Living Wage Ordinance for Milwaukee County

Economists' View: Dr. Michael Rosen, PhD

The good jobs crisis of staggering, skyrocketing income inequality and meteoric rise of low-wage employment must be addressed.

- By 2020, roughly half of all employment will be in low-wage, service-sector jobs that do not pay enough to support a family nor provide health insurance coverage, paid leave or retirement security. Nearly all of the top ten fastest growing jobs are in low-wage, service-sector employment.
- Nearly 200,000 workers in the Milwaukee area bring home paychecks inadequate to meet even the basic needs of supporting a family, much less achieving a middle class standard of living; most of them rely on taxpayer-funded government assistance like food stamps, BadgerCare, and housing & energy assistance.
- Without bold action taken with urgency, this crisis will worsen and potentially cross a threshold that will put the Milwaukee area beyond the point of no return.

The income inequality economy is unfair and unjust, but also it is economically a poor performer.

- Worker wages, adjusted in real terms for inflation, have been stagnant on average for about two decades; for workers on the low end of the labor market, wages actually declined during that period of time.
- Corporate profits in absolute and relative terms never have been higher, and the share of the wealth generated in the economy going to worker wages has not been this low since prior to the Great Depression.
- Since 1968, a typical benchmark for such comparisons, worker productivity, or the ability of our economy to pay higher wages without accelerating inflation, has more than doubled but average worker pay has remained flat.
- The redistribution of wealth upward not only has tremendous adverse social costs and moral implications, the economic ramifications have been disastrous: Lower living standards and lower economic growth, which have created a cycle of further economic polarization and upward wealth redistribution, softer labor markets and lower wages, and still less growth than the capacity of the economy could manage.

The income inequality economy did not arise naturally or because of economic laws; it developed as the result of decisions by policy makers both in terms of action taken and failures to take action

- Economists now recognize that globalization and "skill biased technological change" cannot be the sole explanation for lower wages, increasing inequality, and increased low-wage employment.
- While the explanation is more complex than what can be addressed here, the simple truth is that policy-makers can address this situation by raising wages for low-income workers in so-called low-skill employment.
- In fact, we already have a model based upon economic history, especially here in Milwaukee.
    - At one time, dirty and dangerous employment in factories paid low wages.
    - But because of worker collective action and government policies, wages were raised and America gave rise to history's first middle class. It did not happen by accident.
    - Now, we must turn the low-wage employment of today into decent-paying jobs to rebuild the middle class, raise living standards, and grow the economy, just as we did in the middle third of the 20th century.

    - The industrial jobs of the 1930s through 1980s that have been lost are not coming back; the prevalence of low-wage service-sector employment means that only if standards are raised for these jobs can wages, income and living standards be increased.
- Some commenters say that more education and skills training can address the income gap and prevalence of low-wage employment; but they are incorrect.
    - Only 22% of the jobs require a college degree or advanced skills. Without demand for such higher-skilled or more-trained workers with a number of jobs in higher-wage occupations, no amount of training will lead to generally applicable increases in wages.
    - Education and training lead to individual-level advancement; but right now, the scale of the good jobs crisis requires broader policies to raise wages, which education and training cannot accomplish.

Economists are forming an emerging consensus that establishing and raising wage floors through policy is economically sound and socially preferable to alternatives.

- After years of study, the evidence of economic research seems to show that wage floors, like the minimum wage or local living wage laws, do not create adverse employment consequences.
- Unless setting a wage floor at a ridiculously high level, like $20 per hour or more in a market like Milwaukee, establishing a moderate wage floor between $12 and $15 per hour, can accomplish crucial wage-raising objectives that boosts the local economy while raising living standards, and do so without decreasing employment levels.
    - Only the simplistic, Econ 101 analysis would say that increasing wages decreases aggregate employment. This model assumes perfectly competitive labor markets and does not account for what economists call "dynamic equilibrium," where higher wages lead to more consumption which drives an increase in aggregate demand, leading to even or increased employment levels.
    - The empirical evidence bears this out and should matter more than superficial assertions.
- Those who keep up with the research know and understand that the minimum wage body of knowledge is clear on this point; and most economists apply this to understanding of local living wage laws – especially so because they target a narrower population in industries that typically can bear higher wages.
- In fact, after two decades of living wage laws in 140 communities across the country, all of the research demonstrates that they raise wages as intended and do so without decreasing employment, increasing taxes, or diminishing services. Even though such fear-mongering and wolf-crying has been part of nearly every debate on living wage ordinances, these claims have been found false – which is probably why communities are not only not repealing their living wage laws, but instead looking to enhance and expand them.

Establishing a wage floor at 110% of the poverty level is an important first step, but even that proposal does not go far enough.

- Economists at the Massachusetts Institute of Technology calculated that a "living wage" for the Milwaukee Metropolitan Statistical Area, for someone supporting a family, would be roughly $20 an hour.
- By comparison, the minimum wage of $7.25 is at an historic low in real terms; according to the Economic Policy Institute, had it kept pace with the growth of productivity of American workers, i.e. the ability of the economy to pay higher wages, it would be nearly $19 an hour.
- According to figures from the National Employment Law Project, the median living wage ordinance puts the wage floor at around $13 an hour in 2013 terms.

The corporations affected by the proposed living wage ordinance easily can absorb an increase in labor costs without passing along those costs to the County.

- Firms competing on market share in industries with a relatively small number of supply participants are willing and able to "eat" higher labor costs through a relative increase in wages because slightly lower profit margins on a given contract is preferable to losing a contract all together.
    - Take for example the institutional food service industry. Here, three large players (Compass Group, Aramark, and Sodexo) dominate while 5-10 more compete at various levels. Milwaukee County holds a contract with Aramark, which made $12.6 billion in fiscal year 2012 and soon will go public with a stock offering. Aramark seeks market share, with many contracts aggregating to produce their staggering profits. "Eating" higher wage costs in lieu of increasing a bid on a guaranteed government contract as opposed to losing that market share to Compass or Sodexo, is part of the business model. And with such high firm-level gross and net revenue, they can afford comparatively moderate wage increases.
- Firms that would be affected by the proposed living wage ordinance in almost all cases are large firms, many of them multi-national in scale, and some even held by private equity giants; they are not small, local mom-and-pop outfits.
    - For example, besides the aforementioned Aramark, Milwaukee County does business with SSP America, HMS Host, and Paradies as airport concessionaires, dominant industry giants that operate globally; janitorial firm Clean Power is owned by Marsden Group, a large regional building services firm; security firm G4S/Wackenhut is a global powerhouse.
    - Besides doing business with Milwaukee County, these firms have something else in common: in other markets, they absorb higher wages established through living wage ordinances where applicable, and do so as a cost of doing business with other government bodies.

The proposed Milwaukee County living wage ordinance would directly boost the local economy.

- According to an analysis conducted by economists at the Center on Wisconsin Strategy, passing the living wage ordinance would add about $35-40 million in income to nearly 10,000 low-wage workers and their families
    - This stimulative effect of the living wage ordinance in the range of tens of millions of dollars cannot be matched by any other policy of the County.
- The basic macroeconomic theory here breaks down fairly simply: higher wages for lower-income people means a boost to the economy.
    - Lower-income families have the highest marginal propensity to consume, meaning that every additional dollar they bring home in a paycheck gets spent; more money in the hands of the working poor not only means better living standards for them, it means more consumer spending to boost our local economy.
    - Income distributed to corporate profits or up the wealth/income scale has a much lower multiplier effect on the economy than money in the hands of workers, meaning that directing more money into the paychecks of working people will grow the economy much more than allowing it to pad already obscene corporate profits and exhorbitant executive compensation.
    - The geographic realities matter greatly: the living wage ordinance would ensure that taxpayer dollars benefiting private corporations get paid in wages to their workers here in Milwaukee, who will spend that money here in Milwaukee; without a living wage ordinance, Milwaukee County is

choosing to allow its dollars to end up in Virginia, Georgia, California, and many other places that aren't Milwaukee – where the corporations doing business with the County are headquartered.

In summary, an economist's view of the living wage ordinance is that:

- The low-wage, high-inequality good jobs crisis requires bold action with urgency both for social & moral reasons as well as for economic reasons.
- Increasing wages will lead to positive economic outcomes without adverse employment outcomes.
- Raising wages through a living wage ordinance is a proven, practical policy with 140 communities' and two decades' worth of empirical evidence backing up its soundness.
- Passing a living wage ordinance is the one key tool that local government has at its disposal to address the good jobs crisis.

America has a good jobs crisis with skyrocketing income inequality and a growing prevalence of low-wage employment; this holds back economic growth and increases in living standards. In Milwaukee, the good jobs crisis is particularly acute.

> By 2020, roughly half of all employment will be in low-wage service-sector jobs that cannot support families or a middle class.
>
> Increasingly inequitable distribution of income holds back the economy because low wages for the overwhelming majority retard aggregate demand, which drives economic growth and overall performance.
>
> Turning low-wage employment into decent jobs with wages adequate to support a family will boost the economy.
>
> With half of all jobs paying low wages, unless bold action is taken, the crisis may soon cross a threshold from which we cannot return.
>
> Our nation, and our community, have proven that we can turn poor-paying jobs into good, family-supporting jobs. We built the world's first middle class when turning poverty jobs in factories into the good jobs with high wages. Since those have disappeared, replaced by increasingly prevalent low-wage jobs, the solution is to turn the jobs of today (and tomorrow) into good jobs.

The inequitable, distorted economy leaves wages too low.

> Corporate profits continue to hit all-time highs every year because productivity has increased dramatically. Productivity increases represent the ability of our economy to pay higher wages. However, wages have not kept up. Since the 1960s, average wages in real terms are stagnant and flat, while wages on the lower end of the labor market actually declined.
>
> Take for example the minimum wage: had it grown at the rate of productivity since the late-1960s, it would be roughly $19 an hour in real, inflation-adjusted terms, instead of its current historic low of $7.25 an hour.
>
> Raising the federal and state minimum wage would be ideal. However, political polarization prevents this from happening any time in the near future, so other solutions must be found. Living wage ordinances are just that type of localized solution that works.

Living wage ordinances are proven, practical tools at the disposal of local governments to accomplish positive wage effects in regional economies, without sacrificing employment levels or increasing taxes.

> In 140 communities across the country over the past two decades, living wage laws have been put into place to raise wages.
>
> The research on living wage laws is rather definitive. Despite repeated claims by opponents that their implementation would decrease employment and increase taxes, the empirical data

demonstrates such assertions to be false. Numerous studies conducted by credentialed economists and policy experts find that at worst, no employment effects are found and that in some cases, overall employment actually increased in the jurisdiction. Further, they find that at worst, costs to local governments with living wage ordinances increase negligibly, in the tenths of one percent and in some cases do not increase at all.

This is because of two reasons: first, at a time when corporate profits are incredibly high, and the affected firms operate in industries where market share matters far more than profit-per-contract, these firms can and do accept lower margins on a given government contract in order to retain it and do not pass along significant costs to the local government; second, labor market economists know that when wages go up, so too do productivity and efficiency while turnover decreases, leading to a greatly diminished increase in labor costs.

Overall, research on living wage laws show that despite claims of adverse impacts, these policies are sound, effective means of increasing wages for low-income families. As it stands, a living wage ordinance is the sharpest tool in the box for local government to raise wages and boost the economy; and, no other policy adopted by the County could accomplish nearly as much.

A living wage law for Milwaukee would have a tremendously positive impact, in its own right, and even compared to worst-case scenarios about adverse impacts – which again, have been asserted previously but not borne out in practice.

Perhaps most importantly, an analysis released this morning the Center On Wisconsin Strategy, a think-tank based in Madison, found that the proposed living wage ordinance would put about $35-40 million of additional income in the hands of the directly affected workers. They will spend that money here in Milwaukee, boosting our local economy.

Instead of Milwaukee County tax dollars subsidizing corporate profits for firms located outside of our area, as the affected firms are, the living wage ordinance would keep that money here locally, in the hands of low-income workers who have the highest marginal propensity to consume.

A holistic approach to fiscal effects on government demonstrates another key point: Most of the affected workers live in poverty, qualifying for public assistance programs like food stamps and BadgerCare. By raising their wages, there will be less reliance on government assistance, and consequently, lower potential tax burdens for Wisconsinites.

Labor market economists also know that when wages go up for some workers, standards are increased for other workers. This strengthens local labqr markets while increasing standards of living.

Considering the proven performance of living wage ordinances as successes in 140 communities over the preceding two decades, only one real economic critique can be levied against the proposed Milwaukee County living wage ordinance: that is, it does not go far enough.

Economists at the Massachusetts Institute of Technology estimate the living wage for the Milwaukee Metropolitan Statistical Area to be $19.66 an hour.

The proposed wage level of 110% of the poverty line for a family of four, equating to $12.45 for the current year, is too low. Even the median living wage ordinance sets a wage standard at a little over $13 an hour, and many include health insurance and other benefits.

Labor market economists are coming to a growing consensus, as noted before, that raising wages both is crucial for economic performance and readily within the capacity of our current economic position. Only when wages would be raised, in a market like Milwaukee, to something like $17 to $20 an hour, for all workers in the region and not just the 8,000 who would be affected by the proposed ordinance here, would disemployment effects become real.

As an economist, I recommend that Milwaukee County adopt the living wage ordinance because it will help address the fundamental economic challenge of our time: low wages and increasing inequality that hold back economic performance.

Living wage laws are proven effective both by their presence in 140 jurisdictions across the country and by virtue of the research demonstrating their successes without the instances of adverse consequences for employment and fiscal impact as raised by opponents who routinely cry wolf.

By passing this ordinance, Milwaukee County will ensure that it is doing what it can to improve our local economy.

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CRY WOLF PROJECT

Living Wage ordinances
By Stephanie Luce
Revised January 25, 2011

"Living wage" ordinances are a policy tool used to raise wages for low-income workers. While they vary quite a bit, the most common form is a municipal ordinance requiring businesses that hold service contracts or receive economic development assistance from a city to pay their employees a living wage.

There are different ways to define a living wage but the basic concept is an hourly wage that would bring a worker with a family up to the federal poverty line.

The first such ordinance was passed in Baltimore in 1994. The idea became popular and by the late 1990s and early 2000s, dozens of campaigns emerged. Today, there are over 150 living wage ordinances in effect around the country, in large and small, urban and rural cities and counties - including Los Angeles, New York city, San Francisco, Chicago, Boston, Miami, Detroit, Buffalo and Milwaukee.[1] A few living wage ordinances apply to other kinds of entities, such as universities, airports and stadiums. Maryland broke ground by passing a statewide living wage ordinance in 2007.

Most living wage ordinances mandate employers to pay an hourly living wage and provide health benefits, or pay a higher wage if benefits are not provided. For example, the 2011 living wage rate in San Diego, California is $11 an hour plus health benefits, or $13.20 an hour if health benefits are not provided. Most ordinances are indexed to go up annually with the cost of living, and many include additional benefits such as paid sick days.

Living Wage campaigns are usually run by large coalitions. The main groups involved include community organizations, unions, faith-based groups, and students.

Arguments for the Living Wage

The US federal minimum wage does not increase automatically with the cost of living and can only be raised by Congress. Congress only increased the minimum wage once between 1981 and the early 1990s, so by the mid-1990s the real value of the minimum wage (adjusted for inflation), was far below its historic levels.

Facing resistance at the federal and state level, advocates pushed to raise wages at the local level. They demanded that local officials to address the growing issue of low wage jobs. At the time that the first living wage ordinance was proposed, almost eight million people were considered "working poor" - working 27 weeks or more per year but not earning enough to meet the federal poverty line.[2] Almost 31 percent of the workforce was earning poverty-level wages (Economic Policy Institute 2008). By raising wages to the federal poverty line, living wage ordinances set wages at a level higher than the state or federal minimum wage. For example, the federal minimum wage in 2010 is $7.25 an hour, and the highest state minimum wage, in Washington State, is $8.55 an hour. The living wage rate (where it is set to the poverty line for a family of four) is $10.55 an hour.

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[1] For a full list of ordinances see the National Employment Law Project, www.nelp.org.
[2] Current Population Survey. 1995. "A Profile of the Working Poor."

Living wage ordinances benefit workers by establishing higher hourly wages - however, they do not usually cover a large proportion of workers. For example, the San Diego ordinance covered 974 workers in fiscal year 2009, out of a total labor force of approximately 668,000.[3]

Living wage supporters also argued that cities could spend their money in a way that benefitted residents and the community. For example, the Boston living wage ordinance begins with this statement:

> Whereas, the City of Boston awards millions of dollars in contracts for services each year that result in the creation or maintenance of a wide variety of employment opportunities; and
> Whereas, these contracts are paid for by taxpayer dollars and-should be used to promote the sustenance and creation of jobs that will increase consumer income, decrease levels of poverty, invigorate neighborhood businesses and reduce the need for taxpayer-funded programs in other areas; ..

Some advocates were motivated by moral arguments, suggesting that people should not be working and living in poverty. Others highlighted the economic inefficiencies of allowing employers to pay low wages and then having workers depend on public programs (such as food stamps, subsidized housing, and the Earned Income Tax Credit) to survive. Still others suggested living wages can be good for businesses: If employers raised wages they would benefit from lower turnover and increased productivity.

Arguments against the Living Wage

Business groups are the main opponents of living wage campaigns. This includes the Chamber of Commerce, the American Legislative Exchange Council, industry associations and business-funded think tanks.[4] One of the main organizations that fights the policies is the Employment Policies Institute, a non-profit research and advocacy group funded by the restaurant industry and headed by Richard Berman, a well-known Washington lobbyist for food and alcohol companies. In addition, in most cities the leading newspapers editorialized against the ordinances before the vote. The living wage was not a partisan issue, as many Republican and Democrat mayors and representatives initially opposed the legislation.

Business groups have funded studies used to argue against the passage of living wage and minimum wage laws, generally predicting large job loss and costs for the city. For example, a study of a proposed ordinance in Providence, RI, funded by a coalition that included the Greater Providence Chamber of Commerce, predicted that a living wage ordinance would cost firms $146 million in increased wages and benefits, firms would lose $30 million in profits, employers would cut approximately 30,000 jobs, and the city

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[3] See City of San Diego 2010; 2006-2008 American Community Survey.

[4] Not all business leaders oppose living wage ordinances. Some business owners were vocal advocates for the laws. Some felt they did not want to have to compete with other businesses who paid low-wages when competing for city contracts.

would need to spend $18 million. Another study funded by the Chamber in Los Angeles was conducted by Spectrum Economics Inc., a consultant group that has written economic studies arguing against policies such as public health insurance, environmental regulation and prevailing wage laws.[5] The authors claimed that the L.A. living wage ordinance would cost the city $130 million a year (Merl 1996).

Opponents have a variety of objections to living wage ordinances, and their arguments have shifted over time. Initially, opponents predicted substantial economic harm from the ordinances. St. Paul mayor Norm Coleman said a living wage law would be "dumb and dangerous" because it was "the quickest way to kill jobs" (Ojeda-Zapata 1995). In Los Angeles, the Deputy Mayor for Economic Development said the proposed ordinance would cause "entire industries could be wiped out or move overseas."[6]

Citing mainstream economic models of supply and demand, living wage critics argued that if employers are forced to raise wages, they will compensate by cutting employment. Other businesses might simply move out of the city to avoid the law, or not move in. A 1999 *Washington Post* editorial said the living wage was "a formula for assisted economic suicide." Opponents claimed that businesses would stop bidding for city contracts, resulting in a loss of services for residents. Those businesses who did receive contracts would pass on the cost to the city, which would ultimately result in higher taxes. The *Boston Herald* (1996) editorialized, "The proposal couldn't be better calculated to drive business out of the city and encourage corruption" and an article in the *Oakland Tribune* added "The proposed ordinance will increase the cost for…every single item or service the city buys" (Hamilton 1997).

Others predicted that ordinances would hurt small business. Some business owners who held city contracts stated they would no longer bid for a contract if the living wage was required. One reporter said the owner of ABC Security said that although she had a half million dollar contract with the City of Oakland since 1978, she would drop it (Locke 1998).

Opponents went on to assert that the ordinances would hurt minority and women-owned businesses that do a lot of contracting with city governments. One author wrote this about the proposal in Oakland, CA: "If the proposed ordinance is adopted it will hurt small women- and minority-owned businesses the most, the majority of which are already struggling mightily to do business in this city of higher-than-average costs of doing business" (Hamilton 1997).

A number of opponents argued against the ordinances on the grounds that they would hurt the people they are trying to help. Roy Cordato of the John Locke Research Foundation wrote in 2002, "So what are the likely effects of laws that force employers to pay a living wage? First and foremost, all people whose skills are so low that the value of their productive efforts are below the mandated wage will be eliminated as potential employees." The *Los Angeles Daily News* had a similar perspective, noting unintended consequences as well as suggesting the policies were a form of socialism: "This kind of municipal socialism might be well-meaning, but it's ultimately bad for those it purports to help" (2006).

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[5] See Spectrum Economics, Inc. website for more information on their work: http://www.spececon.com/ecstud.php

[6] As cited in Pollin 1998.

Gregory Mankiw, an economist who chaired the Council of Economic Advisors for a period under George Bush, was outspoken in his critique of the Harvard University living wage campaign. Mankiw argued that the living wage would encourage teenagers to drop out of school to take the higher wage jobs, and then "take jobs that would go to unskilled adults, making it harder for those adults to make the transition from welfare to work" (Mankiw 2001). At the same time, a *Crain's Detroit Business* editorial made the opposite argument, suggesting that living wage laws could hurt teenagers: "..such an ordinance will limit the availability of internships and part-time and co-op jobs that provide youth the skills and experience necessary to gain a foothold in the job market."

A few opponents relied on ideological critiques, suggesting that governments setting wages was akin to socialism. William Tucker wrote in the *Weekly Standard*, "The Living Wage' movement has become the latest effort to impose socialism on the United States, one city at a time." Others simply objected to government intervention in markets.[7] Carl Gipson, Director of the Center for Small Business said, "It always makes for good public relations to instruct companies to pay their workers a 'fair' wage, but to do so truly throws out normal economic realities." Los Angeles Mayor Richard Riordan added, "I do not believe that government should dictate wages. We have seen this fail in Socialist and Communist countries. It will do irreparable harm."

What does the data say?

While there were many voices predicting what living wage ordinances would do, there are far fewer studies of actual outcomes. Many of the claims about living wage ordinances rely on minimum wage studies, but that work is not always applicable to living wage ordinances. There are about a dozen reports that have utilized surveys of employers and workers, interviews with city administrators, and evaluations of city contract bidding to assess the impact of living wage ordinances after they have been passed (a full list is available in the appendix).

The studies are remarkably consistent in their findings, which contradict most of the "cry wolf" claims. First, these studies find little evidence of job loss or harm to the local economy. Studies by Neidt et. al (1999), Brenner (2005), Brenner and Luce (2005), Reich (2005), and Howes (2002) all find no evidence of employment loss due to living wage ordinances. Williams and Sanders (1998) do a one-year assessment of the LA living wage ordinance and find "no significant positive or negative effect on the city's fiscal health or the local economy from the living wage ordinance."

Interestingly, while many newspaper articles and studies predict negative outcomes for small businesses, no follow-up reports present evidence of actual hardship. In fact, some employers were discovered to have made exaggerated claims during the campaign. For example, one employer in Philadelphia held contracts to shred documents for the city, and said they would have no choice but to leave the city if the living wage ordinance passed. However, when campaign organizers investigated, the contractor in question only had two employees that would be covered by the ordinance, and they would be entitled to 40 cents per hour raise (Reynolds 2003).

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[7] Some employers who oppose living wage ordinances already pay their employees a living wage. See Grant and Trautner 2004.

In addition, the studies find positive impacts on covered workers. Detailed surveys of workers covered by ordinances in Los Angeles and Boston find that the majority of affected employees are adult workers who are disproportionately people of color, poor and low-income. For example, the Los Angeles survey found that only four percent of workers covered by the law were under the age of 20, and 58 percent were 35 years or older. 86 percent of the workers were employed full-time, and on average, they had been in the workforce for 20 years (Fairris et al 2005).

Third, there is little evidence that the ordinances result in major expenses or hardships for cities. Richard Sanders and Sean Lokey (1998) conducted a follow-up study to Sander's earlier work (with Douglass Williams 1997). Williams and Sanders (1997) had estimated that the living wage would result in increased labor costs of $30 to $40 million. In the follow-up study, Sander and Lokey note that the total cost came only to about $2.5 million. Andrew Elmore (2003) reviewed data from 20 cities with living wage ordinances and found the costs of living wage ordinances for cities to be much smaller than opponents had predicted. On average, the ordinances cost cities less than one-tenth of one percent of the city budget.

Studies find some mixed evidence on bidding patterns. In a few cases the number of bids on a contract went down, but in other cases it increased. A number of employers report that it's more likely they will bid on a city contract given that they don't have to compete with low-wage employers (e.g. Fairris et al 2005). One security guard contractor in Hartford, CT remarked: "Most companies with any business sense would concentrate on a higher wage niche, because there is more stability involved, and it gives you better control of the business, and allows you to preserve your reputation" (Brenner and Luce 2005). And despite ABC Security's claim that they would no longer bid on city contracts if the living wage was passed in Oakland, the company has since bid on and won contracts from the Port of Oakland, the city of Richmond, and the city of Oakland, CA - all of which have a living wage requirement.[8]

In addition to these studies, there are a handful of publications that use government data sources to estimate the impact of the ordinances. David Neumark used the Current Population Survey (CPS) to examine trends in poverty, employment and wages in cities with and without living wage ordinances (Neumark 2002).[9] He concluded that the ordinances had a positive impact on poverty rates, but also resulted in modest job loss.

Brenner, Wicks-Lim and Pollin have critiqued the Neumark study on methodological grounds. They argue that the CPS - designed by the Bureau of Labor Statistics to measure unemployment - is not an appropriate dataset for measuring living wage impacts. Since living wage ordinances do not affect a large proportion of workers in a city, the CPS may not include any living wage workers in the sample of 60,000 households--in fact, the odds of the CPS sample including a large enough number of workers covered by the ordinance are about one in 244 million. In addition, the authors raise several other concerns with Neumark's methodology, such as sample selection bias

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[8]. (see http://www.abcsecurityinc.com/; http://sireweb.ci.richmond.ca.us/sirepub/cache/2/hw5qy2uvh5hhcwbpywkeaofs/237910823201003431466 4.PDF)

[9] Neumark has published a series of articles with this argument, including Neumark and Adams 2000, and Adams and Neumark 2005a, 2005b and 2005c.

that may have resulted when Neumark truncated his data to include only low-wage workers.

Finally, living wage impact studies find other evidence of positive effects beyond the impact on workers - something not addressed by opponents. Numerous studies find that turnover and absenteeism fall after living wage ordinances are implemented. For example, turnover among airport security screeners at the San Francisco airport went from 95 percent a year to 19 percent after the living wage went into effect (Reich, Hall and Jacobs 2005). In addition, the authors found that employment *increased* in the airport after the living wage ordinance went into effect. Fairris et al (2005) found that turnover in firms covered by the Los Angeles living wage ordinance had one-third less turnover than firms not covered. In Boston, Brenner and Luce (2005) found some employers converted part-time jobs into full-time jobs after the ordinance was implemented. A report by the City of San Diego found that 46 percent of employers covered by the living wage ordinance said they experienced a decrease in absenteeism or turnover, and 47 percent reported that the ordinance "improved their firm's quality of service."

What We've Learned

Despite the dire predictions no studies find that living wage ordinances have a devastating impact on cities, employers or workers. On the contrary, some who initially opposed living wage ordinances later turned into supporters, such as the *Boston Globe* that editorialized against the city's living wage ordinance in 1997 but turned around later to call on the city to enforce the living wage for child care workers and recycling centers. The Alexandria, Virginia City Council almost unanimously opposed the living wage concept when first introduced but later went on to pass it, and then defend it when the state tried to repeal it.

Meanwhile, the living wage increases have real impacts on workers lives. In Boston, workers covered by the ordinance experienced an average of $2.10 an hour increase in wages from 1998 to 2001, alongside an increase in hours worked, raising annual income from $16,990 to $26,990 (Brenner and Luce 2005). With this increase workers reported that they were able to achieve modest but meaningful goals, such as paying off debt, helping a son with college tuition, starting a 401(k), and opening a first bank account.

It is likely that the Chamber of Commerce and other opponents will continue to Cry Wolf about the terrible impacts of living wage ordinances. Policymakers will do best to review the data rather than rely on tired arguments recycled for each campaign.

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University of California-Berkeley Labor Center, http://laborcenter.berkeley.edu/

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Measuring the Impact of Living Wage Laws:
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PRELIMINARY DRAFT

MEASURING THE IMPACT OF LIVING WAGE LAWS:
A Critical Appraisal of David Neumark's
How Living Wage Laws Affect Low Wage Workers and Low-Income Families

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ABSTRACT: Drawing on data from the Current Population Survey (CPS), David Neumark (2002) finds that living wage laws have brought substantial wage increases for a high proportion of workers in cities that have passed these laws. He also finds that living wage laws significantly reduce employment opportunities for low-wage workers. We argue, first, that by truncating his sample to concentrate his analysis on low-wage workers, Neumark's analysis is vulnerable to sample selection bias, and that his results are not robust to alternative specifications that utilize quantile regression to avoid such selection bias. In addition, we argue that Neumark has erroneously utilized the CPS data set to derive these results. We show that, with respect to both wage and employment effects, Neumark's results are not robust to more accurate alternative classifications as to which workers are covered by living wage laws. We also show that the wage effects that Neumark observes for all U.S. cities with living wage laws can be more accurately explained as resulting from effects on sub-minimum wage workers in Los Angeles alone of a falling unemployment rate and rising minimum wage in that city.

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Measuring the Impact of Living Wage Laws:
A Critical Appraisal of David Neumark's
How Living Wage Laws Affect Low-Wage Workers and Low-Income Communities

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Non-Technical Summary of Working Paper

Since 1994 over eighty municipal governments in the United States have adopted so-called "living wage" ordinances. Most of the existing research has recognized the benefits of these laws for those workers who receive living wage increases and for their families But this research has also been clear in acknowledging the limitations of these measures in terms of affecting large numbers of the working poor.

David Neumark's recent study finds that the effects of living wage laws are much broader than this previous research has suggested. Building upon data provided in the monthly Current Population Survey (CPS), Neumark concludes that wages for as much as 11% of the low wage workforce may rise as a result of these measures in cities that adopt them, an estimate that is orders of magnitude beyond previous findings. Neumark also concludes that living wage laws reduce employment for the lowest wage workers in such cities by a significant degree. In short, Neumark finds that living wage laws pose a clear trade-off between higher wages versus fewer jobs.

This paper assesses the robustness of the Neumark's findings, focusing primarily, as he himself does, on the effects on wages. We also consider his findings with respect to employment. We find that Neumark's findings are neither methodologically sound nor robust either statistically or substantively. To begin with, Neumark's analysis relies on a statistical technique that is well-known to produce unreliable results. We briefly discuss the professional literature on this subject, then show that Neumark's results are invalidated when we use an alternative statistical technique that controls for the problems faced by Neumark's methodology.

