Appendixes

Appendix A

Accident Reporting under the Negligence System

It is extremely difficult to get comparable estimates of workers' losses due to nonfatal accidents because of substantial differences in the reporting of such accidents under negligence liability and workers' compensation. Comparisons of the number of nonfatal accidents reported to bureaus of labor before and after workers' compensation show that two to ten times as many serious nonfatal accidents were reported after workers' compensation was introduced. By contrast, fatal accident reporting did not spike upward after workers' compensation was adopted.

The differences in reporting under the two systems may have been caused by the disparate benefits the injured expected to receive under the two regimes. Under negligence liability workers had little incentive to report accidents to employers if they felt that there was little chance of receiving compensation. By reporting such an accident, the worker would be signaling to the employer that he was "accident prone," or worse yet that he was a "troublesome" employee. Either label might lead to negative repercussions. Similarly, employers had little incentive to reveal accidents to bureaus of labor unless they knew that the worker was going to press the issue with a suit or a request for compensation. By reporting relatively more accidents, the employer would have raised the factory inspectors' awareness of problems at his workplace, thus raising the risk of inspections and possible fines. Under workers' compensation, however, all workplace accidents were potentially compensable, so workers had far more incentive to report them and pursue their entitled benefits.[1]

In the course of examining the accident reporting issue, we have collected evidence from a variety of sources. This information is summarized below.

Accident Reporting at the Stonega Coke and Coal Company

One sign of the accident reporting problem within a single firm can be seen by studying the records of the Stonega Coke and Coal Company, which was primarily located in Virginia. The *Annual Reports of the Operations Department* reported a doubling in the rate of serious and slight accidents once workers' compensation was enacted. The number of serious accidents reported rose from 29 per million tons of coal produced during the period 1912 to 1918 (before workers' compensation in Virginia) to 61 per million tons in the period 1919 to 1925 (when workers' compensation was in effect). The number of slight accidents rose from 111 per million tons in 1912–1918 to 232 per million tons in 1919–1925. The reporting of fatal accidents was not nearly as different under the two legal systems. The number of fatalities per million tons held roughly constant at 0.005 per million tons in 1912–1918 and 0.004 per million tons in 1919–1925.[2]

Illinois Coal Mines

Between 1897 and 1906 the *Annual Coal Reports* of the Illinois Bureau of Labor Statistics reported an average of 118.7 fatal accidents per year and 492.4 nonfatal accidents during a period when the average number of employees rose from 33,788 to 61,988 men. In the report of the Illinois Industrial Commission for the calendar year 1919, by which time workers' compensation had been in effect for several years, coal mining experienced 145 deaths and 7,652 compensable injuries while employing approximately 87,000 men.[3]

Iowa Coal Mines

From 1906 to 1910, Iowa coal mines reported an average of 35.4 fatal accidents per year and 108.2 nonfatal accidents per year, while employing an average of 17,491 workers.[4] During the two-year period ending 30 June 1914, when workers' compensation was in effect in the state, the Iowa Industrial Commissioner (1914, 48–49) reported an average of 33 fatal accidents per year, 2 permanent total disability accidents, 22.5 permanent partial disability accidents, 216 temporary disability accidents lasting longer than two weeks, and 684 minor injuries.

Minnesota

In 1909 the Minnesota legislature required employers to report their accidents to the Minnesota Department of Labor and Industries. Extant letters between the department and Minnesota employers show that the department took its responsibility of investigating accidents in the state very seriously. A clipping department within the department scoured the state's newspapers looking for evidence of industrial accidents. If it found a mention of an accident, without receiving notification from the company, a letter of inquiry was generated. In general, the department believed that large numbers of firms were underreporting accidents. In quite a few cases the department sent out incredulous letters to firms that did not send any information about accidents. The archives of the department at the Minnesota State Historical Society in St. Paul contain many letters like the one of 29 September 1911, in which Labor Commissioner Houk sent a letter to the Parker and Topping Foundry asking, "Can it be possible that no accidents have occurred in that length of time [year ending 31 July 1911] to the people in the employ of said company?"

Ohio

Ohio is a valuable state to study the differences in reporting because it was quick to establish an industrial commission to oversee workers' compensation and it had a strong factory inspection law prior to the legislation. The same dramatic increase in the reporting of serious and minor injuries occurred. Just prior to the passage of the state's workers' compensation law, Ohio in 1910 tightened the rules for reporting accidents. As a result there was a substantial increase in the reporting of nonfatal accidents from 1910 to 1911: the number of serious nonfatal accidents rose from 795 to 1,481. Once workers' compensation was established and firmly in place, the number of serious accidents took another sharp turn upward from 1,481 to 7,344. When we compare the figures from 1910 to 1912, the number of fatal accidents in each industry changed very little, from 163 to 195. On the other hand, the number of serious accidents (disability lasting more than seven days) rose nearly tenfold from 795 to 7,344, while the number of minor accidents (disability of seven days or less) rose from 1,499 to 5,161.[5] The accident statistics from the factory inspection reports pale in comparison with the annualized number of industrial accidents reported under workers' compensation for the eighteen-month period from 1 January 1914 to 30 June 1915 (we annualized the report by dividing the number of accidents by 1.5). Because the workers' compensation report included a number of industries not listed in the factory inspection reports, we compare the situations where we believe the industrial categories matched up in the two reports. Typically, the number of fatal accidents reported is similar in both reports. There are, however, dramatic differences in the number of serious and minor accidents reported. The rise in reported accidents is at least double in every category and in some cases there is a tenfold increase. The sheer difference in accident reporting under the two systems suggests that it is extremely difficult to compare the compensation of nonfatal accidents before and after workers' compensation. In appendix C, however, we attempt such a comparison using two different methods. One method uses workers' reports of their accident compensation, which is surely to be plagued with the underreporting problem, and the other uses data from employers' reports of their accident compensation.

Appendix B

Workers' Compensation Benefits and the Construction of the Expected Benefits Variable

Workers' compensation laws established parameters for the payment of benefits to workers injured on the job and the families of workers who were killed in workplace accidents. Injured workers typically received payments of up to two-thirds of their weekly wage each week for the period of the disability, while the families of fatalities typically received weekly payments for a period of up to eight years. The parameters for compensation varied across states and by type of accident. In this appendix we describe the various payment parameters and show how we construct the expected benefits variable that is discussed in chapters 3 and 7.

Fatal Accident Payments

Table B.1 summarizes the provisions related to fatal accidents at the end of the first year of operation of each state's workers' compensation law.[6] In many states the percentage of the wage replaced varied in proportion to the number of family members. To aid comparability, the calculations in all of the benefit calculations are based on the assumption that the deceased's family consisted of a widow age thirty-five, a child age ten, and a second child age eight. We also assumed that the deceased's widow did not remarry and lived another thirty years. The New Jersey law of 1911, which established a pattern followed by many states, offered this family weekly payments equal to 45 percent of the worker's wage for up to three hundred weeks. Weekly benefits could not be lower than five dollars a week or higher than ten dollars a week, and the sum of the payments could not exceed three thousand dollars or be lower than fifteen hundred dollars. In addition, New Jersey offered the family one hundred dollars

Table B.1 **Fatal Accident Compensation in the First Year of Operation for States Adopting Workers' Compensation before 1930**

State	First Year in Effect[a]	National Weekly Wage	Present Value of Stream of Fatal Benefits	Ratio of Present Value of Fatal Benefits to Annual Earnings	Funeral Expenses	Weekly Payment Replaced This Percent of Weekly Wage	Maximum Number of Weeks	Minimum Weekly Payment	Maximum Weekly Payment	Maximum Total Payout	Minimum Total Payout
Cal.	1911	14.83	2,149	2.9	0	—	156	6.41	32.05	5,000	1,000
N.J.	1911	14.83	1,840	2.48	100	0.45	300	5	10	3,000	1,500
Nev.	1911	14.83	2,314	3.12	0	—	—	—	—	3,000	2,000
Wash.	1911	14.83	4,511	6.08	75	—	—	6.9	6.9	—	0
Wis.	1911	14.83	2,719	3.67	0	—	—	7.21	14.42	3,000	1,500
Ill.	1912	15.34	2,548	3.32	0	0.5	—	0	200	3,500	1,500
Kan.	1912	15.34	2,068	2.7	0	0.5	—	6	15	3,600	1,200
Mass.	1912	15.34	1,999	2.61	0	0.5	300	4	10	3,000	1,200
Md.	1912	15.34	2,548	3.32	0	—	—	—	—	—	1,000
Mich.	1912	15.34	1,999	2.61	0	0.5	300	4	10	3,000	1,200
Ohio	1912	15.34	2,771	3.61	0	0.67	312	0	200	3,400	1,500
R.I.	1912	15.34	1,999	2.61	0	0.5	300	4	10	3,000	1,200
N.Y.	1912	15.34	2,302	3	0	—	150	0	200	3,000	0
Ariz.	1913	15.82	2,626	3.32	0	0.5	400	0	200	4,000	0
Minn.	1913	15.82	2,161	2.73	100	0.5	300	6	10	3,000	1,800
Neb.	1913	15.82	2,450	3.1	100	0.5	350	5	10	3,500	1,750
Tex.	1913	15.82	2,888	3.65	0	0.6	360	5	15	5,400	1,800
W. Va.	1913	15.82	3,981	5.03	75	—	—	6.9	6.9	—	0
Conn.	1914	15.84	2,235	2.82	100	0.5	312	5	10	3,120	1,560
Iowa	1914	15.84	2,164	2.73	100	0.5	300	5	10	3,000	1,500
N.Y.	1914	15.84	5,131	6.48	100	0.5	—	0	11.5	—	0
Or.	1914	15.84	6,541	8.26	100	—	—	9.67	9.67	—	—
Colo.	1915	15.79	2,127	2.69	0	0.5	312	0	8	2,500	1,000
Ind.	1915	15.79	2,363	2.99	100	0.55	300	5.5	13.2	5,000	1,500
La.	1915	15.79	2,157	2.73	100	0.5	300	3	10	3,000	900
Mont.	1915	15.79	2,696	3.41	75	0.5	400	6	10	4,000	2,400
Okla.	1915	15.79	—	—	—	—	—	—	—	—	—
Vt.	1915	15.79	1,528	1.94	75	0.4	260	2	10	3,500	520
Wyo.	1915	15.79	1,890	2.39	50	—	—	—	—	2,000	—

(continued)

Table B.1 (continued)

State	First Year in Effect[a]	National Weekly Wage	Present Value of Stream of Fatal Benefits	Ratio of Present Value of Fatal Benefits to Annual Earnings	Funeral Expenses	Weekly Payment Replaced This Percent of Weekly Wage	Maximum Number of Weeks	Minimum Weekly Payment	Maximum Weekly Payment	Maximum Total Payout	Minimum Total Payout
Ky.	1916	17.57	3,345	3.81	75	0.65	335	5	12	4,000	1,675
Me.	1916	17.57	2,289	2.61	0	0.5	300	4	10	3,000	1,200
Pa.	1916	17.57	2,389	2.72	100	0.5	300	5	10	3,000	1,500
N.M.	1917	19.87	2,640	2.66	50	0.5	300	5	15	4,500	1,500
S.D.	1917	19.87	2,604	2.62	0	0.5	—	—	200	3,000	1,650
Utah	1917	19.87	3,096	3.12	150	0.55	312	0	15	4,500	2,000
Del.	1918	24.01	2,670	2.22	100	0.45	270	4.5	13.5	3,645	1,215
Idaho	1918	24.01	4,085	3.4	100	0.55	400	6	12	4,800	2,400
N.D.	1919	27.67	10,004	7.23	100	0.55	—	9.9	16.5	—	—
Tenn.	1919	27.67	3,753	2.71	100	0.5	400	5	11	4,400	2,000
Va.	1919	27.67	2,706	1.96	100	0.5	300	5	10	4,000	1,500
Ala.	1920	33.81	3,749	2.22	100	0.6	300	5	14	5,000	1,500
Ga.	1921	30.77	2,706	1.76	100	0.5	300	5	10	4,000	1,500
Mo.	1927	33.23	5,362	3.23	150	0.67	300	6	20	6,000	1,800
N.C.	1929	34.08	5,333	3.13	200	0.6	333	7	18	6,000	2,450

Notes: The details of the laws come from Clark and Frincke (1921), Hookstadt (1918, 1919, 1920, 1922), Jones (1927), and U.S. Bureau of Labor Statistics Bulletin Nos. 126 (1913b), 203 (1917), 243 (1918), 332 (1923), 423 (1926b), and 496 (1929), with supplementation from the session laws for the individual states. The information comes from the first year that the law was in effect. The present value of fatal accident benefits as a percentage of annual earnings is calculated based on the national average weekly wage in manufacturing. For the years prior to 1927, the average weekly wage was calculated as average weekly hours times hourly earnings from Paul Douglas's series (series D-765 times series D-766 in U.S. Bureau of the Census 1975, 168). We then interpolated values for the years 1927 through 1930 by running a regression of the weekly wage measure on Stanley Lebergott's measure of average annual earnings per full-time employee for manufacturing (series D-740 in U.S. Bureau of the Census 1975, 166) divided by fifty-two. The interpolated values for 1927 through 1930 are equal to 2.021638 + 1.080317 times the Lebergott measure of weekly wages. For years after 1927, the average weekly wage is from the U.S. BLS series (series D-802, U.S. Bureau of the Census 1975, 169–70). Given the weekly earnings, we calculated the present value of the stream of payments allowed by the workers' compenstaion statute using continuous discounting and a discount rate of 5 percent. The worker was assumed to have had a wife age thirty-five and two children age eight and ten. In some states there was an overall maximum payment that was binding. We assumed the families were paid the weekly amount until the time that the maximum total payment (not discounted) was reached; therefore, time in the discounting formula in those states was equal to the maximum total payment divided by the weekly payment. In Nevada, New York, Oregon, Washington, and West Virginia the payments were for the life of the spouse or until remarriage. We assumed that the spouse lived thirty more years without remarrying. Payments to dependents were stopped when they reached the state's defined age of adulthood. Annual earnings were defined as the average manufacturing weekly wage times fifty weeks.

[a]In quite a few states the first year of operation was the year after the law was adopted. Maryland (for miners in 1902), New York (1910), Montana (for miners in 1909), and Kentucky (1914) passed earlier laws that were declared unconstitutional. Maryland also passed a law specific to miners in 1910, while New York passed a voluntary compensation law and a compulsory compensation law in 1910. The compulsory law was declared unconstitutional, and the voluntary law was little used. New York passed a new compulsory law in 1913 after the state constitution was amended.

for funeral expenses. Several states deviated from the New Jersey pattern. Washington, West Virginia, and Oregon established fixed weekly payments. Oklahoma did not pay workers' compensation benefits for fatal accidents. Nevada, California, Maryland, and Kansas chose the total payment level based on three times annual earnings, while Wisconsin, Illinois, and South Dakota based it on four times annual earnings. Wyoming paid a fixed lump sum for fatal accidents based on the number of survivors. Some states chose not to limit either the number of weeks of payment or the maximum total payout.

Nearly all states focused on paying the money weekly over an extended period of time. Most states legally allowed for accident victims to be paid a lump sum after an appeal, usually using a discount rate between 3 and 6 percent to determine the size of the lump sum. Our impression, however, is that the administrators of workers' compensation discouraged the payment of a lump sum.[7] To allow easier comparisons of the fatal accident parameters in each of the states, we have calculated the present value of the stream of weekly payments prescribed by the workers' compensation acts using a discount rate of 5 percent. We assumed that the worker was paid the national average weekly wage at the time of the accident. As an example, the present value of the stream of payments for the New Jersey family in 1911 was $1,840.

The present values for the states adopting later in the period appear artificially high in comparison with those for states adopting earlier because the national average weekly wage more than doubled over the time period. Therefore, we have also calculated a ratio of the present value of fatal benefits to annual earnings, which were calculated as fifty weeks times the national weekly wage. In terms of fatal accident payments, the least generous states were Georgia, Vermont, and Virginia, each with present values that replaced less than two years' income. Generally, these states had relatively low maximum weekly payments. The states with present values that replaced more than five times annual incomes—Washington, New York, Oregon, West Virginia, and South Dakota—generally did not limit the length of time for fatal accident payments or impose maximum total payments. The relative generosity of these high-benefit states is affected more by the discount rate than in the rest of the states. For example, raising the discount rate from 5 percent to 10 percent lowered the ratio from 6.08 to 4 in Washington in 1911, while lowering the ratio in New Jersey from 2.48 to 2.19.

Nonfatal Accident Payments

Another major component of workers' compensation was the benefits paid for nonfatal accidents, which were far more common than fatal accidents. Nonfatal accidents were separated into three major categories: permanent total disability (e.g., full paralysis), permanent partial disability (e.g., loss of a hand), and temporary disability (e.g., broken leg). In most states, the compensation for nonfatal accidents followed the general pattern of that for fatal accidents. During his disability the worker was paid a percentage of his weekly wage, subject to statutory minimum and maximum payments, for a maximum number of weeks. Each state established a waiting period, ranging from three days to two weeks from the date of the accident, during which time no accident compensation was paid. Injured workers who were out of work for a period less than the waiting period received no compensation. In some states at the time of introduction (and later in most states), workers with more serious injuries that lasted beyond four to eight weeks were able to collect compensation forgone during the waiting period, retroactively. The rules for permanent total disability payments, say for full paralysis, were similar to the rules for fatal accident payments (without the funeral expense payments) in nearly every state.

To show how the various states compensated temporary total disability, table B.2 shows the waiting period, the retroactive pay feature, the percentage of the wage replaced, and the minimum and maximum weekly payments. For example, a worker injured for five weeks in New Jersey in 1911 would have started receiving payments for his injury after two weeks. For the remaining three weeks of his injury he was paid half of his weekly wage, and the payment could not be lower than $5 or higher than $10. A worker receiving the national weekly wage of $14.83 would have been paid $7.415 per week for three weeks for a total of $22.245. The present value of this stream of income using continuous discounting and a discount rate of 5 percent was $22.11, which was 1.49 times the national weekly wage. Comparison of all the states in table B.2 in their first year of operation shows that North Dakota in 1919 was the most generous for temporary total disability at 3.31 times the weekly wage, while Missouri and Oregon had ratios of almost 3 times the weekly wage. The three states combined relatively generous maximums with either no waiting period or the payment of retroactive benefits after a relatively short period of time. It is important to note, however, that states starting operation later generally were adopting benefit parameters that were similar to the parameters in other states at that time.

Permanent partial disabilities ranged from the loss of a finger to the loss of a leg. It was anticipated that someone with a permanent partial disability might be able to continue to work, although the type of work depended on the disability. Table B.3 shows the rules for compensating people who lost a hand. Among the states that adopted workers' compensation earlier, the typical pattern was to pay the worker as if he were totally disabled for a period of time and then begin paying the worker a partial disability payment. In New Jersey in 1911, the worker was paid as if he were totally disabled for 15 weeks, which appeared to be common in many states, and

Table B.2 **Workers' Compensation for a Five-Week Spell of Disability in the First Year of Operation**

State	Year	National Weekly Wage	Present Value of Five-Week Disability Pay	Ratio of Present Value of Disability Pay to Weekly Wage	Waiting Period in Days	Retroactive Pay for Waiting Period after This Number of Weeks	Weekly Payment Replaced This Percent of Weekly Wage	Minimum Weekly Payment	Maximum Weekly Payment
Cal.	1911	14.83	38.35	2.59	7	—	0.65	4.17	20.83
N.J.	1911	14.83	22.11	1.49	14	—	0.5	5	10
Nev.	1911	14.83	31.6	2.13	10	—	0.6	0	—
Wash.	1911	14.83	38.34	2.59	1.5	—	0.6	8.05	8.05
Wis.	1911	14.83	37.29	2.51	7	—	0.65	4.69	9.38
Ill.	1912	15.34	30.52	1.99	7	—	0.5	5	12
Kan.	1912	15.34	22.88	1.49	14	—	0.5	6	15
Mass.	1912	15.34	22.88	1.49	14	—	0.5	4	10
Md.	1912	15.34	15.26	0.99	7	—	0.5	0	—
Mich.	1912	15.34	22.88	1.49	14	8	0.5	4	10
Ohio	1912	15.34	36.58	2.38	7	—	0.67	5	12
R.I.	1912	15.34	22.88	1.49	14	—	0.5	4	10
N.H.	1912	15.34	22.88	1.49	14	—	0.5	0	10
Ariz.	1913	15.82	23.58	1.49	14	—	0.5	0	—
Minn.	1913	15.82	23.58	1.49	14	—	0.5	6	10
Neb.	1913	15.82	23.58	1.49	14	8	0.5	5	10
Tex.	1913	15.82	37.75	2.39	7	—	0.6	5	15
W. Va.	1913	15.82	28.31	1.79	7	—	0.5	4	8
Conn.	1914	15.84	23.62	1.49	14	—	0.5	0	10
Iowa	1914	15.84	23.62	1.49	14	—	0.5	5	10
N.Y.	1914	15.84	31.46	1.99	14	—	0.67	5	15
Or.	1914	15.84	47.28	2.98	0	—	0.6	10.81	10.81
Colo.	1915	15.79	15.68	0.99	21	—	0.5	5	8
Ind.	1915	15.79	25.89	1.64	14	—	0.55	5.5	13.2
La.	1915	15.79	23.53	1.49	14	—	0.5	3	10

(*continued*)

Table B.2 (continued)

State	Year	National Weekly Wage	Present Value of Five-Week Disability Pay	Ratio of Present Value of Disability Pay to Weekly Wage	Waiting Period in Days	Retroactive Pay for Waiting Period after This Number of Weeks	Weekly Payment Replaced This Percent of Weekly Wage	Minimum Weekly Payment	Maximum Weekly Payment
Mont.	1915	15.79	23.53	1.49	14	—	0.5	6	10
Okla.	1915	15.79	23.53	1.49	14	—	0.5	6	10
Vt.	1915	15.79	23.53	1.49	14	—	0.5	3	12.5
Wyo.	1915	15.79	24.64	1.56	10	—	1	6.94	6.94
Ky.	1916	17.57	34.05	1.94	14	—	0.65	5	12
Me.	1916	17.57	26.19	1.49	14	—	0.5	4	10
Pa.	1916	17.57	26.19	1.49	14	—	0.5	5	10
N.M.	1917	19.87	19.74	0.99	21	—	0.5	5	10
S.D.	1917	19.87	29.63	1.49	14	8	0.5	6	12
Utah	1917	19.87	38.81	1.95	10	—	0.55	7	12
Del.	1918	24.01	29.82	1.24	14	—	0.5	4	10
Idaho	1918	24.01	47.73	1.99	7	—	0.55	6	12
N.D.	1919	27.67	91.7	3.31	7	1	0.67	6	20
Tenn.	1919	27.67	32.8	1.19	14	6	0.5	5	11
Va.	1919	27.67	29.82	1.08	14	—	0.5	5	10
Ala.	1920	33.81	59.69	1.77	14	4	0.5	5	12
Ga.	1921	30.77	35.78	1.16	14	7	0.5	6	12
Mo.	1927	33.23	99.48	2.99	3	4	0.67	6	20
N.C.	1929	34.08	89.53	2.63	7	4	0.6	7	18

Notes: See table B.1 for the sources to the workers' compensation laws. The information comes from the first year that the law was in effect. The national weekly wage is the same as in table B.1. The present value of the weekly payments was calculated using continuous discounting at a rate of 5 percent. The retroactive pay feature worked as in the case of Alabama in 1920. The waiting period in Alabama was two weeks, which meant that a worker had to be injured for more than two weeks to receive any benefits. If he was injured for three weeks, then the worker would receive a payment only for the third week of the injury. If he was disabled longer than four weeks, however, he received a retroactive payment that paid him benefits for the first two weeks of the injury. For further details see the sources to table B.1 and the text of appendix B.

