Does Nonprofit Law Matter? Evidence from the Enactment of UPMIFA

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[preliminary write-up of results only for NTA submission]

In this set of regressions, I examine the effects of UPMIFA enactment on nonprofit expenditures and portfolio allocation. I see the general framing of the project as an investigation into whether the rules governing nonprofit firm behavior actually affect firm outputs. Since few states allow private enforcement of the rules, and attorney general attention is rare, it would be reasonable to describe the rules as mostly symbolic or hortatory. Evidence that changes in rules actually affect key outputs of the firm would therefore be at least mildly surprising.

The main legal change studied here is UPMIFA, a model statute designed to loosen prior rules that tended to limit spending out of endowment by charitable corporations. UPMIFA was released in 2006 and first adopted by any state in 2007; by 2013 some version had been enacted in 48 states and the District of Columbia. Charitable trusts had already obtained similar reforms in the late 90's.

1. Data

Data derive from the National Center on Charitable Statistics Core-Private Foundations files. NCCS gathers its information from digitized tax returns filed annually by each registered firm. The "Core" files include select data on every firm that files. An alternative database, the "Statistics of Income" files, includes a wider variety of firm-level data that would potentially allow for additional hypothesis testing (in particular, the SOI data separately state donations and government grants, while the Core files lump the two together). However, the SOI is a randomly-drawn sample of firms, with oversampling of larger firms. To avoid the possibility of selection bias (for example, the chance that the treatment we are studying itself affects the likelihood of a firm-year observation even appearing in the data) or sampling error, I use the Core files, which are adequate for most of my purposes here.

In order to allow for identification of pre-UPMIFA trends, I include firm data going back to 2001, and extending forward to 2011. (Only firms with early fiscal years are included in 2011; I expect to add one or more years of data as it becomes available at the NCCS web site). Overall, this gives us 794,000 firm-years.

The NCCS files are notoriously error-filled. Unleveraged firms, in particular, have few incentives to report their data accurately. I first follow several of the "cleaning" protocols suggested in prior literature, such as in Heutel (2011) and Andreoni & Payne (2011). That is, I restate most negative balance sheet expenditures at their absolute value, and drop observations where category totals are less than the sum of their constituent parts (except in the case of total

revenues, where investment returns can be negative). I also drop 251 observations for which assets are listed as zero but adjacent years give values of $100,000 or more; since I use logs of the variables, regressions also omit observations with negative gross assets (about 1,000 observations).[1] In addition, I smooth out middle-different states, entity forms, and ntee classifications.[2] In other words, if a single firm lists their state of incorporation as NC, NV, NC over a three-year period, I change the middle value to NC. And I drop observations giving a negative value for interest or dividend revenue. The summary stats below reflect these changes.