We then proceed by accepting Neumark's statistical approach on its own terms, and within that context, raise other concerns with his analysis. Most broadly, we argue that the CPS cannot be effectively utilized in the manner deployed by Neumark, to detect the effects of living wage laws. In attempting to work with the CPS for this purpose, Neumark broadly classifies workers as among those "potentially covered" by living wage ordinances—i.e. those receiving legally mandated raises because of the implementation of living wage laws—without providing evidence as to whether his system of assigning "potential coverage" is consistent with the actual experiences of cities in implementing their ordinances. His decisions in selecting which workers should be included among those "potentially covered" to receive mandated raises several serious questions.

For example, Neumark includes workers who receive wages below the national or relevant state-wide minimum wage—sub-minimum wage workers—as among those who have "potentially" received mandated living wage increases even though these workers are being paid

below the lower mandated minimum wage standard. Neumark also assumes that virtually all workers are "potentially covered" by living wage laws in cities where those laws apply to firms receiving subsidies or other forms of business assistance from local governments. However, officials in nearly all the relevant cities report that they have not yet applied the living wage mandates to any businesses in their cities simply on the basis of their receiving subsidies or other forms of assistance from government. In addition, while Neumark's data set is nominally constructed to include a wide range of cities that have passed living wage ordinances, in fact most of the observations from cities with living wage ordinances come from a small number of municipalities. Indeed, in a majority of Neumark's econometric models, over half of all relevant observations from living wage cities come from Los Angeles alone.

In our statistical models relying on Neumark's own technique, we show that Neumark's findings are invalidated when 1) workers in living wage cities are reclassified from Neumark's "potentially covered" category to uncovered based on the actual extent of coverage in these cities; or 2) sub-minimum wage workers are reclassified from Neumark's "potentially covered" category to being uncovered by living wage laws. Consistent with the high concentration of observations coming from Los Angeles alone, we also find that 3) Neumark's statistically significant results on wage effects are invalidated when we reclassify sub-minimum wage workers in Los Angeles alone as being uncovered rather than "potentially covered." In other words, Neumark's results on wage effects throughout all U.S. cities in his sample hinge on his having included sub-minimum wage workers in Los Angeles as covered by that city's living wage laws. We also find that, in fact, the rise in wages for sub-minimum wage workers in Los Angeles over the time period investigated by Neumark is being driven to a significant extent by a sharply declining unemployment rate in the city and a rise in the California minimum wage, not by the city's living wage ordinance.

Our overall conclusion is that David Neumark's analysis of the effects of living wage laws in the United States has produced no results that stand up to the scrutiny of this critical replication exercise. Of course, our results to not speak to the broader substantive question as to how living wage laws have affected low-wage workers in terms of either their wages or their employment opportunities. But we expect that many researchers will continue to make progress in addressing these substantive questions, which are, of course, the central matters of concern for understanding how living wage laws are affecting the lives of low-wage workers in the United States.

1. Introduction and Summary of Findings

Since 1994 over eighty municipal governments in the United States have adopted so-called "living wage" ordinances. While the specifics of these various measures differ, their common theme is that they require firms doing business with local governments to pay minimum wage rates that are well above both the U.S. and state-level minimum wage levels. The aim of these laws is to set a wage floor high enough so that a full-time worker can support a family of three or four at a living standard above the official poverty line. Most of the laws apply to large-scale city service contractors, although a limited number also apply to firms receiving financial assistance, tax abatements or other subsidies.

Most of the existing research has recognized the benefits of these laws for those workers who receive living wage increases and for their families (see for example Pollin and Luce 2000, Pollin and Brenner 2000, and Reich, Hall and Hsu 1999). But this research has also been clear in acknowledging the limitations of these measures in terms of affecting large numbers of the working poor. Indeed, most studies of both proposed and enacted living wage ordinances find that these measures affect a very small number of private sector firms in a given locale, and that the benefits of higher wages are concentrated among a small fraction of the overall workforce within any given municipality (e.g. Pollin and Luce, op cit.; Niedt et al. 1999; Nissen 1998).

Recent work by David Neumark (2002) finds that the effects of living wage laws are much broader than this previous research has suggested. Building upon data provided in the monthly Current Population Survey (CPS), Neumark concludes that wages for as much as 11% of the low wage workforce may rise as a result of these measures in cities that adopt them, an estimate that is orders of magnitude beyond previous findings. Neumark also concludes that living wage laws reduce employment for the lowest wage workers in such cities, with an estimated employment elasticity of -.14. In short, Neumark finds that living wage laws pose a clear trade-off between higher wages versus fewer jobs. But because Neumark also finds that the positive wage benefits for low-wage workers have been stronger than the costs in terms of job

losses, the overall effect of these ordinances according to Neumark has been to reduce poverty in cities that have adopted them.

Because of the importance of these questions to an accurate understanding of the dynamics of living wage ordinances, this paper will assess the robustness of the Neumark's recent findings, focusing primarily, as he himself does, on the effects on wages. We also consider his findings with respect to employment. We do not examine his analysis of poverty impacts, since the viability of this analysis will hinge entirely on the prior analysis of wage and employment effects.

Our overall conclusion is that Neumark's findings are neither methodologically sound nor robust either statistically or substantively. To begin with, Neumark's econometric model relies on a truncated sample of workers that excludes higher-wage workers from the data pool. While Neumark's approach is correct in focusing its attention on low-wage workers, the particular manner in which he does so, through truncating the full sample, is vulnerable to sample selection bias. This diminishes the reliability of his results, since they are subject to both bias and inconsistency. We show that Neumark's results are not robust when one takes an alternative approach to truncation, that is, utilizing quantile regression.

But we then also accept Neumark's truncation approach on its own terms, and within that context, raise other concerns with his analysis. Most broadly, we argue that the CPS cannot be effectively utilized in the manner deployed by Neumark, to detect the effects of living wage laws. In attempting to work with the CPS for this purpose, Neumark broadly classifies workers as among those "potentially covered" by living wage ordinances—i.e. those receiving legally mandated raises because of the implementation of living wage laws—without providing evidence as to whether his system of assigning "potential coverage" is consistent with the actual experiences of cities in implementing their ordinances. His decisions in selecting which workers should be included among those "potentially covered" to receive mandated raises several serious questions.

For example, Neumark includes workers who receive wages below the national or relevant state-wide minimum wage—sub-minimum wage workers—as among those "potentially covered" by living wage laws, despite the fact that these workers aren't being paid at the lower minimum wage standard. Neumark also assumes that virtually all workers are "potentially covered" by living wage laws in cities where those laws apply to firms receiving subsidies or other forms of business assistance from local governments. However, officials in nearly all the relevant cities report that they have not yet applied the living wage mandates to any businesses in their cities simply on the basis of their receiving subsidies or other forms of assistance from government. In addition, while Neumark's data set is nominally constructed to include a wide range of cities that have passed living wage ordinances, in fact most of the observations from cities with living wage ordinances come from a small number of municipalities. Indeed, in a majority of Neumark's econometric models, over half of all relevant observations from living wage cities come from Los Angeles alone.

Even in the models in which we accept Neumark's sample truncation technique on its own terms, we still show that all of Neumark's statistically significant findings are invalidated when 1) workers in living wage cities are reclassified from Neumark's "potentially covered" category to uncovered based on the actual extent of coverage in these cities; or 2) sub-minimum wage workers are reclassified from Neumark's "potentially covered" category to being uncovered by living wage laws. Consistent with the high concentration of observations coming from Los Angeles alone, we also find that 3) Neumark's statistically significant results on wage effects are invalidated when we reclassify sub-minimum wage workers in Los Angeles alone as being uncovered rather than "potentially covered." In other words, Neumark's results on wage effects throughout all U.S. cities in his sample hinge on his having included sub-minimum wage workers in Los Angeles as covered by that city's living wage laws. We also find that, in fact, the rise in wages for sub-minimum wage workers in Los Angeles over the time period investigated by

Neumark is being driven to a significant extent by a sharply declining unemployment rate in the city and a rise in the California minimum wage, not by the city's living wage ordinance.

The rest of the paper is structured as follows: In Section 2, we present an explanation and replication of Neumark's basic findings for wages and employment. In Section 3 we discuss methodological shortcomings with the Neumark's analysis. In Sections 4 and 5, we re-estimate his findings for wage effects and employment effects of living wage laws on the basis of both and alternative econometric methods and alternative choices as to which workers should be classified as likely to have received mandated living wage increases. Section 6 concludes.

2. The Neumark Model – Explanation and Replication

In his 2002 Public Policy Institute of California (PPIC) Report "How Living Wage Laws Affect Low Wage Workers and Low-Income Families," Neumark uses data from the Current Population Survey (CPS) to measure the effect of living wages on the wages and employment status of low wage workers during the 1996-2000 period. Specifically, Neumark constructs a pooled cross section of observations from the CPS, utilizing all persons in cities with at least 25 observations in a given month of the calendar year.[1] Taking a difference-in-difference approach, he then attempts to gauge living wage effects by analyzing low wage workers in large cities with and without living wage laws over the period in question. We will first describe his methods for assessing the effect of living wage laws on the earnings of low-wage workers and then turn to his employment analysis. Our remarks will focus on the three models from which he derives his basic findings.

[1] In the wage analysis, the sample is constructed from observations of employed persons earning an hourly wage greater than $1.00 and less than or equal to $100. In both the wage and employment analysis, observations are restricted to individuals between the ages of 16 and 70, inclusive.

Wage Analysis

Equation 1 presents the first model that Neumark employs to assess the effect of living wage laws on low-wage workers' earnings:

(1) $$\ln(w_{icmy}) = \alpha + X_{icmy}\omega + \beta\ln(w^{min}_{cmy}) + \gamma\max[\ln(w^{liv}_{cmy}),\ln(w^{min}_{cmy})] + \delta_y Y_y + \delta_M M_m + \delta_C C_c + \varepsilon_{icmy}$$

Here Neumark estimates a wage equation where w is the hourly wage, X is a vector of dummy variables controlling for gender, race, education, and marital status, w^{min} is the higher of the federal and state minimum wage, and w^{liv} is the higher of the prevailing minimum wage or applicable municipal living wage (our notation throughout mirrors that in Neumark's study). Y, M, and C are vectors of dummy variables controlling for each year, month, and city respectively. The unit of analysis (denoted by the subscript of icmy) is the individual worker, within a given city c, month m, and year y. Note that β captures the effect of the minimum wage (state or federal) on individual worker wages, and γ captures the wage effect of the living wage on workers' wages.

Neumark employs a second model that attempts to discriminate between the wage effect of living wage ordinances experienced by "covered" workers versus those experienced by "uncovered" workers. In classifying workers as covered or uncovered, Neumark distinguishes between two types of living wage ordinances. The first type of law applies only to those firms that perform services under contract with a municipality. These usually include such services as landscape maintenance or security guard and janitorial services. For these "contractor-only" ordinances, covered workers are defined to be those individuals working in service industries (usually about 10-20% of the workforce).

The second type of living wage ordinance he identifies applies to any firm that receives a designated form of financial assistance (such as utility subsidies, tax abatements, or industrial revenue bond guarantees, etc.) from a municipality. For ordinances with such "business-

assistance" provisions, covered workers are defined to be all private sector workers (usually about 90-95% of the workforce), since any business in the private sector could potentially receive financial assistance from its local government. It is important to emphasize that, unlike most other researchers on this issue, Neumark makes no attempt to determine which firms and workers are actually likely to be covered according to the stipulations of the living wage ordinances in any given municipality. He rather relies on broad assumptions as to which workers are "potentially" covered, then proceeds with his estimation exercises through including all "potentially" covered workers in his data sample. As we will see, Neumark's assumptions as to who should be included in the pool of potentially covered workers are crucial to his overall findings.

Equation 2 presents the second model used by Neumark:

$$(2) \quad \ln(w_{icmy}) = \alpha + X_{icmy}\omega + \beta\ln(w^{min}_{cmy}) + \gamma\max[\ln(w^{liv}_{cmy}) \times Cov_{icmy}, \ln(w^{min}_{cmy})] + \gamma'\max[\ln(w^{liv}_{cmy}) \times Uncov_{icmy}, \ln(w^{min}_{cmy})] + \delta_y Y_y + \delta_M M_m + \delta_C C_c + \varepsilon_{icmy}$$

Here Cov is a dummy variable equal to one for covered workers and Uncov is a dummy variable equal to one for workers who are not covered. Note that while β still controls for the wage effect of the minimum wage, γ and γ' capture the separate wage effects of the living wage for covered and uncovered workers, respectively.[2]

In his third model, Neumark returns to the specification in equation 1, but also attempts to distinguish between the effects of contractor-only versus business-assistance living wage laws. Equation 3 depicts this approach:

$$(3) \quad \ln(w_{icmy}) = \alpha + X_{icmy}\omega + \beta\ln(w^{min}_{cmy}) + \gamma\max[\ln(w^{liv}_{cmy}) \times Bus_{icmy}, \ln(w^{min}_{cmy})] + \gamma'\max[\ln(w^{liv}_{cmy}) \times Con_{icmy}, \ln(w^{min}_{cmy})] + \delta_y Y_y + \delta_M M_m + \delta_C C_c + \varepsilon_{icmy}$$

Here Bus is a dummy variable equal to one for workers living in a city with a living wage ordinance that includes a business assistance provision and Con is a dummy variable equal to one for workers living in a city with a contractor-only living wage ordinance. Again, β controls for

the effect of the minimum wage on individual wage levels, and γ and γ' capture the different wage effects of business-assistance type living wage ordinances and contractor-only type living wage ordinances respectively.

Neumark estimates these three models using data on two different subsets of the workforce: 1) workers who fall into the lowest wage decile in a given month of the sample in their respective city, and 2) workers whose wages place them in the lowest quartile of the wage distribution, but not in the lowest decile, i.e. those between the 10th and 25th wage centile in the sample month in their respective city.[3] With each model Neumark also employs three slightly different specifications. The first uses a contemporaneous living wage variable. The second uses a living wage variable that is lagged 6 months. The third uses a living wage variable that is lagged 12 months. We replicate Neumark's basic results in Table 1A (these figures are identical to those found in Tables 5.3-5.5 of Neumark, 2002, p. 52, 63, and 65, respectively). Neumark also presents estimates using data from higher centile ranges. However, almost all of his significant results are limited to the lowest decile. He therefore focuses most of his attention on the model specifications and results presented here.

[INSERT TABLE 1A HERE]

The results which are of primary interest and which are statistically significant in all three models are those using the 12 month lagged living wage variables. For example, Neumark's regression estimates of Model 1 indicate that with a one-year lag, those workers in the lowest wage decile in a living wage city have a wage elasticity of 0.07. That is, given a 100% increase in the living wage one year earlier, workers within the lowest wage decile in a living wage city experience a 7% increase, on average, in their wages. Neumark offers two possible interpretations of this result and similar results in subsequent specifications. The first possible

[2] Neumark adds a set of dummy variables to the vector X of Model 2 to control for level differences between the different worker subgroups that are covered by the various living wage ordinances.

interpretation Neumark offers is that, given that the maximum wage elasticity we would expect for a covered worker would be 1, the .07 elasticity represents the proportion of workers in the lowest decile that receive the full living wage raise—that is, seven percent of all workers in this lowest decile are receiving the living wage increase. The second possible interpretation is that workers earning the living wage increase now earn wages that place them in a higher wage decile. This "bumping up" effect would then mean that the mean wage for the lowest decile would rise, since the decile would now include workers whose wages had previously placed them in the next higher decile. As Neumark notes, this second interpretation of his results can also incorporate disemployment effects. That is, if some workers in the lowest decile become nonemployed due to the living wage and therefore drop out of the pool of wage earners, that would mean that the newly constituted lowest decile would now include workers earning higher wages. In our analysis of Neumark's findings, we find that his first explanation cannot be supported by the relevant evidence, but that his second explanation is empirically possible. However, it is another matter whether this second interpretation is consistent with the full range of relevant evidence.

In Model 2, with a one-year lag, those workers living in a living wage city *and* categorized as "covered" have a wage elasticity of 0.11. Those who live in a city that has a living wage ordinance that was enacted one year previously and are categorized as "uncovered" are statistically indistinguishable from those workers who live in cities that did not have living wage ordinances enacted in the prior year. Finally, in Model 3, with a one-year lag, those workers living in a city that has a living wage ordinance that includes a business assistance provision have a wage elasticity of 0.11. Those workers living in a city that had a living wage ordinance one year prior that targets contractors only are indistinguishable statistically from those workers who live in cities that did not have any type of living wage ordinance enacted one year before. Thus Neumark concludes that only those workers in cities where living wage laws have business-

[3] Neumark also presents estimates using data from higher centile ranges but because virtually all of his significant results are limited to the lowest decile, he focuses most of his attention on the two subsets

assistance clauses appear to have a statistically significant wage effect. Our regression estimates yield identical results, namely wage elasticity estimates of 0.07, 0.11, 0.11 in Models 1, 2 and 3 respectively.

Note that Neumark also finds a statistically significant effect for the living wage variable lagged six months for covered workers in Model 2 and for business assistance cities in Model 3. In both cases the magnitude of the living wage coefficient is approximately half that of the coefficient for the living wage variable lagged 12 months. Although Neumark does not highlight the statistically significant results for the six month lagged specification, in all subsequent analysis we examine this specification as well as the twelve month lag.

In Table 1B we include a model specification that is not presented in Neumark's original work, but which draws directly from it, that is:

(4) $$\ln(w_{icmy}) = \alpha + X_{icmy}\omega + \beta\ln(w^{min}_{cmy}) + \gamma\max[\ln(w^{liv}_{cmy}) \times Bus_{icmy} \times Cov_{icmy}, \ln(w^{min}_{cmy})] + \gamma'\max[\ln(w^{liv}_{cmy}) \times Con_{icmy} \times Cov_{icmy}, \ln(w^{min}_{cmy})] + \theta\max[\ln(w^{liv}_{cmy}) \times Bus_{icmy} \times Uncov_{icmy}, \ln(w^{min}_{cmy})] + \theta'\max[\ln(w^{liv}_{cmy}) \times Con_{icmy} \times Uncov_{icmy}, \ln(w^{min}_{cmy})] + \delta_y Y_y + \delta_M M_m + \delta_C C_c + \varepsilon_{icmy}$$

What we call Model 4 combines the covered/uncovered distinction of Neumark's Model 2, with the contractor-only/business assistance distinction drawn in his Model 3. In this instance positive and statistically significant coefficients on γ (γ') would indicate that covered workers in business assistance (or contractor-only) cities were receiving wage increases as a result of the respective living wage laws. By contrast if θ (θ') displays a positive and statistically significant coefficient, this instead means that uncovered workers in living wage cities are experiencing wage increases. As can be seen from Table 1B, which presents results for the lowest decile and both the 6 and 12-month lagged specification, results from our Model 4 appear to confirm Neumark's findings that it is covered workers in business assistance cities who are receiving wage increases as a result of

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described in the text.

the passage of living wage laws. Specifically we estimate a wage elasticity of .11 for the 12 month lagged specification and .07 for the six month lagged specification.

[INSERT TABLE 1B HERE]

Employment Analysis

The framework Neumark employs for analyzing the effect of living wage laws on employment mirrors his wage analysis discussed above. Specifically Neumark utilizes a linear probability model to assess the effect that living laws have on the employment status of individuals throughout the wage distribution. One significant difference between the wage and employment models, however, is that the employment model incorporates individuals currently outside the labor force into the analysis. As such, Neumark is required to impute wages for non-working individuals, in order to place them within the overall wage distribution. For consistency, Neumark then calculates imputed wages for all workers, including those holding jobs, and utilizes these imputed wage rates, rather than actual wages for the employed workers, in establishing decile thresholds.

We reproduce Neumark's initial employment results in Table 2A (these figures are identical to those found in Tables 6.1 and 6.2 of Neumark, 2002, p. 82-3). Consistent with Neumark's original results, for Model 1 we find a negative statistically significant effect of living wage laws, lagged 12 months, on employment status for those individuals in the lowest wage decile. The estimated coefficient is –5.62. Given that the average employment rate for the lowest decile is approximately .4, this implies an elasticity of -.14 (i.e -.056/.4). In other words, these results suggest that for a 100 percent increase in wages generated through the living wage mandate, the employment rate for the lowest decile of workers would decline by 14 percent.

But note with Neumark's Model 1 results the additional statistically significant findings. That is, in four specifications with data from the 25th-50 and 50th-75th centile ranges, Neumark finds that the living wage laws exert a statistically significant *positive* influence on the employment rate. These results would appear anomalous—that the living wage laws would, first

exert any significant influence on employment for highly-paid workers; and second, that any such employment effect due to rising wages would be positive rather than negative. Neumark provides a one-sentence aside on these findings, offering the possibility that the living wage increase is responsible for producing proportionally more high-end jobs. But he offers no evidence to corroborate this hunch.

Considering now Neumark's Model 3, we also find a statistically significant effect for the living wage 12 months lagged within the lowest wage decile in business assistance cities, with an estimated coefficient of –5.88, implying an elasticity of -.15.

[INSERT TABLE 2A HERE]

One advantage that arises when modeling employment status as opposed to wages is that using working and nonworking individuals dramatically increases the number of cities for which Neumark's inclusion criteria of 25 observations in a given city-month cell are met. In all, Neumark uses a total of 223 cities in his employment analysis, as compared with the 130 cities that he is able to use in his wage analysis. A natural question that arises, however, when juxtaposing results from the wage and employment analysis, is whether the statistical significance of Neumark's employment results would remain if the analysis were conducted on the same set of cities used for his wage analysis. This question is especially germane in light of the fact that Neumark himself notes that "[t]he evidence on employment effects is weaker than the evidence on wage effects," (ibid, p. 86). As seen in Table 2B, Neumark's findings appear to hold for the restricted set of cities. For Model 1 we find a statistically significant effect for the 12 month lag of living wage laws in the lowest wage decile, with an estimated coefficient of –4.8. For Model 3 we also find a statistically significant effect in business assistance cities, with an estimated coefficient of –5.34 for the living wage lagged 12 months. In both cases the coefficients (and implied elasticities) are smaller than those estimated using the larger set of cities. The statistical significance is also weaker in the Model 3 specification.

[TABLE 2B BELONGS HERE]

3. Assessing the Neumark Approach

In this section we discuss several potential limitations to Neumark's methodological approach and examine several potentially problematic features of the features of his data sample.

Sample Truncation and Selection Bias

Neumark's focuses his analysis of living wage laws on workers at the lower portions of the wage distribution. Indeed, as we have seen, he obtains statistically significant effects of living wage laws only when he narrows his data set to include workers in the lowest decile of the overall wage distribution. The methodology Neumark employs here is known as a *truncation* of the full data set.

Given that the living wage laws are of course targeted at benefiting low-wage workers, not all workers, it is appropriate that Neumark's analysis be focused on the lowest-paid workers only. However, as has been demonstrated in the literature, Neumark's method of truncation is vulnerable to selection bias, since he is truncating the full sample based on workers' wages, the dependent variable in the model. This approach to truncation can produce both biased and inconsistent estimates of model coefficients (see Maddala 1983). The recent survey article by Koenker and Hallock (2001) provides several practical illustrations where the type of truncation employed by Neumark can create a seriously distorted statistical picture.[4]

As Koenker and Hallock make clear, a more appropriate econometric technique with which one can focus attention on the lower wage distribution while avoiding the selection bias associated with truncated regression is quantile regression. This is because with a quantile regression, one can choose the central tendency point around which to estimate a regression—for example, wages at the 10th decile rather than the mean—without truncating the sample to exclude the upper 90 percent of workers from the analysis. Koenker and Hallock provide numerous

[4] One of the examples Koenker and Hallock describe is Engel's classic analysis of the relationship between food expenditure and household income. In this case, they show how a truncation on the dependent variable such as Neumark performed "would yield disastrous results." They further comment that "such

examples in which the quantile regression technique has been used in recent years in labor econometric models. In the next section of the paper, we perform a quantile regression on Neumark's data sample.

Sample Size Issues

Unlike analysis at the national level, utilizing the Current Population Survey to study local area labor markets can frequently be difficult, due to relatively small sample sizes. This is particularly true if the analysis is concentrated on a subset of the local-area labor market (in our discussion we take the local area labor market to be the Metropolitan Statistical Area – MSA). Although Neumark utilizes data from a broad range of cities, the analysis fundamentally is one of local area labor markets, since it is within the MSA that the difference-in-difference methodology applies.

This general problem of small sample sizes for local areas is exacerbated by the specific task of detecting the effects of living wage laws in which, according to previous researchers, the number of workers affected by the laws will be a very small proportion of the total low-wage labor force. Take for example the case of Los Angeles. In 1999 there were approximately 4.6 million people in the Los Angeles labor market (i.e. the Los Angeles-Long Beach Primary MSA). Pollin and Luce (2000) had estimated that, as of 1999, the LA living wage ordinance would cover no more than 7,600 employees, that is, no more than 1 in 600 workers in the local LA labor market. Given that in 1999 the CPS sampled approximately 5000 wage earners in the Los Angeles-Long Beach PMSA, this implies that there are likely to be about eight workers covered by the living law within this sample[5]. This number is too small a number to obtain statistically reliable estimates.

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strategies are doomed to failure for all the reasons so carefully laid out in Heckman's (1979) work on sample selection."

[5] The estimate of eight covered workers in the CPS sample is derived as follows: 7,600 is 0.17 percent of the full 4.6 million workforce in Los Angeles. The CPS sample is approximately 5,000 workers, and 0.17 percent of 5,000 is approximately eight.

The crucial question to ask here therefore is: what is the probability that a large enough number of workers receiving a living wage would be included in the CPS sample? Assume we set 25 as the minimum number of workers receiving living wage increases necessary to conduct meaningful statistical analysis. If it is in fact the case that about 7,600 workers are covered by the law, then the odds that 25 or more will be included in the CPS sample of 5,000 are approximately one in 500,000. If we assume 30 to be the minimum necessary number for the CPS sample, then the odds of including that number or more in the CPS sample rise to one in 244 million.[6]

The problem is complicated further by the fact that even if directly affected workers were in the sample, there is no way within the CPS data of identifying who they are. Such a determination would require information about individuals' primary employers, as well as information from the city as to which private sector firms were covered by their living wage law. Because of these data limitations, Neumark adopts the method described above for identifying "potentially" covered workers. Thus, to take the Los Angeles example once again, under Neumark's definition, 97 percent of the Los Angeles low-wage labor market is "potentially" covered by the LA living wage ordinance, which translates into approximately 450,000 workers within the lowest wage decile in Los Angeles being in the "potentially covered" category. Figures of this magnitude are far beyond what any previous observer had suggested was even a most high-end estimate of the coverage range for living wage laws.

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[6] The exact formula for determining the probability of 25 or more workers covered by the LA living wage ordinance appearing in the Current Population Survey is:

$$\sum_{k\geq 25} \frac{5000!}{k!(5000-k)!} p^k (1-p)^{(5000-k)}$$

In this calculation, 5000 is the approximate sample size drawn from the Los Angeles-Long Beach PMSA while k is the number of covered workers in the CPS and p is the probability that a given individual will be a covered worker. Based on the fact that there were approximately 4.6 million labor force participants in the Los Angeles-Long Beach PMSA in 1999, and approximately 7,600 covered workers at that time, p=7,600/4,600,000.

It is important to note, moreover, that Neumark is not referring with this category of "potentially covered" workers to those who may be receiving non-mandated "ripple effect" wage increases—i.e. those workers whose employers give them raises after a living wage law passes even though the living wage law does not mandate that this be done. It is certainly true that living wage laws, like minimum wage laws, do generate some ripple wage increases for noncovered workers. But none of the existing literature on this topic suggests that such ripple effects are orders of magnitude larger in terms of number of workers getting raises than the mandated wage increases, nor does Neumark introduce any arguments or evidence to support such a position.[7] Indeed, Neumark never claims that his category of "potentially covered" workers includes workers receiving non-mandated ripple effect raises. He is clear, rather, that his category of "potentially covered" workers includes all workers employed in industries where businesses could potentially be *mandated* to provide living wage increases (see Neumark 2002, pp. 58 – 61).

Living Wage Effects on Sub-Minimum Wage Workers

Our next major concern is Neumark inclusion of sub-minimum wage workers in his data pool without having provided a careful explanation as to why it is reasonable to do so. Sub-minimum wage workers could possibly benefit through a ripple effect from a mandated living wage increase. But because, by definition, they are not receiving mandated *minimum wage* increases, it is difficult to see how they would qualify as among those "potentially covered" to receive mandated living wage increases. Neumark's decision to include sub-minimum wage workers as among those potentially covered is especially important given that it is only in the lowest decile of the wage distribution that Neumark finds statistically significant effects from living wage laws. Yet, as Neumark reports in Table 1.4 of his study, the upper bound for the 10th decile in many cities is frequently at or only slightly above the relevant minimum wage (Neumark

[7] See Pollin and Brenner (2000), pp. 49–55 for one effort at utilizing the existing literature on minimum wage ripple effects to projecting its potential magnitude for the case of the living wage ordinance in Santa Monica, CA. Some of the more recent literature on minimum wage ripple effects includes Katz and Krueger (1992) and Card and Krueger (1995).

2002, p. 7). In fact, we estimate that over a quarter of workers in the lowest decile are earning less than the relevant minimum wage over the entire sample period, and in many cities this number is considerably higher. The highest proportion is for Los Angeles, in which a full 39 percent of Neumark's data pool for the lowest decile are sub-minimum wage workers.[8]

The Definition of Coverage with Business-Assistance Clauses

Another concern with Neumark's methodology is his approach for defining coverage in cities where business-assistance recipients are covered by living wage laws. As discussed above for Los Angeles, for all practical purposes Neumark is including the entire labor force as potentially covered by the law. While Neumark acknowledges that this classification of business-assistance coverage will be "noisy", we would go further: this classification is not consistent with the available evidence of how living wage laws have actually been implemented. We draw this conclusion after conducting personal interviews with city officials in all the cities classified by Neumark himself as having business assistance clauses in their living wage ordinances. Our goal was to determine the extent to which these cities were in fact applying the business-assistance clause in their living wage laws. Based on our interviews with city staff, we found that, with the exception of San Antonio, none of the cities in Neumark's study had actually implemented the business assistance provision in their living wage law during the period 1995 to 1999.[9]

This has two important implications. First, for all cities currently classified as business-assistance cities, the category of "potentially covered" workers should include, at most, only workers in the likely-to-be-affected service industries (that is, 10-20% of the workforce in those cities rather than 90-95%). Second, adjusting the "potentially covered" category to conform with the evidence runs counter to Neumark's claim that it is the "broadness" of the business-assistance-type living wage ordinances that explains the detectable increase in wages due to

[8] It is also important to note that over the entire sample period approximately 10 percent of the lowest decile is comprised of workers earning exactly the minimum wage. This implies that more than a third of the lowest decile is earning at or below the prevailing minimum wage.

living wage ordinances.[10] Rather, the business-assistance-type living wage ordinances are not, in practice, broader than the contractor-only living wage ordinances.[11]

The Geographic Distribution of Covered Workers

Another methodological issue is the geographic distribution of covered workers in the lowest decile in both Neumark's wage and employment analysis. Panel A of Table 3 presents the number of affected workers in each living wage city for Models 1-3 with a 12 month lagged specification. We focus only on the 12 month lagged specification for the sake of brevity. The geographic distribution of covered workers in the 6 month lagged specification closely follows that presented in Table 3.