Table B.3 **Workers' Compensation for the Loss of a Hand in the First Year of Operation**

State	First Year	Present Value of Hand Disability Pay	Ratio of Present Value to Annual Earnings	Weekly Payment Replaced This Percent of Weekly Wage	Weeks of Payment	Minimum Weekly Payment	Maximum Weekly Payment	Standard Payment Method	Hand Payments Start after Period of Total Disability Payments
Cal.	1911	1,936.53	2.61	0.65	200	4.16	20.83	Weekly	Yes
N.J.	1911	1,116.56	1.51	0.5	150	5	10	Weekly	Yes
Nev.	1911	631.36	0.85	0.15	260	0	No max	Weekly	Yes
Wash.	1911	1,263.16	1.7	—	—	—	—	Lump sum	No
Wis.	1911	2,283.19	3.08	0.65	—	—	—	Weekly	Yes
Ill.	1912	1,364.56	1.78	0.5	—	0	12	Weekly	Yes
Kan.	1912	1,312.72	1.71	0.5	—	3	12	Weekly	Yes
Mass.	1912	2,925.56	3.81	0.5	50	4	10	Weekly	Yes
Md.	1912	1,456.05	1.9	0.5	—	0	No max	Weekly	Yes
Mich.	1912	1,069.61	1.39	0.5	150	4	10	Weekly	No
Ohio	1912	1,298.68	1.69	0.666	—	5	12	Weekly	Yes
R.I.	1912	1,415.06	1.84	0.5	50	4	10	Weekly	Yes
N.H.	1912	1,041.26	1.36	0.5	—	—	—	Weekly	Yes
Ariz.	1913	2,584.03	3.27	0.5	—	—	—	Weekly	Yes
Minn.	1913	1,102.61	1.39	0.5	150	6	10	Weekly	No
Neb.	1913	1,271.41	1.61	0.5	175	5	10	Weekly	No
Tex.	1913	2,180.24	2.76	0.6	50	5	15	Weekly	Yes
W. Va.	1913	1,030.13	1.3	0.5	156	4	8	Weekly	No
Conn.	1914	1,145.3	1.45	0.5	156	0	No max	Weekly	No
Iowa	1914	1,104.35	1.39	0.5	150	5	10	Weekly	No
N.Y.	1914	2,290.35	2.89	0.666	244	5	20	Weekly	No
Or.	1914	1,744.71	2.2	0.6	330	5.75	5.753	Weekly	Yes
Colo.	1915	778.87	0.99	0.5	104	0	8	Weekly	No
Ind.	1915	1,210.38	1.53	0.55	150	5.5	13.2	Weekly	No
La.	1915	1,100.35	1.39	0.5	150	3	10	Weekly	No

(*continued*)

Table B.3 (continued)

State	First Year	Present Value of Hand Disability Pay	Ratio of Present Value to Annual Earnings	Weekly Payment Replaced This Percent of Weekly Wage	Weeks of Payment	Minimum Weekly Payment	Maximum Weekly Payment	Standard Payment Method	Hand Payments Start after Period of Total Disability Payments
Mont.	1915	1,100.35	1.39	0.5	150	6	10	Weekly	No
Okla.	1915	1,433.25	1.82	0.5	200	6	10	Weekly	No
Vt.	1915	1,120.53	1.42	0.5	140	0	10	Weekly	Yes
Wyo.	1915	800	1.01	—	—	—	—	Lump sum	No
Ky.	1916	1,592.03	1.81	0.65	150	5	12	Weekly	No
Me.	1916	1,658.69	1.89	0.5	125	4	10	Weekly	Yes
Pa.	1916	1,412.12	1.61	0.5	175	5	10	Weekly	No
N.M.	1917	1,140.36	1.15	0.5	110	5	10	Weekly	Yes
S.D.	1917	1,515.76	1.53	0.5	150	6	12	Weekly	Yes
Utah	1917	1,524.79	1.53	0.55	150	0	12	Weekly	No
Del.	1918	1,463.03	1.22	0.5	158	4	10	Weekly	No
Idaho	1918	1,674.61	1.39	0.55	150	0	12	Weekly	No
N.D.	1919	4,234.79	3.06	0.666	260	0	20	Weekly	No
Tenn.	1919	1,533.59	1.11	0.5	150	5	11	Weekly	No
Va.	1919	1,394.17	1.01	0.5	150	5	10	Weekly	No
Ala.	1920	1,673	0.99	0.5	150	5	12	Weekly	No
Ga.	1921	1,673	1.09	0.5	150	6	12	Weekly	No
Mo.	1927	3,220.05	1.94	0.666	175	6	20	Weekly	No
N.C.	1929	2,511.91	1.47	0.6	150	7	18	Weekly	No

Notes: See tables B.1 and B.2. The payments are based on the national weekly wage reported in tables B.1 and B.2. In the first year of operation for many of the states adopting workers' compensation early (the states with Yes in the far-right column), the hand payments began after a period of paying the worker for temporary total disability, typically for a period of fifteen weeks. Thus the initial payments for the fifteen weeks followed the rules described in table B.2. In other states (the states with No in the far-right column), there was no period of temporary total disability pay. Nearly all states set up the payments on a weekly basis with an option for the worker to petition for a lump-sum payment. Most workers' compensation administrations discouraged the payment of lump sums. Washington and Wyoming, however, used lump-sum payments as their standard practice.

then he began receiving payments of half his wage for 150 weeks. The weekly payments could not exceed $10 or be lower than $5. For a worker receiving the national weekly wage at the time of the accident, the present value of this stream of payments, discounted at 5 percent, would have been $1,117, which was roughly 1.5 times his average annual earnings (50 weeks times the national weekly wage). In most of the rest of the states, like Michigan in 1912, the injured worker received just the hand payments without any period of receiving temporary total disability payments. The Michigan payment stream for a worker earning the national weekly wage of $15.34 a week led to a present value of $1,070, which was 1.39 times average annual earnings. In Washington and Wyoming the hand payment was typically paid as a lump sum. Most other states allowed the worker to receive a lump sum under appeal, but they generally did not encourage the practice.

Calculating Expected Benefits

The relative generosity of the states sometimes varied for different types of accidents. Table B.4 combines the present values of the accident payments into a measure of expected benefits to develop a summary measure of workers' compensation benefits. For each type of accident we calculated the gross benefit as the present value of the stream of payments for that type of accident. We then converted these gross benefit estimates into an expected benefit measure ($E(B)$) by weighting each of the four types of accident benefits by the probability that each type of accident would occur and then summing the four expected compensation estimates, as in the following equation:

$$E(B) = p_f B_f + p_{pt} B_{pt} + p_{pp} B_{pp} + p_{tt} B_{tt},$$

where B is the benefit paid and p is the probability that the accident will occur. The subscript f denotes fatal accidents, pt permanent total disability, pp permanent partial disability, and tt represents temporary total disability. In essence, the expected benefit shows what an insurance company might expect to pay to the families of workplace accident victims earning the national weekly wage during the course of a year.

The accident probabilities for the expected benefits calculations in tables B.4 and 7.1 are based on the manufacturing average for Oregon and represent the average accident experiences of all Oregon industries (Oregon Industrial Accident Commission 1919, 28–42). The probability of a fatal accident over the course of a year was 0.001895, for permanent total disability 0.000136, for permanent partial disability 0.0099, and for temporary total disability 0.1199. After multiplying these probabilities by the present value of the benefits and scaling down the hand benefits to reflect

Table B.4 **Expected Workers' Compensation Benefits in the First Year of Operation**

State	First Year	National Weekly Wage	Expected Benefit	Expected Benefit as Percentage of Annual Earnings	Present Value of Fatal Accident Payments	Present Value of Five-Week Disability Payments	Present Value of Hand Disability Payments
Cal.	1911	14.83	13.17	1.78	2,149	38.35	1,936.53
N.J.	1911	14.83	8.81	1.19	1,840	22.11	1,116.56
Nev.	1911	14.83	9.86	1.33	2,314	31.6	631.36
Wash.	1911	14.83	16.5	2.23	4,511	38.34	1,263.16
Wis.	1911	14.83	14.95	2.02	2,719	37.29	2,283.19
Ill.	1912	15.34	11.8	1.54	2,548	30.52	1,364.56
Kan.	1912	15.34	9.79	1.28	2,068	22.88	1,312.72
Mass.	1912	15.34	13.15	1.72	1,999	22.88	2,925.56
Md.	1912	15.34	10.16	1.33	2,548	15.26	1,456.05
Mich.	1912	15.34	9.13	1.19	1,999	22.88	1,069.61
Ohio	1912	15.34	12.83	1.67	2,771	36.58	1,298.68
R.I.	1912	15.34	9.88	1.29	1,999	22.88	1,415.06
N.H.	1912	15.34	9.68	1.26	2,302	22.88	1,041.26
Ariz.	1913	15.82	13.77	1.74	2,626	23.58	2,584.03
Minn.	1913	15.82	9.61	1.21	2,161	23.58	1,102.61
Neb.	1913	15.82	10.56	1.34	2,450	23.58	1,271.41
Tex.	1913	15.82	15.13	1.91	2,888	37.75	2,180.24
W. Va.	1913	15.82	13.72	1.73	3,981	28.31	1,030.13
Conn.	1914	15.84	9.86	1.24	2,235	23.62	1,145.3
Iowa	1914	15.84	9.63	1.22	2,164	23.62	1,104.35
N.Y.	1914	15.84	19.17	2.42	5,131	31.46	2,290.35
Or.	1914	15.84	19.74	2.49	6,541	47.28	1,744.71
Colo.	1915	15.79	7.89	1.00	2,127	15.68	778.87

Ind.	1915	15.79	10.53	1.33	2,363	25.89	1,210.38
La.	1915	15.79	9.59	1.21	2,157	23.53	1,100.35
Mont.	1915	15.79	10.69	1.35	2,696	23.53	1,100.35
Okla.	1915	15.79	6.73	0.85	395	23.53	1,433.25
Vt.	1915	15.79	8.36	1.06	1,528	23.53	1,120.53
Wyo.	1915	15.79	8.53	1.08	1,890	24.64	800
Ky.	1916	17.57	14.33	1.63	3,345	34.05	1,592.03
Me.	1916	17.57	11.39	1.30	2,289	26.19	1,658.69
Pa.	1916	17.57	11.06	1.26	2,389	26.19	1,412.12
N.M.	1917	19.87	10.2	1.03	2,640	19.74	1,140.36
S.D.	1917	19.87	12.13	1.22	2,604	29.63	1,515.76
Utah	1917	19.87	14.25	1.43	3,096	38.81	1,524.79
Del.	1918	24.01	12.17	1.01	2,670	29.82	1,463.03
Idaho	1918	24.01	17.66	1.47	4,085	47.73	1,674.61
N.D.	1919	27.67	40.51	2.93	10,004	91.7	4,234.79
Tenn.	1919	27.67	14.88	1.08	3,753	32.8	1,533.59
Va.	1919	27.67	12.1	0.87	2,706	29.82	1,394.17
Ala.	1920	33.81	18.4	1.09	3,749	59.69	1,673
Ga.	1921	30.77	13.42	0.87	2,706	35.78	1,673
Mo.	1927	33.23	29.81	1.79	5,362	99.48	3,220.05
N.C.	1929	34.08	27.02	1.59	5,333	89.53	2,511.91

Sources: See sources to table B.1 and appendix B. The expected benefit is the weighted sum of the present value of fatal accident payments, the present value of hand payments, and the present value of the five-week disability payment. The weights are the probability of this type of accident. The same probabilities were used for all states and are based on averages for manufacturing in Oregon (Oregon Industrial Accident Commission 1919, 28–42). The probability of a fatal accident over the course of a year was 0.001895, for a permanent total disability 0.000136, for permanent partial disability 0.0099, and for temporary total disability 0.1199. We used the fatal accident present value as a measure for the permanent total disability benefits because they were so similar in nearly all the states. We scaled the hand present value down to 21.8 percent of the level listed above because the average value paid for permanent partial disabilities was about 21.8 percent of the hand value (see accident statistics reported in Wisconsin Industrial Commission [1915, 41; 1916, 44; 1917, 6–7] for 1914 to 1917).

that a permanent partial disability typically was about 21.8 percent of the hand benefits, table B.4 reports the expected workers' compensation benefit in each state during the first year of operation. For workers earning the national weekly wage in New Jersey in 1911, an insurer might have anticipated paying out $8.81 per worker, or approximately 1.19 percent of the workers' annual earnings, in workers' compensation benefits. In chapter 7 we discuss the factors determining the choice of benefit levels and table 7.1 compares the expected benefits in each state from the first year of operation through 1930.

In calculating the expected benefits we merged the fatal accident and permanent total disability accident categories together because permanent total disability accidents, like full paralysis, were relatively rare and the payments were very close to the fatal accident payouts. Workers' compensation benefits and the expected benefit measure are based on the workers' weekly wage. We used different weekly wages for expected benefits calculations in different settings. When comparing the workers' compensation benefits across states and time in tables B.1 through B.4 and when analyzing the determinants of expected benefits in chapter 7 and table 7.1, we used the national average weekly wage in manufacturing. In the wage offset regressions discussed in chapter 3 and appendix D, we used the national average weekly wage for each occupation in the sample. In the analysis of household savings in chapter 3, table 3.4, and appendix F, we used the weekly wage for the head of the household, which was reported in the Bureau of Labor Statistics (BLS) cost-of-living sample.

We obtained the statutory descriptions from various bulletins of the U.S. Bureau of Labor Statistics in the Workmen's Compensation and Insurance Series (U.S. Bureau of Labor Statistics 1914, 1917, 1918, 1923, 1926b), Hookstadt (1920, 1922), and Clark and Frincke (1921). We also consulted Jones (1927). When questions arose about the timing of changes in the law, the state's statutes were consulted directly.

For fatal accidents, the typical law allowed weekly payments to be a percentage (up to two-thirds) of the weekly wage for a specified period of time. We calculated the present value (using continuous discounting) of the stream of benefits using a discount rate of 5 percent, which was the typical return on stocks and bonds for the period. The rate of 5 percent also was in the range of statutory rates used when the stream of workers' compensation benefits were converted to lump sums. The calculations were sometimes complicated because states usually imposed maximums on the weekly payment or maximums on the sum total of all the weekly payments. If the percentage times the weekly wage exceeded the maximum weekly payment, we inserted the maximum weekly payment into the present value calculations. In cases where there was a maximum total payment, we assumed the family received the regular weekly payment until the total undiscounted stream of payments reached the maximum total. Thus, we determined the number of weekly payments by taking the maximum total divided by the weekly payment (states did not worry about discounting issues when deciding when a family reached its maximum total benefit).

For the loss of a hand, the typical state paid a percentage of the weekly wage for a fixed amount of time, subject to minimum and maximum weekly amounts. Some states commenced the hand payments after the worker collected a statutory amount of temporary disability pay. Following the recommendations of the International Association of Industrial Accident Boards and Commissions in 1920 (Hookstadt 1920, 77), we assumed that the loss of a hand temporarily disabled the worker fully for fifteen weeks before he could return to work. We calculated the present value of the stream of payments using continuous discounting. It was important to calculate the present value because some states would pay a relatively small amount per week for the rest of the worker's life. Without discounting, the total amount paid would look quite large when, in fact, the present value of the stream of payments was in the range of other states' benefits. In the few cases where a hand payment was not mentioned specifically, we followed the BLS in describing it as a 50 percent disability.

For the permanent partial disability category, we used the loss of a hand as a typical accident because the payment structure for the amputation of a hand was defined in almost all of the states' laws. The typical accident in the permanent partial category, however, was actually much less serious. Based on accident statistics reported by the Wisconsin Industrial Commission (1915, 41; 1916, 44; 1917, 6–7) for 1914 to 1917, we found that the average payment for a permanent partial disability was 21.9 percent of that for the loss of a hand. Thus, in the expected benefits calculations, we scaled down the present value of the hand payment by multiplying the figure by 21.9 percent. We treated the typical temporary disability accident as one in which the injured worker was out of work for five weeks.

For temporary disabilities, workers were paid a percentage of their weekly wage during the period of the disability, which we assumed to be five weeks. These payments were usually subject to minimum and maximum weekly amounts. Nearly all states had waiting periods. In many cases a worker injured for five weeks would receive no payment for the first three to fourteen days of the disability, such that he might receive as few as three weekly payments. In a number of states, the worker would receive nothing during the waiting period, but if the disability lasted beyond four weeks (up to eight weeks in some states) the worker would eventually receive a retroactive payment for the first week or two of the disability. We have made our calculations sensitive to these nuances across states.

In a number of years the statutory parameters of the law changed. For the purposes of the wage regression analysis in chapter 3 and in appendix D, we determined from the states' session laws when the new workers' compensation provisions went into effect. We then used a weighted average of the benefits calculated under the old and new laws, with the weight being the percentage of time during the year that each law was in effect. In the wage regressions we wanted the benefits throughout the year because the wages were typically averages of the wages throughout the years. When we calculated the expected benefit values in tables B.1 through B.4 and in table 7.1, we focused instead on the benefits as they existed at the end of the year. In these situations our focus is on the decisions made by legislatures as to the benefits that they wanted to establish, which was best represented by what was in place after the legislature had met.

In the years prior to the introduction of workers' compensation, the courts and settlements with employers determined the payments to injured workers. We need to come up with measures of the generosity of negligence liability for states without workers' compensation for the wage regressions and the savings analysis in chapter 3 and appendixes D and F. Based on the material presented in table 2.1, we assumed that the family of a worker killed in a workplace accident could expect to receive about half a year's income on average (which takes into account the probability of getting nothing). We then calculated the payment for a hand to be 54.02 percent of the fatal accident benefit and for the five-week disability to be 1.557 percent of the fatal accident benefit. These percentages were based on national averages of the ratios of hand-to-death benefits and disability-to-death benefits from all workers' compensation states during the year 1923. It is clear that the generosity of the liability systems varied across states because insurance companies established state differentials for employers' liability premiums in their ratebooks. The state differentials would typically reflect differences in the liability rules and differences in the court treatments of accident compensation. The differentials are reported in DeLeon (1907, 26–27). To make this calculation we multiplied the benefits above by the state's reported liability differential and then divided by 0.64333, which was the average liability differential reported for the forty-six states plus Arizona and New Mexico (still territories in 1909) in the sample.

It is clear that our estimates of the negligence liability payments suffer from measurement error because we cannot make the calculations with as much certainty as we did for workers' compensation because there were no statutory proscriptions under the negligence system. We have experimented with a variety of measures of the benefits under negligence liability and generally have obtained similar results to the ones reported in chapter 3, appendix D, and appendix F. In both the wage analyses and the savings and insurance analyses, we ran tests in which we assumed that families received nothing under the non–workers' compensation regime and we tried using the benefits without adjusting for the liability differential. The fundamental results remain the same.

The probabilities of each type of accident were derived from different sources for each of the analyses. In the wage regression analysis in chapter 3 and appendix D for the coal industry we started with an average fatal accident rate of 2.043 per million man hours from the sample of coal states used to estimate the wage equation (Fishback 1992, 87). To translate that into a fatal accident rate per full-year worked of 3.37 per one thousand men, we assumed that the men worked 206.4 eight-hour days (from the sample means). The remaining coal accident rates were determined by comparing the relative number of fatal cases (61), permanent total disability cases (3), permanent partial disability cases (82), and temporary disability cases (1,971) receiving compensation in coal mining from the Ohio State Insurance Fund during the eighteen months ending 30 June 1915. For example, the permanent total disability probability is calculated as the probability of a fatal accident in coal mining (0.00337) multiplied by the ratio of the number of permanent total disability cases to the number of fatal cases in Ohio (three to sixty-one). Using the Ohio workers' compensation information to estimate the probability of nonfatal accidents understates the actual probability of an accident because some injured workers were not compensated and, thus, were not included in the official accident statistics. The lumber and building trades accident rates in the wage regressions in chapter 3 and appendix D were obtained from the Oregon Industrial Accident Commission (1919, 28–42). The commission reported the total number of accidents in each accident category and the number of full-time workers covered under the workers' compensation system.

Expected benefits in the wage regression analyses discussed in chapter 3 and appendix D were based on the national average wage for each occupation in each year. We did not use the wage corresponding to each observation because the expected benefits would have been a function of the wage, thus imparting a positive bias to the estimated coefficients of the expected benefits index. Similarly, we could not use the ratio of expected benefits to wages because in some cases maximum allowable benefits became binding and the ratio of expected benefits to wages would have imparted a spurious negative bias. To eliminate these problems, we used the national average wage for each occupation in each year, which allowed the expected benefits index to rise in response to rising wages during the period as well as reflect differences in expected benefits driven by differences in wages at each skill level. Thus, the expected benefits variable becomes an instrumental variable for the actual expected benefits the worker would receive. For a particular occupation, even though our calculation assumed a constant wage across all states, each state's expected calculation measure in a particular year was different because each state's law was unique. In addition, because a state's law might have changed over the period of study, the expected benefits measure for an occupation class would have changed over time (holding state and average occupational wages constant). Further, if we were to hold a state's law constant over time, nominal expected benefits would change because average occupational wages fluctuated over the course of the sample. Thus, the factors that caused each observation to take a unique value were occupational differences, changes in average occupational wages over time, differences in each state's workers' compensation law, and changes in states' laws over time.

In the calculations for tables B.1 to B.4 and 7.1, the national average manufacturing weekly wage was constructed using Paul Douglas's measures of weekly hours and hourly earnings (series D-765 and D-766 in U.S. Bureau of the Census 1975, 168) for the years 1890 to 1926. We then interpolated values for the years 1927 through 1930 by running a regression of the weekly wage measure on Stanley Lebergott's measure of average annual earnings per full-time employee for manufacturing (series D-740 in U.S. Bureau of the Census 1975, 166), divided by fifty-two. The interpolated values for 1927 through 1930 are equal to 2.021638 + 1.080317 times the Lebergott measure of weekly wages.

Appendix C

Measuring the Change in Accident Benefits from Negligence Liability to Workers' Compensation

To show the relative generosity of the nonfatal accident benefits under the two systems, we compare the expected benefits, $E(B)$, that were paid as a share of income, holding accident rates constant. Let

$$E(B) = p_f B_f + p_n B_n,$$

where p is the probability of an accident and B is the average payment for fatal (f) and nonfatal (n) accidents. To make comparisons across time, we divided the expected benefits by average annual income to compare expected benefits as a percentage of workers' annual income or employers' payrolls. Since the number of reported nonfatal accidents rose sharply with the introduction of workers' compensation (see app. A), the nonfatal portion of the expected benefits measure may have risen either because a higher percentage of actual accidents received compensation or because the average percentage of wages paid as benefits increased.