Summary Statistics

	Total			Corps			Trusts		
Variable	**Obs**	**Mean**	**SD**	**Obs**	**Mean**	**SD**	**Obs**	**Mean**	**SD**
Grants Distrib.	272459	510949.3	1.66E+07	182806	520053.9	8666494	82518	512612.6	2.73E+07
Contribs. Rec'd	787789	441242.1	1.37E+07	527582	485181.3	1.11E+07	240640	365346.9	1.85E+07
Interest Rev	787789	27491.26	3035641	527582	20636.66	429613.6	240640	43641.17	5455373
Dividend Rev	787789	131894.5	2553665	527582	139492.7	2389685	240640	117474.6	2964239
Total Rev	787789	827394.3	2.05E+07	527582	896012.3	1.57E+07	240640	705276.5	2.89E+07
Officer Comp	787789	11886.52	154670.2	527582	11782.98	100490.4	240640	12196.47	236513
Invest Exp	787789	36052.47	698094	527582	39942.78	674373.5	240640	27932.08	771245.5
Char. Admin Exp	787789	96349.03	2.54E+07	527582	124647.4	3.10E+07	240640	38031.81	2057428
Total Exp	787789	624848.1	1.42E+07	527582	680071	8667672	240640	521142.4	2.23E+07
Char. Exp Total	787789	527291.1	1.09E+07	527582	582903.7	7645177	240640	420038.4	1.61E+07
Invest Return	787789	436290.5	1.29E+07	527582	465523.4	1.01E+07	240640	383848.1	1.80E+07
Gov Bonds	787789	465045.8	2.40E+07	527582	453918.7	1.26E+07	240640	505119.9	3.91E+07
Corp Stock	787789	2750464	7.61E+07	527582	3021178	6.90E+07	240640	2233226	9.22E+07
Corp Bonds	787789	507428	1.93E+07	527582	505346.4	8862608	240640	522181	3.23E+07
Securities Total	787789	3722917	1.05E+08	527582	3980423	8.14E+07	240640	3260501	1.46E+08
Mortgage Inv	787789	22724.28	3697077	527582	22719.77	4187160	240640	23759.56	2510504
Gross Assets Bk	787789	6673017	1.84E+08	527582	6774286	1.13E+08	240640	6638446	2.88E+08
Total Liabs	787789	322643.6	2.48E+07	527582	314924.4	9831584	240640	353124	4.24E+07
Excise Paid	787789	5910.07	164005.5	527582	6524.799	159068.5	240640	4708.792	180015.2
Corr. Gr. Assets	787789	7313254	1.87E+08	527582	7441815	1.17E+08	240640	7227585	2.91E+08
Tot Exp / Asset	776318	13892.76	1651490	518501	18969.93	2016857	238572	3692.006	183862.1
Firm Age	767078	15.57736	15.06823	516964	14.75456	14.87903	236616	17.00479	15.15601
Year UPMIFA	739571	2008.857	1.196722	503571	2008.849	1.205272	218134	2008.887	1.18466
Post Upmifa?	788773	0.146381	0.353488	528288	0.149256	0.356341	240900	0.139531	0.3465
Corporations	788773	0.669759	0.4703	528288	1	0	240900	0	0
Trusts	788330	0.306754	0.461147	528288	0	0	240900	1	0

[1] I'm not sure what accounts for the large number of firms with negative reported gross assets. Possibly some firms misreport net assets as gross assets. I'd like to experiment with different robustness checks, such as taking the absolute value of gross assets or treating all negative values as 10.

[2] NTEE codes for a category of charitable activity, such as "health and hospitals" or "arts and music."

Stock/Securities	435961	75.65012	33.66503	279267	77.27675	33.98563	146966	72.82398	32.83265
Stock/Gr Assets	778233	8.303522	6280.672	519942	12.08192	7683.128	239027	0.730997	163.8239
Bonds /Gr Assets	778233	8.633024	4703.765	519942	12.6193	5752.957	239027	0.649274	208.9734

Notes: All dollar values in 2009 dollars. "Corrected Gross Assets" are corrected by adding back negative values for sub-components, as described more fully in text.

2. Methodology

I investigate the effects of state adoption of UPMIFA on firm spending and portfolio allocation. The basic regressions are of the form:

$$S_{it} = \alpha + \beta_1 Upmifa_{jt} + \beta_2 X_{it} + \lambda t + \gamma i + \varphi j + \varepsilon$$

where *Upmifa* measures whether UPMIFA has been adopted in year *t* and jurisdiction *j*, X is a vector of control variables, and *i* is an individual firm fixed effect.

For difference-in-difference estimations, the equation takes the form:

$$S_{it} = \alpha + \beta_1 Upmifa_{jt} + \beta_2 Treated_{it} + \delta Upmifa_{jt} * Treated_{it} + \beta_3 X_{it} + \lambda t + \gamma i + \varphi j + \varepsilon$$

where *delta* is the coefficient of interest, the interaction term between UPMIFA enactment and the "treated" population. I code firms as treated if UPMIFA is enacted in their state prior to the end of their fiscal year.