[INSERT TABLE 3 HERE]

A crucial fact that emerges from this table is that most of the covered workers are located in a very small number of cities. Indeed for Models 2 and 3 of the wage analysis, approximately half of all affected workers are found in Los Angels alone. Panel B of Table 3 presents similar estimates for Neumark's employment analysis, and here too Los Angeles has a disproportionate share of the total number of affected workers, including 54% of all affected workers in Model 3. Neumark makes no mention of this geographic concentration of his data pool in his study.

The concentration of affected workers in Los Angeles and a handful of other cities is due to three main factors. The first is the differential size of the cities themselves, with Los Angeles having the largest population among the living wage cities in Neumark's data pool. The second is the nature of the ordinances in the different cities. Using Neumark's classifications, cities with business assistance clauses have a much larger pool of "potentially covered" workers than those with "contractors only" ordinances. However the most important factor is the year in which

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[9] Telephone interviews with Apney (10/30/01), Enman (10/30/01), Gibson (10/18/01), Greyson (10/15/01), Jacobson (10/29/01), and Robbins (10/23/01). Full citations to interviews are in the reference section.

[10] Neumark concludes his analysis of the effects on wages of living wage laws by writing, "the effects are driven by cities in which the coverage of living wage laws is more broad—namely, cities that impose living wages on employees receiving business assistance from the city," (Neumark 2002, pp. 73-4).

living wage laws were adopted in each city. In the cases of Los Angeles and Minneapolis, both adopted their living wage laws in 1997. This implies that in both cities workers are classified as affected for nearly a year more than in every other living wage city. This also explains why cities such as Hartford, where their living wage ordinance was not passed until October 1999, have so few workers classified as affected in the 12 month lagged specification. As we shall see in the next section, the geographic concentration of covered workers is central to explaining Neumark's statistically significant results.

Increasing wages and employment

As we have seen, Neumark finds that living wage laws produce a statistically significant negative employment effect on workers within the lowest decile—that is, there is a lower employment rate among low-wage workers after living wage laws are implemented than before. Of course, Neumark did also find an anomalous statistically significant positive employment effect for high-wage workers due to living wage laws. But focusing on the result for low-wage workers only, Neumark interprets this negative employment effect as representing *disemployment* among low-wage workers, i.e. that low-wage workers are involuntarily facing diminished employment prospects. But Neumark is neglecting here the well-known literature on labor force participation decisions resulting from wage increases, such as would occur through a living wage ordinance. This issue is especially pertinent given that more than two-thirds of the workers in the lowest decile of Neumark's data pool are women, whose labor force participation decisions are well known to be more complex than those for similarly-profiled men (see for example the discussions in Killingsworth and Heckman 1986, Eissa and Liebman 1996, and Eissa and Hoynes 1998). For example, as Eissa and Hoynes have demonstrated with respect to the earned income tax credit program, the increased earning opportunities afforded by the program do not necessarily increase women's labor force participation and are likely to *reduce* participation in

[11] For example, even in San Antonio, the only city to implement their business assistance clause in the period, fewer than 10 businesses were covered by the law during the 1996-00 period.

the case of married women. For these women in the low-wage labor market, an increase in their take-home pay has enabled them to voluntarily cut back on their hours of work. Their reduction in employment is not due to an involuntary loss of employment. At least for some significant subset of low-wage workers, especially women, one might anticipate some analogous effects to operate in the case of living wage laws. Neumark does not explore this possibility.

4. Re-Examining Neumark's Analysis of Wage Effects

In light of the above concerns, we now reexamine Neumark's econometric results on the wage effects of living wage ordinances.

Quantile Regression

In Table 4, we present results from testing Neumark's models 1-3 using quantile regression centered on the 10th wage decile. We report results only for the living wage variable lagged 12 months, that is, the variable on which Neumark himself concentrates his attention. As we see, making this one methodological adjustment on Neumark's model to control for sample selection bias produces substantially different results than Neumark obtained through his truncated regression technique, whose results we reported in our Table 1A.

INSERT TABLE 4 HERE

With Model 1, covering all living wage laws, we now find that the coefficient value on the living wage variable has fallen nearly 10-fold—from 6.95 in Neumark's model to 0.74 and is not statistically significant. With Model 2, in which the sample is divided according to Neumark's categories of "potentially covered" and uncovered workers, we do still obtain a statistically significant coefficient for covered workers at the 10 percent level. However, the coefficient on this variable has now fallen more than 3-fold, from 10.61 under Neumark's specification to 3.04 with the quantile regression. Finally, in our quantile regression of Neumark's Model 3, the coefficient for "business assistance" living wage cities again falls nearly 10-fold and is insignificant. In short, we see that Neumark's results are not robust after controlling for sample selection bias through quantile regression, but making no further

adjustments to his methodology. At the very least, these results raise questions as to the overall robustness of Neumark's findings, and, more generally, his methodology of relying exclusively on truncated OLS regressions. Nevertheless, having raised these questions, we will now proceed through accepting *as is* his truncated OLS methodology, and addressing our other concerns with his methodology.

Correcting the Coverage Classification in Business-Assistance Cities

We now consider the issue of how Neumark chose to classify workers as among those "potentially covered" to receive mandated wage increases due to living wage ordinances. As discussed earlier, no living wage city but San Antonio has actually implemented their business-assistance clause between 1995 and 1999, the relevant years for the 12 month lagged specification.[12] How would Neumark's results be affected by classifying workers in business assistance cities in a manner reflective of the actual experiences in these cities? To assess this, we re-estimated Neumark's Model 2 and our Model 4 classifying workers in business assistance cities as "potentially covered" only if they worked in the service industry.[13] Our revised wage equation results for the lowest decile are presented in Table 5. We present results for both the 6 and 12 month lagged specification.

[INSERT TABLE 5 HERE]

As is clear from the table, reclassifying potentially covered workers to more accurately reflect how living ordinances are being implemented substantially alters Neumark's original findings. Indeed, based on our revised classification, we find that there is no measurable wage effect of living wage ordinances on potentially covered workers for either the 6 or 12 month specification. In fact, we observe a statistically significant, positive wage effect of living wages for *uncovered* workers in the 12 month lagged specification for Model 2, and in both the 6 and 12

[12] It also merits note that in the San Antonio case they have applied their law to no more than a half dozen redevelopment projects since its passage in 1998 Jacobson interview (10/29/01).
[13] Since the service contract component of Minneapolis' living wage law did not go into effect until 2001 we did not classify any workers in that city as covered.

month specification for uncovered workers in business assistance cities in Model 4. Thus, even if we ignore the bigger issues of data adequacy due to small sample sizes within local labor markets or the use of sub-minimum wage workers in our analysis, we still find that classifying workers as potentially covered or uncovered in a more plausible way itself eliminates Neumark's statistically significant effects of living wage laws on potentially covered workers.

Sub-Minimum Wage Re-Specification

We now examine the role that sub-minimum wage workers play within Neumark's original wage analysis. In an attempt to assess that role, we have re-estimated Neumark's wage regressions reclassifying sub-minimum wage workers only as uncovered. Table 6 presents wage equation Models 2 and 4 with the appropriately revised coverage classification.

[INSERT TABLE 6 HERE]

For Model 2, with both the 6 and 12 month lagged specifications, we see the magnitude of elasticity estimates for covered workers declines by more than a factor of 15 as compared with Neumark's original elasticity estimates, and in both cases the new estimates are now statistically insignificant. By contrast, under this revised coverage definition the elasticity estimates for uncovered workers in living wage cities in Model 2 are 3 times larger for the 6 month specification and 20 times larger for the 12 month lagged specification than Neumark's original point estimate. Both are now highly significant statistically. And as we see in Model 4, the statistically significant results are concentrated in the cities with business assistance clauses in their living wage ordinances.

It is also evident from the Model 4 results that the statistically significant results are being generated by the wage patterns of the sub-minimum wage workers. As we see, uncovered workers in business assistance cities are the only ones who experience a statistically significant increase in their wages in Model 4 (for both the 6 and 12 month lagged specifications), and in both cases the elasticity estimates of .3 are three times larger than the largest elasticity estimate produced by Neumark in his original set of results. It is crucial, therefore, to recognize that

almost all of the individuals who now fall into the category of uncovered workers in business assistance cities are sub-minimum wage workers. It is, furthermore, increasingly clear that that the wage patterns for sub-minimum wage workers are highly influential in generating Neumark's original findings.

But let us consider this issue with still greater specificity. As we have seen in Table 3 and our earlier discussion, covered workers in Neumark's wage analysis are highly concentrated geographically, with over half in Los Angeles alone in the business assistance models. We therefore next pose the question: to what degree can Neumark's results be explained by the wage patterns of sub-minimum wage workers in Los Angeles alone? Table 7 presents results that shed light on this question.

[INSERT TABLE 7 HERE]

In this table, we have conducted an analysis similar to that presented in Table 6, except we have reclassified the sub-minimum wage workers in Los Angeles alone as uncovered. In the specification for Model 2 we see that uncovered workers have large and statistically significant wage effects in both the 6 and 12 month lagged specifications. The implied elasticities of .17 and .15, respectively, are larger than those estimated for potentially covered workers in Neumark's original specifications, and are highly significant. For our Model 4, we find even larger elasticity estimates for uncovered workers in business assistance cities, .5 and .47 for the 6 and 12 month lagged specifications, respectively. In short, Neumark's results are not robust to the reclassification of sub-minimum wage workers in Los Angeles from his full national category into the category of uncovered workers.

Wage Dynamics in Los Angeles

These results lead us to consider what actually is happening with sub-minimum wage workers in Los Angeles in our sample period given that, by definition, these workers are unlikely to be receiving mandated wage increases due to the passage of the city's living wage law. While a comprehensive treatment of the dynamics of the low-wage labor market in Los Angeles is

beyond the scope of this paper, two facts are of primary importance as potential sources of upward wage pressure. First, along with the state of California as a whole, Los Angeles experienced dramatic declines in unemployment over the time period used in Neumark's study. Figure 1 displays these unemployment rates for both California and the LA-Long Beach PMSA over the period 1995 to 2000. As the figure shows, the monthly unemployment rate in Los Angeles declined from a high of 9.1 percent in July 1996 to a low of 4.7 percent in December 2000. Economic theory would suggest that, everything else equal, such a sharp decline in unemployment will give even low-wage workers increased bargaining power relative to employers, which in turn should drive up even low-end wages.

[INSERT FIGURE 1 HERE]

A second factor is that California's minimum wage increased from $4.25 to $5.75 over 1996-1998, and the 12 month lagged value of the Los Angeles living wage rises in close correspondence with the largest minimum wage increase (in absolute terms), though, of course, the living wage rate far exceeds the new level for the statewide minimum wage. We can observe this close relationship in Figure 2, which plots both the 12-month lagged living wage value and the contemporaneous minimum wage value for Los Angeles from January 1996 – October 2000. We also plot in Panel A of Figure 2 the mean wage of the lowest decile in Los Angeles. What is clearly suggested by this figure is that a close correlation exists between changes in the mean wage and the minimum wage, but not between the mean wage and the living wage. We also see from Panel B that at no point from January 1996 to October 2000 were *any* of the workers in the lowest decile in Los Angeles making a wage close to the living wage after it was passed.

[INSERT FIGURE 2 HERE]

This descriptive evidence suggests that we consider more formally the effects of the California minimum wage increase and decline in the unemployment rate as factors exerting upward pressure on wages for sub-minimum wage workers in Los Angeles. We present the results of this formal exercise in Table 8. To begin with, in column one, we see that we do not

find a statistically significant positive effect for the living wage variable, lagged 12 months, as we had with tests run on the full Neumark sample of bottom decile workers. As in Neumark's initial specifications, this equation includes the minimum wage with a 12-month lag, which is also insignificant. In column two we add the unemployment rate to our model specification, using a moving average of the current unemployment rate that includes observations three months before and three months after the current period. This seven-month centered moving average is highly significant, and the magnitude of its effect is as large as that of the living wage variable, which unlike the column one results, is statistically significant at the 5 percent level in this specification. The 12-month lagged minimum wage remains insignificant in this specification as well. However, as seen in column three, when we specify the unemployment rate variable with a six-month lag, the unemployment rate variable is still statistically significant at the 10 percent level, while the living wage variable becomes insignificant. When we specify the unemployment rate variable with a 12-month lag, as reported in column four, none of the variables presented in Table 8 are significant, and the living wage variable actually become negative, along with the 12-month lagged value of the minimum wage.

[INSERT TABLE 8 HERE]

The descriptive evidence in Figure 2 suggested that we also consider the minimum wage effects more carefully than we have done thus far, as we have up to this point relied on the 12-month lagged value of the minimum wage as the only specification for this variable. In all of Neumark's models, the minimum wage is included with a lag period identical to the lag for the living wage variable. However there are good reasons to believe that unlike living wage ordinances, minimum wage laws do not entail the same lag in implementation. For one thing, the minimum wage is a long-standing labor market institution, and an increase does not require the long implementation process that has been the case with living wage ordinances. Minimum wage increases also do not depend on city service contracting cycles, as do some living wage ordinances. Thus, minimum wage changes can be expected to take effect relatively quickly.

This all points towards the inclusion of the contemporaneous minimum wage as an alternative specification in our estimates, which we do in columns 5-7 of Table 8. In column 5, as in column 2, we see that with the current unemployment rate variable included, the unemployment variable is significant and with the appropriate sign while the living wage and minimum wage variables are both insignificant. In column 6 however, when the unemployment rate is lagged six months, the 12 month lagged living wage variable becomes insignificant and drops dramatically in magnitude. Moreover, the contemporaneous minimum wage increases dramatically in magnitude, and becomes statistically significant at the five percent level. When we include unemployment lagged 12 months, only the contemporaneous minimum wage is significant (at the five percent level) with an elasticity of 0.78. These results are broadly similar for the living wage variable lagged 6 months.

Overall, these results are consistent with the descriptive evidence in showing the importance of the decline in the unemployment rate and the rise in the minimum wage in pulling up the wages of sub-minimum wage workers in Los Angeles. At the very least, we can conclude that the living wage is not robust as an explanatory variable on wages for this sample of workers, while the unemployment rate is highly robust in its explanatory power. The contemporaneous minimum wage is also more consistently a significant factor than the living wage lagged 12 months. The broad conclusion, then, is that the rise in wages for LA's sub-minimum wage workers is driven substantially by changes in the unemployment rate in the region and, somewhat less clearly, by the California minimum wage changes. The wages of LA's sub-minimum wage market are not being driven to a statistically significant extent, much less a substantively significant extent, by in the LA living wage. This broad conclusion is important, in turn, for the overall interpretation of Neumark's study, since, as we have seen, the effects of living wage ordinances throughout the United States are themselves dependent on including LA's sub-minimum wage workers in his sample.

5. Re-Examining Neumark's Employment Result

We now turn to a brief examination of Neumark's employment results in light of the concerns raised in Section 3 and the examination of his wage results in Section 4. The first major issue we address is whether Neumark's employment effects are in fact caused by workers likely to be covered by living wage laws. As noted above, Neumark's approach to estimating employment effects involves incorporating a large number of individuals who are not currently in the labor force, and therefore who do not possess identifying information on wages or sector of the economy in which they work. Including these individuals outside the labor force in his employment analysis means that Neumark is unable to reproduce the "potentially covered" and "potentially uncovered" classifications that were the centerpiece of his wage analysis. However, drawing upon Neumark, we develop here an approach which does allow us to retain this crucial covered/uncovered distinction. This approach proceeds from the fact that the Current Population Survey incorporates information on sector of activity for all individuals who have been in the labor force at sometime in the past year, even if they were out of the labor force at the time they were surveyed. By restricting our sample to only those individuals who currently are or have been in the labor force in the last year, we are then able to preserve the covered/uncovered distinction. This restriction, moreover, is appropriate in substantive terms, since it is over the set of individuals with at least some marginal attachment to the labor market that we should expect to see any employment effects from living wage laws.

In Table 9 we estimate three employment models, using the specifications from wage Models 1-3, with the restricted sample discussed above. In the first panel, we see that for Model 1, when the living wage variable is lagged 12 months it is statistically significant at the 5 percent level, with a magnitude very close to the -5.62 estimate seen in Table 2A. This suggests that while we are restricting our sample to those who have been at least marginally attached to the labor market within the year of the sample, we are still not making a significant empirical departure from Neumark's original analysis.

[INSERT TABLE 9 HERE]

In the second panel, we find evidence still consistent with Neumark's findings, namely a negative and statistically significant coefficient (at the 10 percent level) of -6.65 for the living wage variable for covered workers lagged 12 months. Interestingly, when we separate cities into those with business assistance laws, versus those with contractor-only laws—i.e. Model 3, presented in panel 3 of Table 9—we find that the living wage does not have a statistically significant effect on the employment status of our sample. It should be noted, however, that although neither the living wage variables for contractor-only nor business-assistance cities are statistically significant at conventional levels, the business assistance living wage variable is significant at a 15% threshold, and therefore could arguably be interpreted as supportive of Neumark's model with the reduced sample size.

It is at this stage that our ability to separate out covered from uncovered workers becomes important. Following our approach from the preceding section, in the bottom panel of Table 9 we adjust Neumark's classification of workers as potentially covered and uncovered to reflect the actual application of living wage laws by cities that have business assistance provisions. In other words, as with our approach with the wage models, we reclassify workers outside the service sector as uncovered, reflecting information on actual implementation of living wage laws provided to us by city officials. As seen in the fourth panel, when this adjustment is made, there is no statistically significant effect for covered workers, and none for uncovered workers either at conventional levels. Uncovered workers do display a negative and statistically significant coefficient for the living wage lagged 12 months if we consider the significance threshold to be 15% instead of 10%. Overall, we again find that Neumark's results are not robust to alternative specifications that are informed by the actual experiences with implementing living wage laws.

6. Concluding Remarks

This critical appraisal of David Neumark's 2002 study challenges his argument that living wage ordinances, particularly those covering business-assistance living wage ordinances,

increase wages for a far larger proportion of low-wage workers than has been previously estimated. We also challenge Neumark's finding that living wage ordinances cause disemployment.

On methodological grounds we have demonstrated why the Current Population Survey is not an appropriate dataset for analyzing either the wage or employment effects of living wage laws. We have also argued, on methodological grounds alone, that Neumark's truncated OLS regressions are vulnerable to problems of sample selection bias. The results we obtained through our quantile regression specification certainly, at the very least, raises concerns about the robustness of Neumark's results using his truncated OLS methodology. But even accepting Neumark's truncated OLS methodology on its own terms, we demonstrate that his statistically significant results hinge on the way in which he treats workers earning below the minimum wage, as well as how he defines covered workers in business-assistance living wage cities. We show that through classifying sub-minimum wage workers as uncovered, as opposed to Neumark's category "potentially covered," Neumark's statistically significant wage results are invalidated for "potentially covered" workers. Moreover, based on interviews with city staff in the cities in question, we have demonstrated that Neumark's approach to defining "potentially covered" workers in cities with business assistance ordinances drastically overstates the number of workers likely to have received mandated wage increases through existing living wage laws. We have also shown that once Neumark's definition of "potentially covered" workers is corrected for this misclassification, we find no statistically significant effect of living wage ordinances on wages or employment for covered workers. Finally, we have shown that Neumark's original results rest on a sample weighted heavily toward just sub-minimum wage workers in Los Angeles. When sub-minimum wage workers in Los Angeles alone are reclassified as uncovered, Neumark's putative "living wage effects" are again invalidated.

The overall conclusion that we reach is clear: David Neumark's analysis of the effects of living wage laws in the United States has produced no results that stand up to the scrutiny of this

critical replication exercise. Of course, our results do not speak to the broader substantive question as to how living wage laws have affected low-wage workers in terms of either their wages or their employment opportunities. But we expect that many researchers will continue to make progress in addressing these substantive questions, which are, of course, the central matters of concern for understanding how living wage laws are affecting the lives of low-wage workers in the United States.

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Table 1A: Effects of Living Wage Laws on Wages,
Alternative Classifications of Workers and Living Wage Laws
Replicating results from Neumark 2002

Model 1: All Living Wage Laws

Centile range:	<=10	10-25	25-50	50-75
Specification 1: Living Wage	-0.53 (2.23)	0.27 (1.62)	0.95 (1.65)	-0.03 (1.63)
Specification 2: Living wage, 6-month lag	1.91 (2.25)	0.84 (1.70)	2.22 (1.76)	0.34 (1.79)
Specification 3: Living wage, 12-month lag	6.95** (2.40)	0.93 (1.78)	-0.01 (1.85)	-1.08 (1.92)
Unweighted Ns:	34,435	42,912	71,135	72,737

Model 2: Potentially Covered and Uncovered Workers

	Uncovered Workers	Covered Workers	Uncovered Workers	Covered Workers
Centile range:	<=10		10-25	
Specification 1: Living Wage	-4.99* (2.97)	2.11 (2.53)	-1.02 (1.82)	1.23 (1.78)
Specification 2: Living wage, 6-month lag	-4.62 (3.07)	5.66** (2.56)	-1.09 (1.92)	1.98 (1.89)
Specification 3: Living wage, 12-month lag	0.61 (3.49)	10.61** (2.72)	-1.28 (2.07)	2.26 (2.00)
Unweighted Ns:	34,196		42,638	

Model 3: Contractor and Business Assistance Living Wage Laws

	Contractor Only Living Wage	Business Assistance Living Wage	Contractor Only Living Wage	Business Assistance Living Wage
Centile range:	<=10		10-25	
Specification 1: Living Wage	-4.50 (3.45)	1.78 (2.76)	-2.82 (2.30)	2.15 (2.09)
Specification 2: Living wage, 6-month lag	-5.22 (3.66)	5.83** (2.66)	-2.21 (2.38)	2.64 (2.21)
Specification 3: Living wage, 12-month lag	0.50 (4.02)	10.54** (2.78)	-1.92 (2.49)	2.72 (2.31)
Unweighted Ns:	34,435		42,912	

Notes: These results are identical to those in Neumark (2002), Tables 5.3, 5.5, and 5.6. The dependent variable is log wages. Estimated coefficients have been multiplied by 100. Standard errors reported in parentheses are robust to heteroskedasticity and non-independence within city-month cells. A city's observations are included in the sample in a particular month and year if it has at least 25 observations for that particular month and year. See text for other controls included in the regression. * indicates statistical significance at the .10 level. ** indicates statistical significance at the .05 level.

Table 1B: Effects of Living Wage Laws on Wages,
Potentially Covered and Uncovered Workers Under Business Assistance and Contractor Living Wage Laws
An Extension of Models 2 and 3

	Model 4			
	Contractor Living Wage Laws		Business Assistance Living Wage Laws	
	Uncovered Workers	Covered Workers	Uncovered Workers	Covered Workers
Centile range:	<=10			
Specification 2: Living wage, 6-month lag	-6.18* (3.67)	0.21 (6.53)	-0.24 (5.91)	6.52** (2.72)
Specification 3: Living wage, 12-month lag	-0.52 (4.11)	6.46 (7.26)	4.72 (6.05)	11.21** (2.88)
Unweighted N:	34,196			

Notes: See notes to Table 1A. * indicates statistical significance at the .10 level. ** indicates statistical significance at the .05 level.

Table 2A: Effects of Living Wage Laws on Employment, Alternative Classifications of Living Wage Laws Replicating results from Neumark 2002

Model 1: All Living Wage Laws

Centile range:	<=10	10-25	25-50	50-75
Specification 1: Living Wage	-1.77 (2.14)	0.02 (1.81)	2.58** (1.18)	1.79* (1.04)
Specification 2: Living wage, 6-month lag	-3.22 (2.26)	1.16 (1.88)	2.31* (1.24)	1.32 (1.08)
Specification 3: Living wage, 12-month lag	-5.62** (2.45)	1.62 (2.02)	1.55 (1.31)	2.44** (1.16)
Unweighted Ns:	83,326	118,541	197,477	199,703

Model 3: Contractor and Business Assistance Living Wage Laws

	Contractor Only Living Wage	Business Assistance Living Wage	Contractor Only Living Wage	Business Assistance Living Wage
Centile range:	<=10		10-25	
Specification 1: Living Wage	-3.26 (3.19)	-0.81 (2.73)	0.59 (2.75)	-0.34 (2.26)
Specification 2: Living wage, 6-month lag	-5.49 (3.40)	-1.74 (2.90)	1.03 (2.85)	1.24 (2.36)
Specification 3: Living wage, 12-month lag	-5.26 (3.79)	-5.88* (3.06)	1.45 (3.06)	1.74 (2.54)
Unweighted Ns:	83,326		118,541	

Notes: These results are identical to those in Neumark (2002), Tables 6.1 and 6.2. The dependent variable is employment status. Estimated coefficients have been multiplied by 100. Standard errors reported in parentheses are robust to heteroskedasticity and non-independence within city-month cells. A city's observations are included in the sample in a particular month and year if it has at least 25 observations for that particular month and year. See text for other controls included in the regression. * indicates statistical significance at the .10 level. ** indicates statistical significance at the .05 level.

Table 2B: Effects of Living Wage Laws on Employment, Alternative Classifications of Living Wage Laws Sample Restricted to Cities in Wage Analysis

Model 1: All Living Wage Laws

Centile range:	<=10	10-25	25-50	50-75
Specification 1:	-1.06	-0.01	2.50**	1.75*
Living Wage	(2.15)	(1.84)	(1.19)	(1.05)
Specification 2:	-2.51	0.97	2.35*	1.44
Living wage, 6-month lag	(2.29)	(1.91)	(1.25)	(1.09)
Specification 3:	-4.80*	1.46	1.71	2.67**
Living wage, 12-month lag	(2.48)	(2.05)	(1.32)	(1.17)
Unweighted Ns:	78,779	112,555	187,605	189,442

Model 3: Contractor and Business Assistance Living Wage Laws

	Contractor Only Living Wage	Business Assistance Living Wage	Contractor Only Living Wage	Business Assistance Living Wage
Centile range:	<=10		10-25	
Specification 1:	-2.58	-0.12	0.56	-0.37
Living Wage	(3.23)	(2.74)	(2.83)	(2.27)
Specification 2:	-4.64	-1.19	0.69	1.14
Living wage, 6-month lag	(3.47)	(2.90)	(2.94)	(2.38)
Specification 3:	-3.99	-5.34*	1.34	1.55
Living wage, 12-month lag	(3.91)	(3.07)	(3.17)	(2.56)
Unweighted Ns:	78,779		112,555	

Notes: The dependent variable is employment status. Estimated coefficients have been multiplied by 100. Standard errors reported in parentheses are robust to heteroskedasticity and non-independence within city-month cells. A city's observations are included in the sample if it appeared in the wage analysis sample. See text for other controls included in the regression. * indicates statistical significance at the .10 level. ** indicates statistical significance at the .05 level.

Table. 3: Composition of Affected Workers in the Lowest Decile
12-Month Lagged Living Wage Specifications

Panel A: Wage Analysis

	Model 1 All Living Wage Laws		Model 2 Covered Workers		Model 3 Business Assistance Cities	
City	Unwtd. N	Wtd. %	Unwtd. N	Wtd. %	Unwtd. N	Wtd. %
Los Angeles	1,196	27.5	1,136	49.1	1,196	52.4
Minneapolis	369	10.4	344	18.2	369	19.8
Detroit	230	5.0	215	8.7	230	9.5
Oakland	106	4.2	94	7.0	106	7.9
San Antonio	79	2.6	73	4.5	79	4.9
San Jose	69	2.6	58	4.2	69	5.0
Hartford	9	0.3	7	0.4	9	0.5
Chicago	476	10.4	44	1.9	--	--
Baltimore	325	12.6	23	1.7	--	--
Milwaukee	250	8.0	25	1.6	--	--
Durham	96	2.3	31	1.4	--	--
Boston	157	3.5	20	0.8	--	--
Portland	360	8.4	13	0.6	--	--
Dayton	95	2.1	1	0.0	--	--
Jersey City	15	0.3	1	0.0	--	--
Total	3,832	100	2,085	100	2,058	100

Panel B: Employment Analysis

	Model 1 All Living Wage Laws		Model 3 Business Assistance Cities	
City	Unwtd. N	Wtd. %	Unwtd. N	Wtd. %
Los Angeles	2,348	26.3	2,348	53.4
Minneapolis	592	8.2	592	16.6
Detroit	472	5.1	472	10.4
Oakland	218	4.2	218	8.5
San Antonio	147	2.4	147	4.8
San Jose	119	2.3	119	4.7
Hartford	21	0.3	21	0.5
Madison	20	0.3	20	0.6
Tucson	17	0.2	17	0.4
Duluth	3	0.0	3	0.1
Baltimore	645	12.2	--	--
Boston	400	4.3	--	--
Chicago	1,109	12.2	--	--
Dayton	168	1.8	--	--
Jersey City	203	2.2	--	--
Milwaukee	434	6.7	--	--
New Haven	109	1.4	--	--
Portland	617	7.4	--	--
Durham	216	2.6	--	--
Total	7,858	100	3,957	100

Notes: In Model 1, affected workers are those workers who live in a city with a living wage ordinance in effect (lagged 12 months). In Model 2, affected workers are those workers who live in a city with a living wage ordinance in effect (lagged 12 months) and are classified as "potentially covered" according to their industry affiliation. In Model 3, affected workers are those workers who live in a city with a business-assistance type living wage ordinance in effect (lagged 12 months).

Table 4: Estimating the Effects of Living Wage Laws on Wages Replicating Neumark's Models 1-3 Utilizing Quantile Regression

Model 1: All Living Wage Laws		
Quantile Specified:	10th	
Living wage, 12-month lag	0.74	
	(1.35)	
N	291,743	

Model 2: All Living Wage Laws	Uncovered Workers	Covered Workers
Quantile Specified:	10th	
Living wage, 12-month lag	0.48	3.04*
	(2.43)	(1.60)
N	291,743	

Model 3: Contractor and Business Assistance Living Wage	Contractor Only Living Wage	Business Assistance Living Wage
Quantile Specified:	10th	
Living wage, 12-month lag	-0.15	1.09
	(2.70)	(1.21)
N	291,743	

Notes: These results are estimated using quantile regression for the 10th centile of the wage distribution. Standard errors are bootstrapped to address heteroskedasticity. * indicates statistical significance at the .10 level. ** indicates statistical significance at the .05 level.

Table 5: Effects of Living Wage Laws on Wages, Alternative Coverage Classification

Model 2: Potentially Covered and Uncovered Workers

	Uncovered Workers	Covered Workers
Centile range	<=10	
Specification 2: Living wage, 6-month lag	0.21 (2.49)	-4.21 (4.16)
Specification 3: Living wage, 12-month lag	5.69** (2.68)	0.36 (4.91)
Unweighted N	34,196	

Model 4: Potentially Covered and Uncovered Workers Under Business Assistance and Contractor Living Wage Laws

	Contractor Living Wage Laws		Business Assistance Living Wage Laws	
	Uncovered Workers	Covered Workers	Uncovered Workers	Covered Workers
Centile range	<=10			
Specification 2: Living wage, 6-month lag	-6.54* (3.67)	-0.60 (6.52)	6.23** (3.18)	-6.48 (5.07)
Specification 3: Living wage, 12-month lag	-1.00 (4.11)	5.75 (7.23)	12.11** (3.19)	-2.70 (6.15)
Unweighted N	34,196			

Notes: See notes to Table 1A. Coverage definition is based on the actual implementation of living wage ordinances (see text for full description). * indicates statistical significance at the .10 level. ** indicates statistical significance at the .05 level.