We have tried to make comparisons of the generosity of workers' compensation benefits in two ways. First, we tried comparing the expected payment of wage benefits (ignoring medical payments) by fixing the probability of compensation and then calculating the payments that injured workers reportedly received for different types of accidents under negligence liability. We then compared these amounts to the statutory payments that workers' compensation guaranteed. Second, we examined employers' reports on the amounts of wage benefits, settlements, and medical payments that they and their insurance companies paid to the families of accident victims under the negligence system. We then compared these figures to similar reports generated by workers' compensation commissions on the amounts of wages and medical benefits that workers received. The comparisons were made in terms of employers' total payments relative to their payroll expenditures.

The employer-based technique has one major advantage over the worker-based technique. When employers' liability commissions sought a sample of accident victims, they usually started with accidents reported to the state bureau of labor. Since the reporting of nonfatal accidents (lasting longer than a week) rose substantially with the introduction of workers' compensation, it is likely that our estimates of the change in payments per nonfatal accident are overstated when we rely on injured workers' reports. By contrast, from the employers' reports, we are likely to get a better picture of their expenditures under negligence liability because the employer did not have to reveal the total number of accidents, only how much was paid in the aggregate. Our measure of employers' expenditures under both legal regimes combines both the probability of the accident and the payout per accident. Therefore, even if the underlying accident probabilities remained the same, the total payments per dollar on the payroll might have risen because of an increase in reported accidents that received compensation and/or an increase in compensation per accident. The total payments might also have risen because of an increase in the underlying probability of accidents resulting from moral hazard. We do not believe that the rise in the underlying probability of accidents is nearly as great as the rise in the reporting of accidents.

Comparisons Based on Injured Workers' Reports

In table B.4 we report a measure of expected benefits under workers' compensation in New York in 1914 as 2.42 percent of annual earnings. This calculation was based on inserting values into the following equation:

$$\text{(C1)} \qquad E(B) = p_f B_f + p_{pt} B_{pt} + p_{pp} B_{pp} + p_{tt} B_{tt},$$

where B is the benefit paid and p is the probability of an accident. The subscript f denotes fatal accidents, pt represents accidents causing permanent total disability, pp is permanent partial disability, and tt is temporary total disability. We then divided the expected benefits by annual earnings. See appendix B for further details.

In our calculation for the negligence system we focus on wage replacement. We believe that medical coverage was better under workers' compensation, but it is much harder to document the extent of medical coverage than it is to document wages. By eliminating medical payments, however, we believe that we are biasing the calculations against finding that workers' compensation was more generous. As a starting point, we use accident probabilities based on workers' compensation experience to calculate the expected payouts under both negligence liability and workers' compensation. The biases that result from this assumption are discussed below. The expected benefits under each system are calculated as:

$$\text{(C2)} \qquad E(B) = 0.001895B_f + 0.000136B_{pt} + 0.009932B_{pp} + 0.119916B_{tt}.$$

The accident probabilities represent the experience in Oregon from 1917 to 1919 across all industries, as reported in Oregon Industrial Accident Commission (1919). We calculated the accident probabilities as the number of compensable accidents divided by the number of workers covered.

The New York Commission on Employers' Liability (1910, 210–11, 246–50) conducted a series of direct interviews of a random sample of workers who had been injured and whose accidents had been reported to the New York Bureau of Factory Inspection. The average annual earnings for workers in New York at the time was \$536 (p. 223), which translates into weekly earnings of \$10.31.[8] The workers experiencing temporary disability received \$10,623 in wage payments on lost earnings of \$66,854. In addition they received \$11,663 in other receipts (we believe these include settlements). Eleven workers had suits pending. We assume that workers won half the cases and received \$200 in each case for an additional \$1,100. Thus, employers were replacing 34.98 percent of the lost earnings. For comparative purposes we assumed the typical temporary disability involved five weeks of lost wages. Therefore, if the average wage replacement for five weeks of lost earnings was about 35 percent, then the average replacement per accident was \$18.

In forty-eight fatal accident cases in the survey (p. 249), employers paid a total of \$22,343 in settlements plus \$2,274 in medical or funeral expenses, and eight suits were pending. We assume that funeral expenses were half of the \$2,274 and that workers' heirs won or settled half of the pending cases, receiving an average payment of \$2,000 for an additional payout of \$8,000. If we include all other receipts of \$1,840 as coming from employers, then total fatal accident payments were \$33,320, or \$694.17 per case.

There were ten permanent complete disability cases, where employers paid settlements of \$965 (p. 247). One case was pending and we assume that it paid \$2,000. The total payout was therefore \$2,965, or an average of \$296.50 per case. If we include all other receipts of \$656, the payout was \$3,621, or \$362.10 per case.

There were sixty permanent partial disability cases, with settlements equal to $7,950. Ten cases were pending. Assuming that workers won half the cases and received $2,000 each, the total payout was $17,950, or $299.16 per case. If we add in other receipts of $4,196, the total is $22,146, or $369.10 per case.

Inserting these payment values across the different types of accidents into equation C2, the calculation becomes

$$E(B) = 0.001895*694 + 0.000136*362 + 0.009932*369 + 0.119916*18.03 = \$7.19,$$

which is 1.34 percent of the $536 annual wage.

Using the same accident rates and the weekly wage of $10.31, we calculated the present value of the benefits that would have been paid for a typical accident in each category under New York's workers' compensation law that went into effect in 1914. We gave the same payment for permanent total disability as we did for a fatal accident. As reported above we treated the typical temporary disability accident as one in which the injured worker was out of work for five weeks. For the permanent partial disability category, we calculated the benefits for the loss of a hand as a typical accident and then adjusted the payment downward by multiplying by 0.219 because the typical accident in the permanent partial category was actually much less serious than a hand amputation. The 0.219 figure is based on actual accident statistics reported by the Wisconsin Industrial Commission (1915, 41; 1916, 44; 1917, 6–7) for 1914 to 1917. For more details about the precise procedure, see appendix B on the construction of the expected benefits variable.

For a worker earning $10.31 a week or $536 per year, the New York workers' compensation law promised expected benefits equal to $12.54, which was 2.34 percent of annual wages and roughly 75 percent more than what was paid under negligence liability.[9]

The rise in expected benefits received by workers when workers' compensation was introduced in New York is probably *understated* in this analysis. First, the estimate of expected benefits per nonfatal accident are probably substantially overstated in the negligence liability case because of problems faced in the reporting of accidents to the New York Bureau of Labor Statistics. We fixed the probability of an accident in the equations based on accident probabilities under workers' compensation. However, as we have suggested above, there was a significant rise in the reporting of nonfatal accidents when workers' compensation was introduced. This suggests that there was substantial underreporting of accidents by employers under negligence liability, and it is likely they would not report injuries where they paid no compensation. Since the sample of injured workers collected by the New York Commission on Employers' Liability was drawn from reports to the New York Bureau of Labor, it is likely that it is missing a large number of nonfatal accidents for which no payment was made to workers. The biggest effect this reporting might have on our calculation is on the payments to temporarily totally disabled workers and permanently partial disabled workers. If we base calculations on the idea that reported accidents for temporary total disablement doubled, we should cut the average payment for temporarily totally disabled workers from $18 to $9 per accident, which would cut the expected benefits to $6.13, or 1.14 percent of annual earnings. If we assume that reported accidents went up 1.5 times for the accidents causing permanent partial disability, the expected benefits fall further to $4.91, or 0.92 percent of expected earnings. Thus the expected benefits might have risen 2.54 times with the introduction of workers' compensation.[10]

The near doubling in benefits also understates the rise in the net amount received by workers after they paid legal fees. Evidence from various sources suggests that workers paid lawyers' contingency fees under the negligence system ranging from 20 to 50 percent. Evidence from Kansas and Minnesota suggests that of payments made to workers, lawyers received about 13 to 23 percent, which is lower than the contingency fees because a number of workers did not hire lawyers. It is likely that some workers hired lawyers to contest workers' compensation payments as well. At most, 5 percent of the total amount that workers received went to lawyers under workers' compensation. Thus, the expected net benefits after paying legal fees rose from 0.75 percent of annual earnings under negligence liability to 2.22 percent of annual earnings under workers' compensation. The expected net benefits under workers' compensation might have been as large as three times the levels received under negligence liability.

Comparisons Based on Employers' Reports

Our second method for making the benefits comparison is to start from reports made by employers about their total accident expenditures that injured workers received. The Wisconsin Bureau of Labor and Industrial Statistics (WBLIS) conducted a survey of employers regarding their accident expenses in 1906 (see WBLIS 1909, 34–35). They received responses from 540 establishments that reported both accident expenses and total wages paid.[11] The total accident expenses include all expenditures on insurance premiums, medical expenses not paid by the insurer, wages paid during disablement, settlements, and payments to insurance companies for workers' collective benefits. The study found that employers spent $164,696 on accident expenses while paying $30,534 million in wages, which translates into a rate of $0.53 for every $100 in wages paid for accident expenses. The WBLIS (p. 31) also found that the percentage of the employers' expenditures on accidents that eventually reached the worker (including compensation and aid before the worker paid attorney fees) was only 45.5 percent.[12] Therefore, workers would have received only about $75,000 of the employers' expenditures (before legal fees) which is $0.245 in accident payments for every $100 in wages paid by the employer. The estimate for Wisconsin is very similar to estimates calculated from information provided by the employers' liability commissions in New York at $0.298 per $100 on the payroll, Michigan at $0.34, and Massachusetts at $0.29.[13] Under workers' compensation in 1913, by contrast, the Wisconsin Industrial Commission (1915, 32–38) calculated that workers received wage and medical benefits of $0.82 for every $100 of payroll. This Wisconsin comparison suggests that workers' compensation was 3.35 times more generous than the negligence liability system.

As mentioned in chapter 3, we were concerned that the Wisconsin comparison may be biased because the industries surveyed in 1906 and 1913 differed. To examine this issue more carefully, we matched industries from the 1906 and 1913 listings and found that in all but one of the twenty-three comparisons workers' compensation benefits exceeded those under negligence liability. These comparisons are presented in table C.1.

We made another comparison based on the experience of the Stonega Coke and Coal Company, largely based in Virginia, before and after workers' compensation. The comparison is based on information reported in the *Annual Reports of the Operating Department* for the years 1916–25. We estimate the percentage of the payroll that went to compensation of workers, excluding medical expenses.[14] For the period 1916 to 1918, the reports of the legal department show the compensation paid to workers through settlements and court suits. We believe that these reports give full coverage of the payments, because numerous situations mention that there was consultation with the liability insurance company. In no other place in the annual operating reports could we find mention of expenditures for payments to workers. We followed the legal reports for years after 1916 to 1918 to see the final results of pending cases in those years. Therefore, we believe that we have a good understanding of what Stonega paid to accident victims in the form of damages for accidents occurring between 1916 and 1918. Stonega spent roughly $52,225 on settlements and court awards for accidents occurring during this period.[15] Stonega's payroll for coal mining during this period was approximately $5,685,506; therefore Stonega spent roughly $0.975 per $100 on the payroll on compensating workers.[16] During the period 1919 to 1923, after workers' compensation was established, we have information on the number of compensable weeks associated with accidents at the Stonega mines. We translated this into workers' compensation payments using the rule that Virginia paid 50 percent of weekly earnings, with a minimum of $5 per week and a maximum of $10 per week in compensation payments in 1919. The minimum rose to $6 per week and the maximum to $12 per week in the 1920 session of the legislature. Motormen during this period typically earned about $5 to $6

Table C.1 **Generosity of Wage and Medical Benefits in Wisconsin before and after Workers' Compensation, by Industry, 1906 and 1913**

	Benefits Workers Received per $100 on Payroll in				
		1913 for Employers		Ratio 1913 to 1906	
Industry	1906	With Insurance	Without Insurance	With Insurance	Without Insurance
Agricultural implements	.13	.59		4.5	
Boots and shoes	.08	.33		4.2	
Chairs	.06	.58	.30	9.2	4.7
Clothing	.08	.16	.26	2.1	3.4
Furniture	.14	.58	.30	4.1	2.1
Knit goods	.05	.16	.26	3.2	5.2
Leather tanning	.13	.12	.37	.95	2.9
Lumber	.38	.58	.78	1.5	2.0
Machinery	.25	.48	.49	1.95	2.0
Malt liquors	.33	.52	.36	1.6	1.1
Paper and pulp	.43	.93	.56	2.2	1.3
Sheet metal	.41	.74	.58	1.8	1.4
Iron		.12	.84		7.1

Sources: Wisconsin Bureau of Labor and Industrial Statistics (1909, 31–35) and Wisconsin Industrial Commission (1915).

Notes: The compensation amount in 1906 represents a gross figure, before attorneys fees are subtracted. It is calculated by multiplying employers' total expenditures on accidents in 1906 by 45.6 percent, which is the percentage of the employers' expenditures that workers actually received after all of the employers' expenses were subtracted, including employers' liability insurance and collective accident insurance premiums. The amount that workers received would have included compensation and medical aid not paid by insurers, and perhaps wages paid during the disability. The benefits received by workers in 1913 from employers with insurance is the "pure premium" listed by insurance companies with policies that had been terminated. The pure premiums reflect the actual amounts insurance companies paid out to the families of killed and injured workers. The expenditures by noninsured employers shows the compensation and medical payments paid out during the year ending 31 December 1913.

Possible biases in the data: The Wisconsin Bureau of Labor and Industrial Statistics (WBLIS) claimed that the entire amount of compensation paid in 1906 may not reflect the amounts actually reported for 1906. However, the survey asked for all expenditures employers had in the year 1906 on account of accidents. Many firms probably reported all expenditures on accounts, including payouts for accidents from prior years, thus the typical annual amount they paid would not be understated.

In 1913, the same questions arise for uninsured employers carrying their own risk. We do not believe this is a problem for the insurance information in 1913 because it focuses on completed policies, where some measure of the total payouts for which the insurer is liable on accidents is included in the totals.

Matching industries: The industry titles in the 1906 and 1913 studies matched precisely for agricultural implements, boots and shoes, furniture, machinery, and paper and pulp. We matched chairs in 1906 with furniture for the insurance spending in 1913 and with woodworking for the uninsured spending in 1913. We matched clothing and knit goods in 1906 with textiles in 1913; iron in 1906 with iron for insured in 1913, and iron and steel for uninsured in 1913; leather tanning in 1906 with other leather for insured in 1913, leather and leather products for uninsured in 1913; lumber in 1906 with logging and lumbering in 1913; malt liquors in 1906 with breweries in 1913; sheet metal in 1906 with metal working for insured in 1913 and stamping works for uninsured in 1913.

per day, or $20 to $24 for a four-day week. Thus coal miners were probably receiving at least $9 per week in compensation for accidents, which would imply payouts of $255,326 on a payroll of $14,983,930, or $1.70 per $100 on the payroll, which is about 74 percent higher than the payouts under negligence liability.[17] We should note that the rise in payouts is smaller than what we see in Wisconsin and in New York in part because Virginia during 1919 was one of the least generous workers' compensation states. Comparisons of the generosity of workers' compensation in table B.4 show that Virginia's expected benefits in 1919 were about 0.87 percent of annual earnings, compared with Wisconsin's index of 2.02 in 1911 and New York's index of 2.42 in 1914. Further, the rise in benefits received by workers may be understated for two reasons. The benefits under workers' compensation do not include medical payments or funeral expenses, whereas it is likely that the payments listed under negligence liability in part covered medical and funeral expenses. As in other settings, we have not yet subtracted the legal fees that workers paid under the two systems, and legal fees were a higher percentage of negligence liability payments than of workers' compensation payments.

Appendix D

Econometric Analysis Used to Estimate Wage Offsets

In chapter 3 we present several estimates of the wage offsets that workers experienced when workers' compensation was introduced. This appendix discusses the techniques used to estimate these wage offsets in more detail (see also Fishback and Kantor 1995). To estimate how the wages of workers adjusted in response to the introduction of workers' compensation, we constructed three separate panel data sets for relatively dangerous industries in the early 1900s. The first sample covers hourly wage rates from payrolls collected by the U.S. Bituminous Coal Commission. The sample contains state averages for ten jobs from the twenty-three leading coal producing states at the end of each year from 1911 to 1922. The second sample is hourly earnings collected from payrolls by the U.S. Bureau of Labor Statistics for ten different jobs in the lumber industry for the years 1910 to 1913, 1915, 1921, and 1923 in the twenty-three major lumber producing states. The third sample is the wage scales listed in union contracts in the building trades for thirteen occupations in seventy-seven cities for each year between 1907 and 1913.[18] All three data sets allow examination of differences across states and over time during the period when nearly all the workers' compensation laws were adopted.

For each of the three industries, we estimate reduced-form, weighted least squares wage regressions with the real hourly wage (in constant 1890–99 dollars) of occupation i in state j in year t (W_{ijt}) as the dependent variable.[19] The reduced-form equation that we estimate can be written as

$$W_{ijt} = (D_{ijt}, B_{ijt}, WT_{ijt}, A_{ijt}, U_{ijt}, O_i, S_j, Y_t, e_{ijt}).$$

The reduced-form equation contains variables affecting the employers' wage offer function and the workers' wage acceptance function. The employers' offers were influenced by fluctuations in the product market and in worker productivity (D_{ijt}). Variables affecting the workers' wage acceptance function include the extent of postaccident benefits (B_{ijt}), measures of restrictions on working time (WT_{ijt}), and the accident rate (A_{ijt} which was available at the state level only for the coal sample). Workers seek higher wages as compensation for lower accident benefits, greater restrictions on working time, and higher accident rates. For the coal industry analysis we were also able to include a vector of information on strikes and union strength (U_{ijt}), which also might have affected the wages that workers sought. A vector of state dummy variables (S_j, city dummies for the building trades) controls for geographic differences in labor market conditions, such as differences in the cost of living and other labor laws specific to individual states. A vector of year dummies (Y_t) controls for labor market differences specific to each year, like the government's greater control of markets during World War I. A vector of occupation dummy variables (O_i) in all of the regressions controls for skill differences and other differences in the supply and demand conditions for those particular jobs.

The regression coefficients presented in table D.1 are generally consistent with the findings of other wage studies. Wage rates were positively related to product prices in the coal and lumber samples, whereas increases in building activity (measured by the real value of building permits per capita) were associated with higher wages in the building trades.[20] Output per man hour was positively associated with wages in both the coal and lumber industries. In all of the estimations, the coefficients of the occupation dummy variables suggest that higher skilled workers earned relatively higher wages. Limitations on working time, as measured by full-time hours, were offset by higher wages in the lumber sample.[21] Unions and strike activity were associated with higher wages in the coal industry, although the union coefficient was statistically insignificant.

Columns (1), (3), and (5) in the table show the regression results when the impact of workers' compensation is measured by a zero-one dummy variable that takes the value one for states and years in which the law was in effect, and zero otherwise. In coal mining the presence of a workers' compensation law was associated with a statistically significant 2.16 percent decline in hourly earnings when evaluated at the mean hourly earnings. Similarly, the lumber industry wage offset was 1.60 percent and statistically significant. In the building trades, however, the decline was smaller at 0.33 percent, and not statistically different from zero.[22]

Columns (2), (4), and (6) of the table show the full regression results when we use the expected benefits measure described in the text and in appendix B to measure the impact of workers' compensation. The expected benefits measure is an index that corresponds better to the generosity of employer-provided accident compensation both before and after workers' compensation. It is calculated using the national average wage for each occupation in each year. We did not use the wage corresponding to each observation because the expected benefits would have been a function of the wage, thus imparting a positive bias to the estimated coefficients of the expected benefits index. Similarly, we could not use the ratio of expected benefits to wages because in some cases maximum allowable benefits became binding and the ratio of expected benefits to wages would have imparted a spurious negative bias. To eliminate these problems, we used the national average wage for each occupation in each year, which allowed the expected benefits index to rise in response to rising wages during the period as well as reflect differences in expected benefits driven by differences in wages at each skill level.[23]

The expected benefits variable is therefore an instrumental variable that measures the monetary value that a risk-neutral worker would place on his expected accident compensation. If workers were risk-averse, however, our measure of expected compensation actually provides a lower-bound estimate of the value that workers would have placed on these postaccident benefits. A coefficient of -1 implies that workers fully paid for increases in the expected benefits that they received, although the worker would not have fully paid for the employer's cost of purchasing insurance to provide those benefits. Coefficients of roughly -1.67 imply that employers were able to pass on their full insurance costs to workers.

We have also estimated the various wage equations using a semilog specification often used in wage studies. Table D.2 gives a summary of the accident benefit coefficients from a variety of different empirical specifications. Column (1) of the table corresponds to the second column for each sample in table D.1. Column (2) of table D.2 shows the change in the wage associated with a one-dollar increase in expected benefits under the semilog specification, evaluated at the sample mean of average annual earnings for each industry. The absolute values of *t*-statistics are listed in parentheses below the estimates of the wage offsets. The estimates based on the semilog specification are similar to the results reported in the level specification.[24]

These results are generally robust to the inclusion or exclusion of the

Table D.1 **Fixed-Effects Weighted Least Squares Wage Regressions**

Variable	Coal Mining Hourly Wage Workers (1)	Coal Mining Hourly Wage Workers (2)	Lumber Mill Workers (3)	Lumber Mill Workers (4)	Unionized Building Trades (5)	Unionized Building Trades (6)
Workers' compensation dummy	−0.603		−0.335		−0.124	
	(3.67)		(2.62)		(0.604)	
Expected present value of accident compensation[a]		−1.72		−1.04		0.020
		(4.14)		(2.11)		(0.031)
Limits on working time[b]	0.053	0.264	−0.359	−0.362	−0.843	−0.843
	(0.138)	(0.704)	(18.5)	(18.6)	(22.7)	(22.7)
Product price or other product demand index[c]	8.46	8.35	0.277	0.286	0.044	0.044
	(17.7)	(17.5)	(6.68)	(6.95)	(5.02)	(5.01)
Productivity measure[d]	6.43	7.66	0.387	0.386		
	(3.46)	(4.12)	(2.44)	(2.43)		
Fatal accidents per million man-hours	−0.029	−0.005				
	(0.505)	(0.086)				
Paid-up membership in the United Mine Workers of America as a percentage of employment	0.111	0.317				
	(0.161)	(0.469)				
Strike days per employee	0.010	0.012				
	(2.56)	(3.16)				
Strike days per employee lagged one year	−0.006	−0.005				
	(2.28)	(2.20)				
Strike days per employee in states other than state *i*	0.072	0.079				
	(4.00)	(4.42)				
Intercept	3.40	2.57	37.4	37.6	77.1	77.0
	(2.52)	(1.91)	(30.5)	(29.5)	(45.7)	(45.5)

Occupation dummies	9 of 10	9 of 10	9 of 10	9 of 10	12 of 13	12 of 13
Geography dummies	22 of 23 states	22 of 23 states	22 of 23 states	22 of 23 states	76 of 77 cities	76 of 77 cities
Year dummies	Included	Included	Included	Included	Included	Included
N	2,690	2,690	1,236	1,236	6,563	6,563

Sources: Fishback and Kantor (1995, 727–28). Average hourly earnings in the coal industry are from Fisher and Bezanson (1932, 254–89, 296–325). The union variable is the percentage of workers with paid-up membership in the UMWA reported in U.S. Coal Commission (1925, 1052), with straight-line interpolations to fill years not reported. The remaining coal data were compiled from U.S. Bureau of Mines Bulletins titled, "Coal-Mine Fatalities in the year . . . ," and U.S. Geological Survey (after 1922 Bureau of Mines) publications titled, *Mineral Resources of the United States, Part II, Nonmetals,* various years. For further details on the coal sample, see appendix B of Fishback (1992, 234–41). Wage and hours-worked data for each of the lumber occupations were collected from U.S. Bureau of Labor Statistics Bulletin Numbers 129, 153, 225, 317, and 363. Lumber price and output data are reported in U.S. Department of Agriculture (1948). The total number of lumber workers in each state was derived using a straight-line interpolation of data reported in U.S. Bureau of the Census (1913, 504–5; 1923, 466–71). Wage and hours-worked data for each of the building occupations were collected from the following U.S. Bureau of Labor Statistics "Union Scale of Wages and Hours of Labor" Bulletins (year(s) of coverage in parentheses): No. 131 (1907–1912), No. 143 (1913), No. 171 (1914), No. 194 (1915), No. 214 (1916), No. 245 (1917), No. 259 (1918), No. 274 (1919), No. 286 (1920), No. 302 (1921), No. 325 (1922), and No. 354 (1923). Building permit data were collected from Riggleman (1934, 263–76). All dollar values have been deflated using Paul Douglas's cost-of-living index (1890–1899 = 100), series E185 in U.S. Bureau of the Census (1975, 212).

