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Using Stock Return Data to Measure the Wealth Effects of Regulation: Additional Evidence from California's Proposition 103

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ABSTRACT

In contrast to literature that has used the event study method to examine the stock market reaction to California's Proposition 103, we estimate the abnormal performance of both equally-weighted and value-weighted portfolios of securities. We also estimate a cross-sectional regression in value terms; that is, regressing the abnormal change in dollar value of the firm's equity against direct premiums written in automobile and other insurance lines in estimating relative exposure to Proposition 103. Results from the value-weighted portfolios indicate that Proposition 103 had little impact on the aggregate dollar value of the insurance industry. Cross-sectional regressions indicate that exposure, income, and size measures are not related to changes in firm value.

Introduction

On November 8, 1988, despite intense insurance industry lobbying efforts, California voters approved Proposition 103, radically reforming the state's insurance laws. Among the referendum's more controversial provisions was a 20 percent reduction in property-liability (notably auto) insurance rates. Proposition 103 outlined which risk factors could and could not be used to underwrite auto insurance. Geography could no longer be used as a primary rating factor.[1] Instead, drivers' records, miles driven, and years of driving experience became the primary determinants of insurance premium rates. Insurers were

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[1] More specifically, Proposition 103 barred insurers from using the county of vehicle registration as a primary determinant of rates, a net wealth transfer from rural to urban regions.

also required to lower insurance premium rates an additional 20 percent for good drivers. No other lines of insurance received such specific attention.

Within two days of the vote, the California State Supreme Court issued a temporary staying order, delaying implementation of the measure. This stay was ultimately overturned, and the elected California insurance commissioner directed automobile insurers to pay rebates in 1993. Consequently, this act has had an impact on the operating profits of California's insurers. This article examines whether the market correctly anticipated Proposition 103's impact on California's property-liability insurers. We also examine whether Proposition 103's impact varies by firm-specific characteristics.

Previous stock market–based research by Fields et al. (1990), Szewczyk and Varma (1990), and Shelor and Cross (1990) on the wealth effects of Proposition 103 report conflicting conclusions. Both Fields et al. and Szewczyk and Varma find a significant negative impact on insurers with high California exposure. In addition to a time-series event methodology, Fields et al. employ cross-sectional regression and find that insurer negative abnormal stock returns are proportional to California exposure. Both Fields et al. and Szewczyk and Varma also conclude that the California reform represents a leading indicator of other states' likely reforms, supported by stock market evidence. By comparison, Shelor and Cross report no significant negative impact of Proposition 103 on California insurers but a significant negative impact (-2.87 percent) on non-California insurers on election day and the following day. These authors offer size effects as an explanation; large insurers were able to diversify away adverse effects of regulation while small non-California insurers were unable to do so.

This article uses the stock market to measure the wealth effects of California Proposition 103. The article differs from previous research in several respects. First, we examine the abnormal performance of both equally-weighted and value-weighted portfolios of exposed and nonexposed securities.[2] Portfolios are drawn from the sample of firms used by Fields et al. and also from a somewhat different sample. Results from the value-weighted portfolios indicate that Proposition 103 had little impact on the aggregate (dollar) value of the insurance industry.

Second, we compare the security return behavior of five firms (Chubb, Fireman's Fund, Ohio Casualty, Safeco, and Zenith National) mentioned in the *Wall Street Journal*'s Heard on the Street (HOTS) column on the day before the election to the stock return behavior of four other firms not mentioned in HOTS but that had roughly comparable California exposure. Thus, the relative performance of these stocks can shed some light on market efficiency.

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[2] Tests based on portfolio abnormal returns will properly account for "event-clustering," that is, cross-sectional correlation among security returns. Since within-industry correlation tends to be positive on average, tests based on the assumption of cross-sectional independence would be biased toward rejecting the null hypothesis. Applying the Bruesch-Pagan (1980) test described in Judge et al. (1985, p. 476), we reject the hypothesis of cross-sectional independence at the one percent level for our sample and also for the one used by Fields et al.

Third, to gauge the impact of this referendum and the subsequent court decision to delay implementation, we estimate a cross-sectional regression in value terms; that is, we regress the abnormal change in (dollar) value of the firm's equity against direct premiums written in automobile and other insurance lines in estimating relative exposure to Proposition 103.