Table 6: Effects of Living Wage Laws on Wages,
Subminimum Wage Workers Classified as Uncovered

Model 2: Potentially Covered and Uncovered Workers

	Uncovered Workers	Covered Workers
Centile range	<=10	
Specification 2:	12.91**	0.21
Living wage, 6-month lag	(2.75)	(2.21)
Specification 3:	11.85**	0.86
Living wage, 12-month lag	(3.07)	(2.40)
Unweighted N:	34,196	

Model 4: Potentially Covered and Uncovered Workers Under Business Assistance and Contractor Living Wage Laws

	Contractor Living Wage Laws		Business Assistance Living Wage Laws	
	Uncovered Workers	Covered Workers	Uncovered Workers	Covered Workers
Centile range	<=10			
Specification 2:	-0.56	5.74	30.27**	1.89
Living wage, 6-month lag	(2.46)	(3.88)	(4.83)	(2.30)
Specification 3:	-0.57	6.71	30.22**	2.07
Living wage, 12-month lag	(2.65)	(4.38)	(5.58)	(2.51)
Unweighted N:	34,196			

Notes: See notes to Table 1A. Subminimum wage workers who were previously classified as covered are classified as uncovered here, along with workers classified as uncovered based on the industry affiliation. * indicates statistical significance at the .10 level. ** indicates statistical significance at the .05 level.

Table 7: Effects of Living Wage Laws on Wages,
Los Angeles Subminimum Wage Workers Classified as Uncovered

Model 2: Potentially Covered and Uncovered Workers

	Uncovered Workers	Covered Workers
Centile range	<=10	
Specification 2: Living wage, 6-month lag	16.54**	-0.43
	(2.99)	(2.15)
Specification 3: Living wage, 12-month lag	15.03**	-0.22
	(3.23)	(2.38)
Unweighted N	34,196	

Model 4: Potentially Covered and Uncovered Workers Under Business Assistance and Contractor Living Wage Laws

	Contractor Living Wage Laws		Business Assistance Living Wage Laws	
	Uncovered Workers	Covered Workers	Uncovered Workers	Covered Workers
Centile range	<=10			
Specification 2: Living wage, 6-month lag	-0.13	-0.14	49.76**	2.43
	(2.43)	(4.71)	(5.36)	(2.19)
Specification 3: Living wage, 12-month lag	-0.37	2.36	46.88**	1.76
	(2.64)	(5.28)	(5.34)	(2.46)
Unweighted N	34,196			

Notes: See notes to Table 1A. Los Angeles subminimum wage workers who were previously classified as covered are classified as uncovered here, along with workers classified as uncovered based on the industry affiliation. * indicates statistical significance at the .10 level. ** indicates statistical significance at the .05 level.

Figure 1 - Unemployment Rate in California and Los Angeles: 1995-2000
(Not Seasonally Adjusted)

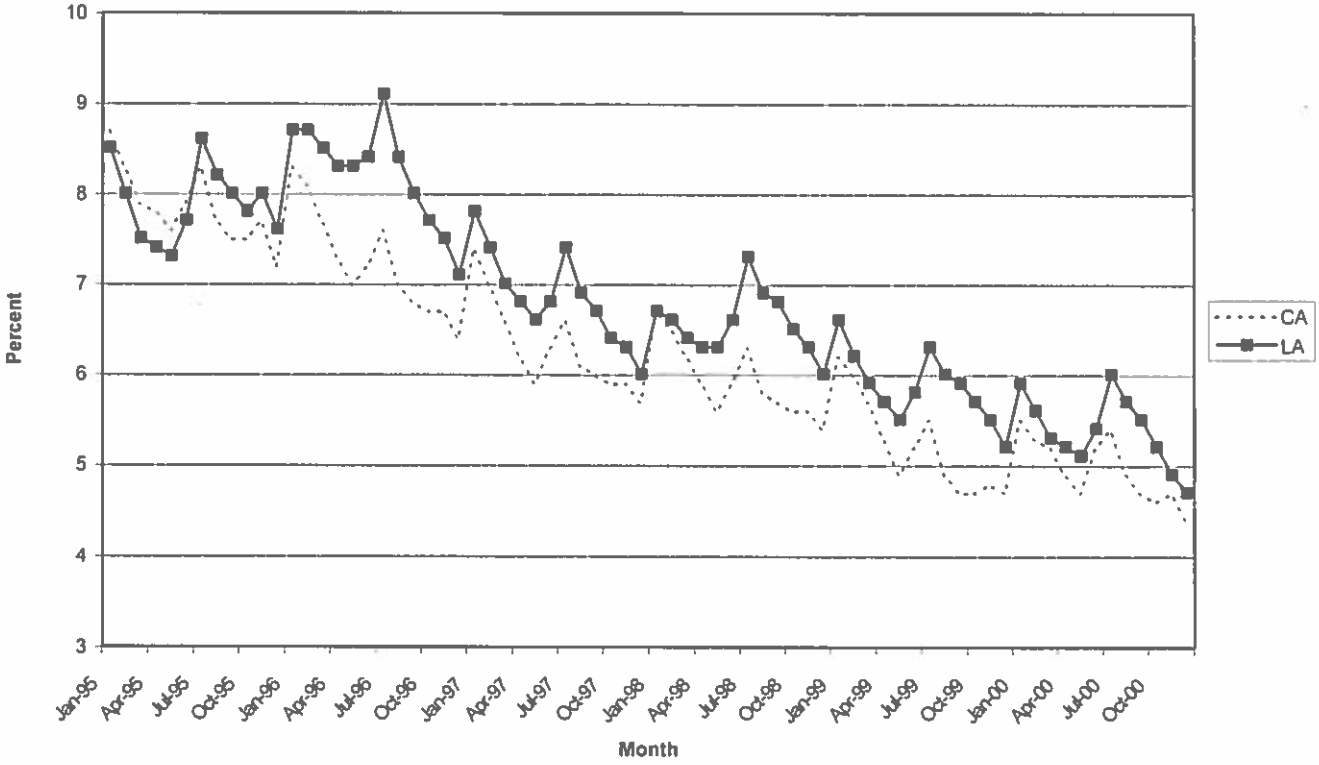
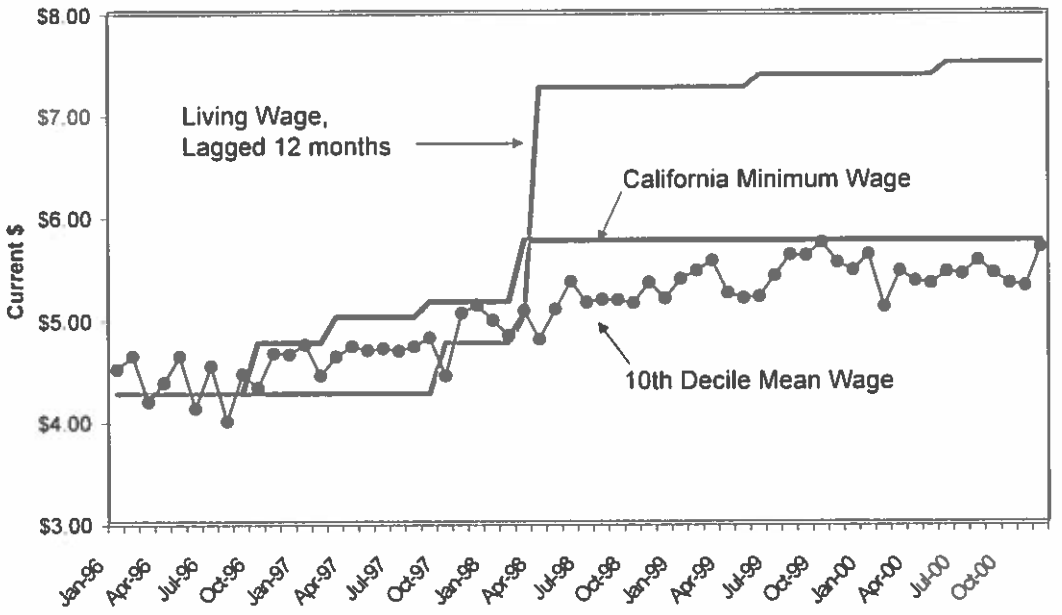


Figure 2. Los Angeles Wage Trends, 1996-2000

Panel A - Comparing Minimum and Living wages to 10th Decile Average Wages.



Panel B - Comparing Minimum and Living wages to 10th Decile Maximum Wages

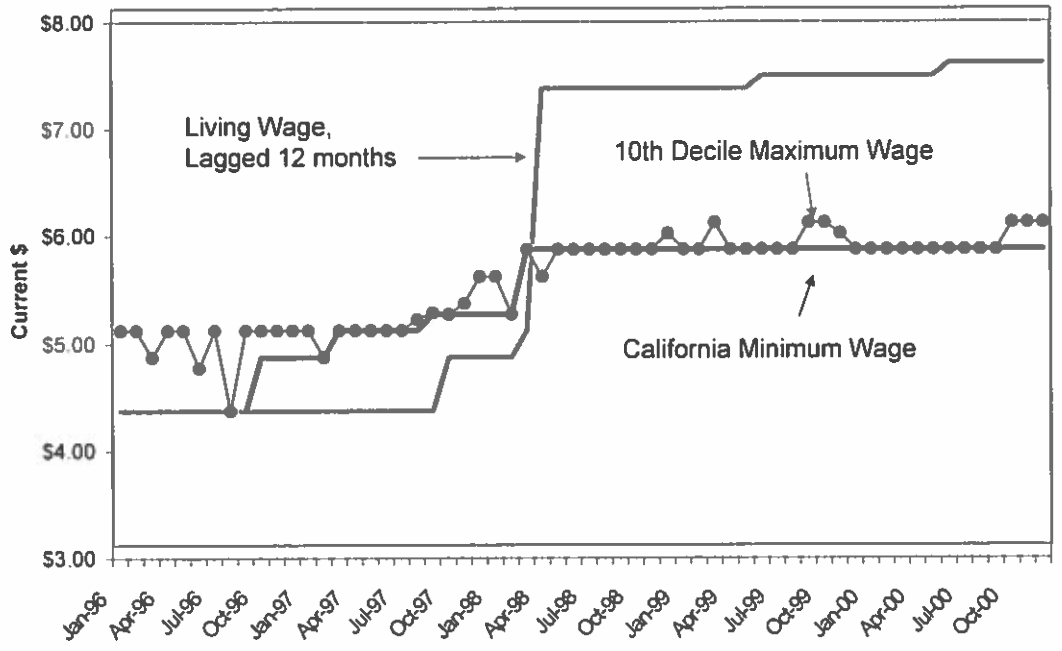


Table 8: Explaining Wages for Subminimum Wage Workers in Los Angeles

Model 1	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Centile range	<=10	<=10	<=10	<=10	<=10	<=10	<=10
Unemployment Rate (7 month moving average, centered)	-	-22.01** (7.45)	-	-	-20.69** (7.02)	-	-
Unemployment Rate, lagged 6 months (7 month moving average, centered)	-	-	-10.06** (3.64)	-	-	-10.26** (3.35)	-
Unemployment Rate, lagged 12 months (7 month moving average, centered)	-	-	-	-3.23 (5.95)	-	-	-5.84 (5.83)
Minimum Wage, current	-	-	-	-	12.49 (35.15)	69.82* (37.21)	76.16** (36.17)
Minimum Wage, lagged 12 months	-14.9 (38.65)	-43.49 (36.52)	-53.91 (39.17)	-19.33 (38.33)	-39.66 (38.69)	-42.84 (38.83)	-10.02 (36.40)
Living Wage, lagged 12 months	1.03 (8.09)	23.71** (10.44)	4.35 (7.12)	-2.48 (11.08)	20.53* (11.74)	-5.73 (9.14)	-16.38 (12.69)
Unweighted N	845	845	845	845	845	845	845

Notes: See notes to Table 1A.
* indicates statistical significance at the .10 level. ** indicates statistical significance at the .05 level

Table 9: Effects of Living Wage Laws on Employment, Alternative Samples and Classifications

Model 1, New Sample [only those in the labor force or NILF and worked last year]

Centile range	<=10
Specification 1:	
Living wage	-0.65
	(2.50)
Specification 2:	
Living wage, 6-month lag	-1.58
	(2.55)
Specification 3:	
Living wage, 12-month lag	-5.38**
	(2.74)
Unweighted N:	47,759

Model 2: Potentially Covered and Uncovered Workers, Neumark's Classification - New Sample [only those in the labor force or NILF and worked last year]

Centile range	Uncovered Workers	Covered Workers
	<=10	
Specification 1:		
Living wage	-1.06	-1.35
	(3.28)	(2.96)
Specification 2:		
Living wage, 6-month lag	-2.26	-3.16
	(3.40)	(3.21)
Specification 3:		
Living wage, 12-month lag	-5.70	-6.65*
	(3.83)	(3.55)
Unweighted N:	46,133	

Model 3, New Sample [only those in the labor force or NILF and worked last year]

Centile range	Contractor Only	Business Assistance
	<=10	
Specification 1:		
Living wage	-2.42	0.49
	(3.58)	(3.26)
Specification 2:		
Living wage, 6-month lag	-2.93	-0.70
	(3.65)	(3.35)
Specification 3:		
Living wage, 12-month lag	-4.85	-5.74
	(3.97)	(3.59)
Unweighted N:	47,759	

Model 2 - Corrected Classification, New Sample [only those in the labor force or or NILF and worked in last year]

Centile range	Uncovered Workers	Covered Workers
	<=10	
Specification 1:		
Living wage	-0.47	-1.45
	(2.62)	(4.71)
Specification 2:		
Living wage, 6-month lag	-2.00	-2.04
	(2.77)	(5.27)
Specification 3:		
Living wage, 12-month lag	-4.69	-6.55
	(3.04)	(6.12)
Unweighted N:	46,133	

Notes: The dependent variable is employment status. Estimated coefficients have been multiplied by 100. Standard errors reported in parentheses are robust to heteroskedasticity and non-independence within city-month cells. A city's observations are included in the sample if it appeared in the wage analysis sample. See text for other controls included in the regression. * indicates statistical significance at the .10 level. ** indicates statistical significance at the .05 level.

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Evaluating Living Wage Laws in the United States: Good Intentions and Economic Reality in Conflict?

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Evaluating Living Wage Laws in the United States: Good Intentions and Economic Reality in Conflict?

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ABSTRACT: This paper first examines the question "what is a living wage" and provides a range of specific dollar amounts derived a conceptual assessment of the term. I then provide a series of cost estimates of living wage laws in various cities. Based on these cost estimates, I examine a set of alternative adjustments that covered firms could make to absorb these costs, including raising prices and productivity, redistributing the firm's income more equally, laying off employees and relocating out of the area covered by the law. I draw upon both prospective and retrospective evidence to reach an overall assessment of the benefits of living wage laws relative to their costs.

JEL CLASSIFICATION: J38

This paper builds from a series of research projects I have undertaken in collaboration with co-workers at the Political Economy Research Institute. I acknowledge with respect and appreciation the contributions of Jeanette Wicks-Lim, Stephanie Luce, and Mark Brenner. I am also grateful for the comments by three anonymous referees and Timothy Bartik, the co-editor of this journal.

"In Economics, "other things" are so often not equal that greater proportionate stress ought to be laid on the necessity of examining each case to see whether the weights are important or not."

-- Alfred Marshall letter to Bowley, February 21, 1901, Vol 2, of Collected Correspondence p. 301

As of October 2002, living wage proposals have passed into law in about 90 municipalities in the United States since the first ordinance was approved by the Baltimore City Council in 1995. But this is not the first living wage movement in the U.S. Indeed the initial establishment of minimum wage laws in the U.S.—first at the state level beginning with Massachusetts in 1912 then moving to the Federal level through various measures between 1933-36—was itself the culmination of an explicit "living wage" movement. One of the most influential works supporting the movement was a 1906 book by Monsignor John A. Ryan titled *A Living Wage: Its Ethical and Economic* Aspects. By the mid-1930s, President Franklin D. Roosevelt made his position on the issue clear, stating that "no business which depends for existence on paying less than living wages to its workers has any right to exist in this country."[1]

The contemporary living wage movement spread throughout the country from its inception point in Baltimore as a response to the declining fortunes of low-wage workers and, more generally, the sharply rising trend in wage and income inequality in the U.S. economy. Thus, the real value of the national minimum wage as of 2001, at $5.15 per hour, was 37 percent below its peak value in 1968 of $8.14 (in constant 2001 dollars), even while average labor productivity rose in the U.S. by roughly 80 percent between 1968 – 2001. This means that if the real value of the national minimum wage had risen exactly in step with the rate of productivity growth, the minimum wage as of 2001 would be $14.65. More to the point, someone who works full-time for 52 weeks at the $5.15 national minimum would earn $10,712 over a year. This figure is 12.2 percent below the 2001 national poverty threshold for a family of two (1 adult, 1

[1] Glickman (1997) is a book length history of living wage movements in the United States during the 20th century. The quote from Roosevelt is cited in Stabile (1993), p. 13, which in turn cites Barkin (1937), p. 7, as the original reference.

child) and a broad range of researchers consider such official poverty thresholds themselves to be between 25 and 50 percent too low.[2]

Similarly, the real value of the average wage for non-supervisory workers stood in 2001 at $14.31, which is 8.9 percent below its peak value in 1973 of $15.71 (in constant 2001 dollars), even after the average real wage rose significantly between 1997-99 and continued rising, though more modestly, in 2000 and 2001. Moreover, these declines in the absolute values of the minimum and average wage have occurred while earnings for high-wage workers rose substantially and compensation for corporate CEOs exploded. Thus, the wage for workers at the 10th wage decile fell from 28 to 23 percent of workers at the 90th wage decile between 1976 – 2001. Meanwhile, average CEO compensation rose from 38 to *449 times* that of an average non-supervisory production worker between 1970 – 2001.[3]

Despite these trends, opponents of living wage ordinances argue that these measures will not benefit, but will actually hurt, the very low-wage workers and their families that the movement is trying to assist. In other words, according to opponents, the living wage movement is a classic case of the "law of unintended consequences" as it operates in economics—that is, well-meaning people ending up doing harm while seeking to do good, through their misapprehension as to how economic policy interventions play themselves out in actual market settings. Opponents point to three major unintended consequences of living wage ordinances:

1) They will cause a contraction of employment opportunities for low-wage workers and/or a displacement of currently employed workers by those possessing higher skills.
2) They will induce firms located in cities with living wage ordinances to relocate out of these areas, as a means of avoiding being covered by the mandates of the law; and
3) They will create significant new costs for municipal governments. This will mean that governments will have fewer resources to support their existing measures aimed at reducing poverty and promoting increased job opportunities.

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[2] Citro and Michael (1995) is the most extensive contemporary examination of the poverty thresholds, a topic to which we return below.

[3] Original sources for data in this paragraph are presented in Pollin (2003).

In the rest of this paper, I address these basic issues concerning living wage laws, drawing on the existing research I have produced with colleagues along with the work by others in the field.

In Section 2, I examine the initial fundamental matter of concern, posing the question, "what is a living wage" in the context of the contemporary U.S. economy. As we will see, the answer is not obvious, even at a conceptual level. But it is even more challenging to move from a conceptual consideration of the issue to establishing quantitative benchmarks that define living wages in terms of specific hourly wage rates. Drawing on earlier research, I provide a range of values for a living wage in the Los Angeles area. This set of figures reflects both different notions of the term "living wage" as well as an accounting for regional variation in the cost of living.

But establishing a set of figures for living wages does not provide information as to whether establishing a minimum wage mandate at any of the threshold figures would trigger significant negative unintended consequences. To begin to address this question, in Section 3 I provide summary figures on how much ordinances are likely to cost. I draw here on estimates my co-authors and I have derived for three different living wage ordinances—that which has been in operation in Los Angeles since 1997, as well as measures that have been considered in both New Orleans, LA and Santa Monica, CA. The key figure in these exercises is our estimate of the full costs to covered firms of a given living wage ordinance relative to some relevant measure—either total costs of production or total sales—of the firms' total operations. As we will see, in all three cases, we find that for the average firms covered by living wage ordinances, the costs of the ordinance range between 1 – 2 percent or less of the firm's total production costs or sales. But we also report on the situation for firms that experience significantly higher cost increases, such as the hotels and restaurants in the Santa Monica case, which would face cost increases on the order of 10 percent of total sales.

In Section 4, I then consider how firms faced with cost increases of the magnitudes we estimate are likely to respond. Layoffs and relocatons are two possible adjustment paths and, of course, both would entail negative intended consequences. But these are not the only possible ways through which firms could adjust to their living wage-induced cost increases. Depending on their cost structures and production processes, firms could also absorb the increased costs through three other means: 1) raising prices; 2) raising productivity; or 3) redistributing income within the firm, either through wage compression or a fall in profit shares. The advantage for covered firms of these three other adjustment mechanisms relative to layoffs or relocations is that, within a reasonable range of small adjustments, firms could implement them more quickly and at lower cost to themselves than either layoffs or relocations.

In terms of costs to municipalities, it similarly does not follow that the higher wage costs would necessarily be passed back to government budgets dollar-for-dollar via more expensive service contracts. It would be reasonable to consider that at least some portion of the higher wage costs might be absorbed by private firms competing for municipal contracts. Again, this is most likely to be true when the costs of a living wage ordinance to private firms are relatively small.

Thus, I evaluate in Section 4 the extent to which firms are likely to pursue the various possible adjustment options they face. I consider the issue first in terms of the evidence I report on prospective costs, what I will term *prospective* evidence. I then consider whether the conclusions I draw based on prospective evidence are consistent with *retrospective* analysis of the experience with implementing living wage ordinances in four cities—Baltimore, Boston, New Haven, CT and Hartford CT. The specific retrospective evidence on which I focus is how awards for city service contracts have changed after living wage ordinances were implemented in these four cities.

The living wage ordinances in these four cities for which we have retrospective evidence, and indeed all of the living wage ordinances that have become law thus far, are designed in similar ways. They are what I term "contractors-only" ordinances because they apply only to

firms either holding service contracts with, or receiving subsidies from, city governments. At the same time, various cities, including New Orleans and Santa Monica, have considered ordinances which apply to all businesses of a given size within a given geographic region, what I term "area-wide" measures. In Section 5, I therefore examine the extent to which our conclusions as regards contractors-only ordinances would also apply to area-wide ordinances. In fact, I argue that there are distinct adjustment issues with area-wide ordinances, specifically around the question of price increases and firm relocations.

In Section 6, I examine the possibilities for labor substitution as a final distinct adjustment issue—that is, the extent to which firms might replace their existing minimum wage employees with workers having better skills and/or credentials. Because the covered firms would tend to pay higher than uncovered firms for comparable job types, openings for the jobs with covered firms would likely attract workers with better credentials. This could occur even if firms did not reduce at all the overall number of workers they employ. To the extent that substitution occurs, it would mean that the low-wage workers employed by firms prior to passage of a living wage ordinance—presumably the intended beneficiaries of the ordinance—would rather, in the end, experience job losses. The discussion in this section attempts to gauge the likely magnitude of this substitution effect. I argue that it is likely to occur, though only to a modest extent.

In the paper's concluding Section 7, I briefly pull together the various findings to provide an overall assessment of the likely benefits and costs of living wage ordinances. Of course, the main issue at this point is not the conclusion itself, but rather how the conclusion was derived. The research approach that I summarize in this paper has been to follow the dictum articulated by Alfred Marshall in the statement that serves as this paper's epigram. That is, my co-authors and I have been careful to recognize the range of possible intended and unintended effects of living wage ordinances. But the task of the researcher is not simply to recognize possibilities, but to examine each case, as Marshall says, "to see whether the weights are important or not." My general conclusion with regard to the ordinances I consider here is that their negative effects,

while present, have not been large enough so as to outweigh the benefits the low-wage workers and their families would obtain through receiving the wage increases stipulated by the ordinances. But it should also be clear from the approach I take that I could not reach this same conclusion *a priori* for other living wage ordinances. Following Marshall again, the key issue will always be determining in any given situation the relative weights of the possible intended and unintended effects.

Before proceeding, I should also be clear that this paper does not attempt to be comprehensive in its scope. Among other important questions, it does not examine the actual family conditions for workers who would be eligible for living wage increases, nor does it consider alternative policy measures, such as the Earned Income Tax Credit, for raising living standards of low-wage workers. It also does not discuss possible positive multiplier effects from living wage laws—i.e. the benefits of higher household incomes on consumer spending and business sales, and the reduced burdens on governments for spending on poverty alleviation. In general, positive multiplier effects are present, but are small, primarily because the scope of all living ordinances that have been implemented to date are themselves small. However, I have addressed all of these questions in other work (e.g. Pollin and Luce 2000, Pollin 2002, Pollin, Brenner, and Luce 2002) and will continue to examine them in the future.

2. What is a Living Wage?

The living wage initiatives that have become law throughout the country are motivated by an initial common initial premise: that people who work for a living should not have to raise a family in poverty. But the term living wage also suggests a more ambitious standard. In *A Living Wage: American Workers and the Making of a Consumer Society* (1997), Lawrence Glickman writes that in the historical development of the living wage movement, supporters used the "living wage" concept to define a wage level that offers workers "the ability to support families, to maintain self-respect, and to have both the means and the leisure to participate in the civic life of the nation, (p.66)."

This Glickman definition of a living wage bears a close correspondence with the ideas of Amartya Sen on defining poverty relative to the achievement of what he calls "capabilities." These capabilities include such things as the ability to read and write, to lead a long and healthy life, to have freedom of movement, and to participate meaningfully in the civic life of the community. But how does one measure the ability to participate in community life? Sen acknowledges the difficulties with this issue, especially when one considers the question according to the level of general affluence of the community in which a person lives. As Sen writes,

> The need to take part in the life of a community may induce demands for modern equipment (televisions, videocassette recorders, automobiles and so on) in a country where such facilities are more or less universal (unlike what would be needed in less affluent countries), and this imposes a strain on the relatively poor person in a rich country even when that person is at a much higher level of income compared with people in less opulent countries. Indeed, the paradoxical phenomenon of hunger in rich countries—even in the United States—has something to do with the competing demands of these expenses (1999, pp 89-90).

Quantifying the concepts

Regardless of whether we define the term living wage narrowly, as adequate to provide a poverty-line living standard, or more generously, in line with both the historical meaning of the term and Sen's conception of attaining adequate capabilities, we still face problems in translating these concepts into concrete monetary amounts. What are the proper dollar values that we should assign to a "poverty-level" living standard or to a higher, but still relatively modest standard that would enable a person to participate meaningfully in one's community life? A considerable amount of value judgment inevitably goes into establishing such standards. Moreover, the cost of living varies considerably between regions and communities in the U.S. Adjustments should therefore be made in light of such cost differences in determining appropriate living-standard thresholds. The only way a researcher can handle these issues is to be explicit in establishing meaningful benchmarks. In what follows, I describe the procedures my colleagues and I used in establishing an appropriate set of "poverty" and "basic needs" thresholds as they applied to

workers in the Los Angeles area, through which we could then derive a range of hourly wage rates consonant with the idea of a living wage.

First, in terms of measuring "poverty-line" living standards, the U.S. Census Bureau, of course, has been producing such measures since 1963. But a broad range of researchers argue that the government's methodology—which has not been significantly altered since its introduction in 1963—is no longer adequate. We therefore attempted to develop some viable guidelines for establishing poverty thresholds for our purposes, drawing both from the Census Bureau estimates and the recent professional literature focused on developing improved methodologies.

In terms of measuring a "basic needs" living standard, the California Budget Project (CBP) in Sacramento has done careful research in estimating this. The CBP divided the State of California into 8 regions, of which Los Angeles is one (that with the largest population). The CBP then attempted to measure a "basic family budget" derived from observed costs of housing, food, health care, child care, transportation, clothing, basic telephone service, and a few other essentials. Unlike the Census Bureau's poverty thresholds, the standard of living that the CBP is attempting to measure is, as they explain, "more than a 'bare bones' existence, yet covers only basic expenses, allowing little room for 'extras' such as college savings or vacations," (CBP 2001, p. 1). The CBP estimate therefore can serve as a good reference point in defining a more generous "basic needs" living wage for workers in the Los Angeles Area.[4]

Measuring Poverty Thresholds

In 1963, the U.S. Census Bureau developed a methodology for establishing detailed poverty thresholds for families of different sizes that remains in force at present. The government's approach began by defining what it termed an "economy food plan"—which is the lowest-cost bundle of food items that could ensure that each family member receives the basic

[4] Various researchers have conducted similar studies for different regions throughout the country. One reference which includes a large number of such community-specific basic needs budgets is Boushey, Brocht, Gunderson and Bernstein (2001).

caloric minimum. This figure would obviously need to be adjusted according to size of families. The government's methodology then assumed that poor families spend approximately one-third of their budget on food. This figure was derived from government surveys in 1963 of the spending patterns of low-income households. Thus, to generate the dollar figures for the poverty thresholds for 1963, the government simply multiplied the dollar value of the "economy food plan" by three. The poverty thresholds that were obtained by this method in 1963 are then updated annually, by increasing the dollar values of the threshold in accordance with changes in the Consumer Price Index. Using this methodology, the official poverty threshold in 2001 for a family of two was $11,569 and for a family of four with two children was $17,960.

In recent years, many researchers have questioned the adequacy of this method for establishing poverty thresholds. The most extensive survey of these issues was that sponsored by the National Research Council (hereafter NRC; Citro and Michael 1995). According to the NRC study, establishing overall poverty thresholds on the basis of food costs alone presents many problems. For one thing, there are large variations in housing and medical care costs by region and population groups. In addition, food costs have fallen relative to those for housing for low-income households since the time the government surveys of spending patterns were conducted forty years ago. The increasing relative costs of childcare over the past 40 years are also not accurately reflected in the government's methodology. This has become increasingly important over time, as labor force participation by mothers has risen.

The NRC study reports on six alternative methodologies to the current official method for measuring absolute poverty for a two adult/two child family.[5] The thresholds generated by these

[5] The NRC study includes consideration of "relative" as well as "absolute" measures of poverty. Relative poverty, as the term suggests, takes account of problems resulting from pronounced inequality in a society, even if that society's average living standard is relatively high; that is, a specific concern addressed with Sen's concept of "capabilities" poverty. However, we focus here only on absolute poverty measures. For an insightful overview on these themes as well as current poverty trends throughout the world, see Keith Griffin, "Problems of Poverty and Marginalization," (2003).

alternative methodologies [6]are all higher than the official threshold, ranging between 23.7 and 53.2 percent above the official threshold. The average value of these alternative estimates is 41.7 percent higher than the official threshold. This standard for an alternative absolute poverty threshold will help establish our benchmark for a low-end living wage estimate.

Regional Living Costs

The alternative poverty thresholds reported by the NRC do not take account of regional differences in the cost of living. Considerable evidence suggests that living costs for low-wage workers in the LA area are significantly higher than those in other parts of the country. I consider two basic sources here, that of the American Chamber of Commerce Research Association (ACCRA) Cost of Living Index and the 2001 California Budget Project (CBP) figures.