Notes: Dependent variable is hourly wage in 1890–99 cents. Absolute value of t-statistics in parentheses below coefficient estimates. F-tests reject the hypothesis that the coefficients of occupation, year, and geography dummy variables are simultaneously zero. The mean of the wage is 27.7 (standard deviation of 28) in the coal sample, 20.4 (10.4) in the lumber sample, and 37.3 (9.4) in the building trades sample. Since the data use information based on means from states with different sample sizes, we used weighted least squares. In the coal estimation the square root of the number of coal workers in the state is used as the weight. In the lumber sample, the square root of the number of sampled workers is used as the weight. The building trades data did not report sample size, so White's (1980) method is used to adjust the standard errors. Using White's standard error correction for the coal and lumber regressions does not change the basic results reported above.

[a]The expected value of accident compensation is scaled to correspond to the hourly measurement of the dependent variable. We divided the expected benefits measures by estimates of hours worked per year. For the coal sample, we assumed that miners worked an average of 206.4 eight-hour days (derived from the sample). We assumed that lumber workers worked a total of 3,000 hours per year and building tradesmen worked 2,250 hours. The latter two estimates were calculated as the average number of hours worked per week given in the sample multiplied by fifty weeks.

[b]Limits on working time in the coal industry is measured as the mean number ($\times 10^{-2}$) of days the state's mines were open in that year and the number of hours in a full-time week in the lumber and building trades industries.

[c]The product price is the average price of coal at the mine mouth adjusted for inflation. The lumber price index is a weighted average (by output) of the real prices of individual species of lumber produced in each state. Product demand in the building trades is measured as the per capita value of building permits. Since these data were reported at the regional level, we matched each city observation to its particular region for the given year.

[d]The productivity variable in the coal industry is total coal output divided by total man-days (number of workers multiplied by average days worked multiplied by average hours per day). Lumber productivity is calculated as the total quantity of lumber cut per lumber worker in each state ($\times 10^{-2}$).

Table D.2 **Wage Offsets Associated with Different Specifications**

Specification	1907 to 1923		Only Compensation and Dummies Included		1907 to 1915		Only Workers' Compensation States and Years	
Sample	Linear (1)	Semilog (2)	Linear (3)	Semilog (4)	Linear (5)	Semilog (6)	Linear (7)	Semilog (8)
Coal	−1.72	−2.50	−1.55	−2.35	−1.56	−2.28	−2.38	−3.48
	(4.14)	(5.07)	(3.53)	(4.38)	(3.64)	(3.78)	(2.85)	(3.63)
Lumber	−1.04	−0.69	−0.68	−0.41	−0.95	−0.67	−1.64	0.70
	(2.11)	(1.18)	(1.21)	(0.58)	(1.94)	(1.43)	(1.48)	(0.57)
Unionized building trades	0.02	−0.17	−0.39	−0.61	0.14	−0.52	4.12	4.09
	(0.03)	(0.27)	(0.56)	(0.91)	(0.19)	(0.75)	(3.83)	(4.15)

Sources: Fishback and Kantor (1995, 732). See table D.1.

Notes: Absolute value of t-statistics in parentheses. The entries for the linear specifications are the actual coefficients from the regression. The entries for the semilog specifications are evaluated at the mean wage rate for each sample. A −1 entry would imply a dollar-for-dollar wage offset. Each regression contained the variables listed in table D.1, although the list of year and state dummies was adjusted to include only years and states included in the samples.

various labor demand and supply variables in the equations. For example, columns (3) and (4) of table D.2 report the wage offsets for the level and semilog specifications when only the expected accident compensation variable, along with the dummies for occupations, years, and geography, are included in the estimation. The same patterns are detected. Coal and lumber experienced near or greater than dollar-for-dollar offsets, although the lumber estimates are imprecise. Unionized building tradesmen experienced no offsets.[25]

The samples cover a period of substantial change in American labor markets. During World War I the federal government played a much larger role in labor and product markets and the war led to substantial shocks to labor and product markets. Nominal wages and the price level rose sharply between 1916 and 1920 and both experienced sharp declines in 1921. Within labor markets the gap between skilled and unskilled wages narrowed and the wages of southern and nonsouthern workers began to converge. While the year, skill, and location dummy variables attempt to control for these effects, another method is to limit the samples to the period prior to 1916. The restricted sample captures the substantial change in postaccident benefits associated with the adoption of workers' compensation but avoids the large wage inflation and wage squeeze associated with U.S. participation in World War I. Comparisons of columns (5) and (6) with columns (1) and (2) in table D.2 show that limiting the sample to the early years does not substantially change the results. The coal wage offset ranges between −1.56 and −2.28, while the lumber offset ranges between −0.67 and −0.95. As before, the contractual wages in the building trades are generally unaffected by the expected benefits variable. These results suggest that the wage offsets in the full samples are not driven by the narrowing of regional or skilled-unskilled wage differentials during World War I.[26]

To further test the robustness of our central results, we limited the sample to states and years when workers' compensation was in effect. This test serves a dual purpose. First, because our estimates of postaccident compensation under negligence liability are less accurate than our measures for workers' compensation, focusing solely on the workers' compensation observations is a way to reduce measurement error.[27] Second, since recent studies using modern data find wage offsets associated with more generous workers' compensation benefits, we would expect to find a similar effect for labor markets in the early twentieth century. We can test the reliability of our data by restricting our attention to the wage adjustment associated with the variations in benefit levels under workers' compensation, ignoring the effect of the big change in expected compensation when the laws were first introduced. As shown in columns (7) and (8) of table D.2, the restricted samples produce the same general patterns found in the full samples. Coal wages in fact show an offset that is larger than the one from the full sample. The lumber offset in the linear case is larger than the one from the full sample but not precisely estimated. The building trades coefficients remain positive and are larger in magnitude and are statistically different from zero.

Appendix E

A Model of Insurance Consumption and Saving Behavior

How did the rise in postaccident benefits associated with workers' compensation influence a household's decision to save for and insure against a workplace accident? The answer to this question largely depends on whether the worker's access to insurance was rationed or not. If a worker's access to workplace accident insurance was not rationed, such that he could freely buy his desired amount of insurance at a price below the opportunity cost of savings, he would have bought less insurance and saved more when postaccident benefits increased. If, on the other hand, access to insurance was limited, such that the worker faced a binding constraint on the amount of insurance he could purchase, then increases in postaccident benefits would have led him to reduce his saving.[28] Increases in postaccident benefits would have affected accident insurance purchases only if the worker's new optimal level of insurance purchases fell below the original binding constraint.

Modeling the Unconstrained Case

We capture the essential elements of the household's demand for saving and insurance using a two-period, expected-utility framework. In the first period total household income includes the earnings of the household head, y, and other household income, denoted n, which might include other family members' earnings and nonwage income, such as rent from boarders. At the beginning of the second period, the household head might have a workplace accident with probability q ($0 \leq q \leq 1$). If the primary wage earner has no accident, then the family again receives y and n in period 2. If the head of the household is killed on the job, the family still earns n from other family members plus a postaccident payment of C. The family can adjust its income stream across time periods by saving an amount s in period 1, which earns an interest rate r. The family can also insure against the income loss from an accident by paying a premium p that insures a payment of I if the household head is killed. We have parameterized the model to treat the consumption goods as the numeraire.

The household's budget constraints can be written as follows:

$$n + y = x_1 + pI + s \quad \text{in period 1},$$

$$n + y + (1 + r)s = x_{2n} \quad \text{in period 2 if no accident occurs, and}$$

$$n + (1 + r)s + C + I = x_{2a} \quad \text{in period 2 if an accident occurs.}$$

Consumption in the first period is denoted x_1, x_{2n} is consumption in the second period with no accident, and x_{2a} is consumption in the second period with an accident. The household's expected utility over the two periods can be written as

$$Z(x_1, x_{2n}, x_{2a}) = U(x_1) + (1 - q)V(x_{2n}) + qW(x_{2a}).$$

The use of different nonaccident utility functions (U and V) for the two time periods implicitly reflects the household's discount rate. Different second-period utility functions for the nonaccident (V) and accident (W) states reflect lower utility for the same income when the family loses a loved one to an accident (see Viscusi and Evans 1992). All three utility functions are assumed to rise at a diminishing rate with increasing consumption (i.e., $U' > 0$, $V' > 0$, $W' > 0$, $U'' < 0$, $V'' < 0$, and $W'' < 0$).

After solving the budget constraints for x_1, x_{2n}, and x_{2a} and substituting them into the utility function, we can derive the comparative statics in the case when insurance is not rationed. In this unconstrained setting, the household chooses a saving level s and insurance purchases I to maximize the following objective function:

$$Z(s,I) = U(n + y - pI - s) + (1 - q)V(n + y + (1 + r)s) + qW(n + C + I + (1 + r)s).$$

The first-order conditions for a maximum are

$$Z_s = -U'(x_1) + (1 + r)(1 - q)V'(x_{2n}) + q(1 + r)W'(x_{2a}) = 0,$$

$$Z_I = -pU'(x_1) + qW'(x_{2a}) = 0,$$

where Z_s and Z_I are the first derivatives of Z with respect to saving and insurance, respectively. The first-order conditions imply that the household chooses saving and insurance levels such that the ratio of the marginal utility in the first period to the expected marginal utility in the second period $\{U'(x_1)/[(1 - q)V'(x_{2n}) + qW'(x_{2a})]\}$ is equal to $(1 + r)$ and the ratio of the marginal utility in the first period to the marginal utility if an accident occurs in the second period $[U'(x_1)/W'(x_{2a})]$ is equal to the ratio of the probability of an accident to the insurance premium (q/p).

The second-order conditions for a maximum are

$$Z_{ss} = U''(x_1) + (1 - q)V''(x_{2n}) + q(1 + r)^2W''(x_{2a}) < 0$$

and

$$Z_{II} = p^2U''(x_1) + qW''(x_{2a}) < 0.$$

$$Z_{ss}Z_{II} - Z_{Is}^2 > 0,$$

where

$$Z_{Is} = pU''(x_1) + q(1 + r)W''(x_{2a}) < 0$$

because $U'' < 0$ and $V'' < 0$.

Denote the demand functions for insurance and saving derived from these first-order conditions as $s^* = s^*(y, n, r, p, q, C)$ and $I^* = I^*(y, n, r, p, q, C)$, respectively. The saving and insurance decisions become functions of the income of the household head (y), the nonwage income and income of other family members (n), the interest rate on saving (r), the probability of a workplace accident (q), the premium paid for accident insurance (p), and postaccident payments (C). Of course, these decisions will also be influenced by differences in household preferences (particularly rates of time preference), which might be based on the age of the household head, the number and ages of children in the family, and the skill levels of the workers in the household or their union status.

The comparative statics for changes in savings and insurance when postaccident compensation changes (ds^*/dC and dI^*/dC) are

$$ds^*/dC = (-Z_{sC}Z_{II} + Z_{IC}Z_{Is})/(Z_{ss}Z_{II} - Z_{Is}^2)$$

and

$$dI^*/dC = (-Z_{ss}Z_{IC} + Z_{sC}Z_{Is})/(Z_{ss}Z_{II} - Z_{Is}^2).$$

From the definition of a maximum and from the assumptions that U'', V'', and W'' are negative, the denominator of both equations is positive. Therefore, the sign of the numerator determines the results.

$$Z_{sC} = q(1 + r)W''(x_{2a}) < 0$$

because $W'' < 0$, and

$$Z_{IC} = qW''(x_{2a}) < 0$$

for the same reason.

Given this information, $-Z_{sC}Z_{II} + Z_{IC}Z_{Is}$ and $-Z_{ss}Z_{IC} + Z_{sC}Z_{Is}$ are both ambiguous in sign at first glance. If we substitute in the values of the derivatives, however, the signs of the above functions become clearer. The sign of ds^*/dC is determined by the sign of $-Z_{sC}Z_{II} + Z_{IC}Z_{Is} = W''(x_{2a})U''(x_1)\, q\, p\, [1 - (1 + r)p]$, which is determined by the sign of $[1 - (1 + r)\, p]$. This expression is positive as long as p is less than $1/(1 + r)$.

The sign of dI^*/dC is similarly affected by the relationship between p

and $1/(1 + r)$. Its sign is determined by the sign of $-Z_{ss}Z_{IC} + Z_{sC}Z_{Is} = W''(x_{2a})U'' q[p(1 + r) - 1] - q(1 - q)(1 + r)^2 W''(x_{2a})V''(x_{2n})$. Since the second term is negative, the whole term will be negative if $p(1 + r) - 1 < 0$. When p is less than $1/(1 + r)$, the model predicts that dI^*/ds is negative. Thus, if the insurance premium p is less than $1/(1 + r)$, we should expect that increases in postaccident compensation would lead to increases in savings and reductions in insurance purchases in the unrationed model.

Historically, $1/(1 + r)$ was probably no smaller than 0.9 because interest rates on saving rarely reached as high as 10 percent. Given that the probability of an accident was at most 0.2, and generally more like 0.02, the insurance premium p would have been much lower than 0.9 if insurance was actuarially fair. For the insurance premium p to exceed $1/(1 + r)$ the load factor on insurance had to have been enormous or the insurance was unavailable or rationed (in other words the premium at the margin was infinite). In fact, if p exceeded $1/(1 + r)$, the worker could not achieve a maximum because the first-order conditions would never hold. From the first-order condition $Z_1 = 0$, $p/q\ U''(x_1) = W''(x_{2a})$. Substitute this into the first-order condition $Z_s = 0$ and simplify, then $U'(x_1)[1 - (1 + r)p] = (1 + r)(1 - q)V'(x_{2n})$. By assumption U', V', $(1 + r)$, and $(1 - q)$ must be positive; therefore, $[1 - (1 + r)p]$ must be positive for the first-order conditions to hold.

Comparative Statics in the Constrained Model

The comparative statical results change markedly when we assume that insurance purchases were rationed. Insurance companies, in response to problems with adverse selection, often establish maximums for the amount of insurance people can buy and in some cases sell no insurance at all. If this constraint is binding, the worker faces a maximization problem with the extra constraint that insurance purchases I equal the maximum M. The maximization problem then becomes a Lagrangian with an objective function,

$$X(s, I, u) = U(n + y - pI - s) + (1 - q)V(n + y + (1 + r)s) + qW(n + C + I + (1 + r)s) + u(M - I),$$

where M is the maximum amount of insurance allowed by the insurance companies and u is a LaGrangian multiplier.

The first-order conditions for a maximum are

$$X_s = -U'(x_1) + (1 + r)(1 - q)V'(x_{2n}) + q(1 + r)W'(x_{2a}) = 0,$$

$$X_I = -pU'(x_1) + qW'(x_{2a}) - u = 0,$$

$$X_u = (M - I).$$

The choice functions for insurance and saving derived from these first-order conditions are now $s^c = s^c(y, n, r, p, q, C, M)$ and $I^c = I^c(y, n, r, p, q, C, M)$, and $u^c = u^c(y, n, r, p, q, C, M)$. X_{ss}, X_{II}, and X_{sI} are all negative (they are the same as the expressions Z_{ss}, Z_{II}, and Z_{sI} above). Also, $X_{su} = 0$, $X_{Iu} = -1$, and $X_{uu} = 0$.

The comparative statics show that increases in postaccident benefits (C) cause workers to save less. The sign of ds^*/dC is determined by the sign of

$$X_{sC} = q(1 + r)W''(x_{2a}),$$

which is less than zero because W'' is negative. The binding constraint on insurance purchases also leads to the result that insurance purchases are unaffected by changes in postaccident compensation ($dI^*/dC = 0$). However, this presumes that the insurance constraint remains binding. It is possible that the optimal level of insurance could fall below the constraint, in which case the impact of higher postaccident compensation would be to lower insurance purchases again.

We can also derive comparative statics for the impact on savings and insurance when insurance companies raise the maximum amount of insurance allowed. The impact on insurance (dI^*/dM) is determined by the sign of $-X_{ss}$, which is greater than zero; therefore, increases in the maximums lead workers to purchase more insurance. The impact on savings (ds^*/dM) is determined by the sign of X_{sI}, which is negative; therefore, increases in the insurance maximum lead workers to save less.

The intuition underlying the differences in the saving response in the unconstrained and rationed cases is relatively simple. If insurance were not rationed and priced near actuarial fairness, the worker would have found it much less costly to buy accident insurance than to use saving for insurance purchases. An increase in postaccident benefits would allow him to purchase lower amounts of insurance, freeing funds for more saving and consumption. If accident insurance, on the other hand, were constrained at the maximum, then saving would have been a more reasonable means of insuring against the risk of an accident, and increases in postaccident compensation would have led to reductions in saving.

Recasting the Models Incorporating Compensating Wage Differentials in Response to a Rise in Expected Benefits

We have also reformulated the model to include the presence of compensating differentials in wages in response to the change in the postaccident payment C. In that case, income y can become a function of C, $y(C)$, where $y'(C) < 0$. When insurance is unconstrained and relatively inexpensive [$p < 1/(1 + r)$], the more complicated model predicts that saving would rise whenever postaccident payments rise. When insurance is rationed, the model predicts that saving could either fall or rise when post-accident payments rise. Thus, even under compensating differentials, the only setting in which saving would be expected to fall with a rise in postaccident compensation is when there are limitations on the availability of insurance.

Appendix F

An Econometric Analysis of the Effect of Increased Expected Benefits on Saving and Insurance Behavior

This appendix reports the results of a regression analysis of how variations in expected postaccident benefits on the part of working-class families influenced precautionary saving and private insurance coverage (see Kantor and Fishback 1996). We use cross-sectional data on families' financial decisions in both workers' compensation states and negligence liability states. Between late 1917 and early 1919 the U.S. Bureau of Labor Statistics (BLS) conducted an intricate analysis of the consumption patterns of working-class families in industrial centers of the United States. The study established the budget weights for the consumer price index (U.S. Bureau of Labor Statistics 1924). Agents interviewed 12,817 families of wage earners or salaried workers in ninety-nine cities in forty-two states. Although the BLS believed that the survey families fairly represented the urban population at the time, the investigation was limited in a number of important ways. The interviewers surveyed only households of wage and salary earners where both spouses and one or more children were present. The salaried workers were not to earn more than two thousand dollars a year and the families had to reside in the same community for a year prior to the survey. Further, the BLS excluded families with more than three boarders, "slum" families, charity families, and non-English-speaking families who had been in the United States less than five years. As a result, craft workers and other high-wage workers were oversampled relative to factory operatives and laborers.

We imposed some additional limits on the sample, restricting it to laborers, operatives, and craft workers for several reasons. First, domestic service workers and farm workers were excluded because workers' compensation laws usually exempted these occupations from coverage. Second, we eliminated managers, professionals and semiprofessionals, salesmen, and clerical workers because our measure of accident risk largely pertains to the workers directly involved in the defining activities of that industry.[29] The exclusions reduce measurement error because the managerial, sales, and clerical workers were typically not exposed to the same accident risk as manufacturing workers. Third, men working in the maritime industry were eliminated from the sample because the nature of their postaccident compensation was in a state of flux at the time of the survey.[30] Fourth, railroad workers were eliminated because of inadequate information on their postaccident compensation. Railroad workers typically fared better than nonrailroad workers under negligence liability because employers could not invoke the fellow servant and contributory negligence defenses after 1908. However, because interstate railroad workers were not covered under workers' compensation, we are uncertain how they fared relative to nonrailroad workers when the laws were enacted. Fifth, government workers were eliminated because the status of postaccident compensation for these workers was poorly defined in many states.[31] After all of these restrictions to the original sample, we were left with a total of 7,475 observations.

The cost-of-living survey contains information on household purchases of accident and life insurance and household saving. "Saving" is defined as the household's total income minus its total expenditures. The BLS survey asked the family how many people in the household had accident insurance. In the sample 90 percent responded that no one had accident insurance, 9.4 percent responded that at least one person had accident insurance, and only 0.6 percent responded that more than one person had insurance. Presumably, most households with just one accident insurance policy were insuring the household head, the primary wage earner. We used this information to create a dummy variable taking the value of one if the household held one or more accident insurance policies, and zero otherwise.

The BLS also asked about the number of household members with five different types of life insurance: old-line (whole life), fraternal, industrial, establishment, and other types. Because life insurance covered so many more people within the household than accident insurance, the life insurance variable by necessity focuses on the purchases of life insurance for all members of the household. We used the information to create a dummy variable valued at one if the household claimed at least one life insurance policy, and zero otherwise. Given the zero-one nature of the variables we estimated probit equations.

The insurance and saving functions that we estimate can be derived from the first-order conditions in appendix E or from extensions of the models that Leland (1968) and Kotlikoff (1989) developed. In such a model the demands for insurance and precautionary saving would be affected by the expected postaccident benefit, accident risk, the interest rate, and income. We included other demand variables pertaining more specifically to the household's financial status. Thus, the regressions include the wife's annual earnings, the children's earnings, income from boarding and lodging, net income from rent and interest, and the number of children between ages zero and four, five and nine, ten and fourteen, and older than fourteen. Moreover, following Haines (1985), our empirical models include a number of variables designed to capture differences in the utility functions across households. We control for possible life-cycle effects by including the age and age squared of the household head, and we include dummy variables controlling for his occupational skill level and whether he contributed to a labor organization. To capture differences in the cost of living that households faced, we included an index of the average cost of living in urban areas in each state for the period 1919 to 1921. We also included regional dummy variables to capture geographic differences in interest rates caused by differences in banking regulations and differences in insurance premiums caused by state-specific regulations or varying costs of selling and monitoring policies across the country.