I employ two distinct sets of difference-in-difference approaches. In the first, I use whether a firm is a corporation as the "treated" variable in the regressions. I hypothesize that UPMIFA may have a larger effect on corporations than on trusts. UPMIFA liberalizes the prior rule limiting charities from spending out of the real "historic dollar value" of their endowments. A similar model act, the Uniform Principal and Income Act of 1997 ("UPAIA"), had provided for similar (albeit forward-looking) reforms for charitable trusts in enacting states. I don't presently have a definitive list of when states adopted UPAIA; I know that as of 2004 there were 36 adopters (including New York and California), plus D.C. Ideally I will be able to identify which states had adopted UPAIA as of the time of UPMIFA enactment to better define the control group of trusts.[3]

UPMIFA also recodifies the definition of prudent investing in a way that arguably makes clearer that firms are not constrained in their ability to invest according to modern portfolio theory; trusts received the benefit of similar rules around 1994 with the adoption of the Uniform Prudent Investment Act. Better portfolio allocation should change the optimal spend / invest frontier, suggesting another avenue along which the impact of UPMIFA on corporate spending

[3] It may also be interesting to extend the analysis back in time to examine whether UPAIA had any effect on trust spending.

might vary from trusts. But corporations basically received an earlier version of the same language in the early 1970's with the adoption of UMIFA. At the time UPIA was adopted, though, some commentators opined that it would also benefit corporations because corporations seemed to be "influenced" by trust-law standards. So perhaps UPMIFA was actually meaningful on this front, but hard to say in the abstract.

The second treatment effect is a measure of how close the firm was to its pre-UPMIFA spending limits. Firms that were already relatively unconstrained in their spending would not have been immediately affected by UPMIFA, and so pooling them together with constrained firms would (at least theoretically) tend to wash out any effect. I therefore employ a set of d-in-d regressions in which firms that were recently close to fiscally constrained are the treatment group. I cannot observe "historic dollar value" in the NCCS data. To approximate it, I employ several different measures of "fiscally constrained" and "recently," although none of the variations seem to change the results. The results reported here define a firm as "fiscally constrained" if lagged spending exceeds current revenues; firms that had spent all of their income in prior years would be spending-constrained in that scenario. I define "recently" as a firm that has hit this constraint within 2 years. Most firms in the data, about 90%, meet this definition.[4] So only about 72,000 firm-years are untreated.

I control throughout for a handful of basic firm characteristics. Because portfolio allocation and spending may affect total assets, I use lagged assets as a control. To capture the quality / professionalism of management, I also control for officer compensation. As a measure of pressure the firm may feel from outside scrutiny, I control for total liabilities, and whether it reports payment of an "excise tax" for violating one of the nine or so federal tax-law limits on private foundation activities. Alternately, one could view compensation and excise taxes as measures of the presence of agency costs.

Although I attempt to control for the age of each firm, that variable is somewhat unreliable. I derive age simply by subtracting the firm's "ruling date" from the current fiscal year. The ruling date is the date on which the IRS recognized the firm as a charity. However, firms sometimes re-apply for rulings when their mission or organizational structure changes, which may potentially give a misleadingly short life span. About 4,000 firm-years also have no recorded ruling date. (I impute ruling dates from later or earlier years where a single firm-year date is missing.) Results are robust to excluding the age variable.

I also include year, firm, entity form (e.g., trust or corporation) and ntee-code fixed effects. Regression designs using corporate form as a treatment omit entity-form effects, and fixed-effects regressions omit state, ntee, and entity form. Firm data are organized by fiscal year, not calendar year. To impose a uniform control for time effects, I generate a calendar-year

[4] 500,000 firm-years meet the definition of constraint, and another 220,000 do not but have met it within the previous 2 fiscal years.

equivalent for each firm based on the calendar year in which the majority of the fiscal year months fall; years ending in June were assigned to the previous calendar year.

The results reported below mostly rely on the use of random-effects models to capture the cross-sectional variation between states in a given year. Since I cannot defend all the assumptions of RE, I double-check these cross-sectional results using both pooled OLS and GEE. GEE is increasingly popular in health and political sciences; see Hubbard et al. (2010) and Harden (2012). Interpretation with GEE is slightly different than OLS; as Harden (2012) puts it, "GEE coefficients denote changes in the dependent variable associated with observations in clusters that differ by one unit on the independent variable. Simply put . . . GEE represents the average effect across a population." Unless otherwise noted, I obtain very similar results with all three methods.

3.0 Results

3.1 Baseline Results

Table One below reports the results of simple OLS regressions, without dividing the sample into treated and controls and without scaling for firm assets. (I call these results the "baseline" in the text that follows.) Columns one and three report the effect of UPMIFA on total firm spending, while columns two and four report its effect on charitable spending. Columns one and two use a random-effects model. Columns three and four report fixed-effect regressions.