Theory and Hypotheses

We test three hypotheses concerning the stock market reaction to Proposition 103: the contagion effect hypothesis, the information effect hypothesis, and the dribs and drabs hypothesis.

The contagion effect hypothesis was applied to bank failures by Aharony and Swary (1983) and subsequently applied to the international debt crisis by Bruner and Simms (1987). Also known as the irrational pricing hypothesis, it states that all insurers will earn negative abnormal returns and that these abnormal returns will not be consistent with the firm's true Proposition 103 exposure. Negative abnormal returns experienced by insurers with little or no California exposure provide evidence of contagion.

The information-effect or traditional semistrong form efficient markets hypothesis is related to the efficient market theory. In an efficient market, new information will be quickly and accurately incorporated into stock prices. Thus, insurer stock prices should rapidly adjust to new information released about Proposition 103, and the changes in share value should be in proportion to each firm's California exposure.

California's Proposition 103 allows a unique opportunity to examine market efficiency and the *Wall Street Journal*'s Heard on the Street column. One day prior to the election, HOTS identified five major California insurers as likely to suffer major losses in earnings if Proposition 103 were to pass. Thus, we compare the abnormal share price reaction of these five firms to another portfolio consisting of firms with equal or greater exposure. Previous research on HOTS by Liu, Smith, and Syed (1990) and Lloyd-Davies and Canes (1978) suggests that this column does cause abnormal changes in share value.

The dribs and drabs hypothesis was originally coined by Cornell and Shapiro (1986) in the context of the international debt crisis, yet is equally applicable to Proposition 103. This hypothesis states that, for partially anticipated events such as Proposition 103, new information is released gradually over long periods of time; there is no single event date on which all relevant information is released. Thus, the stock returns of insurers should reflect their California exposure when returns are measured over long intervals, not necessarily on any given day. This hypothesis is supported in previous research by Fields et al. (1990).

Measuring Exposure

The ideal measure of an insurer's exposure to California's Proposition 103 would comprise the market value of a wholly-owned subsidiary that wrote insurance only in California and only in lines of insurance affected by Proposition 103. Further, the parent of such a subsidiary would write no insurance in California. The ratio of the market value of this hypothetical subsidiary to the market value of the parent would accurately reflect the parent's exposure to regulatory reform in California.[3]

Because data for the ideal exposure measure do not exist, any feasible exposure measure will be an approximation, subject to the restrictions imposed by data limitations. Data on direct premiums written are available by line of insurance and by state from Best's Executive Data Service; this data must be the foundation of any exposure measure. One limitation of this data is that reinsurance is not available by state. Although reinsurance is often written only for the current year, reinsurance agreements can have extended maturities. Thus, an unknown fraction of each firm's California direct premiums written are effectively hedged by the equivalent of a long dated forward contract.

To estimate each firm's relative exposure, affected premiums must be scaled by a variable related to firm size. Fields et al. (1990) divide each firm's California direct premiums written (in all potentially affected lines) by the firm's premiums written nationwide. Although similar measures were reported in the press around election day, other scaling factors, such as total assets and value of common stock equity, are also logical candidates. Interpretation of regression coefficients becomes problematic when several ratios based on alternative scaling factors are available. The ratio that provides the best fit (i.e., the coefficient is significant and the sign agrees with a priori reasoning) may not be a good approximation of the firm's true financial exposure. Thus, statistical tests based on proxies for financial ratios are actually joint tests of market efficiency and the accuracy of the proxy. In the cross-sectional regression (described below), we attempt to avoid the issue of the appropriate scaling variable by estimating the relationship in value terms. That is, the abnormal (dollar) change in firm value is regressed against the dollar value of premiums written in automotive and other affected lines.