To measure the workplace accident risk that each worker in the sample faced, we matched each worker's industry with the premium paid per one hundred dollars on the payroll that *employers* in that industry were required to pay into the Ohio State Workmen's Compensation Fund in 1923. Note that this premium is not the one that workers paid for personal life or accident insurance. We chose the Ohio information because Ohio had a wider range of industries than any other state where premiums were available. The premiums that employers paid should be correlated with fatal and nonfatal accident risk in the workplace because the Ohio Industrial Commission sought to price the insurance so that industries paid for the accident costs they generated. To some extent, this accident risk measure should be correlated with the accident insurance premium that a worker would have paid for private accident insurance, which was priced according to his particular industry and occupation.

To calculate a worker's expected postaccident compensation, we followed the procedures described in appendix B. In this case we used the worker's actual wage to calculate the expected benefits.

Coefficient estimates and t-statistics from the probit estimations of life and accident insurance coverage and ordinary least squares estimation of the saving equation are presented in table F.1. To calculate the impact of a one-standard-deviation change in each of the independent variables on the life insurance and accident insurance probabilities, we followed the standard procedure of translating probit coefficients into marginal effects. That is, we calculated a baseline probability of purchasing insurance by setting the independent variables at their sample means and the dummy variables equal to zero. We then computed the change in the baseline probability caused by a one-standard-deviation change in each independent variable, holding all others constant at their sample means. The marginal effects of the dummy variables represent shifts from zero to one.

The coefficient of the expected-benefits variable in the life insurance probit is negative, but it is not statistically significant. A one-standard-deviation change in the expected postaccident benefits ($7.15) would have

Table F.1 **Coefficients from Probit Analyses of Accident and Life Insurance Coverage and OLS Estimation of Saving**

Variable	Means (1)	Life Insurance Purchased {0,1} (2)	Accident Insurance Purchased {0,1} (3)	Saving (4)	Saving (5)
Intercept		0.320	−6.491[a]	−330.7[a]	−48.78
		(0.184)	(2.394)	(1.995)	(1.055)
Present value of expected benefits	14.67	−0.0061	−0.0165[a]	−1.624[a]	−1.156[a]
	(7.149)	(1.578)	(3.634)	(4.544)	(4.501)
Accident risk	1.203	0.00098	−0.0196	2.016	1.639
	(1.054)	(0.056)	(0.957)	(1.187)	(0.966)
Age of husband	36.93	0.0169	−0.0067	2.148	2.301
	(8.463)	(1.075)	(0.367)	(1.368)	(1.467)
Age squared	1,435	−0.00019	0.00017	−0.004	−0.005
	(679.6)	(0.956)	(0.752)	(0.201)	(0.283)
Husband's annual earnings	1,301	0.00012[a]	0.00040[a]	0.202[a]	0.202[a]
	(363.6)	(2.050)	(6.274)	(35.45)	(35.71)
Wife's annual earnings	18.53	−0.00017	0.00055	0.143[a]	0.138[a]
	(73.99)	(0.686)	(1.911)	(5.733)	(5.580)
Net income from rent and interest	6.003	−0.0015[a]	0.00062	0.456[a]	0.451[a]
	(32.23)	(3.012)	(1.058)	(8.200)	(8.106)
Children's annual earnings	87.93	0.00015	−0.00001	0.194[a]	0.194[a]
	(272.7)	(1.330)	(0.057)	(17.80)	(17.86)
Income from board and lodging	5.165	−0.00006	−0.0011	−0.154[a]	−0.153[a]
	(29.47)	(0.089)	(1.306)	(2.542)	(2.537)
State cost-of-living index	101.5	−0.0024	0.052[a]	1.264	−1.613[a]
	(5.139)	(0.147)	(2.033)	(0.817)	(4.547)
Contributes to union organization	0.320	−0.067	−0.090	−13.93[a]	−13.39[a]
	(0.467)	(1.698)	(1.944)	(3.542)	(3.457)

Craft occupation	0.489	0.151[a]	0.030	4.695	4.374
	(0.500)	(3.054)	(0.492)	(0.954)	(0.889)
Operative occupation	0.301	0.111[a]	0.181[a]	−0.027	−0.569
	(0.459)	(2.167)	(2.942)	(0.005)	(0.111)
Number of children ages	0.917	−0.025	−0.009	−16.94[a]	−17.42[a]
0 to 4	(0.871)	(1.076)	(0.349)	(7.467)	(7.691)
Number of children ages	0.802	0.062[a]	−0.041	−20.05[a]	−20.51[a]
5 to 9	(0.847)	(2.702)	(1.557)	(9.080)	(9.291)
Number of children ages	0.567	0.004	0.030	−26.48[a]	−26.74[a]
10 to 14	(0.804)	(0.156)	(1.045)	(10.45)	(10.52)
Number of children ages	0.312	−0.026	−0.044	−37.07[a]	−37.33[a]
15 and up	(0.711)	(0.584)	(0.843)	(8.34)	(8.409)
Regional Dummy Variables					
New England	0.119	0.643[a]	−0.539[a]	−6.827	
	(0.324)	(6.222)	(3.759)	(0.668)	
Mid-Atlantic	0.189	0.735[a]	−0.644[a]	3.743	
	(0.391)	(8.774)	(5.876)	(0.446)	
East North Central	0.220	0.509[a]	−0.841	14.56	
	(0.415)	(3.693)	(0.414)	(1.081)	
West North Central	0.086	0.406[a]	−0.265	−10.47	
	(0.281)	(3.602)	(1.839)	(0.931)	
South Atlantic	0.119	0.555[a]	−0.996[a]	−11.80	
	(0.324)	(5.021)	(6.877)	(1.085)	
East South Central	0.059	0.466[a]	−0.628[a]	4.121	
	(0.236)	(3.636)	(3.753)	(0.329)	
West South Central	0.048	0.273	−0.120	−1.458	
	(0.213)	(1.830)	(0.933)	(0.098)	

(continued)

Table F.1 (continued)

Variable	Means (1)	Life Insurance Purchased {0,1} (2)	Accident Insurance Purchased {0,1} (3)	Saving (4)	Saving (5)
Mountain	0.055	0.013	−0.578	−63.92[a]	
	(0.228)	(0.051)	(1.436)	(2.478)	
N	7,475	7,475	7,475	7,475	7,475
Adj. R^2				0.214	0.212
Mean of dependent variable		0.852	0.100	71.71	71.71
				(173.1)	(173.1)

Sources: Kantor and Fishback (1996, 434–35). "Cost-of-Living in the United States, 1917–1919," available through the Inter-University Consortium for Political and Social Research, No. 8299. The accident risk measure matches each worker's industry with the workers' compensation premium paid per one hundred dollars on the payroll by Ohio employers in 1923. The premiums are reported in Ohio Industrial Commission (1923). The expected benefits are calculated based on the procedure described in appendix B. The cost-of-living index is calculated from Williamson and Lindert (1980, 323–25). They report a Koffsky-adjusted cost of living for the entire state including rural and urban areas, which was originally derived from urban cost-of-living indices. We reversed the formula to calculate the urban cost of living, by dividing the cost of living they report on pages 323–24 by $(1 - a \times 0.065)$, where a is the percentage of farm workers in the labor force and 0.065 is the percentage difference between the urban and rural cost of living.

Notes: Absolute values of t-statistics are in parentheses. Standard deviations are reported in parentheses below all means. The craft dummy variable has a value of one for all workers listed as craftsmen, foremen, or kindred workers (code numbers 300–398 in the 1940 census occupation codes, see note 29). The operatives dummy has a value of one for all workers listed as operatives and kindred workers (codes 400–496). The category left out of the regressions is laborers (codes 900–988). The BLS survey was taken during the period from August 1917 through February 1919, a period of substantial inflation. We also ran the regressions with the monetary values adjusted to constant dollars using Douglas's cost-of-living index (1890–1899 = 100) as the deflator (U.S. Bureau of the Census 1975, 212). The central results reported in the table remain the same when the monetary variables are deflated.

[a]Statistically significant at the 5 percent level.

lowered the probability of purchasing life insurance by only 0.1 percentage point—from 86.1 percent to 86.2 percent—when evaluated at the independent variables' sample means.[32] Changes in expected accident benefits had more influence on the probability of purchasing accident insurance, as a one-standard-deviation change in the benefits would have lowered the probability of purchasing accident insurance by a statistically significant 1.9 percentage points.

The coefficient on expected benefits in the saving regression in column (4) of the table indicates that each dollar increase in expected benefits was associated with a reduction in saving of $1.62, which is statistically significantly different from zero.[33] To get a sense of the general magnitude of a switch from negligence liability to workers' compensation, consider a worker who moved from Virginia, where negligence liability was still in force, to the neighboring workers' compensation state of Maryland. All else equal, his expected postaccident benefits would have risen by approximately eleven dollars. Such an increase would have allowed him to reduce his precautionary saving by $17.82, or about 25 percent of the mean level of saving in the sample.

The $1.62 estimate may understate the full macroeconomic effect of introducing workers' compensation because income is held constant in the saving regression. If, as we argue in chapter 3, workers paid for increases in postaccident benefits in the form of lower wages, then the reduction in earnings associated with the adoption of workers' compensation itself probably led to reduced saving. Consider a situation in which the household head experienced a wage offset that lowered his annual earnings by the full increase in his expected postaccident benefits. The coefficient on the household head's income in the table implies that saving would have fallen an additional $0.202 for each dollar increase in expected postaccident benefits, thus raising the overall impact on saving to $1.82.

We have performed several tests to ensure that the estimated impact of changes in postaccident benefits on saving behavior is not spurious. There may be worries that the regional dummy variables are capturing some of the impact of workers' compensation, so we have estimated the equation without the regional dummies. The coefficient of the expected-benefits variable is slightly smaller at -1.16 and remains statistically significant. It should be noted, however, that F-tests reject the hypothesis that the coefficients of the regional dummy variables are simultaneously zero.

Another potential criticism is that states without workers' compensation in 1918 tended to be southern states, where saving might have been lower. Further, there may be questions about measurement error in the non–workers' compensation states because we could not rely on explicit laws to estimate the expected accident benefits in such states. Such concerns are unfounded, however. The saving regression includes income and regional dummies that should control for this effect. Further, we estimated the saving equation on a sample that eliminated the non–workers' compensation states and the expected-benefits coefficient was actually somewhat larger at −2.24, with a *t*-statistic of −5.48.

Another more serious concern is that the BLS survey was taken during World War I, which was a period of substantial upheaval in the economy. The government became heavily involved in labor markets and the economy experienced substantial demand and supply shocks that were probably unevenly distributed geographically. The saving result potentially could be spurious if these shocks were correlated in some way with the generosity of workers' compensation benefits across states. We have tested for this possibility by estimating the saving regression on an alternative sample of households from the BLS survey that would have been largely unaffected by the expected benefits under workers' compensation. In this sample we included professional and clerical workers who did not face the risks that operatives, workers, and craftsmen faced, domestic service workers who were not covered by workers' compensation, federal government workers who were covered under federal workers' compensation law, and railroad workers who were involved in interstate commerce and were covered under an entirely different set of liability rules. For each worker in this alternative sample we calculated his expected benefits as if he were covered under his state's workers' compensation law and faced the same accident risk as that of manufacturing operatives and skilled workers in his particular industry. Given that these households' saving decisions were not in actuality affected by the generosity of their states' workers' compensation programs, we would expect to find that the expected-benefits variable would have a small and statistically insignificant effect on saving. That is exactly what we find. The expected-benefits coefficient when regional dummies are included is small at −0.56, roughly one-third the size of the coefficient in the table, and we cannot reject the hypothesis that the coefficient is zero (*t*-statistic of −0.76). In an equation with the regional dummies excluded, the coefficient is 0.21, and again statistically insignificant (*t*-statistic of 0.42).

Most of the other variables in the table tended to affect saving and insurance as expected. The age variables indicate that saving and insurance purchases increased at a diminishing rate with age, but the coefficients are not statistically significant. A husband's higher earnings were associated with more saving and insurance coverage, while the earnings of other household members had a positive effect on saving, but varied effects on accident insurance purchases. Households also saved less as they had more older children. For example, an additional child between the ages of zero and four lowered saving by seventeen dollars, but one more child older than fifteen years reduced saving by thirty-seven dollars. This result may be driven by the fact that saving is measured as a residual (household income minus expenditures), and older children may have consumed more in terms of food and clothing. Alternatively, the result might be interpreted as evidence that families used children as substitutes for precautionary saving. Having children in the household who could be sent to work in case of financial hardship meant that families did not have to rely so heavily on saving as a means of insurance.

Appendix G
Employers' Liability Laws

Employers' liability laws were a group of statutes that outlined the liability of employers for workplace accidents. Because of the wide range of such laws (Clark 1908) we have classified them into several types: laws that had an impact on employers' liability for most nonrailroad workers in the state (these are the laws summarized in table 4.1), laws that restated the common law for all workers, laws that influenced the liability of railroad employers, laws that assigned liability to mineowners for willful violations of the mine safety laws, laws that specified employers' liability in mining, and laws that outlawed contracts in which workers' waived their liability.

In determining the classification of laws we began with Clark (1908) to get a picture of the situation as of 1907. We went backward in time and examined the laws as of 1900, as reported by Fessenden (1900, 1157–210). We then went forward in time and reexamined the laws again as of 1913, using U.S. Bureau of Labor Statistics (1914) and then once again reexamined the laws as of 1925 using U.S. Bureau of Labor Statistics (1925a). For states where there were changes in the law, we examined when the changes were made (often the date of the change was listed in these sources). These laws are complex and in some cases interpreting them requires a heavy reliance on court interpretations; other scholars might choose to reclassify some of our designations.

Laws That Affected Employers' Liability for Nonrailroad Workers

Often employers' liability laws are lumped into one category. In a number of states the employers' liability law referred specifically to railroading or mining, while in other states the law was more general. Railroad workers were covered by the Federal Employers' Liability Acts of 1906 and 1908 if they were involved in interstate commerce. Given that workers' compensation was focused more on nonrailroad workers, we feel it important to identify a separate class of laws that were general or focused on manufacturing. In this grouping of laws, for which annual totals are given in table 4.1, we have tried to focus on those laws that we felt increased the liability of employers. There was a class of laws that restated the common law and thus probably had no impact on the liability of employers, and we have grouped those in a separate category.

The states with laws that appear to have affected employer liability for nonrailroad workers include Alabama (pre-1900), Arizona (1912), Arkansas (1913), California (1907), Colorado (pre-1900, 1901), Indiana (pre-1900, 1911), Idaho (1909), Iowa (1907), Kansas (1903 and 1909), Louisiana (pre-1900), Maine (1909), Massachusetts (1902, 1909), Mississippi (1896 ending in 1903, 1910), Nebraska (1913), Nevada (1905), New Jersey (1910), New York (1902), Ohio (1902, 1904, 1910), Oklahoma (1907), Oregon (1907), Pennsylvania (1907), Utah (pre-1900), Vermont (1910), Washington (1903), Wisconsin (1906), and Wyoming (pre-1900).

Alabama, Colorado, Massachusetts, and New York had detailed employers' liability laws that described contributory negligence, the fellow servant defense, and assumption of risk in detail and seemed to be more than just a restatement of the common law. Colorado in 1901 limited the fellow servant defense. Massachusetts in 1909 further altered the employers' defenses by establishing comparative negligence and limits on liability. The Arizona Constitution of 1912 and consequent legislation in 1912 abrogated the fellow servant defense and established comparative negligence. Utah specified in detail who was considered a fellow servant. Indiana established that the burden of proof for contributory negligence was on the defendant prior to 1900 and then in 1911 set up a more-detailed employers' liability law. Iowa in 1907 established an assumption of risk law that ensured that if an employee notified the employer of a defect in machinery, the employee had not assumed the risk if he continued to work on the machine. Louisiana had a very broad statement that seemed to limit the fellow servant defense because it was based on the Napoleonic Code. In 1912 Louisiana further limited the assumption of risk defense. Mississippi established comparative negligence in 1910. In 1896 Mississippi enacted a law that tried to expand its railroad employers' liability law to apply to all corporations but it was struck down as unconstitutional in 1903 (Clark 1908, 114). Oklahoma's Constitution of 1907 stated that the fellow servant defense was not applicable in mining and railroading. Oklahoma also established that a jury was to decide as a question of fact whether the assumption of risk and/or contributory negligence defenses applied; therefore, we classified it as having an impact on liability. Alabama, Indiana, and Pennsylvania imposed limits on the fellow servant defense. Wyoming's constitution disallowed laws that would limit the amount of damages to be recovered by an injured person. Idaho in 1909 established a general employers' liability bill, as did Vermont in 1910. Maine's 1909 law seemed to cover both nonrailroad and railroad employers. Nevada had a statute similar to Connecticut, which restated the common law, although it was stated differently enough that we have classified it as having an impact on employers' liability.

Ohio established a statute making the employer liable for accidents that resulted from their failure to follow the inspection statutes; in 1904 the state limited assumption of risk; and in 1910 Ohio established comparative negligence and limited the fellow servant defense. Oregon, Washington, and Wisconsin as part of their factory inspection laws made the employer liable for failure to comply with the factory inspection. In Kansas, the statute requiring the installation of fire escapes and safety devices in manufacturing establishments authorized an action for injuries or death from the employer's disregard of the act. Kansas in 1909 later established a factory act like the ones in Washington and Oregon. Nebraska passed a 1913 statute that was similar to the ones in Oregon, Washington, and Wisconsin. Although Rhode Island and New Jersey (in the 1890s) had statutes for fire escapes and elevators, we did not treat them as a main employers' liability law because of their limited focus. New Jersey in 1910 established a general employers' liability act. Arkansas set up comparative negligence and limited assumption of risk in 1913. Colorado in 1911 imposed a liability maximum of five thousand dollars and added a factory inspection clause that imposed liability for noncompliance. Florida in 1913 passed a law establishing comparative negligence and disallowing contracts whereby workers waived their right to sue in the railroad, street railway, telephone and telegraph, boating, and blasting industries. However, we treated this Florida law as primarily a railroad law.

Laws That Restated the Common Law

Clark (1908) claimed that Arizona, California, Connecticut (1902), Georgia, Minnesota, Montana, North Dakota, Oklahoma, and South Dakota (pre-1900) all had statutes or constitutional provisions that simply restated the common law, without expanding employers' liability.

Employers' Liability for Willful Failure to Follow Mining Statutes

The following states included in their mining regulations a statement that willful violation of the mining statute could lead to a rightful claim for damages: Arkansas, California, Colorado, Illinois, Indiana, Iowa, Maryland (1902), Michigan, Missouri, New Mexico, North Carolina, Ohio, Oklahoma, Pennsylvania, Utah (1905), Washington, and Wyoming. We treat this type of employers' liability law as a separate category specific to mining. All of the laws were enacted prior to 1900 unless another year is noted in parentheses.

General Employers' Liability Laws Specific to Mines

Maryland (1902), Missouri (1907), Nevada (1907), and Oklahoma (1907) all had general liability laws for the mining industry.

Laws Preventing Workers from Signing Contracts Waiving Rights to Negligence Suit Prior to Injury

States with laws preventing such ex ante contracts in railroading included Arkansas, Florida, Iowa, Minnesota, Mississippi, Missouri, Nebraska, Nevada (1907), New Mexico, New York, North Carolina, North Dakota, Ohio, Oregon, South Carolina, South Dakota, Texas, Virginia, and Wisconsin. States with general laws were Alabama (1907), Arkansas (1913), California, Colorado, Georgia, Idaho (1909), Indiana, Massachusetts, Missouri (mining only), Montana, Nevada (1907 for railroading, mining, and milling), Ohio (1910), and Wyoming (constitution).

Employers' Liability Laws for Railroads

The states with employers' liability laws for the railroad industry included Arkansas (pre-1900), Colorado (pre-1900), Delaware (1903), Florida (pre-1900), Georgia (pre-1900), Illinois (pre-1900), Indiana (1901), Iowa (pre-1900), Kansas (pre-1900), Kentucky (1903), Maine (1905), Massachusetts (pre-1900), Michigan (1909), Minnesota (pre-1900), Mississippi (pre-1900), Missouri (pre-1900), Montana (pre-1900), Nebraska (pre-1900), Nevada (1907), New Mexico (pre-1900), New York (1906), North Carolina (pre-1900), North Dakota (pre-1900), Ohio (pre-1900), Oklahoma (1907), Oregon (1903), South Carolina (pre-1900), South Dakota (1907), Texas (pre-1900), Vermont (pre-1900), Virginia (1902), Washington (pre-1900), Wisconsin (pre-1900), and Wyoming (1913).

Appendix H

Discrete-Time Hazard Analysis of the Timing of Adoption across the United States

In chapter 4 we discuss how the timing of adoption across states was influenced by interest group pressure, changes in the climate of employers' liability, and political reform movements. In the text we show comparisons of mean values of various factors that influenced adoption for states adopting prior to 1913, between 1913 and 1916, and after 1916. It is important to remember, however, that all of these factors are influencing the adoption process at the same time. Thus, we must look at the impact of these factors on adoption from a multivariate perspective. This section provides the statistical background for the statements made in chapter 4, section 4.3. The analysis was first reported in Fishback and Kantor (1998a).

To examine the marginal impact of each factor on the probability of adoption, we estimated an equation summarizing the first-time adoption decisions by state legislatures between 1909 and 1930 using a discrete-time hazard model with time-varying covariates (Allison 1984, 16–22; Yamaguchi 1991, 16–24):

$$\ln(p(t;\mathbf{X})/[1 - p(t;\mathbf{X})]) = a + b\mathbf{X} + e,$$

where $p(t; \mathbf{X})$ is the conditional probability of adoption at a discrete point in time t given that the event did not occur prior to time t and given the covariate vector $\mathbf{X}$ (which includes variables measuring the liability environment, interest group strength, and the political climate); b is a $1 \times k$ vector of coefficients for the $k \times 1$ covariate vector $\mathbf{X}$; a is the log-odds of a baseline group where the vector $\mathbf{X}$ is all zeroes; and e is an error term.[34] We chose the discrete-time hazard model because there were significant discontinuities in the opportunities for legislatures to adopt the legislation. The legislatures could only adopt the law when they met, and most legislatures met every other year.

Table H.1 presents the variables' economic impacts on the probability of adopting workers' compensation, based on the coefficients from the discrete-time hazard model.[35] For the continuous variables the impact represents the marginal effect on the probability of adopting workers' compensation (in year t, given that the state had not already adopted the law) caused by a one-standard-deviation (OSD) increase in each independent variable, holding all others constant at their sample means. The marginal effects of the dummy variables show the change in the probability when the dummy variables switch from zero to one. Note that a positive effect implies that an increase in the variable leads to a higher probability of enacting the legislation. The absolute value of t-statistics of the coefficients from the hazard model are reported in parentheses to allow the reader to assess the statistical significance of the original hypothesis tests on the coefficients from the model. In the discussion below we focus on the results in column (1).

Changes in the Legal Environment Governing Accident Compensation

There are several signs that changes in the employers' liability climate influenced the timing of first adoption by state legislatures. The presence of an employers' liability law for nonrailroad workers that altered one or more of the three common law defenses raised the probability of adopting workers' compensation by a statistically significant 7.9 percentage points. In contrast, the presence of a law that restated the common law without truly expanding the employers' liability law had only a small and statistically insignificant effect on the probability of adoption.