Cost of Living Estimates

The ACCRA data set provides the most detailed statistics on costs of living in approximately 300 cities within the United States.[7] According to ACCRA, overall living costs in Los Angeles were 26.4 percent above the national average for 1999. Over the 1990s as a whole, this LA living cost differential averages 23.3 percent above the national average for the decade as a whole. From this, it seems reasonable to conclude that for low-wage workers as well as midmanagers in Los Angeles, living costs are approximately 25 percent above the national average.

LA Living Costs and Poverty Thresholds

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[6]

[7] At the same time, the ACCRA index has limitations for our purposes. The problem is that the ACCRA index is explicitly designed to measure relative living costs in different regions at what ACCRA describes as a "midmanagement standard of living." Our aim is to understand living costs for low-wage workers, which, obviously, will be in a different category than that for midmanagers. Thus, to make use of the ACCRA data, we first have to consider the extent to which differences in living costs at this "midmanagement" level reflect similar relative cost differences at a living standard appropriate to low-wage workers. In Pollin and Brenner (2000) we provide evidence that the ACCRA index is a reasonable standard for measuring relative costs-of-living for low-wage workers as well as midmanagers in various U.S. cities.

We are now in a position to establish a workable "poverty line" living wage standard for Los Angeles area workers. It follows from the two basic points that emerge from the material we have reviewed: 1) according to the average of the alternative measures of poverty reviewed by the National Research Council, the national poverty line for a family of four is about 40 percent above the official Census Bureau poverty line; and 2) the cost of living in the Los Angeles area is about 25 percent above the national average.

These two figures suggest that an appropriate poverty-line estimate for the Los Angeles area should be about 65 percent above the official Census Bureau poverty line. To present this result cautiously, let us round down, assuming that an appropriate poverty threshold for Los Angeles would be about 60 percent above the official poverty line. Thus, in establishing living wage figures and poverty estimates below, we utilize a "160 percent of official poverty" threshold as our basic measure. We will also report a "185 percent of official poverty" threshold to measure a "near poor" living standard. Along with these, we will also include the official poverty threshold figures, but will consider these as properly measuring a "severe poverty" standard.

Basic Needs Budget

As mentioned above, the California Budget Project attempts to measure a standard of living that is more than a "'bare bones' existence, yet covers only basic expenses, allowing little room for 'extras' such as college savings or vacations." The CBP estimates typical costs of housing and utilities, child care, transportation, food, health coverage, payroll and income taxes, and miscellaneous expenses such as clothing, personal care and basic telephone service. The study assumes that the typical family rents housing rather than owns a home, and that the rent they pay is at the lower 40th percentile of "fair market value" rents in the area—i.e. that 40 percent of the rental housing in an area is lower than the fair market value and 60 percent is higher. The family does own a car, but drives an average of only 25 miles per day for commuting. No allowance is made for vacation travel or long commutes. The food budget is based on the Department of Agriculture's "low-cost food plan" which is approximately 25

percent above its "economy food plan" used in measuring the official poverty threshold. The CBP assumes that a family includes two children, one below and the other above six years old. The study then estimates basic income budgets for three different family types: a single parent family; two parents, with one wage-earner and the other handling child-care; and both parents earning wages. For example, for a single parent family with two children, the study finds the yearly budget would include (in 2001 dollars) $9,384 for housing and utilities, $10,692 for childcare, $3,288 for transportation, $5,340 for food, $4,380 for health care, $4,092 for miscellaneous items, and $5,662 for taxes, for a total of $42,840.

Overall, the budget estimates generated by this approach correspond well to what we would consider a "basic needs" living standard, or something akin to a minimum amount needed to become capable, in the sense of Sen, of participating meaningfully in community life.

Alternative Estimates of Living Wage Standards

In Table 1, I present alternative estimates for both "poverty-line" and "basic needs" income levels for workers in the Los Angeles area. As we see, the figures are presented for both a three-person/two-child family and a four-person/two-child family. With the four-person/two-child family, the basic needs figures, derived from the CBP study, are presented in two ways, assuming both one and two wage-earnings in the family. The increased income needs for the two wage-earner family reflects the higher costs of childcare when both adult family-members are working full-time outside the home.

[TABLE 1 BELONGS HERE]

As we see from the table, the alternative "living wage" rates range fairly widely, according to what one defines as a living wage. Given our discussion above on the inadequacies of the official poverty thresholds, especially as a standard relevant for the Los Angeles area, it is reasonable to exclude these official threshold levels—what we are terming the "severe poverty" income thresholds—as a level that we could define as corresponding with a living wage. This still leaves wage rates between $10.98 and $20.60 as the range of values

associated with different living wage standards for a three-person family with one working adult. For a four-person family, the corresponding wage rate would be between $13.82 - $16.93 with one wage earner in the family. If both adults in a four-person family were working, the average wage for both would need to be $12.37 for the family to reach the basic needs threshold.

It is clear from these figures that no single dollar amount can be associated with a living wage threshold. Nevertheless, the figures in the table provide a sense of what an appropriate wage level would be, assuming that workers hold full-time jobs and that they are supporting between one and two additional family members on their wages.

In fact, it may be unrealistic to assume that low-wage workers hold full-time jobs over the course of a year. If they do not, their wage rate would clearly have to be higher to earn an income level corresponding with either a poverty-line or basic needs living standard. At the same time, it may not be the case that workers are trying to support additional family members on their wages, in which case a lower dollar amount would be adequate to supply a living wage.

Such additional considerations need to be weighed carefully in constructing an adequate threshold for a living wage (we have attempted to do so in Pollin and Brenner 2000). But this exercise itself strongly suggests that the California minimum wage, which was at $5.75 when we did our research and is now at $6.75, are both far below even the low-end estimate of a living wage for a worker living in the Los Angeles area. Indeed, even the current value for the Los Angeles living wage of $8.27 plus $1.25 for health benefits is still below the low-end range for what would constitute a reasonable living wage figure for Los Angeles.

3. Estimating Costs of Living Wage Ordinances

Regardless of what would constitute a "living wage" according to the standards reviewed above or some alternative set of standards, the fundamental question one must still ask regarding any living wage measure is whether its dominant effects will be negative unintended

consequences, including job losses, firm relocation, and excessive strains on municipal budgets. Most of the research on living wage proposals, including that conducted by my colleagues and me, has therefore focused on estimating the costs of such measures and evaluating how these costs will be absorbed by those who pay them.

The key consideration here is clearly how private firms covered by a living wage ordinance would adjust to their higher labor costs. My colleagues and I have undertaken such cost estimates with respect to the living wage proposals in Los Angeles (Pollin and Luce 2000), New Orleans (Pollin, Brenner and Luce 2002), and Santa Monica, CA (Pollin and Brenner 2000). We utilized varying methodologies and statistical sources depending on the resources available to us in each case. In Table 2, I summarize the findings from these various exercises.

Data sources. As we see first, the studies vary in terms of their statistical sources. In the intitial study for Los Angeles, we relied almost entirely on government statistical sources. With the New Orleans project, we did a large-scale survey of private firms within the city which was our primary data source. We then supplemented the data from the firm survey with government statistical data on employment levels and wage rates. For the Santa Monica study, we surveyed both firms and workers, and these surveys were the major data source for our cost estimates. But as with New Orleans, we also drew upon government statistics, primarily in determining employment levels and wage rates.

Wage and benefit increases. The proposals that we evaluated varied substantially in terms of their basic features. The ordinance for Los Angeles was the most comparable to the measures that have passed in other cities throughout the country. This measure applied only to business firms holding contracts with municipal governments. It stipulated a living wage minimum that was 71 percent above the operative minimum wage at the time, which, for Los Angeles, was the national minimum wage. The Los Angeles ordinance also included provisions for both health care and paid days off for those workers who were not receiving these benefits.

The New Orleans proposal was far less ambitious in terms of the proposed wage increase, stipulating a 19 percent increase over the national minimum. The New Orleans proposal did not include any provisions for benefits. Finally, the measure in Santa Monica, CA proposed an 87 percent increase over the then California minimum wage of $5.75. It also included provisions for health coverage and paid days off similar to those in the Los Angeles measure.

Cost Estimates. There are two components to total costs. The first is the mandated costs for firms of raising their employees to the new living wage minimum as well as the new benefit levels. This includes the pay raises as well as associated increases in payroll taxes. The second component is so-called "ripple effects," which refer to the *non-mandated* increases in wages and benefits above the living wage minimum that businesses provide to some of their workers after a living wage ordinance is implemented. Businesses provide these non-mandated raises to maintain some semblance of the wage hierarchy that prevailed prior to implementation of a new mandated minimum wage. But estimating ripple effects is necessarily more speculative than estimates for mandated raises, for precisely the reason that ripple effect raises are non-mandated.

As we see in Table 2, our estimates for total costs included a range of between 74 – 89 percent for mandated costs, and, correspondingly, 11 – 26 percent for ripple effects. In part, this difference in the relative magnitude of the ripple effect reflects variations in our approach to estimating ripple effects which we developed as our work has proceeded. But it also is a function of the differences in the ordinances themselves. Thus, with the Santa Monica ordinance, because the mandated increases rose higher in the overall pay structure, we found that there would be fewer workers within the general wage range who would be likely candidates for non-mandated increases.

Total Costs Relative To Firms Overall Operations. The key findings of our studies is presented in the bottom row of Table 2, which reports estimates of total expected costs of the three ordinances relative to some measure of firms' overall operations. These measures of overall operations were total production costs in Los Angeles, total operating costs (production costs

minus capital costs) in New Orleans, and total sales in Santa Monica.[8] As we can see, despite the differences in the ordinances and the methodologies used for estimating costs, there are broad commonalities in our figures for living wage costs relative to the measures we used of firms' total operations. In our Los Angeles study, the average living wage cost estimate was 1.5 percent of total production costs. In New Orleans, the figure was 0.9 percent of operating costs. If the denominator in the New Orleans case would have been total production costs as opposed to operating costs, the living wage cost estimate would have likely been closer to 0.7 percent. Finally, we have the case of the Santa Monica ordinance. Here, I report three separate figures on total costs, because the cost impacts bifurcated dramatically among the total pool of covered firms. As we see there, our average cost figure was 3.9 percent for all firms. But this included a roughly 10 percent cost ratio for covered hotels and restaurants and a 2.2 percent ratio for all other covered firms.

Two basic features therefore emerge from these studies. First, regardless of the statistical sources and methodology we employed, as well as the specific scalar we used to measure overall operations of covered firms, the average cost to most firms of living wage ordinances are low, somewhere in the range of between 1 – 2 percent of firm's total operations. The figure falls even lower when, as with the New Orleans ordinance, the mandates of the ordinance are themselves more modest.

But as the figures from the Santa Monica measure also make clear, there will be firms for which the living wage cost increases will be substantially higher. In Santa Monica, there was a particularly high concentration of hotels and restaurants facing higher costs. This occurred, first, because the mandates of the ordinance in terms of both wages and benefits were high. But more importantly, these high overall cost estimates resulted because the Santa Monica ordinance was designed to target large firms operating in the city's tourist zone. This meant that a

[8] More precisely, the measure for Santa Monica was gross revenues, which is therefore inclusive of entities that don't make sales per se.

disproportionate number of covered firms were large hotels and restaurants, and these are the types of firms that employ a high proportion of low-wage workers.

4. Alternative Adjustments for Covered Firms

These cost estimates provide the basis for our analysis of the most likely adjustments covered firms will make in response to living wage ordinances. We can usefully divide the covered firms into two groups. What I term "low-impact" firms are the majority of firms in all of our studies, for which the cost increases they face in implementing living wage mandates is in the range of 1-2 percent or less of their total operations. "High-impact" firms are those for which the cost increases are enough above the 1-2 percent range that we might expect them to respond in significantly different ways to living wage-induced cost increases than the majority of firms. Certainly, the hotels and restaurants in Santa Monica, which face cost increases on the order of 10 percent of total sales, would be high-impact firms. In our New Orleans study, we assumed that firms facing cost increases above five percent should be considered as constituting the high-impact category.

In addition to considering the different firm types, we also of course need to examine the specific features of a living wage ordinance in order to evaluate the adjustment paths that firms are likely to pursue. As of this writing, all living wage laws in place are ones that apply only to firms either holding service contracts with, or receiving subsidies from, city governments. We therefore first focus on the adjustment issues as they apply under these "contractors only" type ordinances. At the same time, "area-wide" ordinances, which apply to all businesses of a given size within a defined geographic region, have also been considered, most seriously in Santa Monica and New Orleans, but also in, among other places, Denver and Houston. It is therefore important to consider the distinct features of the adjustment process that would apply in Santa Monica-type "area-wide" measures as well.[9]

[9] As of writing in June 2003, the City of Santa Fe, New Mexico is about to implement the nation's first area-wide living wage ordinance. The measure was passed by the Santa Fe City Council in February 2003.

Contractors-Only Ordinances

Conditions for Low-Impact Firms

Of course, businesses bidding on city contracts or to receiving city subsidies would want to pass through their increased living-wage induced costs to city governments in the form of more expensive contracts to perform comparable services. However, because the cost increases are small for these firms, it does not follow that cities would have to absorb all these additional costs in order to attract firms to bid on city contracts. Competitive factors, as well as the possibilities for productivity increases and redistribution become important here, again, precisely because the magnitude of the cost increase that needs to be absorbed is small.

Competition. I assume that firms are bidding on municipal contracts in a competitive environment. This means that contract holders cannot be assured of getting their contracts renewed, because at least one serious competitor will also bid on a given contract.[10] Within such an environment, an increase in costs on the order of 1 –2 percent need not push up contract terms. This is because most firms contracting with cities are eager to maintain these associations and would be unlikely to risk relinquishing them to competitors on the basis of negligible cost increases.

Productivity and Redistribution. On average, firms' productivity will be growing as the living wage is implemented. Even if firms' productivity rises annually by only 1 percent—i.e. at a rate that is half the average rate of productivity growth for all U.S. firms over the last full business cycle of 1991 – 2000—that alone would mean that productivity growth would itself cover two-thirds of a 1.5 percent cost increase. But it is also reasonable to expect that firms' productivity growth should rise at least at a modestly more rapid pace due to the living wage ordinance. This would be due to "efficiency wage" effects—i.e. a decline in turnover and absenteeism because higher wages have made the low-wage jobs more desirable. The costs to

[10] Studies of municipal contract bidding, such as Rehfuss (1989) makes clear that this is the most appropriate assumption from which to begin, even after allowing for extensive lobbying, political jockeying and corruption in bidding for municipal contracts.

firms of recruiting and supervising workers should then also decline. Note that allowing for modest productivity gains through efficiency wage effects does not imply that productivity growth would itself *fully cover* the increased costs of the ordinance. If businesses could increase productivity dollar for dollar through raising wages, an incentive would clearly exist for them to raise wages independent of any living wage mandate. But this incentive to raise wages will diminish when productivity increases only partially offset the cost increases from paying living wages.[11]

Of course, allowing that a firms' productivity increases be used to absorb higher living wage costs does also entail a redistribution of income within such firms, since it would mean that roughly one year's worth of productivity gains would be channeled toward the firms' low wage workers, as opposed to its owners or other employees. But after the one-year adjustment, the distribution of wages and profits within the covered firms could remain constant, while low-wage workers would have received their living-wage increases. All of these adjustments could occur even while the income that firms receive from city contracts would not rise at all.

Obtaining reliable figures on how large these efficiency wage effects are likely to be is very difficult, and estimates therefore range widely. Pollin and Brenner (2002) reviewed much of this evidence as it was relevant for the case of Santa Monica. We attempted to account roughly for the cost savings resulting from all factors—including reduced turnover, absenteeism, supervisory costs as well as increased worker effort—and concluded that "it seems reasonable to suggest that the productivity benefits of the higher living wage for some firms could be as high as 20 – 25 percent of their total living wage costs (p. 87)." This estimate would necessarily vary according to size of the mandated wage increase, since efficiency wage effects are a function of workers receiving wage gains large enough to induce changes in their level of job commitment.

[11] The literature on efficiency wage effects is voluminous. Pollin and Brenner (2000, p. 93) provides a brief set of major references.

Relocations and layoffs. There is no reason to expect significant layoffs or relocations to occur in considering low-impact firms under contractors' only ordinances. With respect to relocations, there is, in fact, not even a logical basis for anticipating these should occur. This is because a contractors-only living wage ordinance will cover firms that have contracts with a city regardless of whether the firm is located within or outside the city's limits.

Concerning layoffs, there would be no incentive for firms to reduce their scale of operations by shedding labor if the firms are able to absorb their living wage costs through small changes in productivity and distribution within the firm. The key here, again, is that the cost increases of the ordinance would be small; and as such, the benefits of maintaining the desired scale of operations is likely to exceed the costs of moving into compliance with the living wage mandates. Assuming a small employment effect for low-impact firms under living wage ordinances is certainly consistent with the most recent evidence on employment elasticities of minimum wage laws. In summarizing differences that remain among researchers on this question, Freeman writes, "The debate is over whether modest minimum wage increases have "no" employment effect, modest positive effects, or small negative effects. It is *not* about whether or not there are large negative effects," (1995, p. 833; emphasis in original).

Note also here that the productivity gains derived from efficiency wage effects—i.e. the gains from reduced turnover and absenteeism as well as lower costs of supervising and replacing employees—also does not imply layoffs of a firm's staff of production workers as the means to improving productivity. It rather entails generating modest increases in output from a given supply of employees through reducing administrative costs.

Overall then, with respect to low-impact firms, we would not expect there to be significant negative consequences either in the form of layoffs, relocations or significant budgetary burdens for city governments.

Conditions for High-Impact Firms

In our study of the Los Angeles ordinance, we found that approximately seven percent of covered firms would experience cost increases of more than 10 percent. Clearly, in cases such as these, the covered businesses would need to pass through a significant proportion of their increased costs if cities were to continue providing the services they are purchasing from these high-impact firms. Nevertheless, even here, significant parts of the increased costs could be absorbed through means other than pass-throughs to *city governments*. The most important factor here is that, at least in the case of Los Angeles and probably in other cities as well, a large proportion of the high-impact firms are concessionaries. These firms operate on city property, such as airports, zoos, or sports arenas, selling goods and services to the public. These firms thus have the option of attempting to pass through their increased costs in the form of product market price increases. Whether they can succeed in doing so will of course depend on the demand elasticities for the products they sell, a topic we will consider in more depth as it relates to the area-wide ordinances. For now though, the main point to emphasize is that, by definition, concessionaires leasing city property operate within a very confined market area, such as an airport or zoo. This means that their competitors are also operating within the same highly localized market. The competitors would also be covered by the living wage ordinance and would correspondingly face comparable cost increases. Firms in this situation should face relatively weak price elasticities for their products within a range of price increases of less than 10 percent implied by the corresponding living wage cost increases.

There will still be some share of covered firms—perhaps around five percent of total firms in the Los Angeles case—for which the city will need to anticipate large pass-throughs. In the Los Angeles case, if we reasonably allow that this five percent of firms accounted for about 10 percent of total living wage cost increases, that implied for 1996, the year on which our data for this study was based, an outside estimate of pass-throughs on the order of $7 million. This obviously is a significant figure in absolute dollars, but it is still an amount that was about 0.2 percent of the city's total budget for that year.

Considering all of these factors, the major expectation is that with contractors' only type ordinances, the typical situation will be one in which no single entity facing cost increases—i.e. neither the low- or high-impact covered firms, the city governments, nor consumers making purchases at city-leased concessions—would necessarily bear an onerous cost burden, that is, a cost burden that will induce significant negative unintended consequences in the form of layoffs, relocations, or heavy new cost increases for city budgets.

Retrospective Studies on Contractors' Only Ordinances

We now examine the major findings from retrospective studies as to whether this body of evidence is consistent with the conclusions of prospective studies. To date, there have been only three studies that have examined how the implementation of living wage ordinances has affected bidding patterns for city-service contracts and the awarding of contracts with city governments.[12] The first study, by Weisbrot and Sforza-Roderick (1996), examined contracts immediately after Baltimore implemented a living wage minimum that was 44 percent above the national minimum. Niedt, Ruiters, Wise, and Schoenberger (1999) then conducted a follow-up analysis of of the Baltimore experience three years later. More recently, Brenner and Luce (2003) have examined changes in contract patterns associated with ordinances in Boston, as well as Hartford and New Haven Connecticut.

The initial Weisbrot/Sforza-Roderick study examined 46 contracts in Baltimore whose value of $19.3 million represented 72 percent of the total value of contracts that were covered under the ordinance. The key finding of their analysis was that the total value of winning bids after implementation of the living wage ordinance rose by a neglible 0.2 percent in nominal terms relative to the value of the comparable pre-living wage contracts. In real terms, this amounted to a decline of 2.4 percent in the total value of contracts after implementation of the living wage

[12] In addition to these retrospective studies of contracting patterns, Neumark (2002) has produced a retrospective analysis of the effects of living wage laws throughout the U.S. on wages, employment, and poverty. However, the Neumark study is neither methologically sound nor robust either statistically or substantively. Brenner, Wicks-Lim, and Pollin (2002) document in detail the weaknesses with Neumark's study.

ordinance. These results by Weisbrot/Sforza-Roderick were then broadly confirmed in the later study of Baltimore by Niedt et al. They examined a total of 26 contracts that could be directly compared before and after the living wage law went into effect. They found that the aggregate cost increase of the winning contract bids was 1.2 percent, again an increase that was below the rate of inflation for any given year between 1995 – 1999.

The findings by Brenner and Luce are similar, in that they find that costs for a given supply of contracted services did not consistently increase—and indeed more frequently declined—subsequent to implementation of living wage ordinances in Boston, New Haven and Hartford. In considering the Brenner and Luce study, we focus primarily on the results regarding Boston, since the Boston ordinance is far more extensive than those in Hartford or New Haven. The Boston ordinance has covered 166 contracts since its inception in 1999. The New Haven ordinance has covered nine contracts since 1997, while that in Hartford has covered only two contracts since 2000.

Among the 166 covered contracts in Boston, Brenner and Luce restricted their analysis to those that were likely to employ a sufficient number of low-wage employees such that these firms might experience a significant cost increase from the ordinance. The criterion Brenner and Luce used for including a firm in their sample was whether they employed five or more workers earning between $8.71 and $12.00 in 2000, with $8.71 being the Boston living wage minimum in 2000. This reduced their sample of covered contracts to 29 firms that could potentially experience a relatively "high impact" from the living wage ordinance. Of these, 23 were contracts for the supplying of various types of special education services.

The results of the Brenner/Luce contract analysis for all three cities are presented in Table 3. First, with Boston, as we see, the weighted average annual change in amount of awarded contracts fell by 9.2 percent. This is over the fiscal years 1999- 00 to 2001-02, during which period the living wage mandate ranged between 57 and 35 percent above the statewide minimum wage (Massachusetts increased its minimum wage from $5.25 in 1999 to $6.75 in 2002). And

again, these results include only contracts awarded to firms that might possibly face significant cost increases from the Boston ordinance. The results from the nine contracts analyzed for New Haven are similar. As we see, the weighted average annual change in the awarded contract value was a decline of 10.9 percent.

This pattern of declining contract values is broken in the case of the two contracts awarded in Hartford after implementation of their living wage ordinance in 2000 – 01. These two contracts were for security guard services and temporary office assistants. As we see, the weighted value of these contracts did rise substantially, by 33.4 percent. Clearly, with these two cases, the contracting firms were able to receive substantial pass-throughs to cover their increased labor costs.

Of course, there is no reason to expect that living wage ordinances would *cause* the value of covered contract awards to decline, as they have, on average, in both Boston and New Haven. The implication of these declines in contract values is rather that, for the most part, factors other than living wage mandates have been more significant to firms in choosing how to make what turned out to be winning bids for city contracts.

But as we also saw with the two contracts in Hartford, there will be cases where the implementation of living wage ordinances is associated with substantial increases in contract values. We anticipated such results in our prospective analyses, in considering the situation for the few high-impact firms. Firms such as those in Hartford providing security guards and tempororary office assistance are likely to be among the high-impact firms, in that they both employ a high number of low-wage workers and those workers constitute a high proportion of such firms' total labor costs. But even with such high-impact cases, we cannot assume that the living wage ordinance alone *caused* the awarded contract values to rise by 33 percent, since no such causal relationship emerged with the 29 high-impact firms in Boston.

In fact, Brenner and Luce observed that the particulars within any given bidding situation are important to determining the final selection of a winning bidder. In the Hartford experience

with the security guards' contract, the establishment of the living wage mandate persuaded a total of nine firms that they could offer a competitive bid, seven more than had bid the contract before the living wage stipulations were included in the request for proposals. The initial two bidders were those that maintained labor costs as low as possible while the additional seven entrants competed more on the basis of quality than costs alone. This created the conditions in which the firm receiving the contract offered a bid 33 percent above the pre-living wage contract value. But this experience was distinct from that in Boston, in which Brenner and Luce found that contract values generally fell after implementation of living wage mandates in cases where the number of bids on a contract increased.

5. Adjustments Under Area-Wide Ordinances

As we have seen, the magnitude of cost increases for firms of living wage ordinances are comparable regardless of whether one is considering a contractors-only or an area-wide ordinance. But the structure of an ordinance will affect the options available to firms in adjusting to their increased living wage costs. Of course, under area-wide ordinances, firms could adjust to their increased living wage costs through raising productivity or redistributing the firm's overall income just as they could under a contractors-only ordinance. Moreover, the cost pressures under which firms might decide to lay off workers would also be comparable under both an area-wide as opposed to a contractors-only ordinance. But there are two adjustment options that would be distinct under an area-wide measure—raising prices or relocation. We consider these in turn.

Price increases. For the case of contractors-only ordinances, we have already discussed the role that price increases can play as an adjustment mechanism in cases where cities have given out concession contracts. With area-wide ordinances, raising prices to reflect increased costs would likely be the least costly and disruptive adjustment path for a high proportion of covered firms. But whether firms can succeed in such a strategy depends on the competitive environment in which they operate and the price elasticity of demand for their products.

As part of their path-breaking work on minimum wages, Card and Krueger (1995) concluded through observing a variety of situations that price increases were a primary meaus through which firms absorbed their increased costs resulting from higher minimum wages. Indeed, after New Jersey raised its statewide minimum wage in 1993 by 18.8 percent over the national minimum, Card and Krueger found that New Jersey fast-food outlets were able to raise their prices by about the same amount as their total costs were increased, which amounted to about 3.4 percent. But their results are focused on prices at fast-food restaurants. The question we examined in our New Orleans study was how well such results could be generalized to the range of businesses that would be covered by an area-wide municipal living wage ordinance.

Of course, all firms within New Orleans or any other municipality would face the same new living wage requirements. But firms that compete with other firms within the municipality, such as those in the fast-food industry, will likely be better able to raise their prices, since their competitors will have experienced similar mandatory cost increases. Businesses that compete in markets that extend beyond the municipality will correspondingly have more difficulty marking up their prices.

As such, in our New Orleans study, we divided all firms in the city according to whether they compete primarily either with firms outside or inside New Orleans, or whether they face some combination of competitors both inside and outside the city. The first significant result of this exercise concerned firms competing outside the city. Of course, such firms should be least able to pass along their cost increases through raising prices without risking a loss of their customer base to their out-of-town competitors. However, we also found that the firms in New Orleans that fit this category, including manufacturing, mining and legal services firms, would all face only negligible cost increases due to the city's living wage ordinance, since low-wage workers are a correspondingly negligible share of total production inputs for such firms. It is likely that in other cities as well, a comparable set of firms would also be the ones that are both a)

most exposed to outside competition; but also b) only marginally affected by the mandates of living wage ordinances.

For industries in which firms compete mainly with other firms within the city, it is likely that the situation will approximate that analyzed by Card and Krueger for the fast-food industry in New Jersey. That is, these firms should be able to raise their prices to reflect their higher costs, since all firms in the market would face similar cost increases. The most important industries that are included in this category in New Orleans are restaurants and hotels, which is to say, a large part of what one purchases with a New Orleans hotel room or restaurant meal is its location within the city. This is why hotel room prices are roughly double inside New Orleans proper relative to its immediately surrounding areas.

But how much of the living wage total cost increase can be covered through raising prices will also depend both on price elasticities for individual products as well as the magnitude of the total cost increases. In the case of New Orleans, where the living wage increase was 19 percent above the national minimum, the price increases needed to cover the increased costs would generally be significantly below the 3.4 percent cost increase estimated by Card and Krueger for the New Jersey fast food industry. The average cost increase for New Orleans restaurants was 2.2 percent while that for hotels was 1.7 percent.[13]

Price elasticities for the hotels and restaurants will depend on the market segment one is considering. With relatively high-end hotels and restaurants, both the industry and research literature have long recognized that customers are not strongly price sensitive. For example, a 1997 paper by Lewis and Shoemaker explains how price can serve as a crucial indicator of quality to relatively high-end hotel and restaurant clients. As such, demand in these markets is

[13] Though it is notable that for New Orleans fast-food restaurants, we estimated cost increases to be 3.9 percent, that is, very close to the Card/Krueger figure for New Jersey fast-food restaurants. But this result should not be surprising, given that the increase in the minimum wage in the two cases was virtually identical, 18.8 percent in New Jersey compared to the 19 percent increase that would have applied in New Orleans.

likely to be relatively insensitive to price increases on the order of two percent—that is, increases that would fully cover the cost increases associated with the New Orleans living wage ordinance.

Of course, the situation is different when the relevant cost increases are much higher, as with the 10 percent increases estimated for hotels and restaurants in Santa Monica, based on a living wage that would be 85 percent above the statewide minimum. However, even here, one needs to examine the particular market conditions to evaluate the extent to which raising prices may still serve as a viable adjustment mechanism. Thus, in the Santa Monica case, the hotels operate in a market in which the supply of rooms is limited by government growth restrictions. In that situation, high-end hotels were able to raise prices by an average of 14 percent per year (in inflation-adjusted dollars) between 1994 – 99 without experiencing any fall off in occupancy rates. These hotels also recovered quickly from a fall in revenues resulting from the September 11, 2001 terrorist attacks. The hotels' pricing power is a reflection of the rents hotels are able to obtain through operating in a restricted market.[14] By contrast, restaurants in Santa Monica do not operate in a restricted market and therefore do not possess the same capacity to earn rents from their operations. We had therefore anticipated that they would be far less capable of using price increases as a means of adjusting to their increased living wage costs.

There is, finally, the situation for firms that compete both inside and outside the borders of a municipality. Firms in this situation are likely to be very heterogeneous. In New Orleans, for example, we estimated that this category of firms included advertising and building maintanance firms, wholesale traders, and retail outlets, including food stores. Conditions will vary considerably among these firm types, so one cannot generalize about the role of price increases as one adjustment mechanism Nevertheless, we can say that in some cases, price

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[14] Stiglitz (1993) explains the concept of economic rent as follows: "The economic concept of rent has its historic origins in the payments made by farmers to landlords for the use of land, but today its application is much broader. The critical characteristic of land in this regard is that it is inelasticallly supplied, so that high payments for land (higher rents) will not elicit a greater supply....Many other factors of production have the same inelastic character," (p. 357-58).

increases will be able to absorb at least some of the cost increases associated with the costs of living wage ordinances.