Sample and Methodology

Sample

The final sample comprises 33 insurers and holding companies. Insurers that are subsidiaries of conglomerates or other noninsurance firms are eliminated

[3] Two firms (Twentieth Century Industries and Mercury General) specialize in the California market. These publicly-traded firms write insurance only in California, and they offer primarily automobile insurance. Although these two firms appear to offer an opportunity for direct examination of the effect of Proposition 103, earnings announcements confound the results and we cannot attribute the abnormal returns to Proposition 103. Twentieth Century Industries announced a 98 percent increase in third-quarter earnings on November 10, 1988, the same day that the California State Supreme Court issued an order staying the implementation of Proposition 103. A second difficulty is created by these firms' specialization. These firms appear committed to the California market and thus are not truly representative of all insurers selling property-liability insurance in that state. Inferences about Proposition 103 drawn from these two firms alone would likely overstate industry-wide effects.

from the sample due to the difficulty that arises in interpreting the results. Firms that made material announcements (e.g., acquisitions or spinoffs) are also eliminated.[4] Two California insurers, Twentieth Century Industries and Mercury General, are also dropped from the sample due to a confounding event (an earnings announcement by Twentieth Century). Finally, Farmer's Group was removed from the sample since it was taken over by B.A.T. Industries.

For each of the 33 securities, daily stock returns are obtained from the data tapes provided by the Center for Research in Security Prices for a 253-day interval beginning 216 days before and ending 36 days after the election. Data on direct premiums written for lines of property-liability insurance written in California during 1987 were obtained from Best's Executive Data Service. This list of firms, together with exposure ratios for auto and other insurance lines, is presented in Table 1.

Methodology

This section discusses the tests and regressions that are the foundation of our analysis. First, we describe our method of estimating abnormal returns. Second, we describe the cross-sectional regression with abnormal changes in firm value as the dependent variable and direct premiums written (auto and nonauto) and earnings before tax as the explanatory variables.[5]

The event parameter approach.[6] In the event parameter approach, abnormal returns are estimated by appending a set of (0,1) dummy variables to the right-hand side of the single index market model. Designating election day (November 8, 1988) as day zero, the model is estimated over a 253-day interval from day -216 to day 36. For each day in the event period (day -65 to day 36), there is a (0,1) dummy variable that is equal to one on that day and is zero otherwise; thus prediction errors are estimated directly as regression coefficients.

Using the event parameter approach, the following equation is estimated for each firm or portfolio j:

$$R_{jt} \quad \alpha_j \quad \beta_j R_{mt} \quad \overset{S}{\underset{s\ 1}{}} \quad \delta_{js} D_{st} \quad \varepsilon_{jt}\ ,\ t \quad 216, \ldots, \quad 36, \qquad (1)$$

[4] Every firm in the sample released third-quarter earnings and dividend news during the period surrounding election day. Thirty-four firms reported regular dividends, and one firm increased its dividend. Seventeen firms in our sample released earnings news during the second week prior to election week (i.e., days -16 to -11). Given the proximity of election day to the end of the third quarter, the presence of earnings announcements within the event window is unavoidable and a problem common to all Proposition 103 studies.

[5] In addition to the test procedures described here, we also estimated regressions with intercept and slope shift dummy variables during the event period. No significant increases in systematic risk were obtained.

[6] This exposition of the event parameter approach is adapted from Karafiath (1988). For a discussion of the event parameter approach, see Binder (1985a and 1985b) and Thompson (1985).

