. We find that a one percentage-point increase in the poverty rate in a state will result in an estimated three percent increase in giving. In contrast, state spending on social services is lower in states with higher poverty rates.

The importance of crowdout estimates to public policy is such that it would be useful to turn to a variety of other data sources and specifications. Our current data restricts us to considering crowdout of donations by tax itemizers with adjusted gross income under $200,000. More could be learned from survey data. In particular, crowdout of non-monetary donations (volunteer labor and in-kind gifts) may prove important. Federal grants may also interact with non-tax-financed state expenditures such as user fees. Finally, for-profit firms may respond to federal cutbacks by providing service on a fee basis.

One would also want to know more about the behavioral response of recipient nonprofit firms, which could only be learned from a survey of nonprofits (Schiff and Weisbrod, 1986, make a start here). Does the increase in donations following a federal cutback come "for free" to the nonprofits, or does it result from increased and expensive solicitation efforts which reduce the net productivity of donations? What share of incremental donations is allocated to incremental service provision? Ben-Ner and Van Hoomissen (1989) make a start here by examining crowdout of labor employed by nonprofit firms, perhaps a better proxy for output than is donations. One would also want to know about crowdout for more finely focused outputs - is crowdout of education spending different from crowdout of aid to the poor? Finally, one would want cross-national comparisons to fully assess the theory of federalism.

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