Overall then, price adjustments—and therefore the passing on of increased living wage costs to product market customers—is likely to be an important adjustment mechanism to area-wide living wage ordinances for a substantial proportion of covered firms. But to obtain a clear understanding of how important price adjustments will be in any given situation will require an analysis of the individual cases. In each case, one would need to obtain a sense of the magnitude of cost increases for firms relative to their overall scale of operations; the broad competitive environment for the covered firms; and the elasticity of demand for firms' products.

Relocation. Raising the living wage minimum for all firms within a given municipality does create an incentive for firms to escape coverage by moving to a neighboring municipality. However, the force of this effect will depend, first, on how large the firms' newly mandated costs are relative to other considerations. With low-impact firms, the incentives to relocate will be small under all circumstances.

But even with a large proportion of high-impact firms, the costs of relocation will exceed the expected benefits. Restaurants and hotels are likely tied to their specific locations—one cannot stay, for example, in a New Orleans hotel unless the hotel is actually within New Orleans. In the case of other types of businesses, such as those providing janitorial services, the firm itself need not remain located within the given municipality. But if the employees of the firm were still working within the boundaries of the municipality, they would still fall under the terms of the living wage ordinance. These firms as well would have little incentive to relocate.

The types of firms for which the benefits of relocation would exceed costs would be high-impact firms whose business is not location-specific. In both our Los Angeles and New Orleans studies, we have estimated the proportion of firms that would likely fall into this category. In both cases, it is a small number of the total of covered firms. In New Orleans, for example, where our estimates are more precise because they are based on our own survey of local

business firms, we estimated that there were 209 firms whose cost increases would exceed five percent of their total operating budgets and whose businesses are not obviously location-specific. These 209 firms constitute 1.6 percent of the total universe of 12,439 New Orleans firms. Whether relocation would be viable for any given firm among this group would depend on the nature of their business. These firms were located fairly evenly across four industrial categories in New Orleans, wholesale trade, business services, retail trade other than restaurants and hotels, and "other services." In some cases, such as with retail trade, firms could risk losing sales through relocation if their customers' purchasing habits are at least partially tied to convenience. In other cases, such as advertising agencies, firms could move without losing customers. There are also cases such as we have discussed above with janitorial firms, in which the firm would still have to pay living wages to workers whose jobs were located within the city, regardless of where the firm's offices were located. In short, it is likely that even among these roughly 200 firms, only a relatively small subset would be in a situation in which the benefits of relocation would outweigh the costs.[15]

Moreover, the relevant issue with this subset of firms would be what the costs would be to the City of New Orleans if these firms did indeed relocate. In our previous work, we worked from the reasonable assumption that the firms would be relocating strictly to avoid paying the higher minimum wage. As such, we would expect the firms to move just outside the city limits, so as to retain, if from a different specific location within the metropolitan area, their same New Orleans-based operations and customer base. One crucial implication of this point is that no net employment losses would occur due to these firms' relocation. Workers would be able to retain

[15] On balance, this conclusion is buttressed by research on a related topic—the effects of differential property tax rates on the location decisions of export-oriented firms within the United States. Thus Bartik (1991, 1992) has shown that export-oriented firms are likely to be highly sensitive to differences in property tax rates between suburban locations. However, the effects of tax differentials appear to be substantially weaker in comparing central city locations with the suburbs. Bartik argues persuasively that this is likely the case because central cities may not be good substitutes for suburban locations.

their same jobs without moving, which in turn would mean no change in the city's housing market.

The primary loss to the City of New Orleans would therefore be the loss of the its authority to tax these firms. In our earlier work, we made an upper-limit assumption that roughly one-half of the 200 firms would move out of the city. Based on that assumption, and on the stipulations of the City's tax codes, we estimated that the City would lose about $2 million in sales and use taxes through the relocation of 100 firms. This is a large absolute amount, of course. But to put it in perspective, it is equal to 0.4 percent of the City's $499.1 approved budget for 1999.

Finally, from our examinations of Los Angeles and Santa Monica as well as less formal considerations in other cities, I would anticipate a relocation effect of roughly this order of magnitude for other cities as well through implementation of an area-wide living wage ordinance.

6. Labor Substitution

Even if firms neither relocated nor reduced their number of employees at all in response to a living wage ordinance, a negative unintended consequence of such measures could still result through labor substitution—i.e. firms replacing their existing minimum wage employees with workers having better skills and/or credentials, which could occur even in the absence of any net job loss. Because the firms covered by a living wage law would pay higher than comparable positions with uncovered firms, openings for the jobs with covered firms would likely attract workers with somewhat better credentials, on average, than those in the existing labor pool.

But how would employers be able to distinguish more qualified workers in this expanded pool of job seekers? This is not an obvious question. For most of the jobs that would be covered by living wage ordinances—e.g. janitors, nurse's aids, garderners, parking lot attendants, elevator operators, hotel maids, restaurant dishwashers, and retail cashiers—the qualities that would distinguish one worker from another will not be based primarily on formal qualifications such as years of schooling . Hiring "better" workers would rather most likely entail hiring people who

work harder and are more conscientious in their duties. But employers will not be able to observe those on-the-job work habits until an employee is actually on the job. The employers are therefore likely to rely on formal qualifications such as years of schooling or English language skills as proxy measures—however inadequate—of workers' job specific skills. Thus, the primary way in which labor substitution would occur after a living wage ordinance were implemented would be through better credentialed workers seeking jobs covered by a living wage ordinance that would not have applied if the wages for these jobs were still closer to the national minimum wage level.

We addressed this issue in both our New Orleans and Santa Monica studies using the same technique and analytic framework. We first examined differences in personal characteristics for two groups of workers—those who fell within the wage range close to the pre-living wage minimum and those who would fall within the living wage minimum. Thus, in the New Orleans case, we examined workers earning between $5.15 and $5.64 in the first group; and between $6.15 and $6.64 in the second group. To obtain a sample of low-wage workers large enough to make this exercise reliable, we had to enlarge the pool to include not workers employed in New Orleans itself, but rather a sample encompassing workers in all of Louisiana as well as Alabama, Arkansas, Georgia, and Texas. The results of this exercise are shown in Table 4.

TABLE 4 BELONGS HERE

As we see from the table, the percentage of those without high school diplomas falls by 15.8 percentage points in moving from the $5.15 to the $6.15 wage category. Correspondingly, those with high school diplomas, some college, and college degrees each rise by between 4.5 – 6.5 percentage points. Not surprisingly, the percentage of teenagers falls by 18.8 percentage points in moving from the lower to the higher wage category. The $6.15 wage category has fewer females but, surprisingly, more non-native English speakers. The differences in personal characteristics between the low- and high-wage categories are somewhat higher in our Santa

Monica study simply because the living wage increase is itself larger. Thus, the high-end wage band that we examined with the Santa Monica study was between $9.10 and $10.75 rather than $6.15 - $6.64.

Nevertheless, in both cases, it is important to emphasize that such figures are useful only in establishing an *upper limit* as to the likely degree of labor substitution. This is because, in considering these figures, we are effectively asking whether, if covered firms were newly hiring their entire low-wage work force, and if they were advertising their job openings at a wage rate in the range of $6.15 rather than $5.15 (to take the New Orleans case), how would the profile change of the newly hired workers?

Having thus defined the upper limit of labor substitution effects through these figures, the next step is to recognize why any actual labor substitution effects are likely to be far more modest. This is first of all because, in reality, businesses are unlikely to newly hire their entire workforce after a living wage law was enacted, nor would they want to do so. Rather, as we have discussed, workers earning the higher minimum will be less inclined to leave their jobs, and their work effort should correspondingly rise. By the same token, businesses are not likely to terminate their existing workers, even if they have relatively poor formal credentials, as long as their performance is satisfactory.[16] Recognizing these various factors, we would still expect some substitution to occur, both by educational credentials and age, though, the magnitude of such substitutions is likely to be modest.

7. Conclusions

The findings I've summarized with respect to labor substitution effects are reflective of my overall evaluation of the evidence concerning negative unintended consequences, including

[16] Another reason the Table 6 figures represent an upper limit measure of the substitution effect is that the minimum $5.15 category includes a relatively high proportion of teenagers who do not have high school diplomas simply because they are still in high school. If $6.15 became the new minimum wage for New Orleans, then the proportion of high school students would rise in this new minimum category, thereby also increasing the total share of those in this category with less than a high school diploma.

layoffs and labor substitution, relocations, and costs to city governments. An analysis of these potential negative consequences is central to any assessment of living wage ordinances. They could certainly be significant and even overriding features of any given ordinance. But when the impact of living wage ordinances on most covered firms is of the order of 1 –2 percent of these firms total production costs or sales, the likely adjustments firms will make will be of a comparably modest magnitude. As such, raising prices and productivity by a small amount, or redistributing the firm's income downward modestly, are likely to be the predominant means through which covered firms will absorb their increased costs. In such cases, the gains of living wage ordinances to low-wage workers and their families will be larger than the per unit costs of the ordinances that would be borne by covered firms, consumers, taxpayers or city governments. To put this another way: a well-designed living wage ordinance has the characteristic that its benefits will be concentrated among low-wage workers and their families while the costs can be broadly diffused among covered firms, consumers, taxpayers and city governments.

Moreover, as we discussed, even in cases where cost increases are large, as with the hotels and restaurants in under the area-wide Santa Monica ordinance, it still doesn't follow that firms will necessarily experience significant difficulties absorbing the cost increases via price, productivity, and redistribution. The Santa Monica hotels earn substantial rents through the restrictions on supply established by the city government's growth restriction policies. In that particular case, the adjustment that the hotels would make through redistribution would simply imply a broader sharing of the policy-induced rents that the firms have heretofore been able to maintain for themselves. Restaurants in Santa Monica, by contrast, do not benefit through growth restriction policies. They would have far greater difficulties absorbing cost increases on the order of 10 percent.

In other cases, as with the security guard contract under the Hartford contractors-only ordinance, we should expect that high impact firms will successfully pass through to city government their increased costs. But our evidence shows that this does not hold as a general

rule. It clearly did not happen as the general case with the Baltimore, New Haven, and Boston ordinances. As such, cities should certainly be prepared to bear some of new costs of ordinances that would apply to high-impact firms. But even here, these cost increases are likely to be a small fraction of the overall budgets of city governments, as we saw with the Los Angeles case. The only situation in which the cost increases would be large relative to a city's overall budget would be when 1) an ordinance would cover a large number of high-impact firms; and 2) the contracts with these high-impact firms would be a significant share of a city's overall budget.

Of course, these conclusions are based on living wage ordinances in which the mandated raises ranged between 19 and 87 percent above the relevant minimum wage. This is a wide range, but, as we have seen, it still does not encompass the full scope of values that one could plausibly define as a "living wage." As we saw for Los Angeles, a reasonable case can be made for defining a living wage as being somewhere in between $11 - $20 an hour, depending on family sizes and types. On the other hand, a $20/hour living wage would mean a three-fold increase over the current California minimum of $6.75 The cost increases associated with a living wage at this level would obviously create much more susceptibility to all the potential negative consequences of these measures.

Two questions capture this basic tension concerning the evaluation of living wage laws: 1) What constitutes a reasonable standard for living wages in the United States according to the approaches articulated by Sen and others; and 2) What is the tipping point at which a living wage mandate would trigger negative unintended consequences to a significant degree? This paper presents evidence concerning both a reasonable range of living wage values and the effects of various living wage ordinances. But more importantly, I have tried to be clear in presenting a methodology for the detailed analysis of living wage laws according to Alfred Marshall's dictum, that we place "greater proportionate stress" not simply on recognizing potential effects in an 'all else equal' world; but on measuring the relative weights of these potential effects under various circumstances.

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Table 1.
Alternative Living Wage Income and Wage Levels for Los Angeles Area

(figures are in 2001 dollars)

	Poverty-level Income			*Basic Needs Income*	
	Severe Poverty *(official poverty line)*	**Poor** *(160% of Official Poverty Line)*	**Near-Poor** *(185% of Official Poverty Line)*	**One Wage Earner**	**Two Wage Earners**
3 Person/2 Child Family					
Annual Income	14,269	22,830	26,398	42,845	---
Hourly Wage-Rate for Full-Time Job	6.86	10.98	12.69	20.60	---
4 Person/2 Child Family					
Annual Income	17,960	28,736	33,226	35,207	51,459
Hourly Wage Rate for Full-Time Job	8.63	13.82	15.97	16.93	12.37 for both jobs

Sources: Current Population Survey (2001); California Budget Project (2001).

Table 2. Cost Estimates of Alternative Living Wage Ordinances

	Los Angeles (1998 study)	New Orleans (1999 study)	Santa Monica (2000 study)
Statistical sources for estimates	Primarly government statistics	Firm survey supplemented with government statistics	Firm and worker surveys supplemented with government statistics
Coverage of ordinance	Govt. contractors over $25,000 and subsidy recipients over $1 million or $100,000 annually	All firms in city with more than $500,000 in annual sales	All firms in city's tourist zone with more than $3 million in annual sales
Mandated wage increase above minimum (percentages)	71%	19%	87%
Benefits	Health—29% of minimum wage; 12 paid days off	Not included	Health—22% of minimum wage; 15 paid days off
Direct costs as proportion of total costs	80.4%	74.4%	89.2%
Ripple effects as propotion of total costs	19.6%	25.6%	10.8%
Total costs of ordinance relative to firms' overall operations	1.5% of production costs	0.9% of operating costs	Percentages of total sales: All firms—3.9% Hotels and restaurants—10% Other firms—2.2%

Sources: Pollin and Luce (2000), Pollin, Brenner and Luce (2002), Pollin and Brenner (2000).

Table 3. City Service Contract Awards in Boston, New Haven and Hartford, Before and After Living Wage Ordinance

	Boston	New Haven	Hartford
Fiscal year of implementation	1999 – 2000	1997-98	2000-01
Mandated wage increase above statewide minimum	+ 57% in 1999 + 35% by 2002	+43% in 1997 +46% in 2001	+33% +86% with health benefits
Average annual change in contract values (weighted by contract size)	-9.2% [29 contracts]	-10.9% [9 contracts]	+33.4% [2 contracts]

Source: Brenner and Luce (2003).

Table 4.
Personal Characteristics of Low-Wage Workers in Five Southern States, 1999

	Hourly Wage Categories *(1999 dollars)*		Differences between two wage categories *(column 2 – 1)*
	(1) $5.15 – 5.64	(2) $6.15 - $6.54	(3)
Less than high school diploma	46.0%	30.2%	-15.8%
High school diploma or GED	31.5%	38.0%	+6.5%
Some college	20.7%	25.2%	+4.5%
Bachelor's degree or more	1.9%	6.6%	+4.7%
Under 20 years of age	32.2%	13.4%	-18.8%
Average age (years)	30.6	33.9	+3.3
Female	65.5%	61.5%	-4.0%
English as second language	13.9%	20.8%	+6.9%

Source: Current Population Survey, Outgoing Rotation Group 1999
Note: In addition to Louisiana, the southern states in the sample are Alabama, Arkansas, Georgia, and Texas.

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Abstract: Drawing upon both prospective and retrospective evidence, this paper reviews the economic effects of local living wage ordinances. I concentrate my attention on the higher costs these measures create for covered firms, as well as their budgetary implications vis-à-vis cities that adopt them. I also briefly review the effect that living wage laws have had on bidding environment for city service contracts. Based on a range of cost estimates associated with living wage laws, I then examine the adjustment channels firms are likely to pursue when adjusting to higher labor costs, including: raising prices; increasing firm productivity; redistributing income within the firm; laying off employees; and relocating out of the area covered by the living wage mandate.

This review draws from research that I and my colleagues at the Political Economy Research Institute have conducted over the past several years. I am grateful to Robert Pollin and Jeannette Wicks-Lim for their thought-provoking and fruitful collaborations. I am especially indebted to Stephanie Luce for her on-going collaboration, and her careful comments on an early draft of this paper.

I. Introduction

Over the last decade, the United States has witnessed the rapid expansion of a municipal policy initiative known as the living wage ordinance. The aim of these laws is to set a wage floor high enough so that a full-time worker can support a family of three or four at a living standard above the official poverty line (Luce 2002). Most living wage laws apply to large-scale city service contractors, and a limited number apply to firms receiving financial assistance such as tax abatements or other subsidies. In recent years these laws have also been extended to cover private sector businesses with no direct financial ties to the city, and living wage initiatives have emerged in variety of other arenas such as college campuses.

These measures are an effort to improve the stagnating living standards of low-wage workers in the U.S., and to stem the more general trend of rising wage and income inequality witnessed over the past generation. Integrally linked to both phenomena has been the steady erosion of the national minimum wage, which has declined 38 percent from its 1968 peak of $8.27 (in constant 2002 dollars). To gain a sense of the magnitude of this decline, consider the fact that at its highpoint, an individual working full-time, full-year for the minimum wage had earnings *20 percent above* the poverty line for a family of three. By 2002, a similarly situated worker earning the current minimum wage of $5.15 fell *25 percent below* the three-person poverty line, itself considered by a broad range of experts to be between 25 and 50 percent too low (see the discussion in Chapter 2 of Citro and Michael 1995).

By the end of 2002 more than 100 cities, counties, school boards or other local government bodies across the country had enacted living wage laws. Table 1 profiles the different types of coverage legislated by these measures. From these data we see that more than three quarters (77 percent) of existing living wage ordinances have placed wage standards on

service contractors, while a much smaller fraction establish standards for economic development assistance (38 percent) or city employment (22 percent). While not immediately apparent from this table, the modern living wage movement has penetrated deep into the fabric of local economic policy-making in the United States. One measure of this success is the fact that by January 2003 cities with living wage ordinances comprised approximately one fifth of the population residing in municipalities of 10,000 people or more, and close to 40 percent of the population in cities larger than 100,000.[1]

[TABLE 1 NEAR HERE]

But these statistics beg the question, what economic impact have existing living wage measures had? Opponents consistently warn that these initiatives will not help, but will actually harm, low-wage workers and their families. They argue that living wage laws will set off a series of "unintended consequences" which, because of the way wage floors are theorized to operate in market settings, will actually lower the welfare of their intended beneficiaries. Two sets of effects are of particular concern. The first involves the firms covered by living wage requirements, where it is feared that higher wage floors will induce employers to scale back their usage of low-wage labor through means such as workplace reorganization, substitution of new machinery or equipment, or replacement with higher skilled (and already higher paid) workers. In the extreme, the response could even entail relocating out of the area in an effort to avoid the living wage mandate. The second set of effects concerns the cities that pass living wage ordinances. Any of the aforementioned adjustment methods would impose new costs on covered firms, and at least in the case of city service contractors much of this could be passed back to cities in the form of higher contract costs. Such costs to local government could be significant, and they carry with them the risk of reducing the level of existing city services, or creating the

need for additional taxes. These are matters of serious concern, given the precarious economic position of the lowest-paid segment of the U.S. workforce, the fragile condition of local government finances in the current recession, and the rapid proliferation of living wage measures throughout the country.

This paper will address these basic issues, providing an overview of the available evidence as to the economic effects of living wage ordinances on the local governments that pass such measures and the firms they cover. Because of their recent emergence and rapid proliferation, there is far less research available on the economic impact of living wage laws when compared to state and federal minimum wages. We are fortunate, however, that this body of research has already produced some important insights into the dynamics of local wage mandates, and that it is expanding at a considerable pace.

Before turning to this empirical evidence, it is first useful to examine some of the theoretical and methodological issues involved in studying the economic effects of living wage ordinances. In the next section I review the recent literature on the effects of state and federal minimum wage mandates, drawing out the important differences between living wage ordinances and these other, more universal, wage floors. One such difference, which stems from the targeted nature of living wage laws, is the feasibility of employing publicly available data together with conventional statistical techniques to examine the economic effects of living wage laws. This is an issue of considerable importance, given the recent work of David Neumark and Scott Adams (forthcoming (a) and (b)). As we shall see, precisely because of the important differences between living wages and minimum wages, the approach adopted by Neumark and Adams is methodologically inappropriate for detecting the effects of living wage laws, and non-robust even on its own terms. Ultimately their work offers us only one strong conclusion, namely that

the most fruitful avenue for evaluating the economic impact of living wage ordinances is at the local level, using various primary data collection methods.

Fortunately virtually all other existing research has followed this more localized approach to examining living wage ordinances, and I review these findings in sections three and four. I start in section three by summarizing the results of a series of economic impact studies conducted prior to the passage of living wage ordinances in several large municipalities. I refer to these findings as *prospective* (or ex ante) evidence. This sort of analysis is a mainstay of public policy-making in a variety of arenas from environmental regulation to tax reform, and is particularly useful when there is no sufficiently comparable historical experience to draw upon while crafting legislation. Such was the case in the early years of the living wage movement, and for many of the early living wage adopters prospective evidence of this sort was a central element to the public debate. Fortunately, there is now also a broad range of evidence concerning the budgetary and employment impact of local living wage ordinances which draws on the actual experiences of cities that have passed such measures in recent years. I refer to this body of literature as *retrospective* (or ex post) evidence, and review the salient findings in section four below.

As we shall see, both the prospective and retrospective evidence indicates that for most firms covered by living wage ordinances the costs created by these measures are small, in the range of 1 to 2 percent or less of total firm sales or operating costs. Research also suggests that for a small subset of covered firms these costs can be substantially greater, on the order of 10 percent of sales. Understanding the magnitude of these costs is crucial if we are to accurately gauge how firms will respond to living wage mandates. Although living wage opponents most frequently warn of lay-offs and firm relocations, these are but two of the many ways in which

businesses can respond to any additional costs associated with living wage mandates. In fact, as I summarize in section five, based on the available evidence it appears that firms are adjusting to living wage-induced costs through a combination of three other means, including raising prices, increasing productivity, and redistributing income within the firm. These alternative adjustment mechanisms appear to have distinct advantages for covered firms within the relevant range of mandated costs, and they may also help explain why most studies to date do not find significant increases in city contract costs following living wage implementation. These conclusions are strikingly similar to the findings put forth in the "new economics of the minimum wage" literature discussed in section two below, and they illustrate the more general point that firms appear to respond to wage-floors in ways that cannot be readily anticipated using theoretical models. I close in section six, synthesizing the available evidence and offering concluding remarks.

II. Examining Living Wage Effects: Theoretical and Methodological Issues

The relationship between the minimum wage and employment has been an issue of longstanding and frequently intense controversy. The reasons for this are straightforward. Standard competitive models of the labor market are unambiguous as to the consequences of a binding wage floor. Firms are expected to utilize less low-wage labor, all else constant, which diminishes the economy-wide employment prospects for workers in the lowest tier of the labor market. However, the predictions of the competitive model are just that, *predictions*, which much be evaluated against available empirical evidence.

Recent work in this area, now known as the "new economics of the minimum wage" literature, has cast serious doubt on the robustness of earlier findings linking minimum wage

increases with lower employment in the aggregate (e.g. Card et al. 1994; Card and Krueger 1995; Neumark and Wascher 1994). This research has also produced substantial new evidence that firms faced with higher minimum wage mandates do not behave in the manner predicted by competitive models, and that average firm employment does not decline but may in fact increase slightly following minimum wage increases (e.g. Katz and Krueger 1992; Spriggs 1993; Card and Krueger 1994; Card and Krueger 2000). There is also individual-specific evidence that low-wage workers do not face lower employment prospects following minimum wage increases (e.g. Card and Krueger's 1995 re-analysis of Lineneman 1982 and Currie and Fallick 1994; Zavodny 2000).

Both the findings on the economy-wide and firm-level effects of the minimum wage have been subsequently challenged, the debate turning on issues of statistical technique and data quality (Burkhauser et al. 2000; Neumark and Wascher 2000). It is important to note that the preponderance of the individual and firm-level evidence is tilted towards the finding of either no statistically significant employment effect or a small positive one.[2] However, the larger issue has been well summarized by Richard Freeman: "The debate is over whether modest minimum wage increases have "no" employment effect, modest positive effects, or small negative effects. It is *not* about whether or not there are large negative effects," (1995, p. 833; emphasis original).

As we will discuss in section five, there are many possible explanations for why individual and firm-level analysis consistently fails to find negative employment effects following minimum wage increases. On a theoretical level one explanation may be that alternatives to the competitive model in fact offer a better description of labor market dynamics than more traditional competitive models. For example, models emphasizing monopsonistic competition admit a variety of possible outcomes in the face of an increased minimum wage,

including a positive relationship between minimum wages and employment, among other potential equilibria (e.g. Card and Krueger 1995; Bhaksar, Manning, and To 2002; Manning 2003). Others have suggested that efficiency wage models may also be more appropriate than competitive models when characterizing the labor market, highlighting the ways in which higher direct labor costs may be offset by higher productivity and lower indirect labor costs, such as monitoring, recruitment, and training (e.g. Akerlof and Yellin 1986; Stiglitz 1987).[3]

While these theoretical interpretations of recent minimum wage research are a significant advancement in our understanding of labor market dynamics, they unfortunately ignore many "older" explanations of firm adjustments to higher mandated wages, such as those advanced by the post-war institutionalists over fifty years ago. While these scholars did not employ the formalized models that dominate the economics profession today, individuals such as Clark Kerr, Richard Lester, Lloyd Reynolds, and Sumner Slichter anticipated many of the arguments that ground today's models. In the current context, these include such insights as the possibility of a minimum wage increase leaving employment levels unaffected (e.g. Lester 1946, 1947, and 1960), and the deleterious role that labor turnover plays in modern firm management (e.g. Slichter 1919 and 1920). One important commonality among these scholars was an acknowledgement of what Lester called "a range of indeterminacy," not just in wage determination, but also in other areas of firm operations, including the procurement of non-labor inputs and the setting output prices. This approach implicitly recognizes that it is very difficult to identify a real world situation in which *all else will be held constant* when the minimum wage increases, as the standard competitive model requires. By allowing firms some discretion in areas such as purchasing other inputs and setting output prices, these researchers in practice found themselves considering the range of methods by which firms might adjust to higher wage

mandates, including such channels as raising prices, increasing sales, changing production techniques, or raising productivity. Changing the level of employment, in this context, becomes only one of many ways in which firms may respond to higher wage mandates. Lester's survey of Southern manufacturers is a prime example of this method of approaching the minimum wage (Lester 1946).

Interestingly, more recent evidence indicates that it is precisely these alternative adjustment channels that firms prioritize in the face of higher minimum wage mandates. For example a little appreciated aspect of Card and Krueger's fast food study in New Jersey and Pennsylvania is that "pretax prices rose 4 percent faster as a result of the minimum-wage increase in New Jersey – slightly more than the increase required to fully cover the cost increase caused by the minimum-wage hike" (Card and Krueger 1995, p. 54). Similar evidence is presented in Aaronson (2001), who finds that restaurant prices in the United States and Canada generally rise with changes in the wage bill, and that these changes are typically concentrated in the first quarter following a minimum wage increase. Understanding how firms use these alternative adjustment channels to cope with increased labor costs following the imposition of binding wage floors may provide the key to reconciling both the recent findings from the new economics of the minimum wage literature as well as those from the prospective and retrospective living wage research reviewed in sections three and four.

While these alternative theoretical and empirical perspectives may indeed prove useful in explaining recent research on the minimum wage, the findings themselves can provide only limited guidance as to the economic impact of living wage ordinances. This is first of all because most living wage laws do not produce the *modest* wage increases that the "new economics of the minimum wage" literature has traditionally examined. More important, however, is the fact that

living wage laws have dramatically more limited coverage than traditional minimum wage mandates. Although each ordinance varies substantially, these measures typically cover far less than one percent of a city's labor force. As one example, Pollin and Luce (2000) estimated that by 1999 the Los Angeles (LA), California living wage ordinance would apply to no more than 7,600 workers in the LA labor market. Yet if their calculations are accurate this means that these covered workers would have comprised a tiny segment of the 4.4 million individuals in the Los Angeles area workforce at that time, approximately 0.17 percent. This implies that whatever employment changes may have occurred in the tiny covered sector, their effect is not discernable in the labor market as a whole due to the much larger size of the uncovered segment.

These substantial differences in coverage between minimum and living wage laws make it impossible to utilize the data sets and statistical techniques that are the mainstay of modern labor economics. This conclusion is particularly important, as it calls into question recent research by David Neumark and Scott Adams (Neumark and Adams forthcoming (a) and (b)). In their work the authors apply the popular "difference-in-difference" statistical technique to data from the Current Population Survey (CPS), in an effort to detect the effects of living wage laws on the wages, employment and poverty status of individuals in the bottom decile of wage-earners. Unfortunately, their research design suffers from a host of methodological and empirical problems that ultimately invalidates their findings.

For example Neumark and Adams utilize the CPS, which does not allow researchers to identify a respondent's place of employment. Such information is not necessary when studying minimum wage laws, where relatively few labor force participants are exempt from the law and those that are can be readily identified by sector of employment. However employer information is crucial for researchers interested in utilizing publicly available data such as the CPS to study

living wage ordinances, since the overwhelming majority of low-wage workers are not covered by living wage laws, even in those sectors where covered firms are concentrated.

However, even if it were possible to identify with precision whether individual respondents in the CPS worked for employers covered by a particular living wage law, this survey does not sample a sufficient number of covered workers at the local level for statistically reliable estimation. As one example of this problem, Brenner et al. (2002) calculated that in Los Angeles, with one of the broadest living wage ordinances in the country, there were likely to be only about eight covered workers contained in an annual CPS sample of approximately 5,000 labor force participants. This causes particularly acute identification problems for the "difference-in-difference" statistical technique employed by Neumark and Adams. In their work, the authors propose to "identify" the effects of living wage laws by comparing changes in wages and employment for workers in cities with living wage laws to those in cities without such measures, controlling for other sources of variation. Any difference in the trend between workers in the two different types of cities is attributed to the living wage policy. However as the authors themselves readily acknowledge, living wage ordinances typically cover far less than one percent of the workforce in the cities that adopt them (Neumark and Adams forthcoming a). This implies, almost mechanically, that any changes taking place within living wage cities must themselves be driven by the workers unaffected by living wage laws.

On methodological grounds alone, such a degree of mismeasurement in the key policy variable calls Neumark and Adams' recent findings into question. However, as my colleagues and I have also demonstrated (Brenner et al. 2002), there are substantial empirical problems with the authors' approach to identifying the effects of living wage laws. As one example, Neumark and Adams find that the statistically significant wage and employment effects are concentrated in

cities where living wage ordinances cover business assistance recipients. They conclude that perhaps the business assistance provisions have had much broader effects that commonly assumed. Unfortunately this explanation does not correspond to actual experience. After interviewing living wage administrators in all the relevant cities, Brenner et al. found that with the exception of one city there have been no economic development projects to which living wage requirements have been applied.[4] Thus, in practice, living wage laws covering business assistance have been less, not more, comprehensive than those covering city service contracts. This of course does not explain Neumark and Adams' statistical results, it merely invalidates the interpretation of their findings that they advance.