Table 1
Sample Firms' Exposure Ratios

Tick	Firm Name	Exposure Ratio Total	Auto	Other
First Quintile				
INDH	Independent L&A	0.0001	0.0000	0.0001
AMV	Viking Brothers	0.0003	0.0000	0.0003
MAKL	Markel Corporation	0.0004	0.0004	0.0000
AIFC	American Indemnity	0.0007	0.0000	0.0007
CPH	Capital Holding	0.0010	0.0010	0.0000
AAME	Georgia Casualty & Surety	0.0011	0.0000	0.0011
FAMR	First American	0.0012	0.0000	0.0012
Second Quintile				
GRN	General Re	0.0100	0.0000	0.0100
AGC	American General	0.0332	0.0122	0.0210
HSB	Hartford Steam	0.0346	0.0000	0.0346
STPL	St. Paul	0.0682	0.0138	0.0544
GEC	GEICO Corporation	0.0706	0.0660	0.0045
AVE	AVEMCO	0.0770	0.0000	0.0770
NWNL	Northwestern National	0.0786	0.0194	0.0593
Third Quintile				
HINS	Hanover	0.0843	0.0313	0.0530
MLA	American Modern Home	0.0870	0.0097	0.0773
LNC	Lincoln National	0.0878	0.0351	0.0527
AIG	American International	0.0935	0.0129	0.0806
ECRC	Employers Casualty Group of Texas	0.0938	0.0464	0.0474
AET	Aetna Life & Casualty	0.1104	0.0294	0.0810
KEMC	Kemper	0.1203	0.0317	0.0886
Fourth Quintile				
CI	Cigna	0.1451	0.0244	0.1207
OLDR	Old Republic	0.1563	0.0251	0.1312
BKLY	W. R. Berkley	0.1676	0.0109	0.1566
SAFC	Safeco	0.1702	0.0750	0.0952
CB	Chubb	0.1892	0.0211	0.1681
PGR	Progressive	0.2970	0.1364	0.1606
OCAS	Ohio Casualty	0.3481	0.0765	0.2717
Fifth Quintile				
ZNT	Zenith National	0.4810	0.2512	0.2298
ABC	Home Insurance	0.5201	0.0907	0.4294
FFC	Fireman's Fund	0.6057	0.0788	0.5269
AMW	AmWest	0.8158	0.0000	0.8158
OC	Orion Capital	0.9057	0.1126	0.7932

Note: Exposure = potentially affected property-liability net premiums written (according to Best's Executive Data Service) for calendar year 1987 divided by the market value of common stock equity (according to the Center for Research in Security Prices) as of December 31, 1987.

where R_{jt} = return to security or portfolio j on day t,
α_j = intercept,
β_j = slope coefficient,
R_{mt} = return to the value-weighted market index on day t,
δ_{js} = abnormal return to security j on day s,
D_{st} = a dummy variable that is set equal to one on day s in the forecast window and is zero otherwise, and
ε_{jt} = disturbance term for security or portfolio j on day t.

For each firm or portfolio, cumulative prediction errors (CPEs) may be obtained by summing the estimated (δ) coefficients:

$$CPE_j \qquad \overset{S}{\underset{s\ \ 1}{}} \qquad \hat{\delta}_{js}, \tag{2}$$

where $CPE_j(t_1, t_2)$ is the cumulative prediction error over the interval t_1 to t_2 for the jth firm or portfolio.[7] Using ordinary least squares regression, each of the portfolios and individual securities described below are used as the dependent variable in equation (1).

Following Karafiath, Mynatt, and Smith, (1991), we compare differences in abnormal returns by forming hedged portfolios consisting of a long position in one portfolio and a short position in the other:

$$R_{ht} = R_{lt} - R_{st}, \tag{3}$$

where R_{ht} = return to the hedged position,
R_{lt} = return to the long position, and
R_{st} = return to the short position.

The hedged portfolio return is used as the dependent variable in equation (1). If the event under investigation has no differential impact on the two portfolios, the gain on the long position will be exactly offset by the loss on the short position and vice versa, and the hedged portfolio will not earn abnormal returns. We use this technique to compare the abnormal returns of the HOTS portfolio to the abnormal returns of a matching portfolio with equal or greater exposure. The total direct premiums written in California in affected lines of insurance is scaled by the market value of equity on December 31, 1987, to measure the overall exposure of each firm to Proposition 103.

[7] A major advantage of the dummy variable regression represented by equation (1) is that the statistical package will correctly calculate the variance of the CPE as

$$s^2 \quad T_2 \qquad \frac{T_2^2}{T_1} \qquad \frac{T_2^2 (R_{m2} \qquad R_{m1})^2}{\underset{t\ 1}{\overset{T_1}{}} \quad (R_{mt} \qquad R_{m1})^2},$$

where s^2 is the variance of the regression residual from t=1 to T_1, T_1 is the number of observations in the nonevent period (i.e., there are no dummy variables assigned to this period), T_2 is the number of observations in the event period, and R_{mi} is the mean value of the market return in period T_i.

We form four portfolios from the sample:

1. After ranking the 33 firms in the sample on the basis of overall exposure measure, the lowest five form the nonexposed portfolio.
2. The remaining 28 firms are the exposed portfolio.
3. Five firms mentioned in the November 7, 1988, Heard on the Street column (Chubb, Fireman's Fund, Ohio Casualty, Safeco, and Zenith National) are the HOTS portfolio.
4. Four firms not mentioned in HOTS but with similar or higher exposure (Amwest Insurance Group, Home Group, Orion Capital, and Progressive) are the not-HOTS portfolio.