Table One: Baseline Effects of UPMIFA on Total and Charitable Expenditures in All Firms

VARIABLES	(1) Lntotexp RE	(2) Lncharexpend RE	(3) Lntotexp FE	(4) Lncharexpend FE
upmifa	0.0156**	0.00126	0.0194***	0.0120
	(2.202)	(0.102)	(2.717)	(0.958)
L.lngrossassets	0.486***	0.549***	0.381***	0.420***
	(136.2)	(146.7)	(78.11)	(74.29)
lntotliab	0.0372***	0.0144***	0.0232***	0.0142***
	(38.23)	(8.869)	(20.42)	(7.675)
lnexcisepd	0.0870***	0.161***	0.0640***	0.0910***
	(49.53)	(61.96)	(33.82)	(31.72)
corporation	0.246***	0.0752		
	(3.477)	(0.619)		
Trust	-0.0715	0.00724		
	(-1.011)	(0.0599)		
Coop	0.0720	0.0335		
	(0.471)	(0.162)		
partnership	0.421	0.0513		
	(1.517)	(0.0615)		

association	-0.0732	-0.455***		
	(-0.901)	(-3.389)		
n/e charit. trust	-0.253**	-0.158		
	(-2.121)	(-0.921)		
lnofcomp	0.0518***	0.0596***	0.0437***	0.0511***
	(51.85)	(37.53)	(31.85)	(21.97)
State Effects?	Y	Y	N	N
Year Effects?	Y	Y	Y	Y
N	631,366	631,366	631,366	631,366
Number of clusters	103,460	103,460	103,460	103,460

Notes: Standard errors clustered by firm. Z scores in (parentheses). Regressions include ntee code fixed effects. **: significant at the 5% level ***: significant at the 1% level

Enactment of UPMIFA appears to increase overall spending, but not spending designated as related to charitable mission. Depending on specification, the increase ranges from a 1.56 to 1.94 percentage point bump. Pooled OLS and random-effects regressions produce results similar to the RE/GEE outcomes.

To account for possible serial correlation, I also attempt alternative regressions in which I use first differences of spending as the dependent variable. In the base specification, these produce no significant results for the upmifa variable.

Next, I restrict the regression only to corporations. Here, upmifa increases spending by 2.7% in baseline RE/GEE panel specifications and 3.2% in baseline FE specifications. The coefficient on charitable expenditures is positive but insignificant, with a 95% confidence interval from -1.7 to 4.5%. First-differences panel regressions yield coefficients about half as large, around 1.5%, and only significant at the 10% level. Pooled OLS regressions in levels yield no significant results. Regressions restricted only to trusts yield effects that are not statistically distinguishable from zero.

3.1 Expenditures Scaled By Assets

Ideally, I'd also like to run these regressions while scaling for organizational assets. But the organizational asset data are…peculiar. Some firms report negative *gross* assets. Others report assets that are less than 1/10 of the assets reported in adjacent years. Even when excluding all these outlier firms (on the order of one thousand each), I still end up with firms that report spending hundreds of millions per asset dollar. Regressions scaled for assets using these two simple fixes yield no results.

As an additional set of fixes, I create a new measure of assets ("corrected gross assets" in the summary table) in which I add back into gross assets any reported negative sub-components of assets. More specifically, I increase gross assets by negative balances for cash, mortgages, or other investments. Additionally, about 44,000 firm-years report having gross asset of "1" (as compared to 220 firm-years reporting $2 in assets). On average the adjacent years for these

firm-years are in the hundreds of thousands of dollars. Accordingly, I drop observations for which gross assets are listed as 1.

I then scale the relevant variables by this alternate asset measure, and run the regressions again. Specifications including the entire pool of firms are insignificant. However, when limiting the regression to corporations, I find about a 2% total expenditure increase in upmifa states, but no significant effects for charitable spending; FE results are similar but significant only at the 10% level. I also re-run the baseline regressions above using the alternative measure of assets as a control in place of the uncorrected assets measure (and again dropping observations with $1 in reported assets), with similar results to those reported above.