Other, more serious, empirical problems appear to explain why Neumark and Adams find statistically significant wage and employment effects in living wage cities. Of particular note is the close correspondence between living wage implementation and changes in the minimum wage. Indeed, upon examining their data more closely we find that more than half of the living wage observations come from cities where approximately one year after the living wage ordinance was adopted the state or federal minimum wage was increased. Because Neumark and Adams find statistically significant living wage effects only with a one year lag, this creates a severe identification problem. Indeed, one of the great weaknesses of the "difference-in-difference" methodology is that it is unable to accurately separate the effects of different policy interventions if their timing coincides. As Brenner et al. (2002) have argued, increases in the minimum wage are a much more plausible source of the effects which Neumark and Adams have attributed to living wage laws, particularly given the large number of workers earning exactly the minimum wage in their sub-sample of the CPS. Ultimately, the combination of these methodological and empirical problems makes it impossible to draw any policy conclusions,

either positive or negative, from Neumark and Adams' research. Their work, however, does reinforce the methodological imperative to examine the effects of living wage ordinances at the local level, using data more suited to the task. Fortunately, this has been the approach adopted in virtually every other treatment of living wage effects, which we review in the next two sections.

III. The Economic Impact of Living Wages: Prospective Evidence

As noted in the introduction, the relatively recent emergence of living wage ordinances necessitated, at least initially, impact analysis using prospective means. The various studies reviewed in this section draw on a range of data sources, including city data on the number and size of affected contracts, surveys of potentially covered firms and workers, and government data on workers and firms including the Census, Current Population Survey, County Business Patterns, and local-area unemployment insurance (ES-202) data. The entire range of prospective work that touches on the cost of living wage laws (including internal city reports, consultant studies and other material) is not reviewed. Instead, the analysis is limited to a subset of this work that meets two criteria: (1) studies that present an estimate of the number of workers likely to be affected by the living wage law under consideration; and (2) provide some sense of the relative cost of each proposal. The former point precludes discussion of many of the internal analyses conducted by city staff or other public officials, while the latter limits the consideration of several other consultant reports such as the analysis of Los Angeles by Sander et al. (1997) and that of Chicago by Tolley et al. (1999). Table 2 summarizes the ten prospective studies which meet these two criteria and which will be discussed in this section. These data will be a prime input in our analysis of how firms are likely to adjust to higher wage mandates, an issue which we take up in section five below.

[TABLE 2 NEAR HERE]

Scope of Ordinances

As shown in Table 1, the living wage rubric has come to encompass a variety of discrete policy initiatives throughout the country, and the measures represented in Table 2 are no exception. In total, these ten ordinances comprise five distinct types of coverage. First is the most prevalent form of living wage policy, which placing wage requirements on city service contracts. This is a component of the living wage measures in Los Angeles, Miami-Dade County, San Jose, Detroit, and San Francisco. Both the San Francisco and New York measures also cover workers in the second category of social service provision, particularly homecare and childcare, while Los Angeles, San Jose, and Detroit also cover workers in the third category of economic development assistance. The living wage ordinances in Los Angeles, San Francisco, Oakland, and New York extend living wage coverage to the fourth category of lessees and other tenants on city property, while the Santa Monica and New Orleans living wage measures comprise the fifth category of geographically-based wage policies. Each of these measures also differs in terms of the provision of other benefits, with all the laws except the New Orleans city-wide minimum wage proposal offering some sort of health benefits coverage. In addition, the living wage ordinances in Los Angeles, San Jose, Oakland, and Santa Monica also making provisions for a minimum number of paid days off.

Turning to the mandated wage increases, as can be seen from the second column of Table 2, these measures vary widely in this regard as well. The range spans the relatively modest 19 percent increase represented by the $6.15 per hour minimum wage proposed for the city of New Orleans, to the 117 percent increase over the California minimum wage which the $12.50 per

hour proposal for the city of San Jose represents. Taken together, the ordinances stipulate, on average, a 71 percent increase over the prevailing minimum wage.

The third column of Table 2 presents estimates of the number of workers likely to benefit from each living wage measure. Here too, there is wide variation among the different types of living wage measures, with the number of estimated beneficiaries ranging from as low as 1,561 in San Jose, to as high as 62,000 in New York City. By and large the number of workers covered by each ordinance corresponds to the breadth of coverage in each case. For example, the citywide minimum wage proposal in New Orleans was expected to covers close to 47,000 individuals – more than 10 percent of the city's population. The San Jose living wage measure, when applied to the 235 firms holding eligible city service contracts is projected to cover only 1,561 individuals – a mere 0.2 percent of the San Jose population.[5] One anomaly appears to be the case of New York, where approximately 62,000 individuals are expected to gain from the proposed living wage law. While this figure is much higher, in absolute terms, than any of the other cities under consideration, upon closer inspection it appears to be roughly in line with the scope of the most comparable ordinance under consideration here, namely San Francisco's living wage law.[6]

Total Costs Relative to Economic Activity

We now turn our consideration to the key finding from each of the 10 studies presented in the fourth column of Table 2, the estimate of total expected costs relative to some measure of firm economic activity. Because of the range of data and methods employed in these studies, the measure of economic activity varies across the cases listed in the table. For San Jose, Oakland, Santa Monica, and New York the total cost of the living wage ordinance was measured against firm revenue, with Los Angeles differing only slightly in that firm output (the value of sales plus

inventory) was used for relative calculations. For the estimates in Miami-Dade County, the city of San Francisco, and Detroit, the total cost of covered contracts was used as the metric for measuring living wage costs, while studies in New Orleans and the Port and Airport of San Francisco compared total costs to firm operating costs (production costs net capital costs).

As we see from Table 2, these cost estimates display a fairly wide range, from 0.3 percent of firm revenue for New York City subsidy recipients, to 10.4 percent of gross receipts for luxury hotels in Santa Monica California. Several factors influence the magnitude of these relative costs. The first is obviously the size of the mandated wage increase, with more modest wage increases generating correspondingly modest average cost increases. The most prominent example of this is the case of New Orleans, where the 19 percent increase in the minimum wage was found likely to elicit a 0.9 percent increase in operating costs on average. At the other extreme is the case of San Jose, where the living wage level initially proposed was a full 117 percent above the California minimum wage. This relatively high living wage level helps explain why the average cost increase for San Jose city contractors was approximately 3 percent, as compared to the 1.5 percent average cost increase anticipated for Los Angeles city contractors, who faced a 71 percent increase over the operative minimum wage.

A second factor that influences the magnitude of the relative cost estimates in Table 2 is the incorporation of a host of indirect costs not mandated by the living wage ordinance, but likely to occur once the law is implemented. These *non-mandated* costs are part of the total cost calculation in the estimates for Los Angeles, New Orleans, Santa Monica, and the Port and Airport of San Francisco and Oakland. The distinction between the direct costs mandated by living wage ordinances, such as the higher wages and associated payroll taxes, and other *non-mandated* costs likely to result from the law is an important one. As has been well established in

the minimum wage literature, higher wage mandates exert an influence on wages beyond those directly mandated to receive pay raises.

Commonly referred to as "ripple effects" these non-mandated increases are a well-established empirical phenomenon, despite some ambiguity as to their precise magnitude. In the context of the minimum wage, available research indicates that ripple effects are typically much smaller in proportional terms than mandated increases, and they do not reach very far up the wage distribution, perhaps no more than $1 to $2 above the newly mandated minimum wage (e.g. Spriggs 1993; Card and Krueger 1995; Lim 2002). Of course, establishing the magnitude of any ripple effects that may result from a living wage is necessarily a more speculative exercise than the estimation of its direct costs. However, in the five cases where ripple effects are estimated, they display a reasonable degree of consistency, ranging from 11 percent to 26 percent of total costs. Clearly effects of this size are large enough to influence estimates of total relative cost, and any estimates which include these non-mandated costs will be correspondingly larger than those focused only on direct costs alone.

Finally, a third major factor influencing the magnitude of relative costs is the degree to which living wage coverage is concentrated in relatively low-wage industries. When such concentrations occur, relative cost figures will be slightly higher on average, in the range of four percent of revenues or operating costs. This can be seen, for example, in cost estimates for the ports of Oakland and San Francisco where the living wage ordinance falls most heavily on the restaurant and retail sectors. There may also be sectors, such as Santa Monica's hotels and restaurants, where the relative costs exceed even these figures. What is equally important to note, however, is that many, if not most firms covered by living wage ordinances will face much more modest cost increases. One example of this is the case of New York City subsidy recipients –

large firms concentrated in high wage industries such as financial services, media, and manufacturing – where cost increases are much more modest than the figures discussed above, on the order of 0.3 percent of firm revenue. Indeed, research in Los Angeles has demonstrated that, in fact, the vast majority of firms covered by that city's living wage law fall into this "low impact" category. According to Pollin and Luce (2000) a full 86 percent of covered businesses were anticipated to have cost increases of one percent or less. Thus, while it is important to acknowledge the much larger impact that living wage laws will have on a small subset of covered firms, the experience of these firms appears to be the exception and not the rule.

To summarize, despite the vastly different scope of each living wage measure, coupled with the range of methodologies and data utilized to estimate the economic impact in each case, there is a striking degree of consistency in the projected cost figures for each city. Most cost estimates fall into the range of one to two percent of firm revenue, or two to four percent of covered contract value. There are notable exceptions to these averages, such as the hotels and restaurants of Santa Monica, expected to face cost increases of approximately 10 percent of revenue. However even in the Santa Monica case if we calculate the average cost increase for affected firms other than hotels and restaurants we find that these businesses are likely to face a cost increase of approximately 2.2 percent. Establishing the magnitude of likely cost increases is crucial if we are to accurately gauge how firms will respond to living wage mandates. Before turning to these questions, however, I examine evidence that draws on the actual experiences of cities that have passed living wage measures in recent years.

IV. The Economic Impact of Living Wages: Retrospective Evidence

This section analyzes the impact that living wage laws have had on city contract costs and bidding patterns. As noted earlier, one of the principle fears voiced by opponents to living wage laws is that these measures have negative financial implications for city budgets. It is argued that if living wage laws generate substantial costs, particularly for city service contractors, these costs will ultimately be passed back to city governments. Such costs could leave cities with the unpleasant choice of either reducing the level of existing city services or raising additional revenue through higher taxes. A second concern is that living wage laws may cause some bidders to reconsider the desirability of city contracting, adversely affecting the competitiveness of the bidding process for city service contracts.

Changes in City Contract Costs

The evidence considered with regard to these questions comes from two sources: information reported by cities themselves in official documents or in interviews with scholars; and evaluations of city records conducted by independent researchers. In terms of the independent assessment of the effects of living wage laws, there are three major evaluations. Two analyze Baltimore MD, one of the first cities in the country to pass a living wage law. These reports compare approximately two dozen contracts before and after the living wage law went into effect (Weisbrot and Sforza Roderick 1996; Niedt et al. 1999). The third study considers similar evidence on changes in contract costs following living wage implementation in three New England cities: Hartford, CT, New Haven, CT, and Boston, MA (Brenner and Luce 2003). These estimates are presented in Table 3, which also provides figures from local officials concerning the changes in contract costs following living wage implementation. These include information on several social service contracts in Dane County, WI and San Francisco, CA (Elmore 2003) and the 31 covered contracts in Corvallis, OR (Brewer 2001).

[TABLE 3 NEAR HERE]

Turning to the evidence, the most important point is that cities have had a wide variety of experiences with living wage laws as they relate to city service contract costs. For example, in the Baltimore case, both studies found only modest increases in nominal contract costs (in the aggregate) after the implementation of the living wage law. Weisbrot and Sforza-Roderick (1996) reported a nominal increase of less than a quarter of a percent in the total cost for all 19 contracts in their study following living wage implementation, while Niedt et al. (1999) incorporating an additional year's worth of data and several additional contracts, found a nominal increase of 1.2 percent for the 26 contracts they analyzed.[7] In both cases, these nominal increases were lower than the rate of inflation, implying that in real terms city contract costs decreased in Baltimore following the implementation of that city's living wage law. This experience of declining real contract costs is not unique to Baltimore. As is evident from Table 3, both Boston, MA and New Haven, CT also witnessed a decline in the real value of covered contracts, and the nominal increase in contract costs for covered social services in Dane County, WI and San Francisco, CA was below the rate of inflation in each locality.

Although aggregate costs declined in real terms in each of these cases, it is important to note that many of these studies documented substantial variation across individual contracts. For instance, one small janitorial contract in Baltimore increased in nominal terms by 47.1 percent, while the contract for summer food services declined by 11.6 percent. Meanwhile the contract for bus services, by far the largest covered contract in Baltimore, rose by only 2.1 percent (Niedt et al. 1999). Given this wide range of experience *within* cities, it is not surprising that there are also substantially different experiences *between* cities, with several reporting rising aggregate contract costs following living wage implementation. In Corvallis, OR, the Finance Director

reported a 9.1 percent increase in costs for contracts under the living wage mandate. These increases were sharpest for bus repair and maintenance (34%), for custodial services (21%), and for the humane society contract (17%) (Brewer 2001). Brenner and Luce (2003) also report cost increases of this magnitude for the city of Hartford, CT, where the cost of security guard services and temporary office help together rose 33.4 percent following living wage implementation. One important characteristic of these services in Hartford is that they are bid on the basis of an hourly rate, with an estimated but not guaranteed number of hours of service to be performed over the life of the contract. The authors argue that soliciting bids on the basis of an hourly rate is causally linked to the higher contract cost increases witnessed in Hartford. They conclude that this method of bidding blunts the very forces of competition that have held down cost pass-throughs in other cities.

Elmore (2003) also records a wide range of experience in his review of living wage implementation in 13 cities. He notes first that in each case he considered, city officials reported higher service contract costs in absolute dollar terms, although these increases range from very slight (approximately $9,000 in Ypsilanti, MI) to quite substantial (over $3.7 million for all human service contracts in San Francisco, CA). Elmore also documents the enormous variation in cost increases among individual contracts. He describes cases where contract costs rose by a substantial amount, such as a janitorial contract covered by Warren MI's living wage law which rose by 22%, as well as cases where contract costs declined in absolute terms, as with three human service contracts in Dane County, WI. This variation only serves to underscore our earlier observation that many factors influence city contract costs, with the living wage ordinance being only one among them.

Changes in Bidding Patterns

We next turn to the effect of living wage laws on bidding patterns. Here too city experiences vary widely. In Baltimore, for the 14 contracts that were re-bid (as opposed to renewed) following living wage implementation, only three displayed an increase in the number of bidders, while eight saw a decrease. In total, there were 93 bids tendered for these fourteen contracts prior to the living wage, and only 76 bids tendered following the law (Weisbrot and Sforza-Roderick 1996). These figures suggest that Baltimore's living wage law may have had an impact on the number of bidders willing to compete for city contracts following living wage implementation. On the other hand, Brenner and Luce (2003) suggest that the effect of living wage laws on bidding patterns can sometimes have the opposite effect. They find, for example, that covered contracts in Hartford saw a 20 percent increase in the number of bids tendered following living wage implementation, while competitively bid contracts in Boston saw no change in the number of bids tendered. By contrast, New Haven saw the total number of bids tendered decline by 3, from 32 to 29.

Regardless of the precise quantitative effect of living wage laws on bidding patterns, results from Elmore's (2003) study indicate that many city officials view living wage laws as at least compatible with, if not conducive of, a more competitive bidding process. As one example, a policymaker in Ypsilanti Township, MI remarked that following living wage implementation, the Township's major contracts had "more bidders than ever before, at even better rates" (Elmore 2003, p. 15) This city official attributed the lower bids to the living wage law, noting that the ordinance subjected contracts to a competitive bidding process with fixed wage and benefit requirements, which in turn forced bidders to "be tighter and provide less of a profit margin" (ibid). Elmore also documents a similar experience in Alexandria, VA, where city officials stated that "[t]here have been some competitive advantages to re-bidding, we have seen

some incumbents who lost on the second go-round, and it may be due to the bidding process" (ibid). Although fewer cities have examined this issue directly, there is some evidence that bidding has not been adversely affected by living wage laws. For example, the Corvallis, OR Finance Director reported that although several firms indicated they would not bid on city business because of the living wage, in fact, every vendor contacted has submitted a bid, and "and the bids have continued to be competitive" (Brewer 2001, p. 1). Similarly, in Hayward, CA the Acting Finance Director reported that all contracts have remained competitively bid, and that it was the "staff's opinion that productivity and service quality has not been adversely affected" (Finance Director's Office 2000, p. 3).

Upon reflection, it is not surprising that living wage laws have had a relatively benign effect on the bidding process for city service contracts. Available evidence indicates that when savings accrue from the contracting out services, the majority of these savings are the result of providing lower wages and benefits to workers performing the newly privatized services (Kettl 1993; López-de-Silanes 1997). In such a context living wage ordinances can reduce the ability of some bidders to undercut their competition by lowering wage and benefit levels. Thus, a living wage law has the effect of "leveling the playing field," forcing contractors to compete with one another along other dimensions such as service quality. Similar sentiments among city officials in New England are documented by Brenner and Luce (2003).

Several conclusions emerge from the evidence discussed above. First, although cities have had a wide range of experiences with living wage laws, the preponderance of evidence indicates that living wage ordinances are unlikely to cause large increases in city contract costs. There are, of course, specific contracts or types of services for which cost increases will occur, but even in these cases the method of bid submission and the competitiveness of the bidding

process can modulate cost pass-throughs to the city. With regard to the bidding process itself, here, too, the affects of living wage laws are highly variable. There are some instances – such as in Baltimore – where living wage ordinances appear to shrink the pool of willing bidders, although there are also examples where living wage ordinances appear to have strengthened the bidding process. Available evidence indicates that city officials do not see these measures as an impediment to competitive bidding, and may, in fact, consider them an inducement. This heterogeneity in bidding experiences also serves to underscore the fact that the living wage ordinance is only one of many factors influencing the competitiveness of city procurement. It also reinforces the conclusion drawn earlier that firm behavior in the face of higher wage mandates is not nearly so straight-forward as often assumed in the realm of theory.

V. How Do Firms Adjust to Living Wage Mandates?

Thus far, we have gained an understanding of the range of costs businesses are likely to experience when faced with living wage mandates as well as a sense of how city service contract costs and bidding practices have been affected by living wage laws. I will now try draw out the links between these two elements, turning attention to the various means by which covered firms can confront higher wage mandates. The objective is to briefly assess which of the various adjustment mechanisms firms will deploy when faced with living wage-induced cost increases. The assessment is based on the relative magnitude of these costs, the range of outcomes observed at the city-level, and, where possible, direct evidence on firm behavior following living wage implementation.

When reviewing how firms respond to wage floors, two types of adjustments – layoffs and relocations – are the most frequently discussed. Yet these are not the only options firms have

at their disposal, nor do they appear to be the most likely means by which firms address the higher labor costs associated with living wage laws. Indeed there are three other adjustment channels that firms can employ: raising prices; increasing firm productivity; and increasing the share of firm income going to low wage workers. Available evidence indicates that adjustment is more likely to occur through some combination of these three mechanisms, as they can be accomplished more readily and at lower costs than either layoffs or firm relocation.

Raising Prices

For most firms covered by living wage laws, the process that would be least costly and disruptive would be to simply raise prices to reflect increased costs. But a firm's ability to raise prices depends on the competitiveness of the market in which they operate as well as how price sensitive their customers are (i.e. on the elasticity of product demand). If we first consider city service contractors, it is important to recognize that these firms will typically face at least one serious competitor during the bidding process (Rehfuss 1989). If living wage costs are modest, on the order of 1 to 2 percent, bid prices from covered vendors may not be affected, as these firms are unlikely to sacrifice the reliable income stream and health margins associated with government contracts over such modest changes in operating costs. This helps explain why so many cities have not witnessed rising contract costs following living wage implementation, since most covered vendors fall into this category.

If living wage costs are more substantial, however, it is likely that firms will press the city to absorb some or all of these costs. The absolute number of these "high impact" firms appears to be small. For example, Pollin and Luce (2000) estimate that roughly seven percent of covered contractors in Los Angeles faced cost increases greater than 10 percent. It is important to recognize a significant portion of the higher costs can be absorbed by the firm through other

means, obviating the need to pass them back to city governments. However, it is nonetheless true that even full pass-through of such costs is a small fraction of the total city budget – 0.2 percent in the Los Angeles example discussed above. Cities can also use competition (or the threat of it) to inhibit full cost pass-through, as illustrated by the experience in the city of Pasadena, CA. There, covered vendors agreed to absorb between 40 to 55 percent of the higher costs associated with living wage if the city extended their current service agreements rather than put them out for competitive bidding. There are also ways in which cities can modulate cost pass-throughs by changing the structure of contracting. One example of this includes Multnomah County, OR, where the living wage policy increased service delivery costs by 27 percent. However, the county was able to limit their contract cost increase to only 5 percent by consolidating three formerly separate janitorial services (Facilities and Property Management Division, n.d.). Brenner and Luce (2003) have also showed that pass-throughs are highest for contracts bid on a unit-cost basis (sometimes known as "cost-plus" bidding), indicating that cities can also limit cost pass-throughs by changing the terms on which bidding occurs.[8]

What about those firms not directly contracting with city governments? The largest set of these firms operate concessions on city property, selling goods and services to the public in airports, ports, and other public facilities. While their ability to pass along higher costs will ultimately be governed by their demand elasticities, it is important to stress that these firms operate in highly circumscribed markets, in places such as airports and sports arenas.[9] To the extent that there are few readily available alternatives, this strengthens their ability to pass costs on to consumers in the form of higher prices. Available evidence also indicates that price increases on the order of 3 to 4 percent are within a range which does not undermine demand (Card and Krueger 1995).

Productivity and Redistribution

In examining these two adjustment channels, we start from the premise that covered firms are experiencing productivity gains on the order of 1 percent per year prior to living wage implementation. This is a reasonable assumption, given that it is half the annual average for all U.S. businesses over the last full business cycle. It also implies that for most covered firms a large portion – if not the entirety – of living wage costs can be absorbed with productivity increases that are the normal course of business development. There are at least two reasons, however, that we would expect firm productivity to grow faster following living wage implementation. First, firms are likely to be more attentive to potential cost saving measures, as well as more open to changes in work organization, that could also compensate for the slightly higher labor costs they now face when doing business with the city. Second, the higher wages associated with living wage laws are also likely to have a salutary effect on employee performance. In the latter case, the benefits of higher wages have several sources. For example, as the efficiency wage literature discussed in section two has made clear, paying higher wages is likely to increase the individual effort and motivation of those workers receiving raises. These higher wages are also likely to reduce other less appreciated labor costs, such as turnover and absenteeism. While their precise costs are difficult to quantify, the evidence is clear that living wage laws can dramatically effect on both turnover and absenteeism. The most striking example of these effects is from the San Francisco Airport, where Reich et al. (2003) have shown that turnover fell by up to 80 percent for several low wage occupations following living wage implementation. Similarly, Howes (2002) estimates that the San Francisco living wage law contributed to a 20 percent decline in turnover for covered homecare workers. Turnover reductions of this magnitude are not likely to generate savings sufficient to fully offset living

wage costs. However they will no doubt ease the financial pressure which otherwise would spur firms to pass such costs back to the city in the form of higher contract costs.

Of course, channeling productivity increases into higher wages for low-paid employees entails a redistribution of income within covered firms. By the same token, for most firms this adjustment would occur only once, in the first year of living wage implementation. Subsequent productivity increases could be absorbed by the firm in whatever manner they deem appropriate, with lower paid workers still receiving a living wage.

Layoffs and Firm Relocation

There are few reasons to expect that living wage laws will lead to layoffs or other negative employment consequences. This is sensible given the modest costs for most firms covered by living wage laws. However, even for "high impact" firms, there are many reasons to believe that some combination of prices, productivity, and redistribution will preclude firms shedding labor to comply with living wage laws. Recent empirical evidence also supports these conclusions. Three examples stand out. At the San Francisco airport, Reich et al. (2003) report that total employment in covered firms increased by 15 percent following living wage implementation. Brenner and Luce (2003) report that firms covered by the Boston living wage ordinance also saw employment increase by 15 percent over pre-living wage levels. Howes (2002) estimates that the number of homecare workers covered by San Francisco's living wage law increased by 54 percent. In the Boston case, another particularly important finding is that employment grew *faster* for those firms who were force to raise wages to comply with Boston's living wage law than for those who did not (Brenner and Luce 2003). On a full-time equivalent basis, employment growth was nearly 50 percent higher for firms raising wages than for those who did not, with the bulk of this increase resulting from firms shifting part-time workers to full-

time schedules. While this does not suggest that higher wage floors "caused" higher employment, it does demonstrate that the two are at least compatible, particularly in a context of rapid economic growth. It also bolsters the contention that other macroeconomic factors are decisive in determining the precise level of employment.

When considering relocation, there are few reasons to believe that living wage laws will create substantial incentives for firms to move out of their current geographic area. First, as noted many times before, the magnitude of living wage costs are very modest for most covered firms, making relocation infeasible on a purely cost basis. Of even greater consequence is the fact that most living wage laws regulating city service contracting apply their wage mandates to firms regardless of their location, so firms are unable to avoid compliance by relocating out of a given locality. Relocation incentives pose a more serious issue for some geographically-based living wage measures, such as the Santa Monica and New Orleans living wage proposals. However, even in these two cases the available evidence indicates that that the most heavily impacted businesses –such as the hotels and restaurants that comprise the core of both cities' tourism industry – are also heavily tied to their current locations (Pollin and Brenner 2000; Pollin et al. 2002). Relocation will no longer allow firms to compete in the tourist market, which is the core of their current operations. This, in turn, implies that the costs of such a measure will likely exceed the expected benefits.[10] Thus, on balance, the evidence indicates that due to the specific manner in which living wage laws function, firms under their mandate will find adjustments through some combination of prices, productivity, and redistribution preferable to the more disruptive options of layoffs and relocation.

VI. Conclusion

This paper has reviewed the economic effects of local living wage ordinances, drawing on a range of prospective and retrospective research. We have seen that these laws are highly specific, localized measures, which precludes using the national data sets and statistical techniques that are the mainstay of modern labor economics can offer us little insight into living wage dynamics. We have also seen that, much like recent results for more conventional minimum wage measures, the evidence suggests that firms respond to living wage laws in ways not readily anticipated by competitive models of the labor market. There appear to be several explanations for this empirical regularity. First, most firms covered by living wage laws experience only modest costs – on the order of 1 to 2 percent of total economic activity – which carries with it the implication that layoffs or firm relocation are likely to be far more disruptive and costly than other channels of adjustment. For some firms, the costs associated with living wage compliance are much greater: 10 percent or more of economic activity. However even for this set of firms, the evidence suggests that some combination of price increases, productivity enhancements, and internal redistribution are the primary means by which they adjust to these measures.

While these alternative adjustment measures make it unlikely that firms will shed workers or move from their existing base of operations, they nonetheless have potentially serious implications for city budgets. While there is clearly evidence that living wage laws raise the price of certain city service contracts, there is also a preponderance of evidence that this is not a generalized phenomenon. Indeed, both internal adjustments such as productivity increases or redistribution, as well as the external force of competition, appear to preclude the full pass-through of living wage-related costs to city budgets. While it is important to realize that even full cost pass-through by high impact firms is a negligible percentage of city finances – less than 0.2

percent for our example of Los Angeles – it is also important to acknowledge that cities can affect the degree to which firms pass along living wage costs by both altering the terms for certain services, as well as changing bidding procedures for others. The weight of the accumulated evidence indicates that living wage laws have an important impact on the living standards of a modest number of beneficiaries, while diffusing the costs broadly among city service contractors and the general public.

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Table 1: Number of Living Wage Ordinances with Selected Coverage

Ordinances that cover:	Number	% of total
Service contractors	74	77%
Recipients of economic development assistance	36	38%
Subcontractors	31	32%
City employees	21	22%
Concessionaires, lessees, or tenants	9	9%
Airports	4	4%
Private employers in geographically-based zone	3	3%
Total ordinances	96	100%

Source: Luce (2003).
Note: Includes ordinances passed through December 2002, with the exception of cities for which copies of the ordinance could not be obtained. The table also does not include university living wage ordinances.

Table 2: Economic Impact of Various Living Wage Ordinances
Prospective Evidence

City (Source)	Wage Increase as a Percentage of the Minimum Wage	Workers Covered	Total Cost Relative to Economic Activity	Relative Measure
Los Angeles, CA (Pollin and Luce 2000)	71%	7,626	1.5%	Firm Output
Miami-Dade, FL (Nissen 1998)	66%	1,956	1.8% [a, b]	Contract Value
San Jose, CA (Benner and Rosner 1998)	117%	1,561	3.0% [a, b]	Firm Revenue
San Francisco, CA (Reich et al. 1999a)	91%	12,380	3.9% [a, b]	Contract Value
Detroit, MI (Reynolds et al. 1999)	62%	2,300	2.5% [b]	Contract Value
San Francisco, CA Airport and Port (Reich et al. 1999b)	91%	14,190	2.7% (Airport) 4.6% (Port)	Operating Costs
New Orleans, LA (Pollin et al. 2001)	19%	47,050	0.9%	Operating Costs
Oakland, CA Airport and Port (Zabin et al. 1999)	44%	3,111	1.5% (Airport) 4.31%(Port) [c]	Firm Revenue
Santa Monica, CA (Pollin and Brenner 2000)	87%	2,078	3.9% (All) 10.4%(Hotel) 9.6% (Restaurant)	Firm Revenue
New York City, NY (Sonn et al. 2002)	57%	62,000	0.3% [b,d]	Firm Revenue

a. Proportions are for city service contracts only.
b. Calculations include direct costs only.
c, Figure is for the Real Estate Division at the Port of Oakland only.
d. Figure is for New York City subsidy recipients only.

Table 3 Economic Impact of Various Living Wage Ordinances
Retrospective Evidence

City (Source)	Fiscal Year Living Wage Implemented	Wage Increase as a Percentage of the Minimum Wage	Average Annual Increase in Real Contract Costs [a]	Number of Contracts Reviewed
Baltimore, MD (Weisbrot and Sforza-Roderick 1996)	FY 96-97	44%	-1.9%	19
Baltimore, MD (Niedt et al. 1999)	FY 96-97	44%	1.2% [b]	26
New Haven, CT (Brenner and Luce 2003)	FY 97-98	56%	-10.9%	9
Boston, MA (Brenner and Luce 2003)	FY 99-00	57%	-7.3%	29
Dane County, WI (Elmore 2003)	FY 99-00	54%	2.8% [b,c]	12
Corvallis, OR (Brewer 2001)	FY 00-01	38%	9.1%	31
San Francisco, CA (Elmore 2003)	FY 00-01	57% (with health) 78% (without)	1.0% [b,c]	-
Hartford (Brenner and Luce 2003)	FY 00-01	43% (with health) 71% (without)	33.4%	2

a. Percentages are weighted by contract value.
b. Contract cost increases are measured in nominal terms.
c. These figures are for the human services contracts covered by the living wage law in each locality.

Notes

[1] These figures do not include the nine cities where living wage laws have been passed and the subsequently repealed.

[2] It should also be noted that neither the Katz and Krueger (1992) analysis of the minimum wage effect on fast food employment in Texas, nor the similar analysis conducted by Spriggs (1993) in North Carolina and Missisippi, are from areas or periods where the minimum wage was relatively high.

[3] Although empirical assessments of efficiency wages are much rarer than theoretical treatments, there appears to be solid evidence of their existence (e.g. Cappelli and Chauvin 1991; Levine 1992; Campbell 1993). More importantly, there is a solid empirical connection between higher wages and lower indirect labor costs such as turnover and absenteeism whether due to efficiency wages or not (e.g. Freeman and Medoff 1984; Raff and Summers1987; Krueger and Summers 1987; Reich et al. 2003; Howes 2002).