We estimate the mean California exposure for the HOTS portfolio to be 35.88 percent and for the not-HOTS portfolio to be 63.46 percent.[8] We also estimate abnormal returns to the not exposed and the exposed portfolio from the sample used by Fields et al. (1990). The Fields et al. portfolios and our first two portfolios are estimated both as value-weighted and equally-weighted portfolios.

Cross-sectional regression. An important issue concerns the sources of abnormal returns. Variables including the amount of exposure, types of insurance underwritten, and prior profitability may cause Proposition 103 to have a differential impact across affected firms. Cross-sectional regressions are estimated over several individual days and intervals to test for differential effects. The cross-sectional regression takes the form

$$\text{Value}_{js} = b_0 + b_1\ \text{AUTO}_j + b_2\ \text{OTHER}_j + b_3\ \text{EBT}_j + \varepsilon_{js}, \tag{4}$$

where Value_{js} = the abnormal change in firm value: the abnormal return on day s times the value of common stock equity on day s-1; the previous day,

AUTO_j = dollar value of direct premiums written in California automotive lines of insurance during 1987,

OTHER_j = dollar value of direct premiums written in California in all other lines of insurance potentially affected by Proposition 103, and

EBT_j = earnings before tax for all lines except accident and health from 1987 data.

If equation (4) is estimated by ordinary least squares, the estimated coefficients will be unbiased and consistent; however, since stock returns (used to calculate Value_{js}) are cross-sectionally correlated and heteroskedastic, it is likely that the ordinary least squares covariance matrix $s^2(X'X)^{-1}$ is biased.[9]

[8] To ensure that the HOTS and not-HOTS samples are hedged, we cross-sectionally average the stock returns by dividing according to the number of securities in the portfolio. Adding a fifth firm, W. R. Berkley, into the not-HOTS sample at the request of a reviewer did not change the results.

[9] See Judge et al. (1988, sect. 8.1.1). Additional details are available from the authors upon request.

Thus, we estimate the true least squares covariance matrix as $(X'X)^{-1}X'\Omega^{-1}X(X'X)^{-1}$, where Ω is the variance covariance matrix of the error term. For cumulative intervals, the dependent variable is summed in the same way as abnormal returns are cumulated; the covariance matrix of parameter estimates is adjusted *mutatis mutandis*.

Results

Event Study Results

Table 2 presents the daily and cumulative abnormal returns to nonexposed and exposed portfolios drawn from the sample used by Fields et al. (1990).

Table 2
Daily and Cumulative Abnormal Returns for
Equally-Weighted and Value-Weighted Portfolios
from the Fields et al. (1990) Sample

Interval	*Equally-Weighted*	*t-test*	*Value-Weighted*	*t-test*
Panel A: Nonexposed Portfolio				
(-01, 01)	-1.583	-0.810	-3.098	-0.970
(-02, 02)	-0.264	-0.104	0.950	0.228
(-05, 05)	-0.392	-0.102	-3.159	-0.502
(-10, 10)	-0.906	-0.165	-3.340	-0.372
(-20, 20)	-1.717	-0.213	-8.156	-0.619
(-03, -03)	0.916	0.818	0.882	0.482
(-02, -02)	-0.084	-0.075	-0.341	-0.186
(-01, -01)	-0.789	-0.702	-2.718	-1.481
(00, 00)	0.358	0.320	-0.234	-0.128
(01, 01)	-1.153	-1.029	-0.146	-0.080
(02, 02)	1.403	1.254	4.389	2.399*
(03, 03)	-0.168	-0.148	0.694	0.375
Panel B: Exposed Portfolio				
(-01, 01)	-0.982	-1.351	-0.828	-1.155
(-02, 02)	-1.424	-1.507	-1.203	-1.064
(-05, 05)	-3.276	-2.288*	-2.073	-1.108
(-10, 10)	-3.739	-1.831	-1.745	-0.873
(-20, 20)	-7.516	-2.507*	-3.022	-1.232
(-03, -03)	0.128	0.307	-0.232	-0.560
(-02, -02)	-0.306	-0.735	0.124	0.229
(-01, -01)	-0.672	-1.609	-0.402	-0.964
(00, 00)	-0.100	-0.241	-0.168	-0.405
(01, 01)	-0.210	-0.504	-0.257	-0.619
(02, 02)	-0.136	-0.327	-0.499	-1.199
(03, 03)	-0.154	-0.366	-0.311	-0.742

* $p < 0.01$.