3.2 Difference-in-Differences

I next attempted to run a set of difference-in-differences regressions, using non-corporations to control for the effect of UPMIFA on corporations. Again, the argument here is that UPMIFA should affect spending among corporations but not trusts. As summarized in Table 3, below, I again find that upmifa's impact on corporate spending was in the range of about 2.6%. I find slightly larger, if less precise, coefficients for the impact on charitable expenditures, with a 95% confidence interval ranging from .3 to 6.6%.

Table 3: Effect of Upmifa on spending, DiD Regressions w/ Corporation as Treatment

	(1)	(2)
VARIABLES	Spending RE	Charitable Spending RE
Upmifa x corp	0.0263***	0.0348**
	(2.871)	(2.177)
corp	0.294***	0.132***
	(22.37)	(6.387)
Upmifa	0.0142	0.0442**
	(1.469)	(2.572)
L.lngrossassets	0.485***	0.541***
	(134.8)	(143.8)
lntotliab	0.0383***	0.0204***
	(38.98)	(12.62)
lnexcisepd	0.0853***	0.152***
	(48.47)	(58.87)
lnofcomp	0.0528***	0.0643***
	(52.59)	(40.25)
Observations	626,276	626,276
Number of ein	103,407	103,407

Notes: Standard errors clustered by firm. Z scores in (parentheses). Includes state, year, and ntee code fixed effects. **: significant at the 5% level ***: significant at the 1% level

Fixed effects regressions are similar, with coefficients of 2.9% and 4.4%, respectively.

I obtain similar results for total spending when I scale the variables by corrected assets, as reported in Table 4, below.

Table 4: Effect of Upmifa on spending, DiD Regressions w/ Corporation as Treatment, Variables Scaled by Firm Assets

	(1)	(4)
	DiD Total Spend by assets	DiD Char spending by asset
VARIABLES	GEE	GEE
Upmifa x corp	0.0344***	0.0177
	(3.702)	(1.184)
corp	0.490***	0.198***
	(44.84)	(12.73)
Upmifa	-0.0855***	-0.0744***
	(-7.900)	(-4.188)
ln_liab_per_asset	0.105***	0.0449***
	(102.0)	(29.39)
ln_excise_per_asset	0.171***	0.238***
	(79.21)	(80.54)
ln_comp_per_asset	0.112***	0.0975***
	(108.2)	(64.56)
Observations	728,057	728,057
R-squared		
Number of ein	108,764	108,764

Notes: Standard errors clustered by firm. Z scores in (parentheses). Includes state, year, and ntee code fixed effects. **: significant at the 5% level ***: significant at the 1% level

However, unlike the unscaled regressions, these results are not robust to switching to fixed effects; FE regressions yield no significant results for total expenditures.

An alternative approach is to assign firms relatively distant from the pre-UPMIFA spending constraint to the control group and all others to the treatment group. Again, in the regressions reported about 89% of firms are treated. This suggests that my assumptions about which firms are constrained are probably too conservative. I assume that firms are constrained if their spending exceeds lagged revenues, but many such firms in fact are not constrained because they have prior investment returns they can draw on without having to spend out of historic dollar value. So my definition of constraint is probably a necessary but not sufficient definition. It is, at least, an assumption that cuts contrary to our hypothesis: we will be pooling together constrained and unconstrained firms, diluting any measurable impact of true constraint.

Only the first-difference regressions show significant results. (Running the constrained regression limited only to corporations yields a coefficient of about 1.6% in non-differenced OLS, but significant only at the 11% level). Table 5 reports the results from these regressions; on average, upmifa increases total spending by 4.8% in constrained firms, and increases charitable spending by 3.3%. Because these results are sensitive to specification, and because I can't come up with a better definition of constraint, I would lean against including them in the paper.