Table H.1 **Economic Impact of Changes in Variables on the Probability of Adopting Workers' Compensation, 1909–30, Derived from Parameters Estimated in Discrete-Time Hazard Model**

Variables	Mean (std. dev.)	Impact of Changes on the Probability of Enacting Workers' Compensation (absolute value of *t*-statistic of underlying regression coefficient)[a] (1)	(2)
Baseline probability		0.043	0.043
Changes in workplace accident liability			
Employers' liability law limiting common law defenses	0.430 (0.496)	0.079 (2.27)	0.078 (2.21)
Employers' liability law restating the common law	0.157 (0.365)	0.002 (0.05)	−0.0001 (0.004)
Ratio of employers' liability and accident insurance premiums to life insurance premiums	0.113 (0.048)	0.036 (2.39)	0.039 (2.49)
Index of workplace accident supreme court cases (1904–6 = 1) lagged one year	1.897 (2.63)	0.023 (1.97)	0.020 (1.71)
Manufacturing accident risk index	1.789 (0.659)	0.013 (0.60)	0.006 (0.29)
Interest group influence			
Percentage of workers in manufacturing establishments with less than 5 workers	7.44 (6.37)	−0.016 (0.89)	−0.015 (0.77)
Percentage of workers in manufacturing establishments with more than 500 workers	23.7 (11.0)	0.047 (1.92)	0.044 (1.77)
Manufacturing value added per worker (thousands; constant 1967 dollars)	4.451 (1.39)	0.049 (1.96)	0.054 (2.06)
Percentage of labor force employed in manufacturing	22.12 (11.08)	0.076 (1.78)	0.086 (1.89)
Percentage of labor force employed in mining	2.32 (3.61)	0.002 (0.12)	0.001 (0.06)
Manufacturing unionization index	9.484 (4.604)	0.110 (3.03)	0.102 (2.84)
Life insurance premiums per worker (constant 1967 dollars)	45.76 (18.48)	−0.008 (0.49)	−0.013 (0.81)
State spending on labor-related bureaucracy per worker (constant 1967 dollars)	0.159 (0.177)	−0.016 (1.25)	−0.014 (1.02)

Table H.1 (continued)

Variables	Mean (std. dev.)	Impact of Changes on the Probability of Enacting Workers' Compensation (absolute value of t-statistic of underlying regression coefficient)[a] (1)	(2)
Political climate			
Power shift in at least one branch of legislature	0.132 (0.339)	0.078 (1.59)	0.071 (1.48)
Power shift in both branches of legislature	0.083 (0.399)	0.009 (0.17)	0.013 (0.25)
Progressive law index	4.65 (1.82)	0.019 (1.13)	0.012 (0.70)
Progressive vote for Roosevelt in 1912 presidential election	15.60 (10.37)	0.093 (3.77)	0.083 (3.37)
Percent of presidential vote for socialist	3.673 (3.296)	0.010 (0.55)	0.012 (0.64)
Southern state dummy variable	0.541 (0.499)	0.074 (1.82)	0.069 (1.66)
"Contagion effect"			
Percentage of nearby states that had adopted workers' compensation $t - 1$	0.277 (0.316)		0.016 (1.03)

Sources: Fishback and Kantor (1998a, 322–23). See appendix I.

Note: The information covers the forty-eight states for the period 1909 to 1930 when the state legislatures met up until the year of adoption by the legislature in each state, with a total of 242 state-years.

[a]The impact of changes for continuous variables is based on a one-standard-deviation increase in each of the continuous variables, holding the other variables constant at their sample means. The marginals of the dummy variables are based on switches from 0 to 1, centered at the mean for the variable, holding all else constant. The baseline probability was computed at the sample means of all the variables. The t-statistics in parentheses are the ones from the estimated coefficients of the discrete-time hazard model; they cannot be used to construct confidence intervals for the measures of the impact of the variables.

A rise in the supreme court case index, our measure of increased legal uncertainty, was associated with a statistically significant increase in the probability of adopting workers' compensation. An OSD increase in the index increased a state's probability of enacting the legislation by 2.3 percentage points.

To capture the expansion in employers' liability insurance premiums and greater interest in insurance purchases, we included the ratio of employers' liability insurance premiums to life insurance premiums that were collected by commercial insurance companies in each state. Consistent with the view that expanding employers' liability led to the adoption of workers' compensation, an OSD increase in the insurance ratio raised the adoption probability by a statistically significant 3.6 percentage points.[36]

To examine the impact of the shift in manufacturing employment toward more dangerous jobs, we created an accident risk index based on the manufacturing industrial mix in each state. Shifts of manufacturing employment into more dangerous industries did not appear to stimulate earlier adoption of the law. The risk index had a small and statistically insignificant impact on the probability of adopting workers' compensation. This result may imply that greater public awareness of accident risk was not nearly as important as changes in the employers' liability climate.

Interest Group Influence

Greater strength of the manufacturing lobby raised the probability of adoption. In states where the manufacturing share of employment was OSD higher than the mean, the probability of adoption was a statistically significant 7.6 percentage points higher. A greater presence of large firms and more productive firms also increased the probability of adopting workers' compensation. Within the manufacturing lobby, an OSD increase in the percentage of establishments with over five hundred workers raised the likelihood of adoption by 4.7 percentage points, while an OSD increase in manufacturing value added raised the probability of adoption by 4.9 percentage points. Meanwhile, an OSD increase in the percentage of workers in firms with less than five workers, the types of firms typically exempted from the law, had a small negative but statistically insignificant impact on the probability of adoption.

Organized labor joined manufacturing interests in strongly supporting the passage of workers' compensation. We developed a union index in each state, which reflects the degree to which the industries represented in the state were unionized at the national level. An OSD increase in the union percentage increased the probability of adopting the law by 11.0 percentage points, and the effect is statistically significant.

To measure the general strength of the insurance lobby, we included the total life insurance premiums paid (in 1967 dollars) divided by the number of workers in the state. The life insurance premiums per worker had virtually no impact on the probability of adopting workers' compensation. Finally, an OSD increase in state spending on labor issues had a small (−1.6 percentage point) and statistically insignificant impact on the probability of adoption.

Political Climate

We examine the impact of the Progressive Movement with four measures: the electorate's support for socialist presidential candidates, the support for Theodore Roosevelt's progressive presidential candidacy in 1912, variables that measure shifts in party control of state legislatures, and an index of progressive laws that each state had adopted to date.

The percentage of the vote won by Theodore Roosevelt's progressive presidential campaign offers a rough measure of the extent to which voters in each state supported the nationwide progressive platform in 1912. An OSD increase in the percent voting for Roosevelt raised the probability of adopting workers' compensation in any one year by 9.3 percentage points.[37] It should be noted, however, that the support for socialist candidates had only a very small effect on the probability of adoption, and we cannot reject the hypothesis of no effect.

One way to capture the effect of reform movements at the state level is to measure their success in adopting other forms of progressive legislation. Therefore, we developed an index of progressive legislation that each state had in place at any point in time. The index measures the number of laws from the following list of progressive proposals that the state had passed: ballot initiatives, referenda, direct primaries, a mothers' pension law, a state tax commission, compulsory school attendance legislation, a state welfare agency, a merit system for state employees, a minimum age for child labor, and a state commission to regulate electricity rates. An OSD increase in the progressive law index raised the probability of adoption by 1.9 percentage points, but the effect was not statistically significant.

Another way to capture the effect of reform movements is to create dummy variables that track the major political party shifts occurring within each state's legislature. The first legislative power shift variable takes a value of one if in at least one branch of state *i*'s legislature, the majority party of the previous session lost its majority coming into year *t*'s session.[38] When only one branch changed parties, it typically produced a situation in which different parties controlled each of the state's two legislative chambers.[39] The second dummy variable has a value of one if both branches of the legislature experienced a political power shift. For every double-chamber power shift in the sample, both chambers always shifted to the same party. We would expect that the probability of enacting workers' compensation would have increased if both branches of a state's legislature experienced a power shift, because otherwise each branch would have had veto power over the decisions of the other.[40] A single-chamber power shift increased the probability of adoption by 7.8 percentage points, but we cannot reject the hypothesis of no effect. A double-chamber shift had a very small influence on the probability of adoption.

The analysis also includes a southern dummy variable to control for any "southern" effect that is not already captured by the other independent variables. States in the South were more likely to adopt workers' compensation, holding the other factors constant.[41]

"Contagion" Effect

When workers' compensation was first being considered, employers raised concerns that the legislation would put them at a cost disadvantage relative to their competitors in neighboring states. Given this attitude, employers may have been more willing to endorse workers' compensation when they were assured that their rivals in other states had similar labor costs. In an attempt to measure such a "contagion" effect, we reestimated the hazard equation including a variable that measures the proportion of nearby states that had adopted workers' compensation by the end of year $t - 1$.[42] The results, reported in column (2) of table H.1, suggest that states were more likely to enact workers' compensation if nearby states had adopted before them. An OSD change in the contagion variable raised the probability of adopting the law by only 1.6 percentage points, however, and the coefficient from the hazard analysis is not statistically significant.[43] Comparisons of the two columns of results in the table show that inclusion of the contagion effect has relatively little impact on the other findings.

Appendix I

Data Sources and Descriptions of Quantitative Variables

This appendix describes the sources of the variables used in the state panel that underlies the comparisons of means in chapter 4 and the hazard model analysis reported in appendix H.

Workers' Compensation Laws

The years in which states enacted their workers' compensation laws and the details of the laws described in various chapters throughout the book come from Clark and Frincke (1921), Hookstadt (1918, 1919, 1920, 1922), Jones (1927), U.S. Bureau of Labor Statistics Bulletins 126 (1913b), 203 (1917), 243 (1918), 332 (1923), 423 (1926b), and 496 (1929), and from closer inspection of the state statutes for the timing of changes in the various parameters of the law.

Variables Characterizing Employers' Liability Laws

Information on the status of each state's employers' liability laws was collected from a variety of sources: Fessenden (1900, 1157–210), U.S. Department of Labor (1903, 1363–64), Clark (1908 and 1911, 904–11), and U.S. Bureau of Labor Statistics Bulletins 111 (1913a), 148 (1914), and 370 (1925a). See also appendix G.

Insurance Measures

The employers' liability and accident insurance premiums that were collected in each state are reported in the Spectator Company's *Insurance Year Book,* 1900 through 1930. For many states there were not separate listings for accident and employers' liability insurance, which is why we have combined the two types of insurance. Although the *Year Book* was published each year, the volumes sometimes did not contain information for the current year, but repeated data from an earlier year. When data were missing in the years before workers' compensation was introduced, we filled the gaps using a straight-line interpolation of the years surrounding the missing year. In some cases the employers' liability and accident insurance data were missing for the year of adoption. In that case we estimated the data using an extrapolation procedure in which we multiplied the ratio of accident and employers' liability insurance to life insurance from the previous year and multiplied that ratio by the amount of life insurance sold during the year of adoption. We could not use straight-line interpolation to estimate the value for the adoption year because workers' compensation dramatically changed the nature of the employers' liability insurance market.

The life insurance premiums are the sum of ordinary and industrial life insurance premiums, also from the *Year Book.* For years with missing insurance premium data, we multiplied the reported life-insurance-in-force measure by the ratio of premiums to insurance-in-force over the period for which we had data. In some cases we still had missing data, because insurance-in-force was not reported, so we filled those years with straight-line interpolations between adjacent years. We deflated the life insurance premiums using the CPI (1967 = 100).

Index of Workplace Accident Supreme Court Cases

The index of state supreme court cases dealing with workplace accidents is based on counting all nonrailroad, street railroad, and railroad nontrain cases in each state's supreme court reporter. In searching for cases, we began with the following headings in the reporters indexes: master-servant liability, negligence, employer liability, assumption of risk, fellow servant, contributory negligence, personal injuries, and other headings referenced in those. We read each case to ensure that it dealt with a workplace accident and not, for example, a dispute over wages. For each state, we then created an index that used the average number of cases from 1904–6 as the base. We use an index of the cases, instead of the actual number of cases adjudicated in our adoption regression equation, because there were differences in the structure of court systems across states that might have led to differences in the number of cases reaching the state supreme court level.

We eliminated railroad train cases because railroad workers who were injured in the course of interstate commerce after 1908 were covered under the Federal Employers' Liability Act. Workers' compensation laws covered nonrailroad cases, street railroad cases, and some railroad cases if the person was not involved in interstate commerce. Typically, railroad workers in the nontrain category were ones who were not likely to be involved in interstate commerce and probably were covered by a workers' compensation law.

Index of Accident Risk

The index of manufacturing accident risk is based on the workers' compensation premiums that Ohio employers paid into the Ohio State Insurance Fund in 1923, and the distribution of manufacturing employment in 1899, 1909, 1919, and 1929. The index is a weighted average of the accident risk in each state's manufacturing industries, where the relative danger in each industry is held fixed over time. The only reason a state's index would change over time is because of changes in the relative employment in each industry. Our measure of the accident risk in each industry is workers' compensation premiums that employers in a wide range of industries paid per one hundred dollars of payroll into Ohio's state-run compensation fund in 1923. The premiums were reported in Ohio Industrial Commission (1923). The workers' compensation premiums are a reasonable measure of the relative danger across industries because the Ohio Industrial Commission experience rated the premiums such that industries paid higher premiums if they generated relatively more accident costs. We chose Ohio premiums because the state had a broader set of industries than most other states for which data were available. The employment data for each industry in each state, which are used as the weights in the weighted average calculation, represent the average number of wage earners in the industry. The data are from U.S. Bureau of the Census (1902, vol. 7; 1913, vol. 9; 1923, vol. 8; 1933, vol. 3). The risk index for the intervening years was calculated using a straight-line interpolation.

Manufacturing Firm Size and Value Added

The percentage of manufacturing establishments employing less than five workers and more than five hundred workers were reported by the census for the years 1899, 1909, 1914, 1919, 1929, and 1939. We used straight-line interpolations to fill the intervening years. The data were collected from the U.S. Bureau of the Census (1902, vol. 7, 336–67; 1913, vol. 8, 469; 1917, 422–25; 1923, vol. 8, 90; 1933, vol. 1, 72–73; 1943, vol. 1, 169).

Manufacturing value added per manufacturing worker was reported in the manufacturing censuses of 1899, 1904, 1909, 1919, 1921, 1923, 1925, 1927, 1929, and 1931. The values were deflated using the CPI (1967 = 100, series E135 in U.S. Bureau of the Census 1975, 211). Values for the intervening years were determined using straight-line interpolation. Hand trades were excluded. Data from 1899, 1904, and 1909 are from U.S. Bureau of the Census (1913, vol. 8, 542–44); 1914 data are from U.S. Bureau of the Census (1917, 171–73); data for 1921, 1923, and 1925 are from U.S. Bureau of the Census (1928, 1283–87); 1919, 1927, and 1929 are from U.S. Bureau of the Census (1933, vol. 1, 17–20); and 1931 data are from U.S. Bureau of the Census (1935, 21).

Employment Shares in Agriculture, Manufacturing, and Mining

The percentages of gainfully employed workers in agriculture, manufacturing, and mining were reported in the population censuses for the years 1900, 1910, 1920, and 1930. See U.S. Bureau of the Census (1902, vol. 2, 508; 1913, vol. 4, 44–45; 1923, vol. 4, 48; 1933, vol. 5, 54). Straight-line interpolation was used to fill the intervening years.

Unionization Index

The union index implicitly assumes that the national unionization rates for each industry in 1899, 1909, 1919, and 1929 were the same across states. For each of the four manufacturing census years, we calculated a weighted average of the unionization rates across each state's manufacturing industries. The weights are the shares of the manufacturing wage earners in each industry. We used Whaples's (1990, 434–47) estimates of the unionization rates in each manufacturing industry from 1909. We then followed Whaples's procedure to recalculate his 1919 unionization rates across industries and to derive estimates for 1899 and 1929 using information on union membership from Wolman (1936). The average number of wage earners was reported by the U.S. Bureau of the Census (see the earlier section on accident risk for sources).

To fill in the years between 1899, 1909, 1919, and 1929 for each state, we interpolated based on movements in the ratio of U.S. trade union membership (Wolman 1936, 16) to nonagricultural employment (series D-127 in U.S. Bureau of the Census 1975, 137).

State Government Spending on Labor Programs

The costs to state government of labor programs include spending for factory inspection, labor bureaus, mining inspection, bureaus of labor statistics, boards of arbitration, boiler inspector, and free employment bureaus. The data were collected from appropriations to state labor departments reported in the states' statutes. For each state-year observation we collected the appropriations for factory inspection, boards of conciliation and arbitration, bureaus of labor, bureaus of labor or industrial statistics, free employment bureaus, boiler inspection (but not ship boiler inspection), mining inspection, industrial welfare commissions, and industrial commissions from the states' session laws. In many states appropriations were given for all labor spending without separating out what share went to each division. In a few states, Iowa for example, the statute volumes offered the exact amounts spent by the state treasurer. Some states were either missing appropriations volumes or the appropriations were unnecessarily obtuse. In those states we used interpolations to fill any gaps. In interpolating we tried to be sensitive to the fact that many states were on a two-year cycle and often gave the same amount of appropriations in both years of the cycle. Maryland and Michigan offered extremely uninformative appropriations information. For Michigan we collected the appropriations data from the Michigan Auditor General's *Annual Report* for years between 1900 and 1920. For Maryland we collected information from the Maryland Bureau of Statistics and Information, *Annual Reports.*

We deflated the expenditures using the CPI (1967 = 100) and then divided the real expenditures by an estimate of the number of workers gainfully employed in the state. The employment estimate was determined by calculating the share of total U.S. citizens gainfully employed in each state for the years 1900, 1910, 1920, 1930, and 1940 from series D-26 in U.S. Bureau of Census (1975, 129–31). The shares between the census years were calculated using straight-line interpolations. We then multiplied the shares for each state and year by total employment in the United States in each year (series D-5 in U.S. Bureau of Census 1975, 126) to create an estimate of employment in each state.

The Political Composition of State Legislatures

The variables indicating political power shifts in each state legislature are based on the number of Republicans, Democrats, and other party members in each chamber of the state's legislature at each legislative session. For each state we sought information on the political structure of the state legislature from legislative manuals, state bluebooks, House and Senate journals, newspapers, and historical listings. In many of the southern states the legislatures were overwhelmingly Democratic and many of the bluebooks did not bother to list party affiliations.

To fill in any gaps we encountered, we used information from the New York Secretary of State *Manual* for the years 1925 to 1940. The data there seem reasonably accurate when matched up against information we collected from states' bluebooks. For the earlier years we collected information from the *Chicago Daily News Almanac and Yearbook,* 1918–30, *Tribune Almanac,* 1900–1909, and *World Almanac and Encyclopedia,* 1910–18.

There is still probably some measurement error in the data. This is due to some disagreement among the sources as to the exact party splits of the legislatures either because some legislators may have changed parties midcourse or because people died and vacancies were filled.

We determined whether the legislature was in session by examining the frequency of each state's legislative sessions, as reported in U.S. Bureau of the Census (1918, 62–63). However, because some states held special sessions during the period under investigation, we examined the statute volumes for each state to determine all of the years that the legislatures met.

Presidential Voting Information

The percentage of votes for Republican and socialist presidential candidates are from Congressional Quarterly (1975, 281–91). Socialist votes include votes for socialist candidates and in 1924 votes for LaFollette. The values for years between presidential elections are based on straight-line interpolations between election years. The progressive voting measure in 1912 is the percent voting for Roosevelt for president in 1912. For years between 1908 and 1912 values were derived from a straight-line interpolation between zero in 1908 and the value in 1912. After 1912 the values are assumed to be the 1912 value on the grounds that the progressive ideas espoused by Roosevelt in 1912 were subsumed under other parties.

Southern States

The dummy for southern states gives a value of one to Alabama, Arkansas, Florida, Georgia, Kentucky, Louisiana, Maryland, Mississippi, North Carolina, South Carolina, Tennessee, Texas, and Virginia.

Labor Laws in the Various States

We created a series of dummy variables that took on a value of one if the state had enacted various labor laws, and zero otherwise. The index includes the following laws that were supported by organized labor: minimum wage laws, union trademark laws, laws protecting labor organizations, mothers' pension laws, antiblacklisting laws, armed guard laws, laws stating that labor agreements were not conspiracies, laws preventing the false use of labor membership cards, laws limiting injunctions, laws exempting labor organizations from antitrust laws, laws against employers not telling incoming workers about the existence of a strike, laws allowing the incorporation of labor unions, and laws prohibiting contracts that restrain workers from joining labor unions. The index also considers laws that unions opposed: antiboycotting laws, laws preventing conspiracies against workers, laws preventing the enticement of workers, laws preventing interference for workers in all industries, laws preventing interferences for workers only in railroad industries, laws against the intimidation of workers, and laws against picketing. We also created a dummy variable indicating whether the state had a general women's-hours law, which took on a value of one if there was a women's-hours law covering manufacturing, mercantile, or other types of occupations. The information regarding the status of these labor laws in each state was obtained from the Department of Labor's series "Labor Laws of the United States and Decisions of the Courts Related Thereto." The volumes include U.S. Commissioner of Labor (1892, 1904, 1908) and U.S. Bureau of Labor Statistics Bulletins 148 (1914), 370 (1925a), 552 (1931), and 590 (1933). The women's-hours laws were collected from Smith (1929). Data for intervening years and the years that the laws were adopted were obtained from the states' statutes.

To compute the overall labor law index we vector-added the dummy variables that represent the prolabor laws described above and the women's-hours law and subtracted those dummy variables that indicate laws that were inimical to labor's interests. Finally, to this measure we added an estimate of the share of men covered by men's-hours laws. For each state we calculated the number of men working in industries and occupations—such as public employment, railroads, street railroads, and mining—that were covered by an hours law and divided this number by the total male employment in 1910. Although some states had a general law declaring men's-hours restrictions, the vast majority of these laws were passed in the 1800s and were rarely enforced. In order to give some weight to the fact that a state had a general men's-hours law, even though it was rarely enforced, we added 0.1 to our estimated percentage of men covered by an hours law. We used 1910 gainful employment as the basis for our calculation because we wanted the labor law index to capture changes in the laws and not changes in employment. The men's-hours law data are from Brandeis (1966, 540–63). The number of males age ten and over gainfully employed in each industry by state in 1910 was collected from U.S. Bureau of the Census (1913, vol. 4, 96–151).

Progressive Law Index

To create the progressive law index, we created a series of dummy variables that took on a value of one if the state had enacted the law and zero otherwise. The index is the sum of the dummies for the following laws: compulsory attendance at school, establishing a state tax commission, establishing a state welfare agency, establishing a merit system, initiative and referendum, direct primary, minimum age for child labor, mothers' pension, and a state commission to regulate electricity rates. The information on commissions regulating electric rates is from Stigler and Friedland (1962). The remaining variables were collected from Jack Walker's ICPSR

Data Set Number 0066, "Diffusion of Public Policy Innovation Among the American States." The information on the mothers' pension laws was obtained from the U.S. Commissioner of Labor (1892, 1904, and 1908). Information for later years was obtained from U.S. Bureau of Labor Statistics Bulletins 148 (1914), 370 (1925a), 552 (1931), and 590 (1933).