Table 3
Daily and Cumulative Abnormal Returns for
Equally-Weighted and Value-Weighted Portfolios

Interval	*Equally-Weighted*	*t-test*	*Value-Weighted*	*t-test*
Panel A: Nonexposed Portfolio				
(-01, 01)	-0.368	-0.224	-0.940	-0.537
(-02, 02)	0.598	0.280	-1.377	-0.605
(-05, 05)	1.357	0.419	-3.358	-0.974
(-10, 10)	-1.285	-0.278	-4.244	-0.864
(-20, 20)	-6.482	-0.956	-4.527	-0.627
(-03, -03)	0.490	0.521	-0.570	-0.569
(-02, -02)	0.245	0.260	0.473	0.471
(-01, -01)	0.020	0.020	0.024	0.024
(00, 00)	-0.499	-0.530	-0.084	-0.084
(01, 01)	0.112	0.118	-0.880	-0.877
(02, 02)	0.721	0.766	-0.910	-0.908
(03, 03)	-1.021	-1.073	-1.138	-1.124
Panel B: Exposed Portfolio				
(-01, 01)	-0.838	-1.233	-1.093	-1.155
(-02, 02)	-1.110	-1.257	-1.310	-1.064
(-05, 05)	-2.672	-1.997**	-2.065	-1.108
(-10, 10)	-4.509	-2.365*	-2.319	-0.873
(-20, 20)	-8.706	-3.117*	-4.810	-1.232
(-03, -03)	0.287	0.738	-0.028	-0.051
(-02, -02)	-0.723	-1.854	0.028	0.051
(-01, -01)	-0.786	-2.014**	-0.737	-1.355
(00, 00)	0.334	0.859	-0.194	-0.358
(01, 01)	-0.386	-0.992	-0.162	-0.299
(02, 02)	0.450	1.157	-0.245	-0.452
(03, 03)	-0.495	-1.259	-0.382	-0.697

* $p < 0.01$ ** $p < 0.05$

Neither the equally-weighted nor the value-weighted nonexposed portfolio data presented in Panel A show any negative abnormal performance. The value-weighted nonexposed portfolio does show a positive abnormal return of 4.389 percent (t = 2.399) on day 2. The equally-weighted exposed portfolio data presented in Panel B show negative abnormal returns over the (-5, 5) and (-20, 20) intervals. The t-ratios are -2.288 and -2.507, respectively. When this portfolio is value-weighted, there are no statistically significant abnormal returns over any day or interval.

Drawing on our sample of 33 securities, daily and cumulative abnormal returns to nonexposed and exposed portfolios are presented in Table 3. Neither the equally-weighted nor the value-weighted nonexposed portfolio data presented in Panel A show any abnormal performance, positive or negative. The equally-weighted, exposed portfolio data presented in Panel B show negative abnormal returns over the (-5, 5), (-10, 10), and (-20, 20) intervals. The t-ratios are -1.997, -4.509, and -8.706, respectively. When this portfolio is value-weighted, the results are similar to the Fields et al. sample: there are no statistically significant abnormal returns over any day or interval.

HOTS Results

The five firms listed in Heard on the Street on day -1 (Chubb, Fireman's Fund, Ohio Casualty, Safeco, and Zenith National) earned an average abnormal return of 2.025 percent (t = 2.653) on that day (see Table 4). Also the not-HOTS portfolio (four firms with equal or greater relative exposure—Amwest Insurance Group, Home Group, Orion Capital, and Progressive) earned a 14.461 percent abnormal return (t = 2.115) over the (-20, 20) interval and a 2.332 percent abnormal return (t = 2.456) on day 2.