Table 5: D-i-D with Fiscal Constraint Treatment, First Differences

VARIABLES	(1) D.lntotexp RE	(2) D.lncharexpend RE
Upmifa x constrained	0.0482***	0.0331**
	(5.916)	(2.498)
constrained	-1.013***	-1.100***
	(-160.5)	(-129.0)
Upmifa	-0.158***	-0.181***
	(-13.16)	(-9.711)
L.lngrossassets	0.00119	0.0273***
	(0.446)	(12.04)
lntotliab	-0.000663	-0.00843***
	(-0.953)	(-10.25)
lnexcisepd	-0.00806***	-0.0146***
	(-4.347)	(-7.534)
lnofcomp	0.00493***	0.00181***
	(8.707)	(2.838)
Observations	631,587	631,587
R-squared		
Number of ein	103,557	103,557

Notes: Standard errors clustered by firm. Z scores in (parentheses). **: significant at the 5% level ***: significant at the 1% level

3.3 Portfolio Allocation

In basic specifications, I find no evidence that UPMIFA affects portfolio allocation, as shown in Table 6, below. For an outcome variable, I use the share of firm securities invested in stock, multiplied by 100. I estimate the outcome variable in levels because, by definition, each unit change is equivalent to a one percentage-point change.

Table 6: Effects of UPMIFA on share of firm securities invested in stock

VARIABLES	(1) Share of stock, RE	(2) 1st Diff. Share of stock , RE	(3) Share of Stock, FE
upmifa	0.175	0.0405	0.276
	(1.017)	(0.324)	(1.582)
L.lngrossassets	-0.588***	-0.000481	-0.388***
	(-12.17)	(-0.0204)	(-6.288)
lntotliab	0.130***	0.0293	0.119**
	(2.794)	(1.522)	(2.135)
lnexcisepd	-0.453***	0.00289	-0.327***
	(-16.32)	(0.176)	(-10.21)
corporation	-0.941	-0.280	
	(-0.598)	(-0.411)	
Trust	-4.768***	-0.291	
	(-3.034)	(-0.427)	
Coop	-7.090**	-0.379	
	(-2.238)	(-0.444)	
partnership	14.63***	0.970	
	(3.144)	(0.791)	
association	-6.631***	0.0545	
	(-3.621)	(0.0777)	
n/e charit. trust	-4.476	0.181	
	(-1.520)	(0.178)	
lnofcomp	-0.193***	0.0366***	0.0298
	(-8.866)	(6.313)	(1.069)
Observations	366,476	351,791	366,592
R-squared			0.006
Number of ein	62,165	59,588	62,211

Notes: Standard errors clustered by firm. Z scores in (parentheses). Column one includes state, year, and ntee code fixed effects; column three includes year effects. **: significant at the 5% level ***: significant at the 1% level

A problem for this approach is that about half of the firm-years report zero securities holdings. Firms reporting zero securities are not necessarily small; the largest such firms have in the hundreds of millions in gross assets, with the mean at $ 2.7 million. Possibly these are firms with large real property holdings but no other assets. But I suspect a fair degree of misreporting, which could be attenuating the coefficients. Ideally what I'd like to do here is to run a heckit regression (i.e., a pair of linked regressions in which I first run a probit on the determinants of non-zero securities, and then one of the regressions above). But my computer has a meltdown when I try that; apparently it's very computationally intensive to do in stata.

Another approach might be to use stock as share of total assets, but this runs into the problems with the gross assets variable reported above. I find no effect of UPMIFA on the share of stock over all assets, regardless of whether I use reported assets or my corrected versions.

It is also possible that firms may be relatively slow to respond to new portfolio allocation rules. To allow for this, I run regressions using lagged upmifa enactment, and find small positive coefficients, equivalent to about 6 basis points, in basic specifications, but nothing doing with first-differences, as summarized in Table 7.

Table 7: Lagged Effect of Upmifa on Stock Share

	(1) Lag upmifa on stock share	(2) Lag upmifa on 1st diff stock share	(3) Lag upmifa on stock share	(4) Lag upmifa on first diff stock share
VARIABLES	RE	RE	FE	FE
L.upmifa	0.652***	0.101	0.569***	0.0557
	(3.359)	(0.760)	(2.850)	(0.374)
L.lngrossassets	-0.778***	0.00225	-0.391***	-0.0530
	(-16.00)	(0.127)	(-6.345)	(-0.500)
lnexcisepd	-0.448***		-0.327***	
	(-16.08)		(-10.22)	
lntotliab	0.140***	0.0293	0.120**	0.127*
	(3.019)	(1.535)	(2.147)	(1.804)
lnofcomp	-0.241***	0.0366***	0.0327	-0.00315
	(-11.18)	(6.564)	(1.177)	(-0.142)
Observations	366,592	351,904	366,592	351,904
State Effects	Y	N	N	N
Year Effects	Y	Y	Y	Y
NTEE Effects	Y	N	N	N
R-squared			0.007	0.001
Number of ein	62,211	59,633	62,211	59,633