Appendix J

State versus Private Insurance: An Ordered-Probit Analysis of the State's Choice

To examine the impact that interest groups and political reformers had on the state's involvement in writing workers' compensation insurance, we added further to the analysis reported in Fishback and Kantor (1996). We estimated an ordered probit. The dependent variable is an ordinal ranking depending on which form of insurance the state had in effect in 1930—either private insurance (coded 0), a state fund that was designed to compete with private carriers (1), or a monopoly state fund (2). In the majority of states, the ultimate state fund decision was made in the year workers' compensation was first adopted. In a handful of states, however, workers' compensation was first enacted without a state fund for political expediency, but was subsequently amended to include state insurance.[44] For those states our analysis considers the year in which the legislature adopted the state fund (or lack thereof) because it represented their long-term choice. The analysis is based on a sample of the forty-four states that had enacted a workers' compensation law by 1930.[45] The data for each observation are drawn from the year in which each of the forty-four states made its decision relating to the form of state insurance it had in place in 1930. For example, Washington had a monopoly state fund in 1930 that was enacted in 1911. Therefore, the independent variables reflect Washington's political economic environment in 1911.

Since state insurance represented such a radical change in public policy, we anticipate that it would have taken a major change in a state's political environment to enact the legislation. To test this hypothesis, we include two dummy variables that indicate whether the state insurance question was decided by a legislature that had experienced a power shift since its last meeting. The first legislative power shift variable takes a value of one if in at least one branch of the state's legislature, the majority party of the previous session lost its majority. The second dummy variable has a value of one if both branches of the legislature experienced a party shift in the year the state insurance decision was made. In our sample it is always the case that if both legislative chambers switched parties, they changed to the same party. We speculate that the probability of enacting a state fund was much more likely if both branches of a state's legislature experienced a power shift, otherwise each branch would have had veto power over the decisions of the other.

We also include two other ways to test the impact of progressive reformers. The first is the percentage of the electorate that voted for Theodore Roosevelt in 1912 when he ran on the progressive platform. This method is imperfect because it measures the support for the national progressive platform in 1912, even though many states made the state insurance choice in other years. We also included an index of progressive legislation that had been passed in the state at any point in time. The index measures the number of laws from the following list of progressive proposals that the state had passed: ballot initiatives, referenda, direct primaries, a mothers' pension law, a state tax commission, compulsory school attendance legislation, a state welfare agency, a merit system for state employees, a minimum age for child labor, and a state commission to regulate electricity rates.

The impact of special interests is captured by several variables. Unions, who ardently supported state funds, are represented by a manufacturing union index, which combines the national percentage of each industry that was unionized with the distribution of employment across industries within each state. The strength of insurance interests, who strongly opposed state intervention in the insurance industry, is measured as the ratio of life insurance premiums collected in the state to the state's employment. The percentage of gainfully employed workers in agriculture captures the relative strength of farming interests within each state. The percentage of manufacturing workers employed in plants with more than five hundred workers is used to capture the influence of large firms. We have also included a dummy variable for southern states to see if there was a separate southern region effect.

The results of an ordered probit estimation are reported in table J.1. The results suggest that both political coalitions and narrowly defined economic interest groups played important, and statistically significant, roles in the adoption of state funds. As predicted, and holding political shifts constant, states that had a greater union presence were more likely to adopt a state fund, while in states that had relatively strong insurance and agricultural interests, employers were more likely to retain their freedom to insure through private firms. As a measure of the impact that these interest groups had on the probability of enacting state insurance, we calculated the marginal effect that a one-standard-deviation (OSD) change in each of the independent variables had on the baseline probability of adopting a monopoly state insurance fund. The baseline probability is a prediction of the probability that the state imposed a monopoly fund, based on the sample means for the independent variables. An OSD

Table J.1 **Ordered-Probit Results of States' Choices between No State Fund, Competitive State Insurance, and Monopolistic State Insurance**

Variables	Means (std. dev.)	Coefficient (t-stat.)	Change in Variable	Marginal Effect on Probability of Choosing Monopoly State Fund, Assuming Changes in Variables[a]
Intercept		6.47 (2.015)		
Union index	12.52 (4.76)	0.206 (3.05)	4.76 increase in union index	0.086
Percentage of gainfully employed in agriculture	32.24 (15.37)	−0.182 (−3.30)	15.37 decrease in agriculture variable	0.689
Ratio of life insurance premiums in 1967 dollars to employment	0.56 (0.20)	−11.39 (−2.62)	0.20 decrease in life insurance variable	0.485
Percentage of workers in manufacturing plants with over 500 workers	27.07 (12.1)	−0.068 (2.22)	12.1 percent decrease in workers in large plants	0.062
Percentage of voters voting for Roosevelt in the 1912 presidential election	25.96 (9.35)	0.0639 (1.36)	9.35 percent increase in the percent voting for Roosevelt	0.034
Index of progressive laws	5.93 (1.55)	0.313 (1.38)	1.55 increase in progressive law index	0.025
Power shift in at least one branch of legislature	0.34 (0.49)	0.891 (1.20)	Power shift in only one legislative branch	0.053
Power shift in both branches	0.091 (0.29)	2.175 (2.72)	Power shift in both legislative branches	0.429
Southern state	0.273 (0.45)	1.26 (1.06)	Southern state	0.083
N	44	44		

Sources: Information on state funds came from Clark (1911, 906–9) and the following Bulletins from the Bureau of Labor Statistics series on Workmen's Compensation and Insurance: 126, 185, 203, 240, 243, 272, 275, 301, 332, 379, 423, 496. Where these sources left confusion, we examined the states' statutes directly. For details on the sources of the independent variables, see appendix I.

Notes: Standard deviations are in parentheses below the means. Value of t-statistics are reported under the coefficients. The estimated ordered-probit threshold coefficient is 1.63, with a t-ratio of 2.33.

[a]The impact for each interest group variable and the Roosevelt progressive vote variable is the change in the baseline probability associated with a one-standard-deviation change in each variable, holding the other variables constant. The marginals of the power-shift variables are switches from 0 to 1, holding all else constant. The baseline probability of 0.011 was computed at the sample means of the variables.

increase in the union index increased the chances of choosing state insurance by 8.6 percentage points. A decrease in the life insurance variable, on the other hand, increased the probability of choosing a monopoly state fund by 48.5 percentage points. Farm interests had a strong influence over the state insurance decision. An OSD fall in the percent of the gainfully employed in agriculture increased the chances of enacting monopoly state insurance by 68.9 percentage points. Finally, large firms also appear to have reduced the probability of choosing a state fund in a statistically significant fashion. An OSD decrease in the percentage of workers in plants with over five hundred workers led to a 6.2 percent increase in the probability of choosing the state fund.

Thus, with the right combination of strong unions and weaker than average agricultural and insurance interests, it was theoretically possible to obtain the passage of state insurance even without a political power shift in the legislature. This set of circumstances occurred in two states—Nevada and Wyoming. Nevada adopted a state fund in 1913 without a measured power shift. Nevada's predicted probability of adopting monopoly state insurance in 1913 was 0.57, which was driven by a unionization rate that was 1.6 standard deviations above the sample mean for all the states, an insurance value 0.51 standard deviation below, and an agriculture value 0.76 standard deviation below the mean. Similarly, Wyoming adopted a monopoly state fund in 1915 in part because the union variable was 1.36 standard deviations above the mean and the insurance variable was 0.79 standard deviation below.

Progressive reform movements also played an important role in the choice of a state fund. In several states power shifts in both chambers of the legislature played a significant role in the decision to adopt a monopoly state fund. If only one branch of the legislature experienced a power shift, the effect was small and statistically insignificant. It appears that the presence of an entrenched political party in one branch limited whatever ambitions the new political coalition espoused in the other branch. On the other hand, if both branches shifted, the probability of choosing a monopoly state fund changed in a dramatic and statistically significant manner. By adding a power shift in both branches of the hypothetical baseline state, the probability of enacting a monopoly state insurance fund increases by 42.9 percentage points. Of the four states in our sample that experienced a power shift in both branches of the legislature, Ohio adopted a monopoly state fund, and Colorado, Idaho and Utah adopted competitive state funds.

The other measures of progressive reform have a positive relationship with the choice of a state fund, although the effects are not statistically significant. An increase in the percentage of people voting for Roosevelt gives an indication of the general interest in national progressive platform as of 1912. In states where the percentage voting for Roosevelt was a standard deviation higher than the mean, the probability of adopting a monopoly state fund rose by 3.4 percentage points, although the ordered-probit coefficient was not statistically significant. A larger index of progressive legislation was also associated with a higher probability of adopting a state fund, although again the coefficient was not statistically significant.[46]

Finally, there does not appear to have been a southern regional bias against the adoption of state insurance funds. The coefficient of the southern variable is not negative and we cannot reject the hypothesis of no effect.

The ordered-probit analysis results, taken as a whole, indicate that the adoption of monopoly state insurance was relatively unlikely. In Nevada and Wyoming it came about because of an unusual combination of strong unions and relatively weak insurance interests. If the proper combination of narrow economic interest groups did not exist, then monopoly state insurance was unlikely to be enacted unless union leaders melded their demands with the broader socioeconomic agenda of a strong political reform movement, as discussed in chapter 6.

Appendix K

A Quantitative Analysis of Workers' Compensation Benefit Levels

In chapter 7 we describe the results of a quantitative analysis of workers' compensation benefit levels. This work was originally reported in Fishback and Kantor (1998b). Our quantitative analysis examines both the initial benefit parameters that states established as well as changes in those benefits through 1930. We have collected information on the extent of union strength, the extent of accident risk, potential differences in the degree of the wage offset across states, firm size, and a measure of manufacturing productivity. In addition, as in our other empirical analyses, we have included variables that capture the influence of broad-based political coalitions and the impact of the workers' compensation bureaucracy after it was in place.

It is worth commenting briefly on two parameters of the model for which empirical information is not easily obtained: the extent of experience rating and the extent of wage offsets. We discuss the role of experience rating in influencing employers' and workers' choices of benefit levels in chapter 7. We may be able to test what influence the less-complete experience rating of smaller firms had on the determinants of benefit levels. Given that insurers actively sought to adjust premiums across industries, our crude measures of accident risks across industries are unlikely to capture fully the subsidies that sometimes arose from temporary mismatches between the insurance rates that industries paid and their actual accident experiences.

Another important consideration, but not easily measurable, is the extent to which workers paid for higher benefits through market adjustments in their wages. Chapter 3 shows that wage offsets varied across industries and by union status. Nonunion miners experienced wage reductions that fully offset the insurance costs of providing workers' compensation benefits; lumber workers experienced wage offsets that covered the value of their expected benefits, but did not fully cover the employer's insurance costs of providing the benefits; while unionized coal workers and building tradesmen experienced much smaller wage offsets. We do not have enough direct information on the extent of wage offsets in each state, although we may be able to gain some insights by examining the relationships between benefit levels and the union index, the percentage of workers in mining, and the productivity variable.

The dependent variable is the natural log of each state's expected workers' compensation benefit index (in 1967 dollars), which is shown in table 7.1.[47] The regression equations are estimated in semilog form, controlling for year and state fixed-effects. The year effects control for unmeasured influences, such as World War I, that were common to all states during a particular year. In addition, they play an important role in controlling for changes over time in the weekly wage used to calculate the workers' compensation benefits. The benefits for each state are calculated using the national average weekly manufacturing wage, which rose substantially over time. Failing to control for year effects might lead to a spurious positive relationship between the average manufacturing wage in a state, which was typically correlated with changes in the national average manufacturing wage, and our measure of the benefit levels in that state. The state dummy variables control for unmeasured characteristics of a state that did not change over time. These might include the rules governing the state legislature, the underlying political strength of employers and workers, and political attitudes that remained unchanged during the period under consideration. To the extent that some of our independent variables did not vary over time within a particular state or did not vary across states during a particular year, then the state and year fixed-effects, respectively, will diminish the importance of these variables in explaining the benefit levels. Thus, after controlling for the state and year effects, the coefficients of the remaining variables capture the relationship between the benefit parameters and the components of the right-hand-side variables that were not state or year specific. *F*-tests reject the hypothesis that the coefficients of the year and state effects are jointly equal to zero.[48]

Our sample is an unbalanced panel because states are only included in the analysis after they had adopted workers' compensation. The determinants of benefits, therefore, are estimated conditional upon the state's enactment of a workers' compensation law. To control for any selectivity bias that might arise from the conditional nature of the analysis, we used the following procedure. First, we estimated a reduced-form probit equation where the dependent variable reflects the presence of a workers' compensation law for the sample of forty-eight states in each year from 1910 to 1930. From this equation we calculated the inverse Mill's ratio, which was then used as a sample-correction variable in the fixed-effects benefits regression. We report the results in table K.1 both with and without the selectivity correction, but we focus the discussion on the results with the selectivity correction because the *t*-test for the coefficient of the inverse Mill's ratio suggests the presence of selectivity bias when estimating the benefits equation (see the last column of the table).[49]

The results are consistent with the suggestion that legislators chose benefit levels by weighing the demands of both employers and workers. Even though workers were more politically numerous and thus a worker was likely to be the median voter, it appears that legislatures also heeded the demands of employers. Meanwhile, in the states where workers had more political clout through unionization, they were successful in shifting the benefit levels upward. Benefit levels were higher, ceteris paribus, in states with greater unionization, where political upheaval led to shifts in party control of the state legislature, and where bureaucracies, as opposed to the courts, administered the workers' compensation system.

One sign that state legislatures responded to the demands of high-accident-risk employers is that benefits were lower in states where more workers were employed in dangerous industries. The comparative statics predict that employers in more dangerous industries would prefer lower benefit levels, while the impact of higher risk on the workers' demands for benefit levels is theoretically unclear. The analysis includes two measures of accident risk: one that proxies accident risk in manufacturing, which is based on the industrial mix in each state, and the other that is the percentage of workers in mining, one of the most dangerous forms of employment during this time period. The coefficients of both the manufacturing accident risk index and the percentage of workers in mining are negative and statistically significant. A one-standard-deviation (OSD) increase in the risk index (0.57) from its sample mean (1.48) would have reduced benefits by about 19.1 percent. Similarly, an OSD increase (4.4) in the percentage of workers in the dangerous mining industry was associated with a 10.6 percent reduction in benefit levels.

Another sign that legislators were attentive to the demands of employers is the absence of a statistically significant effect of wages on benefit levels. Theoretically, higher wages would have caused workers to seek higher benefit levels. On the other hand, the employer's choice of benefit levels was

Table K.1 **Coefficients from Regression Analyses of Expected Workers' Compensation Benefits, 1910–30**

Variables	Means (std. dev.)	Coefficients: No Correction for Selectivity Bias	Coefficients: Correction for Selectivity Bias
Constant		3.84	3.74
		(21.46)	(14.50)
Instrument for average manufacturing wage in the state (1967 dollars)[a]	59.8	0.0047	0.0038
	(10.1)	(1.14)	(0.64)
Manufacturing accident risk index	1.48	−0.259	−0.335
	(0.57)	(5.36)	(4.98)
Percentage of labor force employed in mining	3.3	−0.026	−0.024
	(4.4)	(5.97)	(3.71)
Manufacturing value added per worker (thousands; constant 1967 dollars)	5.65	−0.0133	−0.002
	(1.43)	(0.81)	(0.08)
Percentage of workers in manufacturing establishments with more than 500 workers	32.0	−0.0017	0.0006
	(11.7)	(1.44)	(0.32)
Percentage of workers in manufacturing establishments with less than 5 workers	5.3	0.0042	−0.0075
	(4.1)	(0.99)	(1.14)
Manufacturing unionization index	14.5	0.0048	0.0067
	(7.7)	(2.83)	(2.58)
Political climate			
Power shift in at least one branch of legislature	0.130	0.0384	0.051
	(0.34)	(2.49)	(1.89)
Power shift in both branches of legislature	0.037	−0.0048	−0.026
	(0.19)	(0.19)	(0.58)
Percentage of presidential vote for Republican	52.7	−0.0004	0.0002
	(11.2)	(0.43)	(0.17)
Percentage of presidential vote for socialist	6.5	−0.00006	0.0004
	(8.2)	(0.08)	(0.34)
Workers' compensation administered by commission or state fund in prior year	0.741	0.075	0.073
	(.439)	(4.61)	(3.40)
Lambda (inverse Mill's ratio)[b]			0.249
			(3.81)
Year dummies (all except 1930)		Included	Included
State dummy variables (all except Connecticut)		Included	Included
R^2		0.87	0.88
N	702	702	702

Sources: Fishback and Kantor (1998b, 124). See appendix I.

Notes: The dependent variable is natural log of expected benefits in constant 1967 dollars (see table 7.1). Use of deflators besides the CPI with a 1967 base does not change the results. Absolute value of t-statistics are in parentheses below the coefficients and standard deviations are below the means.

Table K.1 (continued)

[a]The instruments for the wage are predicted values from a wage regression equation using the accident risk, union, mining, productivity, firm size, power shift, and presidential vote variables listed above, plus percent black, percent foreign born, percent illiterate, percent urban, strike activity, percent of the gainfully employed in agriculture, and percent of the gainfully employed in mining. We have also experimented with using a five-year lagged value of wages as an instrument. We chose a five-year lag because the wage information was interpolated over five-year periods prior to 1919 and two-year periods after 1919. The coefficients and t-tests using the five-year lagged wage were similar to those reported in the table above.

[b]The selection equation based on whether states had adopted workers' compensation or not was run on a sample of 1,002 observations with the following variables: presence of an employers' liability law that substantively affected the common law, presence of an employers' liability law that simply restated the common law, and all of the variables above, except the state and year dummies, the wage instrument, and the dummy for the presence of the commission. We did not include the state and year dummies because of multicollinearity problems when they are included.

unaffected by the wage in the theoretical model. This does not imply that the employer was indifferent to changes in benefit levels that were caused by wage changes. Instead, his optimal benefit choice was the same whether the wage was high or low; therefore, the employer would have been opposed to changes in the benefit level that were driven only by wage changes, holding other relevant determinants constant. To examine the impact of wages on benefit levels, the regression includes an instrument for the current year's wage in each state to avoid potential simultaneity problems.[50] The wage coefficients in the table show that an OSD increase leads to a 3.8 percent increase in benefits, but we cannot reject the hypothesis that there was no relationship. Thus, in states where wages were higher, employers appear to have been successful in limiting the benefit increases that workers sought because their wages rose.

Our theoretical prediction is that employers with higher product prices and/or labor productivity would likely seek higher benefits. The combined impact of product prices and labor productivity in manufacturing is captured by the measure of manufacturing value added per worker. In the table the coefficient of the value-added variable is positive, but very small and statistically insignificant. The weakness of this relationship might be the result of wage offsets that would dampen the impact that higher benefits had on a worker's overall remuneration package and, thus, productivity. Employers at high productivity firms might have had more inelastic demands for labor, which might have reduced the size of the wage offset that workers experienced. If so, employers with higher value-added may have balanced the productivity benefits from higher workers' compensation benefits against the fact that they were likely to pay a higher percentage of those benefits, thus weakening their support for higher benefits.

To test the impact of firm size on benefit levels, we also included measures of the percentage of manufacturing workers working in establishments with one to five workers and in those with more than five hundred workers.[51] In the theoretical model, the sign of this effect is uncertain. The size variables might also be influenced by incomplete experience rating within industries, since larger firms paid premiums that were more fully experience rated than smaller firms. Employers and workers in the smaller firms therefore might have pressed for higher benefits. On the other hand, workers' compensation laws allowed small firms to avoid joining the workers' compensation system, which may have left them largely indifferent in the benefits debate. The effects of both measures of firm size on benefit levels are small and statistically insignificant.

The political strength of workers generally was greater in areas with more unionization. We developed an index of unionization in manufacturing that combines the national percentage of each industry that was unionized with the distribution of employment across industries within each state. In essence, the variable captures the extent to which industries that were strongly unionized at the national level were represented in each state. Greater unionization was not only associated with greater political strength, but unionized workers also had relatively strong incentives to push for higher benefit levels because they were more effective at preventing wage offsets. On the other hand, the lower wage offset meant that employers had an incentive to lobby for lower benefit levels. The regression results indicate that in states dominated by industries where organized labor had a greater national presence, organized labor was successful in overcoming the pressure from employers for lower benefit levels. An OSD increase in the unionization index (7.7) led to a statistically significant 5.2 percent increase in expected benefits.

Legislators may have had broader agendas than simply pleasing narrowly focused economic interest groups. To ensure reelection they also had to pay at least some attention to the demands of the electorate, which may have meant balancing workers' compensation issues with other social, political, or economic issues. We have tried to measure the impact of broader political agendas within each state in several ways. One way to measure the political attitudes of voters is to include voting results from presidential elections. The results suggest, however, that changes in voter support for national Republican, Democratic, or more socialist views had little impact on the benefit levels at the state level. After controlling for the state and year effects, the effect of changes across states and time in the percentage of the electorate voting for Republican candidates and socialist candidates were small and statistically insignificant.[52]

Since workers' compensation legislation was state-based, we also have tried to measure the influence of legislative upheavals at the state level. Strong political reform movements during the Progressive Era often caused the party composition of state legislatures to shift dramatically around the time of the introduction of workers' compensation. Thus, during these initial upheavals, we might expect to see higher benefits in settings where political control of the legislature shifted from one party to the other. Further, when the reformers' opponents attempted to retake the legislature, they may have competed to obtain the support of the reform-minded sectors of the electorate by proposing even higher benefits. For example, in the 1914 gubernatorial race in Ohio, conservative Republicans campaigned for higher benefits in their efforts to unseat a progressive Democrat (Fishback and Kantor 1996).

We have included dummy variables that measure situations in which the political dominance of one party shifted in at least one branch of the legislature and in which it shifted in both legislative chambers. The debate over accident benefits was certainly a controversial one, although perhaps not as rancorous as the state insurance debate, and the empirical analysis suggests that political party shifts also played an important role in helping workers secure higher benefits. If one branch of a state legislature shifted parties in a particular year, expected benefits rose by a statistically significant 5.1 percent. There was no additional effect on the benefit levels to have both branches shift simultaneously. These results contrast with our findings for the same variables in examining the overall adoption of workers' compensation laws. It is also important to note that our measures of shifts in political coalitions are incomplete and offer only a lower-bound estimate of the importance of progressive political groups that sought significant socioeconomic reforms in the early twentieth century.

Once workers' compensation was adopted, workers appear to have been joined in the struggle for higher benefits by the bureaucratic agents that administered the new system. States typically chose to administer their workers' compensation programs in one of two ways. By 1930, ten states administered the laws through the courts, with employers and workers establishing agreements regarding accident compensation subject to the statutory guidelines and the courts settling disputes. Thirty-eight states administered workers' compensation with a bureaucratic commission, which directly oversaw the disbursement of accident compensation.[53] The state commissions had the potential to act as advocates for changes in the workers' compensation laws because they were often a key source of information for legislators, although their attitudes could have been influenced by either employers or workers. We examine the bureaucrats' impact on benefit levels by including a dummy variable that takes a value of one if the state had a workers' compensation commission in operation at the end of year $t - 1$, and zero otherwise. The dummy variable indicates the presence of a commission in the previous year because the bureaucracy could lobby the legislature in the current year only if it was already in existence. The coefficient of the commission dummy implies that the presence of an administrative body led legislatures to establish benefits that were 7.3 percent higher, which is statistically significant.[54] Thus, it appears that workers benefited disproportionately from the administrative agencies' lobbying.