Table 4
Daily and Cumulative Abnormal Returns for the
HOTS and Not-HOTS Equally-Weighted Portfolios

Interval	*HOTS*	*t-test*	*Not-HOTS*	*t-test*	*t-test*[a]
(-01, 01)	-1.999	-1.503	-0.110	-0.066	-0.904
(-02, 02)	-1.364	-0.789	-4.168	-1.934	1.032
(-05, 05)	-3.808	-1.454	-3.464	-1.061	-0.083
(-10, 10)	-3.928	-1.052	-5.915	-1.271	0.338
(-20, 20)	-8.252	-1.505	-14.461	-2.115*	0.721
(-03, -03)	-0.238	-0.313	0.421	0.444	-0.552
(-02, -02)	0.051	0.068	-1.726	-1.815	1.484
(-01, -01)	-2.025	-2.653*	-0.035	-0.037	-1.660
(00, 00)	0.788	1.035	0.299	0.315	0.409
(01, 01)	-0.762	-1.000	-0.374	-0.394	-0.324
(02, 02)	0.582	0.766	-2.332	-2.456*	2.438*
(03, 03)	-0.525	-0.683	0.418	0.436	-0.781

[a] The t-test in this column is a test on the difference in abnormal returns between the HOTS and not-HOTS portfolios.
* $p < 0.01$

We further examine the relative performance of the HOTS and not-HOTS portfolios with a t-test on the difference in abnormal returns. These results (reported in the last column of Table 4) show that there was not a statistically significant difference in abnormal returns on day -1 (the day of the HOTS column) or over the interval (-20, 20). However, there is a significant difference in abnormal returns on day 2, the first trading day on which the referendum results were unambiguous.

Cross-Section Results

Results from our cross-sectional regression are presented in Table 5. We find no relationship between abnormal changes in equity value and California premiums written for any day or interval. We included earnings before tax since it may have been possible that a more profitable firm may withstand the impact of Proposition 103 better than a less profitable firm. Contrary to Fields et al. (1990), our results indicate no relationship between abnormal changes in equity value and earnings before tax.[10]

Table 5
Cross-Sectional Regression Results

Interval	*Intercept*	*t-test*	*Auto*	*t-test*	*Other*	*t-test*	*EBT*	*t-test*
-20, 20	-46,097	-0.030	-1.636	-0.031	0.216	0.011	0.004	0.000
-10, 10	-22,487	-0.042	-0.750	-0.041	0.002	0.000	0.122	0.012
-05, 05	-20,578	-0.105	-0.700	-0.104	0.163	0.062	-0.125	-0.032
-03, 03	-4,194	-0.071	-0.403	-0.199	0.034	0.044	-0.120	-0.102
-03, -03	-62	-0.002	-0.361	-0.387	0.014	0.039	-0.079	-0.146
-02, -02	3,229	0.606	0.091	0.505	-0.057	-0.816	-0.007	-0.069
-01, -01	-4,458	-0.837	-0.112	-0.617	0.053	0.756	0.028	0.271
00, 00	3,419	0.646	-0.310	-1.720	-0.041	-0.591	0.038	0.363
01, 01	2,180	0.422	-0.018	-0.100	0.035	0.512	-0.128	-1.236
02, 02	-5,661	-1.094	-0.033	-0.188	0.021	0.300	0.011	0.105

Note: Results are from the cross-sectional regression $\text{Value}_{js} = b_0 + b_1\,\text{AUTO}_j + b_2\,\text{OTHER}_j + b_3\,\text{EBT}_j + \varepsilon_{js}$, where the dependent variable is the abnormal change in the value of the firm's common stock. AUTO = the value of direct premiums written in California for lines of automotive insurance potentially affected by Proposition 103. OTHER = the value of nonautomotive direct premiums written in California for other lines of insurance potentially affected by Proposition 103. EBT = earnings before tax for all lines except accident and health. The test statistics are corrected to account for contemporaneous correlations and heteroskedasticity among the regression error terms.

Discussion

General Sample Results

None of the evidence presented in this article supports the contagion effect hypothesis. Portfolios of nonexposed securities did not earn negative abnormal returns, whether they are equally-weighted or value-weighted. Rather, the data presented in Tables 2, 3, and 4 illustrate the importance of selecting appropriate portfolio weights. The exposed portfolios, when equally weighted, earned negative abnormal returns over the longer intervals, especially the (-20, 20) interval. This result is not sample specific; it is obtained for our sample of 28 exposed securities and the Fields et al. sample of 31 exposed securities.