Notes: Standard errors clustered by firm. Column 1 includes entity-form effects. Z-statistics in (parentheses). *** p<0.01, ** p<0.05, * p<0.1

Lastly, I test for whether UPMIFA may have had larger portfolio-adjustment effects in corporations. Again, in theory trusts should have already obtained portfolio flexibility with the prior enactment of UPIA or judicial decisions. When I limit the regressions above only to corporations, the coefficients are not statistically distinguishable from those report in Table 7; for example, a regression duplicating column 1 of Table 7 but limited to corporations yields a coefficient of .70, with a 95% confidence interval that almost entirely overlaps with that reported

in the first line of Tbl.7 col. 1. Difference-in-differences regressions yield slightly smaller coefficients for the effect of upmifa in corporations, about .50, whether or not scaled by assets.

Lags of UPMIFA also show no effect on share of stock out of all assets.

4.0 Robustness

One potential concern with these results is that enactment of upmifa may be endogenous to nonprofit spending. For example, states experiencing fiscal downturns may have a greater perceived need for increased charitable expenditures, and therefore be more likely to enact upmifa. A strength of the D-i-D approach is that it should help to rule out this story. Nonetheless, as an additional robustness check, I re-estimate key regressions while controlling for basic state-level variables, such as population, unemployment rate, gdp, and median income, that might affect demand for charity. I find that these state-level variables have no significant effect on total expenditures, and that the coefficients reported above are essentially unchanged. However, in the difference-in-differences test reported in Table 3, I lose some precision, so that the key results are significant only at the 10% level.

Prior literature has also suggested that panel data involving policy change at the state level may be subject to spurious results due to serial correlation. (Bertrand, Duflo & Mullainathan 2004). To help rule out that possibility, I follow BDM (2004) in constructing a placebo treatment effect, which I define as enactment of upmifa two years prior to its actual enactment. I find no significant effects of the placebo upmifa on either total or charitable spending.

BDM (2004) also suggest that serial correlation can be eliminated by treating the panel as a two-period sample. When the treatment effect is phased in over time, they suggest first regressing the state-year average outcome variable on state and year fixed effects and covariates. Residuals from this regression for the treatment states are then divided into two time periods, pre- and post-treatment, and averaged by state. Then the regression is repeated using this two-period panel. When I perform this procedure, I obtain a coefficient of 3.55% for the effect of upmifa, with a 95% confidence interval ranging from .6 to 6.5%. I also run a pair of alternative measures in which I code a state as "treated" only in the first full calendar year after upmifa takes effect; in these regressions the 95% confidence interval runs from approximately 1.3 to 8.5% (the two alternatives vary in whether I code the upmifa enactment year as "pre" or not; in the latter the bottom of the range is a hair lower, 1.2%).

[As another set of robustness checks, I'd like to also re-estimate key results using Cameron, Gelbach & Miller (2008) bootstrap standard errors. CGM argue that their method better accounts for serial correlation and other problems in clustered panels. Similarly, Harden

(2012) shows with Monte Carlo simulation that GEE with bootstrapped errors is more efficient than conventional alternatives.]

5.0 Other Directions for Further Investigation

1. Quantile regressions. The results here capture the average effect of upmifa across all firms. Does upmifa have a larger impact for low- or high-spending firms? We might use quantile regressions to find out, if that seems like an interesting question.

2. What makes a hortatory law matter? We could control for AG enforcement capability and state law on donor standing to sue.

3. Does reducing spending constraints tend to benefit stakeholders, or just allow for managerial rents? Given that we find inconsistent evidence that upmifa increases charitable spending, a possible hypothesis is that more poorly governed firms will tend to take more advantage of looser constraints. We don't have a ton of data on good governance, but there are some variables in the database that reflect self-dealing.

4. UPAIA. As mentioned above, the adoption of UPAIA in the late 90's should likely have led to similar spending freedom for trusts.