Notes

1. The one caveat to this statement would be that if the worker's accident was relatively minor, and he was only expected to lose a few days of work, then the accident would probably go unreported. Because all states imposed waiting periods before accident benefits would commence, those accidents that were relatively minor would still go unremunerated because the injured workers would be back at work before the waiting period was over.

2. These data are from the Stonega Coke and Coal Company Records, *Annual Report of Operations Department* (1925, 155).

3. For earlier information see Hoffman (1908, 461) and Fay (1916, 12–13). For later information see Illinois Industrial Commission (1920) and Adams (1923, 41). The nonfatal injuries for 1919 were arrayed as follows: 11 permanent total injuries, 1,275 specific losses, 297 disfigurements, 66 permanent partial disablements, 5,983 temporary total disablements, and 20 temporary partial disablements.

4. Iowa Employers' Liability Commission (1912, 106).

5. See Ohio Department of Inspection of Workshops, Factories, and Public Buildings (1906–1913). The coverage of industries is the same in all of these comparisons.

6. The year of adoption may differ from the first year of operation because quite a few states made their workers' compensation laws effective in the year after the law was adopted.

7. A lump-sum settlement typically required a paternalistic hearing before a state board to determine whether a lump-sum payment was in the best interest of the family. Conyngton (1917, 119–21, 137–44) found that less than 13 percent of the families of fatal accident victims in Ohio and Connecticut received their benefits as lump-sum amounts.

8. The commission reported that they had some problems in 1909 locating the families of workers injured in 1907. They originally sought 3,264 cases, but there were 1,777 for whom information could not be obtained. The information was based on the memories of the families in direct interviews. The distribution of accidents investigated and reported across industries were similar.

9. Note that this replacement percentage is different from the one reported for New York in tables 3.1 and B.4. Although the method of calculating the expected workers' compensation benefits is the same here and in the table, the assumed wage rates are different.

10. A second way in which the rise in benefits is understated stems from discounting, but the effect is very small. We discounted the stream of benefits paid out under workers' compensation using a 5 percent interest rate. We did not discount any of the negligence liability benefits because we assumed they were lump-sum payments. However, there typically were delays in the payment of the benefits under negligence liability. Some of the delay was similar to what we see for workers' compensation given the waiting period. On the other hand, at least one-third of the death and permanent disability cases involved delays of at least a year in payments being made, and at least 20 percent of the death and disability cases involved delays of over two years. This would lower the expected benefits by about four to five cents, which is a very small percentage of annual income.

11. The sample size of survey respondents and nonrespondents appears to have been about the same. Some companies did not fully state wages paid during disablement (but there is some question about this).

12. The 45.5 percent figure is similar to those reported in other states. Over the

period from 1902 to 1911, insurance companies doing employers' liability business in Iowa paid out 51.1 percent of the premiums they collected in the form of losses, or direct payments to workers. See Iowa Employers' Liability Commission (1912, 103).

13. The New York Commission on Employers' Liability (1910, 254–55) also reported information on what 327 employers employing 125,995 workers paid for accidents under negligence liability. The total amount they paid was $254,636, which is $0.374 per $100 on the payroll. Of that amount, after eliminating legal fees and conservatively estimating the insurance companies' expense ratio at 33 percent, the amount received by workers was approximately $202,580 or $0.298 per $100 on the payroll.

We have used the following reasoning to deflate the $254,636 total to the $202,580 amount eventually received by workers. Firms with liability insurance only spent $45,925 and had total wages of $22,438,829. Given an expense ratio of 0.33, workers at these firms would have received about $30,770 in medical expenses and wage payments, which is $0.137 per $100 on the payroll. For firms having both liability insurance and other expenditures, their total wages were $22,896,808. They spent $69,277.72 on liability insurance and $48,700 on other expenditures. Of the liability insurance expenditures using an expense ratio of 0.33, $46,416 would have gone to compensating workers. Of the $48,700 in other expenditures, $11,725 were contributions to benefit associations (which may not count as required, although it may have reduced the probability of lawsuits), $5,274 went to payments of claims, $6,180 went to legal expenses, $9,194 went to medical expenses, $12,363 went to wages, $2,157 went to pensions, $1,449 went to funeral expenses, and $358 went to other expenses. Summing the claims, wages, pensions, funeral expenses, payments to benefit associations, medical expenses, and other payments yields a total of $42,520. So the maximum amount received by workers from employers for wage replacement and medical expenses would have been $88,936, which is $0.39 per $100 on the payroll.

Firms without liability insurance had total wages of $22,748,898. Of the $91,251 they spent on accidents, $1,640 was contributions to employee benefit associations, $57,504 was paid in claims, $8,377 went to legal fees, $10,252 went to medical expenses, $12,196 went to wage payments, $241 went to funeral expenses, and $1,040 went to other expenses. Eliminating legal fees, the maximum amount going to workers was $82,814.

The total amount that workers received for wage replacement and medical expenses papers was $202,580. The total amount paid in wages was $68.0845 million, which is about $0.298 per $100 on the payroll.

The Michigan Employers' Liability and Workmen's Compensation Commission (1911, 10–11, 75–100) conducted a survey of 466 industrial employers, employing 99,134 workers, and asked them information on their costs of accidents. The amount of money that they spent that went to workers was in the range of $0.353 per $100 on the payroll. A collection of mining companies employing 21,080 workers spent less than $0.34 per $100 on the payroll on compensating workers (see Michigan Employers' Liability and Workmen's Compensation Commission 1911, 10–11, 14, 84, 86–89, 98, 108–11). The employers paid $20,159 in wages during the workers' disabilities, $43,225 in settlements, and $14,229 in other aid, first aid expenses of $4,831, medical costs of $14,127, hospital costs of $9,574, and legal costs of $2,707. The total, eliminating legal costs, is $106,145. They paid $120,111 for insurance. Assuming an expense ratio of 0.33, the workers would have received $80,074 from employers.

14. Medical expenses are excluded because we have no good measure of medical

expenses under workers' compensation or under negligence liability. To the extent that the payments made to workers under negligence liability also covered the workers' medical expenses, the payouts overstate the wage replacement that the workers received.

15. Of the $52,225, we are certain that $48,075 was paid for accidents occurring during the time frame. In several cases in the report of 1916, the date of the accident was not reported, but we could tell pretty well from context if it occurred in 1916, thus the additional $3,250 in payments are for accidents we are reasonably certain occurred in 1916.

16. For 1916–18 we have the costs of coal production from the operating statements of the company. We assumed that 74.1 percent of the operating costs went to paying workers, based on evidence on the labor share of operating costs in 1921, where we have information on both operating costs and wages paid.

17. For all years we have operating costs and for some years we also have direct measures of labor costs. In the years when we have both, the labor costs are 74.1 percent of operating costs. Thus, we calculated labor costs in years where only operating costs were available as 74.1 percent of operating costs. The 1916–18 percentages may be biased upward because they include coking accidents in the numerator, while payments to coke workers are not reported in the denominator. On the other hand, we cannot be certain that we have captured all of the information on payments to workers, although the coverage seems fairly complete. The 1919–23 weeks of compensable accidents may overstate the amounts actually paid, because it is the company's estimate of the severity of accident losses. Total weeks of compensation for the period 1919–23 is from the *Annual Report of the Operating Department Stonega Coke and Coal Company Records* (1925, 160). Information on labor costs and operating costs comes from operating statements for the years 1912–19, 1920, 1921–29. Information on payouts to workers during the period 1916–18 come from the lawyer reports in the *Annual Report of the Operating Department,* for the years 1916–21. The wage scales are in the *Annual Report of the Operating Department* for 1929. All are in the records of the Stonega Coke and Coal Company, at the Hagley Museum and Library, Wilmington, Delaware.

18. We have focused the analysis on thirteen occupational classes from the forty specific occupations for which the U.S. Bureau of Labor Statistics reported wage scales by 1923. The occupations chosen reflect a wide and representative characterization of the important building trades in the early twentieth century.

19. All the equations are estimated with weighted least squares because each observation is a state (or city) average from samples of different sizes, which implies that the variance of the error terms is inversely related to the number of workers in each state. In the coal and lumber samples we used the square root of the number of workers sampled for each observation as the weight. In the building sample, the data sources did not provide information on the number of workers sampled. Therefore, we used White's 1980 correction for heteroskedasticity. We have also estimated the lumber and coal equations using the White correction and the main results are roughly the same as those reported below.

20. The prices for lumber and coal are the prices at the mill and the mine. Even though both lumber and coal were competitive, national markets, prices at individual mines and mills varied substantially due to differences in the transport costs of sending the product to market and variations in the quality and type of the product.

21. The full-time hours per week in the lumber and building trades are not measures of labor supply. They reflect the constraints on working time offered by the employer, such that workers might demand higher wages if full-time hours were

shortened. Similarly, in the coal industry the days variable is the number of days the tipples operated, representing a measure of the maximum amount of working time available to workers. Workers therefore made their labor-supply decisions subject to the constraints on full-time hours.

22. All calculations are based on the mean earnings in the sample. When the equations are run in semilog form, the wage reductions in response to workers' compensation are similar, coal at 1.51 percent, lumber at 1.91 percent, and the building trades at 0.35 percent.

23. We have also experimented with using the maximum allowable benefits as an instrument for the expected benefits for all observations, and the results suggest a negative and statistically significant wage offset in all cases. We did not focus on these results because the wages in many occupations were not high enough to hit the legal maximums.

24. We have also tried a specification for the coal sample that interacts the fatal accident rate and expected benefits variable. The wage offsets are close to the ones reported in the text. We cannot do the same interactions in the lumber and building trades samples because we have no information on differences in accident rates across states and years.

We have experimented by including lagged benefits in the various equations. In every sample when we included a lagged benefits term (up to three-year lag) along with the contemporaneous benefits, the coefficient on the lagged term was consistently small and not statistically different from zero.

25. To further investigate whether the dramatic changes in the American labor market during the period under consideration has generated spuriously measured wage offsets, we collected wage data for a relatively safe industry in an attempt to determine whether these workers experienced a wage offset, even though they stood to benefit very little from the change in expected postaccident benefits. If we detected a strong wage offset, then this finding may imply that there is a spurious inverse relationship between benefits and wages during this time period. Cotton textile work was much less risky than the others we have considered. Workers' compensation insurance premiums for cotton mills, for example, were approximately one-tenth of the premiums of coal and one-fifth the premiums for lumber and the building trades (these comparisons are based on the average workers' compensation premiums that employers in each industry would have paid in Ohio, New Jersey, Illinois, and Wisconsin in 1912, as reported in Washington Industrial Insurance Department 1912, 277). We estimated regressions for a cotton textile sample covering eight states with twenty-nine occupations for the years 1910–14, 1916, 1918, 1920, and 1922. The results revealed a large positive coefficient on the benefits variable, which suggests that we should have few worries that the time period analyzed here has imparted a spurious negative bias to the relationship between wages and benefits. We are reluctant to draw definitive conclusions from the cotton sample because the small number of states considered sharply limits the effectiveness of our empirical tests.

26. We have investigated the variation further by running separate regressions for each occupation in each industry. As Gruber and Krueger (1991, 128–29) also found, the wage-offset coefficients for individual occupations vary widely around the estimates from the pooled sample of all occupations. The wage offsets for each occupation in the coal and lumber industries are typically -1 (or more in absolute value), although some are not precisely estimated. In the building trades, there was a mixture of positive and negative coefficients, but we could detect no consistent pattern of differences in the offsets for skilled, semiskilled, or unskilled workers. As another test, we reestimated the full sample with interaction terms between the

state dummy variables and the occupation dummy variables. Inclusion of these interaction terms does not change our main conclusions.

27. We have tried several methods of calculating benefits under negligence liability, including inserting a value of zero for benefits under negligence liability and assuming benefits based on death benefits of 50 percent of annual earnings, without an adjustment for liability differentials. The results show the same pattern as reported in the text.

28. Finding that workers' access to insurance was rationed prior to workers' compensation does not necessarily imply that the new regime was a more efficient legal system than negligence liability, as far as workplace accidents are concerned. A worker consuming his desired level of insurance might not hold a socially optimal amount. Viscusi (1991, 82) notes that as workers reach their optimal levels of insurance, there may be increases in moral hazard, which raise the accident costs of insurers and employers. In fact, studies of accident rates and modern problems with fraudulent claims suggest that moral hazard problems have increased with the passage of workers' compensation (Fishback 1987; Moore and Viscusi 1990). A comparison of the relative efficiency of the two systems would require, at a minimum, a complete examination of the employers' and workers' costs of accident prevention, the damages incurred by injured workers, the administrative costs of the two systems, and other transaction and information costs.

29. We thank Martha Olney for making Claudia Goldin's occupation codes for these data available to us. Goldin matched the listed occupation in the cost-of-living survey with the occupation codes developed for the 1940 Census Public Use Sample. In restricting the sample we eliminated professional and semiprofessional workers (codes under 98), farmers and farm managers (98–99), proprietors, managers, and officials (100–156), clerical and kindred workers (200–266), salespeople (270–98), domestic service workers (500–520), protective service workers (600–614), service workers (700–798), farm laborers and foremen (844–88), and nonclassifiable occupations (998–99).

30. Originally, most maritime states with workers' compensation laws claimed jurisdiction over maritime industries, but the U.S. Supreme Court in *Southern Pacific Co. v. Jensen,* 244 U.S. 205 (1917), claimed that U.S. admiralty and maritime law made state compensation laws inapplicable to maritime injuries. On 6 October 1917, Congress enacted the Johnson amendment which allowed state workers' compensation laws to include maritime industries, but the law was declared unconstitutional by the Supreme Court in *Knickerbocker Ice Co. v. Stewart,* 40 S. Ct. 438, 485 (1920). See French (1920).

31. For example, Arizona, Delaware, and Texas did not include public employment under workers' compensation; New Hampshire and New Mexico did not mention public employees in the compensation act; Iowa exempted firemen and policemen; Kentucky, Maine, Massachusetts, Oregon, and Rhode Island allowed individual municipalities to choose whether their public employees would be covered by the compensation law; Minnesota public employees were covered, except for employees of the state and employees of cities whose charters provided their own compensation schemes; Ohio exempted policemen and firemen in places where pensions were established; and, finally, Oklahoma and Washington limited coverage to public employees who were engaged in hazardous work. For further discussion of these intricate rules, see Clark and Frincke (1921, 21–68).

32. We experimented with estimating separate probits for old-line life insurance, industrial life insurance, and fraternal insurance. The coefficient of expected benefits in each equation was small and statistically insignificant, just as we see when we aggregated all life insurance policies in table F.1.

33. We have also experimented with other specifications in the saving regression by adding an interaction term between the accident-rate measure and the expected-benefits variable. The magnitudes and *t*-tests of the expected benefits and accident risk coefficients are similar to the ones reported in table F.1.

34. This technique was used in Pavalko's (1989) and Buffum's (1992) earlier studies of the adoption of workers' compensation and is widely used in tests of search models. We focus on the period from 1909 to 1930 because of the substantial changes in the attitudes of employers and labor unions during the course of the period 1900 to 1908. As noted in the text, organized labor's attitude toward workers' compensation reversed in 1909, thus the measure for organized labor would have a different impact before and after 1909. When we estimate the hazard equation including information from the 1900 to 1930 period, we obtain largely the same set of results, but not surprisingly, the effects are muted relative to those reported in table H.1.

35. We also ran a cross-sectional ordinary least squares regression with years since 1910 to adoption on the left-hand side of the regression and all but the legislative change variables on the right-hand side. We got very similar results to the ones we report in table H.1. The variables that significantly reduce the time to adoption are the presence of an employers' liability law limiting the common law defenses, the ratio of employers' liability premiums to life insurance premiums, manufacturing value added per worker, the percentage of workers in manufacturing, the union index, and the presidential votes for Roosevelt in 1912. In this analysis the effect of the state supreme court cases is no longer statistically significant. We believe that the reason for this difference is that this variable in the various states changed substantially over time, a feature missed by a cross-sectional analysis.

36. Recall that total employers' liability premiums may have increased either because rates increased or because more employers chose to insure their accident liability risks. Since the empirical analysis controls for changes in manufacturing accident risk and for the change in employers' liability laws, the measured impact of the insurance ratio might suggest that employers saw workers' compensation as a means to control their insurance costs.

37. The general impression of the development of progressivism at the national level is that it rose to a peak in the 1912 presidential election. After 1912 many of the progressive ideas were incorporated in both the Republican and Democratic party platforms. To capture this rise and leveling off in each state, we constructed the Roosevelt voting variable to start at zero in each state in 1908 and then to rise through straight-line interpolation to the value in 1912. From 1912 onward the variable retains its 1912 level. We have also run the analysis using the 1912 values throughout with very little change in the results reported. We also reran the analysis allowing the progressive variable to fall back to zero by 1916. In those cases the progressive variable has very little impact.

38. Buffum (1992, 48) found that a power shift in either legislature enhanced the probability of adopting a workers' compensation law.

39. There was one exception to this observation. In 1919 the Non-Partisan League gained control of the upper house of the North Dakota legislature, while the lower house had been captured by the Non-Partisans in 1917.

40. We chose a general power shift measure, as opposed to a party shift measure, because there was substantial variation across states in the attitudes of Republicans and Democrats. In many settings both the Republican and Democratic parties established support for a workers' compensation measure in their state platforms. Out of seventeen power shifts identified in our sample, ten were shifts from

Republican to Democrat, five were shifts in the other direction, one was a shift from Republican to an even split, and there was one shift from Republican to Non-Partisan League in North Dakota.

41. We were sensitive to the issue of unmeasured heterogeneity across states in the sample, so we experimented with dummy variables representing much smaller geographical groupings. Estimation of the model with dummy variables for eight of the nine census regions led to results similar to those reported in table H.1. We also estimated the model using dummy variables for groupings of two and three states, and the results were qualitatively similar to those reported here. We are unable to estimate the model with a dummy variable for all but one of the states due to problems of perfect collinearity with the remaining variables in the analysis.

42. Nearby states include states in the same census region (of nine regions) and other contiguous states. We have experimented with other measures of contagion, such as the total number of states in the United States that have adopted and a time counter. The basic results remain the same.

43. When we experimented with other variables that may capture the contagion effect, such as a time trend or the number of other states within the entire United States that had adopted the legislation, the results were nearly identical. When a time trend and the neighborhood adoption variable were included together, the impact of the neighborhood adoption variable remained strong and statistically significant, while the coefficient of the time trend was small and statistically insignificant. The results of the remaining variables were very similar to those reported in column (2) of table H.1.

44. New York adopted workers' compensation without a state fund in 1910 and later added a competitive state fund in 1913. Similar processes occurred in California (no fund 1911, competitive fund 1913), Arizona (no fund 1912, competitive fund 1925), and Nevada (no fund 1911, monopoly fund 1913). In no case did a state create a state fund and then revert to competitive insurance. In Kentucky the workers' compensation law of 1914 was declared unconstitutional, and another law was passed in 1916. We chose 1914 for the sample, although estimates with 1916 are very similar. The Missouri legislature passed several laws that were struck down in referenda between 1919 and 1925, some with state insurance and some without. We chose the 1925 law because that was the law that made it through the referendum. We have also estimated an ordered probit equation where we added observations representing each of the earlier decisions described above, and the results changed very little. In addition, we have re-estimated the equation with corrections for possible selection bias related to the year of the decision and found essentially the same results.

45. By stopping in 1930, Florida (which adopted in 1935), South Carolina (1935), Arkansas (1939), and Mississippi (1948) are left out of the sample. None of these states adopted a state fund. Since these states were notably nonunion, agricultural, probably low in insurance coverage, and had almost no change in the party power in the legislature, we expect the results reported in table J.1 to be largely unchanged if these states were included in the sample.

46. We have also experimented with including a measure of spending on state labor issues. It turns out that this variable is strongly related to the progressive measures in part because it was a policy decision that progressives and labor leaders pressed, like the state insurance fund. When we include the state labor spending, the coefficients for the progressive, state spending, power shift, and labor index measures are all positive, as expected, but the coefficients are not statistically significant individually; however, we can reject the hypothesis that they are all zero.

47. We have also estimated the equations using the expected benefits as a per-

centage of the annual wage as the dependent variable, and the results are very similar to the ones reported here.

48. Comparisons of the coefficients from estimations with and without the state and year effects show that the basic results are unchanged for the accident risk, mining percentage, unionization, power shift, and state commission variables. When the state and year effects are excluded, the coefficients of the value-added, plant size, and Republican presidential voting variables are negative and statistically significant, and the presidential vote for the socialists has a positive and statistically significant effect on benefits. Given the construction of the benefits variable, it is not surprising that the exclusion of the state and year effects leads to a strong positive relationship between the benefit level and the wage variable.

49. Although the identification of the benefits equation in the two-stage process is satisfied because of the nonlinearity of the probit, we have included dummy variables for employers' liability laws in the selection equation and not in the benefits equation. The presence of an employers' liability law was likely to influence the initial adoption of the law, but not necessarily the level of benefits.

50. We wanted to eliminate the negative bias that arises when wages adjust downward in the labor market to offset higher benefits. Inclusion of the contemporary wage does lead to a smaller coefficient on the wage than those reported in table K.1. We have experimented with several other instruments for the wage and the results are essentially the same as those reported in the table.

51. It should be noted that this is not a measure of firm size because the manufacturing census reported the number of workers in an establishment. That is, if workers for a firm were divided into two plants of four hundred workers each, then these workers would not be included in the over-five-hundred measure.

52. Part of the impact of political sentiment, as measured by votes for presidential candidates, might be captured by the state effects if voter attitudes did not change over time. When we estimate the equations without the fixed effects, the Republican voting measure is negative and statistically significant and the socialist measure is positive and statistically significant. The socialist measure includes votes for candidates to the left of the Democrats, including LaFollette progressives in 1924. In the analyses reported in table K.1, we have treated votes for Theodore Roosevelt as Republican votes in 1912. We treat the two types of progressives differently because Roosevelt progressives tended to be far more conservative than the later LaFollette progressives. In our analysis of the overall adoption of workers' compensation and the choice of state insurance (see appendixes H and J), we have experimented with using the vote for Roosevelt in 1912 as a separate measure of underlying progressive attitudes. In this model the measure would be collinear with the vector of state dummies, so we cannot use it separately. We have also rerun the equations including the state and year effects and separating the Republican votes and the Roosevelt progressive votes. The progressive votes are interpolated upward from zero in 1908 and back down to zero by 1916, following the fortunes of progressive candidates at the national level. The socialist and Republican votes have small and statistically significant effects, while the progressive vote has a negative effect.

53. Wyoming administered the law through the courts but had a monopoly state insurance fund, so we have treated it as having an administrative body in the regression analyses.

54. There is potentially a simultaneity bias in this variable to the extent that legislatures that created commissions when they first enacted a workers' compensation law might have also favored higher benefit levels if legislatures chose a package favorable to organized labor. On the other hand, legislatures may have chosen

a compromise piece of legislation in which workers traded a commission for higher benefits, or vice versa. We tried developing an instrument for this variable but we could find no variables that belonged in an instrument equation for the commission that would not also be in the selectivity equation. Thus, the coefficient of the commission variable might be overstated if the commission and high wages were packaged together to appease unions, or understated if the commission was a compromise that unions accepted in return for lower benefits.