However, when these portfolios are value-weighted, there is no evidence of negative abnormal performance; the abnormal returns to the value-weighted portfolios are substantially smaller over the (-20, 20) interval and are not sta-

[10] We also tested formally for a size effect by including the market value of common equity in a cross-sectional regression. This regression excluded earnings before tax since multicollinearity tests indicated that earnings before tax and size were highly correlated. No statistically significant size effect was found; thus, we did not include these results here.

tistically significant.[11] The results from value-weighted portfolios indicate that Proposition 103 had little impact on the aggregate (dollar) value of the insurance industry; this conclusion is supported by the cross-sectional regression. Direct premiums written, in automobile and other lines, are not correlated with the abnormal change in the value of insurers for any day or interval.

HOTS Results

Comparing our results from the HOTS portfolio to the matching portfolio (a portfolio that has greater mean California exposure but eluded being reported in HOTS), we find no evidence of market inefficiency. Individual firm exposure to California (i.e., dollar value of premiums written by line of insurance) was publicly available from Best's Executive Data Service long before the Proposition 103 vote. In an efficient market, the HOTS column should have provided no new information to investors.

The HOTS portfolio does show a negative abnormal return on the HOTS publication day, but the not-HOTS portfolio does not show any negative abnormal return on that day. However, a t-test on the difference in abnormal returns is not rejected at the five percent level. Thus, we reject the null hypothesis (of no abnormal returns) for the HOTS portfolio, fail to reject the null hypothesis for the not-HOTS portfolio, and fail to reject the null hypothesis that there is no difference in abnormal returns. A consistent interpretation of these results is that either the HOTS test contains a Type II error, or the other two tests contain a Type I error.[12] Thus, a conservative interpretation of our results is that we fail to obtain evidence that the market reacted to the HOTS column on day -1.

Conclusion

This article uses stock market data to measure the wealth effects of California's Proposition 103 referendum. A sample of 33 securities is partitioned into exposed and nonexposed portfolios. Portfolios are a solution to the

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[11] The effect of value-weighing can be illustrated by comparing two firms, American International Group (AIG) and AmWest Insurance (AMW). The former, a multiline multinational firm that sells substantial amounts of life insurance and also has significant international sales, had a capitalized value of about $2 billion at the end of 1987. The latter specializes in only one line of insurance, surety bonds, earned about 75 percent of its revenue from California in 1987 and had a capitalized value of about $200 million at the end of 1987. When these firms' stock returns are equally weighted, AMW's abnormal return (about 25 percent) dominates AIG's (about 4 percent, not significant); with value weighing, AIG's return will receive about 10 times the weight of AMW's.

[12] The nature of the problem is that our results show that on day -1, HOTS is not different than zero and not-HOTS is different than zero, yet not-HOTS is not different than HOTS. In the absence of sampling error, this is a contradiction. Thus, either we incorrectly rejected the null hypothesis for HOTS (a Type I error) or incorrectly failed to reject the null hypothesis (a Type II error) for the other two tests.

"event clustering" problem in studies of regulatory events; our tests use both equally-weighted and value-weighted portfolios.

None of the evidence presented in this article supports the contagion effect hypothesis. Portfolios of nonexposed securities did not earn negative abnormal returns, whether they are equally-weighted or value-weighted. The evidence supports the dribs and drabs hypothesis only when exposed portfolios are equally-weighted: negative abnormal returns are obtained over the interval from 20 days before to 20 days after election day. Value-weighted portfolios, however, did not earn negative abnormal returns.

We also examine the market response to the information released in the November 7, 1988, *Wall Street Journal* Heard on the Street column (one trading day prior to the referendum). The performance of a portfolio of four firms mentioned in this column was no different than that of another portfolio with equal or greater exposure. Thus, we concluded that the HOTS column provided no new information to investors.

The results from value-weighted portfolios indicate that Proposition 103 had little impact on the aggregate (dollar) value of the insurance industry. Cross-sectional regressions indicate that exposure, income, and size measures are not related to changes in firm value. Smaller firms, less profitable firms, and firms with relatively large California exposure were not disproportionately affected by Proposition 103.

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