[4] It is important to distinguish between monitoring or enforcing living wage laws and actually applying them to business assistance recipients. All business assistance cities were indeed enforcing their laws in the sense that they were monitoring economic development projects to see if any fell under the strictures of the living wage law. However, except for San Antonio, TX, Brenner et al. (2002) concluded that no business assistance cities had actually had a case where the law was applied to an actual business assistance recipient.

[5] Population figures are from 1999, drawn from the most recent Census Bureau estimates: http://eire.census.gov/popest/archives/place/SC100K-T1.txt.

[6] In fact, the San Francisco ordinance, with a city-wide population of approximately 747,000 in 1999, actually covers a proportionally greater share of the city population than does New York, with a city-wide population of 7.4 million, 1.7 percent versus 0.8 percent respectively.

[7] It bears note that the Weisbrot and Sforza-Roderick study was attacked by the anti-minimum wage Employment Policies Institute shortly after its release for fabricating data and distorting results [Employment Policies Institute, 1998 #78]. These charges, however, were without substance, as demonstrated by the Preamble Center staff [Preamble Center, 1999 #77]. More importantly, as we discuss, the Weisbrot and Sforza-Roderick conclusions were supported by the subsequent analysis of Niedt et al.

[8] Unit-cost bids are typically submitted as an hourly rate for services performed, such as for security guard services or temporary office assistance. This form of bidding is most prevalent when cities are unsure of the exact level of services required in the future, and want to maintain the contractual flexibility to increase or decrease their usage as concrete needs are identified.

[9] It is also true that most firms highly impacted by geographically-based measures also have very localized product markets. This was the case for firms covered by living wage proposals in Santa Monica, CA, and New Orleans, LA (Pollin and Brenner 2000; Pollin et al. 2002).

[10] Pollin et al (2001) and Pollin and Brenner (2000) estimate that price increases, even on the order of 10 percent, are not an impractical response to living wage mandates, given the sharp price differences that already exist between the tourist and non-tourist segments of the hotel and restaurant market within Santa Monica and New Orleans.

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Mandated Wage Floors and the Wage Structure: New Estimates of the Ripple Effects of Minimum Wage Laws

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Mandated Wage Floors and the Wage Structure: New Estimates of the Ripple Effects of Minimum Wage Laws

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Abstract:

Minimum wage laws have become a key political issue, following on the heels of over 130 successful living wage campaigns around the country. In the debates surrounding these mandated wage floors, one recurring issue has been whether the legislation has wider-ranging impacts on wages than the legally-required raises alone. Advocates on both sides of the debate dispute the potential magnitude of 'ripple effects'- the non-mandated raises given by employers to maintain a similar wage hierarchy before and after a change in the wage floor. These ripple effects have the potential to greatly expand the overall impact of mandated wage floors. This study uses data from twenty years of the Current Population Survey to assess the magnitude of ripple effects in the context of variations in minimum wage laws, and looks specifically at the retail trade sector to model the potential magnitude of ripple effects under living wage ordinances, where the 'bite' of the legislation would encompass a larger share of the workforce.

JEL Code: I3, J31, J38, J48, J88
Keywords: ripple effect, wage spillover, wage norms, minimum wage, living wage, wage distribution, retail trade, low wage

Mandated Wage Floors and the Wage Structure:

New Estimates of the Ripple Effects of Minimum Wage Laws

Introduction

Statewide minimum wage ballot initiatives have made minimum wage laws a key political issue for the 2006 mid-term elections. These political campaigns follow on the heels of over 130 successful living wage campaigns to enact municipal-level wage minimums in the last ten years. Moreover, the relatively high wage floors set by living wage laws (nearly double minimum wage levels, on average) distinguish them from most minimum wage laws, raising the stakes of winning or losing these political fights (Brenner and Luce, 2005). This recent upsurge of interest in establishing new and higher wage floors has recharged the academic, as well as, political debate around the economic impact of mandated wage floors.

One reoccurring issue in these debates is whether mandated wage floors have a more wide-ranging impact on wages than the legally-required raises alone. In response to new wage minimums, some employers give non-mandated raises, or "ripple effect" raises, in order to maintain a similar hierarchy of wages before and after the change in the wage floor. By expanding the number of workers who receive raises, these ripple-effect raises have the potential to considerably change the overall impact of mandated wage floors.

Both sides of the political debate around mandated wage floors have a stake in arguing that ripple effects are both large and small. Large ripple effects allow proponents

to argue that mandated wage floors have an even greater potential to reduce poverty by expanding the number of beneficiaries. Opponents, however, can argue that large ripple effects seriously intensify the economic strain on employers from such laws. Small ripple effects enable proponents to minimize estimates of the economic costs to employers of mandated wage floors. But, likewise, small ripple effects allow opponents to minimize their estimated benefits. Regardless of one's political position, understanding the extent and size of ripple effects is an important part of evaluating mandated wage floors.

Past research on mandated wage floors does not provide a clear picture of the size and extent of ripple effects. I attempt to fill this gap by providing detailed empirical estimates of the ripple effects produced by state and federal minimum wage increases in the United States from 1983-2002 using Current Population Survey (CPS) data.

I find that adding ripple-effect raises to mandated raises dramatically increases the number of minimum wage beneficiaries. These raises substantially increase the costs of minimum wage increases to employers: the average ripple effect "multiplier" is 2.5, meaning that the total cost increase from a minimum wage increase—including both mandated and ripple-effect raises—is 250 percent of the cost increase from mandated raises only. This occurs even though the extent of ripple effects is limited. Raises from ripple effects only extend up to workers earning 123 percent of the minimum wage prior to the increase.

I also look at the special case of the retail trade industry[1] where minimum wage workers are concentrated. If mandated raises from a minimum wage increase impacts the

[1] Because of the conversion from the SIC to NAICS industry classification system in 2003 in the CPS, retail trade includes different industries prior to 2003 than in the years 2003 and on. In particular, prior to 2003, retail trade included "Eating and Drinking places." The NAICS system introduced a new category,

retail trade wage structure more dramatically than in any other industry, do retail trade employers also make more dramatic adjustments to their wage scales through ripple-effect raises? I find that retail trade employers do not make more extensive adjustments despite the stronger "bite" of the minimum wage in this industry. Instead, mandated raises make up a larger proportion of the overall cost increases faced by employers than ripple-effect raises. The ripple-effect multiplier in retail trade is smaller at 1.9 than in the general case.

Accounting for ripple effects also changes the demographic profile of minimum wage beneficiaries. Adult workers with greater financial responsibilities in their families make up an even larger majority of minimum wage beneficiaries when ripple effects are accounted for, increasing from 61 to 69 percent.

These ripple effect estimates provide an important insight into the likely impact of the increasingly popular living wage laws. The retail trade analysis indicates that when a mandated wage floor has a stronger bite, the ripple-effect multiplier shrinks. Because living wage laws have an even stronger "bite" among covered employers than in the case of minimum wage laws among retail trade employers, the results of this research suggests that the cost increases that employers bear (and the wage benefits that workers obtain) from living wage laws will be primarily from mandated raises rather than ripple-effect raises. This insight provides a much needed guideline for assessing the economic impact of living wage laws.

What are Ripple Effects?

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separate from retail trade called "Accommodation and Food Services" that includes "Food Services and Drinking Places." Retail trade in this paper refers to the SIC industry category.

Ripple effects are the raises that employers feel compelled to give workers beyond those legally required when a mandated wage floor is increased. Consider this basic scenario: If the current $5.15 federal minimum is increased to $6.15, employers are legally required to raise the wages of all covered workers earning less than $6.15. However, without a ripple effect, workers earning $6.15 prior to the minimum increase will fall in their relative wage position: their wage position falls from $1.00 above the bottom of the wage structure to the bottom of the wage structure. Moreover, these workers will be earning the same wages as workers who had previously earned inferior wages. Such a fall in relative wage position could damage worker morale, and therefore, productivity. To avoid this, employers extend raises above the wage floor to maintain a consistent wage hierarchy. As a result, workers earning $6.15 prior to the increase may receive a "ripple effect" raise to keep their wage position above the bottom of the wage structure.

Ripple effects may alternatively be caused by employers substituting low-skilled workers with high-skilled workers. In response to an increase in the wage floor, employers may increase their demand for high-skilled workers who typically earn wages above the minimum. This increased demand for high-skilled workers can push their wages upwards. Consequently, not only are the wages of workers earning the minimum receiving raises but so too are workers at higher wage rates. Regardless of the cause, minimum wage increases have the potential to raise the wages of more jobs than those bound by the minimum through ripple effects.

The Extent and Size of Ripple Effects

This study advances beyond past research by providing detailed estimates of the extent and size of ripple effects while controlling for macroeconomic trends and changes in workforce composition.[2,3] Specifically, I use Current Population Survey data from 1983 to 2002 and regression analysis to estimate how state and federal minimum wage changes impact wage growth at various points of states' wage distributions as defined by wage percentiles.

The regression estimates indicate that the extent of the ripple effect is limited to a narrow band of wages above the minimum wage and that the raises quickly diminish the higher the worker's wage rate. The impact of minimum wage increases is strongest at the 5th wage percentile which is, on average, equal to the minimum wage (see figure 1). The estimated wage elasticity of 0.44 indicates that for every 10 percent increase in the minimum wage, the 5th wage percentile increases 4.4 percent. Take for example, the last federal minimum wage increase of 1997 from \$4.75 to \$5.15, an eight percent increase. Workers earning wages around the old minimum of \$4.75, on average, received a raise of four percent (0.08 x 0.44 = 0.35), or \$0.17, to \$4.92 (see table 1).

Why doesn't the fifth wage percentile reflect increases equivalent to the minimum wage increase (i.e., have a wage elasticity of one)? Because the 5th wage percentile drifts

[2] The regression analysis allows for both contemporaneous and lagged minimum wage effects on the wage percentiles.

[3] Neumark, Schweitzer and Wascher (2004) also offer a serious attempt to address these weaknesses. Using regression analysis, they estimate how minimum wage changes affect wage growth across the wage distribution during 1979-1997. Unfortunately, their study likely produces unreliable results because their model specification appears to exacerbate the measurement error in their wage measure (see Wicks-Lim (2005) for a full critique of their methodology). Consequently, their unlikely results—such as a positive minimum wage effect on the wages of workers earning very high wages and a negative minimum wage effect for almost every segment of the wage distribution one year after a minimum wage increase—appear to reflect problems in their methodology.

above and below the minimum wage over time and across states, the wage elasticity reflects an average of the wage responses to minimum wage changes among a combination of sub-minimum wage workers, minimum wage workers, and workers earning wages very close to, but above, the minimum wage. From other analyses not presented here, I found that sub-minimum wage workers do not receive wage raises equal to minimum wage increases.[4] Workers earning wages very near but above the minimum do not require equivalent wage increases to minimum wage increases to meet or exceed the new minimum. These two influences push the wage elasticity of the 5th wage percentile below one. While those workers earning exactly the minimum wage likely move in tandem with the minimum wage, the workers earning wages *around* (i.e., above and below) the minimum move less than the minimum wage.

Workers with wages around the 10th wage percentile receive, on average, a 2.5 percent increase for every 10 percent increase in the minimum (i.e., a wage elasticity of 0.25). The 10th wage percentile is typically 115 percent of the minimum wage prior to the increase. Applying this to the 1997 federal minimum increase, workers earning around $5.22 got two percent raises (0.025 x 0.08 = 0.02), roughly ten cents to $5.32.

The highest point in the wage distribution with a detectable impact from minimum wage changes is the 15th wage percentile. Since there is no detectable impact at the 20th wage percentile, the 15th wage percentile approximates the lower-bound of the ripple effect's upper limit. Workers with wages around the 15th wage percentile typically earn 123 percent of the minimum wage. These workers receive, on average, a 1.4 percent raise for every 10 percent increase in the minimum wage (i.e., a wage elasticity of 0.14).

[4] One reason for this is that some sub-minimum wage workers are subject to tip credit allowances which do not automatically change when minimum wage levels are changed.

The eight percent federal minimum increase in 1997 produced a one percent raise for workers earning around $5.74 (0.14 x 0.08 = 0.01), or seven cents, to $5.81.

These estimates indicate a compressed wage distribution after the minimum wage increase. In the wage distribution after the minimum wage increase, the 15th wage percentile sits within about 65 cents of the $5.15 wage floor. Prior to the minimum wage increase, the 15th wage percentile was almost one dollar higher than the $4.75 wage floor.

The Overall Impact of Ripple Effects

Despite the limited extent of ripple effects, these wage raises nevertheless considerably expand the overall impact of minimum wage increases. This result is driven by the fact that the number of workers with wages just above the minimum greatly outnumbers workers earning the minimum. To illustrate, I present in table 1 estimates of the wage raises that occurred in response to the 1997 federal minimum wage. The third column presents the number of workers that earned wages around the 5th, 10th, and 15th wage percentiles prior to the federal minimum increase. Since the 5th wage percentile sits right at the minimum wage level, these workers are assumed to receive mandated raises. Based on this example, almost triple the number of minimum wage workers (those earning wages around the 5th percentile) earned wages around the 10th and 15th wage percentiles. In other words, the high concentration of workers near but above the minimum wage produces a large ripple effect.

To provide a measure of how important the ripple effect is in the overall impact of minimum wage increases, I calculated a "ripple-effect multiplier" (see table 1). To do

this, I estimated the total change in employers' annual wage bills caused by the 1997 federal minimum wage increases due to mandated raises and ripple-effect raises separately. The ripple-effect multiplier then quantifies how much the ripple-effect raises multiply the change in employers' annual wage bills from mandated wage raises alone. Specifically, I multiplied the number of workers that earned wages around each wage percentile (column 3) by their average raises as estimated by the regression analysis (column 4), their average hours per week (column 5) and their average weeks worked per year (column 6).

I estimate that employers responded to the federal minimum wage increase by providing roughly 4 million workers with $741 million in mandated raises (column 7). Ripple effects provide another 11.5 million workers with $1.3 billion dollars in raises (column 8), nearly quadrupling the number of minimum wage beneficiaries and almost tripling the overall increase to employers' annual wage bills. In other words, with regard to the overall change in the wage bill, ripple effects multiply the cost increase to employers by 270 percent ((741.0+1280.3)/741.0) producing a ripple-effect multiplier of 2.7. Similar calculations based on the other three federal minimum wage increases of the 1990s produce an average ripple-effect multiplier of 2.5.

Is the Ripple Effect Stronger When the Minimum Wage Has a Stronger "Bite"?

To examine whether minimum wage increases with a stronger "bite" produce stronger ripple effects, I did a separate regression analysis on retail trade workers. I focused on the retail trade industry because a greater proportion of retail trade workers

earn the minimum wage than in any other industry. In 2002, for example, the Bureau of Labor Statistics reported that three percent of all workers paid hourly earned wages at or below the federal minimum. Among hourly-wage workers in the retail trade industry the proportion is more than twice as high at eight percent. Clearly, the minimum wage has a stronger "bite" in the retail trade industry compared to other industries. If a minimum wage increase requires retail trade employers to give a higher proportion of workers mandated raises as compared to other employers, retail trade employers may also need to make more extensive adjustments to their wage scales in the form of ripple-effect raises.

The impact of minimum wages in the retail trade industry has virtually the same pattern as that found for the entire economy, both in terms of extent and magnitude (see figure 2). Instead of producing a qualitatively different effect, the high concentration of minimum and near minimum wage workers in the retail trade industry provides a more detailed view of the minimum wage effect because each wage percentile characterizes a narrower range of wages.[5] As a result, the high wage elasticity of 0.73 for the 10^{th} percentile of the retail trade industry is a more precise estimate for minimum wage workers than the lower wage elasticity of 0.44 for the 5^{th} wage percentile across industries.

The limited extent of the ripple effect in combination with a relatively high concentration of minimum wage workers in the retail trade industry suggests that ripple-effect raises will contribute less to the overall impact of minimum wage increases in this

[5] To see this, consider that the 10th wage percentile is equal to, on average, 99 percent of the minimum wage. The 15th wage percentile, the next percentile for which I produce estimates for, is only four percentage points higher than the 10^{th} wage percentile in terms of its position relative to the minimum, at 103 percent of the minimum wage. Across industries, the 5th wage percentile—the percentile closest to the minimum—is, on average, 100 percent of the minimum wage. The 10th percentile, the next percentile for which I produce estimates for, is much further from the minimum than the 5th wage percentile, at 112 percent of the minimum wage.

industry. In table 2, I repeat the calculations of table 1 for the retail trade industry only. As expected, the heavy concentration of workers at the wage floor in this sector diminishes the size of the ripple-effect multiplier to 1.6. Similar calculations for the other three federal minimum wage increases of the 1990s suggest an average ripple-effect multiplier for the retail trade industry of 1.9.

The Impact of Ripple Effects on Cost Increases to Businesses

The actual economic impact of the costs increases associated with minimum wages can only be assessed when put into context with some measure of businesses' capacity to absorb these costs. One way to evaluate this capacity is to compare the increased costs to businesses' sales revenue. I did such a comparison in a 2004 study with Robert Pollin and Mark Brenner of the economic impact of proposed $6.15 state minimum wage in Florida, increasing the effective minimum wage by one dollar from the federal minimum of $5.15. We estimated the associated cost increases of the minimum wage proposal for both mandated and ripple-effect raises as a percentage of businesses' sales revenue. The estimated ripple effects were of a similar, though somewhat larger, magnitude as those reported in this current study.

We found that, on average, the total cost increases associated with the Florida minimum wage proposal amounted to less than one percent of a business' sales revenue. The mandated raises alone accounted for less than one-half of one percent of businesses' sales revenue. Viewed in this context, it is clear that the large multiplier effect of ripple-effect raises basically does not change the economic burden of minimum wage laws on

businesses. If the typical business in Florida wanted to cover fully the total costs of the minimum wage increase through price increases, they would have to raise their prices by less than one percent. This is true, even though the number of workers receiving ripple-effect raises in Florida was 550,000, nearly double the 300,000 workers receiving mandated raises under the $6.15 proposal.

The Impact of Ripple Effects on the Demographic Profile of Minimum Wage Beneficiaries

These minimum wage ripple effect estimates adds new insight in the debate over whether the benefits of minimum wage laws are well targeted. A longstanding critique of using minimum wage laws to reduce poverty is that some minimum wage beneficiaries are secondary earners (wage earners that do not contribute a large share to their family's income). If ripple effects almost triple the number of workers affected by minimum wage increases, the demographic profile of minimum wage beneficiaries may change dramatically when ripple-effect raises are considered.

Table 3 provides one illustration of how the demographic profile of affected workers can change when ripple-effect raises are taken into account using the 2000-2002 Annual Social and Economic Supplements of the CPS.[6] The first column presents the average demographic characteristics of workers earning wages around the 5th wage percentile in 2000 ($5.50, averaged across states). These workers would most likely receive only mandated raises if the federal minimum rose by 13.4 percent—the same

[6] These years are chosen in order to pick a point in time that is neither very close to nor very far from a federal minimum wage increase because the type of workers near the bottom of the wage distribution may be affected by the relatively high or low real value (respectively) of the federal minimum.

amount as in 1990—to $5.85. In the second column, I present the demographic characteristics of all workers expected to receive wage increases due to a minimum wage increase, i.e., workers earning up to $7.15, just beyond the average 15th wage percentile in 2000 ($6.83, averaged across states).

The most striking difference between the two demographic profiles is the fall in the proportion of teenagers and traditionally-aged students (16 to 24 years old)—the types of workers usually identified as secondary wage earners. The proportion of teenagers falls from 33 percent to 25 percent and the proportion of traditionally-aged students falls from 31 percent to 24 percent. Expanding the scope of minimum wage effects to include ripple effects substantially reduces the overall proportion of these young workers from 39 percent to 31 percent among the expected beneficiaries of a minimum wage increase. This demographic shift reflects a greater presence of primary wage earners among affected workers when ripple-effect raises are taken into account. The average worker's contribution toward his/her family's income is higher among all affected workers (39 percent) than among only workers expected to receive mandated raises (34 percent), a statistically significant difference.

Implications for Living Wage Laws

These results also provide insight into the likely role of ripple effects in the context of living wage laws. Living wage laws typically call for much higher wage floors than state and federal minimum wage laws because their levels are explicitly tied to wage rates that provide a "livable income" such as the federal poverty-level income threshold

for a family of three. Minimum wage rates, on the other hand, are not. The difference between these two types of mandated wage floors is dramatic: Living wage laws typically raise the wage floor to levels that are almost twice as high federal and state minimum wages. For example, the current New Mexico state minimum wage is set at $5.15, equivalent to the federal rate. In the city of Santa Fe, New Mexico, however, a citywide living wage rate is set at $9.50, 84 percent higher than the state minimum.

Do the higher wage minimums of living wage laws, relative to state and federal minimum wage laws, cause employers to provide more extensive ripple-effect raises? The estimates from the retail trade analysis partly answer this question. Similar to the case of retail trade, living wage laws require that a significantly larger proportion of workers receive mandated raises than in the general case of minimum wage laws. For example, when Santa Fe's living wage was originally set at $8.50 in 2004, Pollin (2003) estimated that 16.4 percent of Santa Fe's workforce would receive mandated wage raises—more than four times the percentage of workers likely to receive mandated wage raises from a federal minimum wage increase (see table 3). Based on the retail trade analysis, I found that this factor does not produce more extensive ripple effects. The implication for living wage laws is that their higher wage minimums will not produce more extensive ripple effects than those found in the case of minimum wage laws.

In fact, because many more workers of covered employers receive mandated raises from living wage laws than minimum wage laws, the role of ripple-effect raises in the overall impact of living wage laws should be smaller than in the case of minimum wage laws. In other words, the cost increases that employers face (and the wage benefits that workers receive) from living wage laws will likely be primarily due to mandated

raises rather than ripple-effect raises, converse the general case of minimum wage laws. As a result, a reasonable guideline for assessing the economic impact of living wage laws is that ripple-effect raises will make up a smaller—probably significantly smaller—proportion of the overall change in the wage bill than in the case of minimum wage laws.

There are a couple reasons why living wage laws may produce ripple effects that behave differently from the ripple effects estimated from minimum wage laws. First, the workers just above the higher living wage levels (reaching as high as $15.52 in Burlington, Vermont) may have greater bargaining power than workers just above minimum wage levels (the highest is only $7.63 in Washington state), and thus better able to obtain raises when a living wage law is enacted. In that case, living wage laws may produce more extensive ripple effects because different *types* of workers—perhaps more skilled or more unionized—sit just above the new wage floor.

Second, living wage laws typically cover only employers with financial ties to their municipal government, such as city contractors. As a result, the coverage of living wage laws is typically no more than two percent of workers within a city (Neumark and Adams, 2003). Minimum wage laws tend to have virtually universal coverage. This aspect of living wage laws introduce the possibility of ripple effects *across* firms in addition to ripple effects up the wage scale within covered firms. Employers who are not covered by living wage laws may increase the wages of their workers as they compete for workers within the same local labor market as covered employers. This additional source of ripple-effect raises may cause the ripple effect to larger than suggested by the above analysis.

However, the empirical evidence thus far supports the conclusion that the ripple-effect multiplier from living wage laws will tend to be much smaller than that of minimum wage laws. Two case studies, one of San Francisco's living wage law, and the other of Los Angeles' living wage law, suggest that ripple-effect raises added only 13 to 35 percent to the cost increases from mandated raises (Reich, Hall, and Jacobs, 2003; Fairris, Runsten, Briones, and Goodheart, 2005).

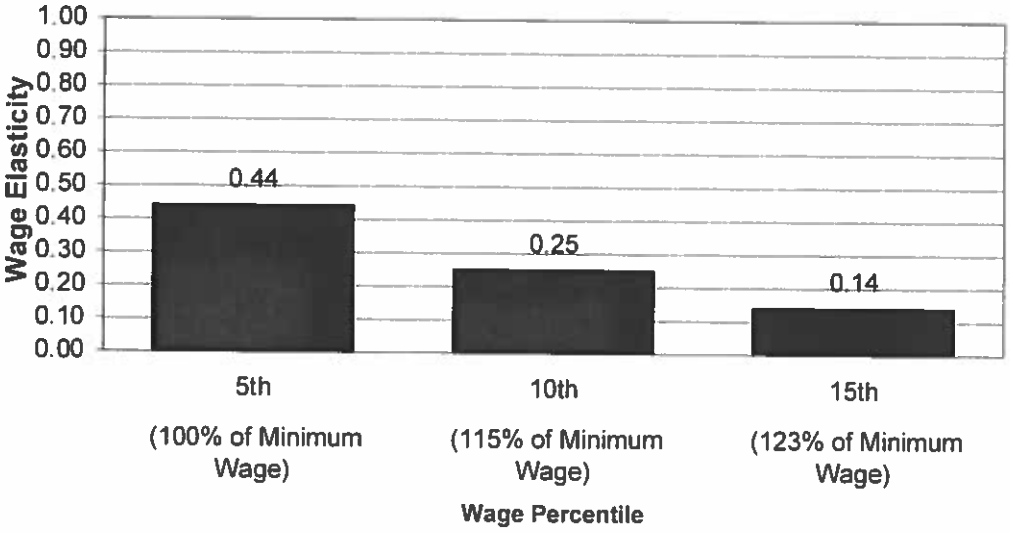
Conclusion

This research brief presents the results of new research on the question of whether mandated wage floors have a wider impact on wages—through ripple effects—than required by law. The results suggest that ripple effects have the potential to change dramatically the overall impact of mandated wage floors. However, how much ripple-effect raises contribute to the overall impact of mandated wage floors varies.

In the case of minimum wage laws, ripple-effect raises can as much as triple the costs associated with a minimum wage increase and nearly quadruple the number of workers who benefit from such an increase. However, because the cost of legally-required raises—as measured as a percentage of businesses' sales revenue—is so small, adding ripple-effect raises to the overall costs associated with minimum wage increases still generally represent a very small cost burden for employers. Ripple effects do tend to make minimum wage laws somewhat better targeted. With ripple effects, the pool of minimum wage beneficiaries includes more low-wage adult workers and fewer teenage and traditionally-aged student workers.

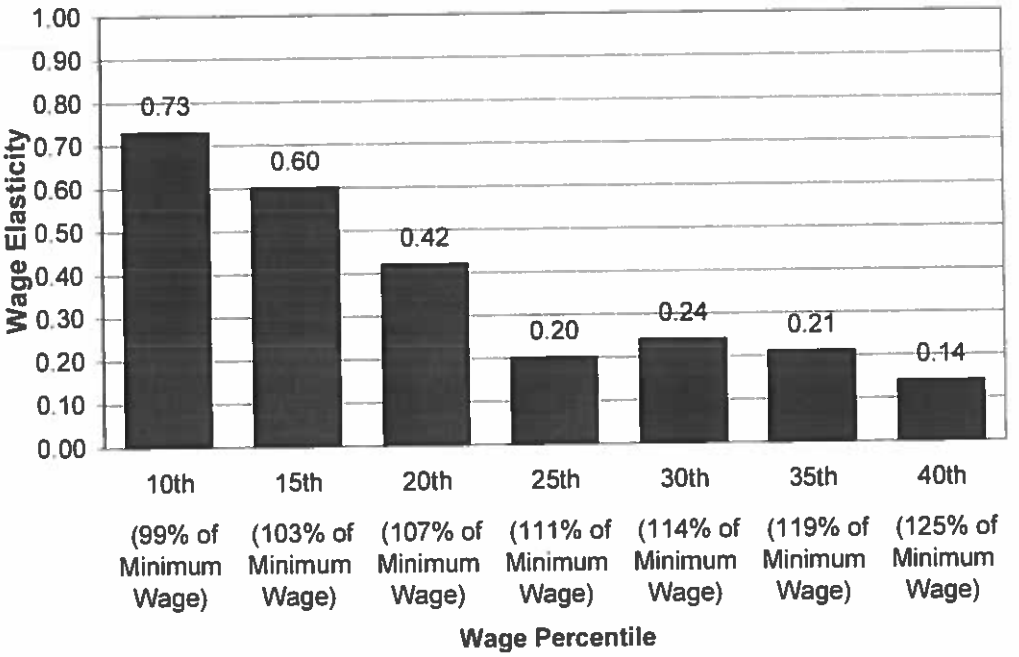
The minimum wage ripple effect estimates suggest that the case of living wage laws—with their much higher wage floors—is different. In particular, the analysis of minimum-wage ripple effects within the low-wage retail trade industry provides a look at what happens when a change in the mandated wage floor has a more dramatic impact on the wage structure through mandated raises alone—similar to living wage laws—than the general case of minimum wage laws. I find that the minimum wage does not produce a more dramatic ripple effect even though it has a stronger "bite" in retail trade. In fact, in retail trade, mandated wage raises are more prevalent than ripple-effect raises. Consequently, the ripple-effect multiplier is smaller when the minimum wage has a stronger "bite." This result implies an even smaller ripple-effect multiplier in the case of living wage laws which have an even stronger "bite" among covered employers than in the case of minimum wage laws and retail trade employers. In other words, living wage laws should be expected to generate cost increases for employers, as well as the wage benefits for workers, mainly through mandated wage increases rather than ripple-effect raises, converse the general case of minimum wage laws.

Figure 1: Estimated Wage Elasticities by Wage Percentile
All Industries



Source: Current Population Survey, 1983-2002.
Notes: Wage effects combine immediate and lagged minimum wage effects. For a full methodological discussion see Wicks-Lim 2005. Refinements to that analysis to control for spurious results cause the estimates to vary slightly from original results. Details may be obtained from author by request.

Figure 2: Estimated Wage Elasticities by Wage Percentile
Retail Trade Industry Only



Source: Current Population Survey, 1983-2002.
Notes: See Figure 1.

Table 1: Estimate of the Ripple-Effect Multiplier, All Industries

Based on Federal Minimum Wage Increase of September 1997: $4.75 to $5.15

(1) Wage percentile	(2) Average wage before[a]	(3) Number of workers (thousands)[b]	(4) Average raise due to minimum wage increase[c]	(5) Average hours worked/ week before change[d]	(6) Average weeks worked/ year[e]	(7) Mandatory raises (millions of dollars)	(8) Ripple-effect raises (millions of dollars)
5th	$ 4.73	3,982	$ 0.17	28.0	39	$ 741.0	$ -
10th	$ 5.22	6,118	$ 0.10	30.5	40	$ -	$ 757.5
15th	$ 5.74	5,385	$ 0.07	32.5	43	$ -	$ 522.7
						$ 741.0	$ 1,280.3
						Multiplier =	2.7

Source: Current Population Survey, 1997.

Notes: [a]Average wages are estimated from workers earning between the wage percentile ± 2. For example, the average wage of workers for the 5th wage percentile is based on workers earning above the 3rd wage percentile and below the 8th percentile."Before" refers to January to June, 1997. [b]Number of workers is estimated from the same sample of workers as the average wages. [c]Average raise based on wage elasticities presented in figures. [d]Average hours are estimated from the same sample of workers as the number of workers. [e]Average weeks worked per year estimates are not available from the CPS ORG data. Instead, approximations were taken from analysis presented in Table 3; see notes to Table 3.

Table 2: Estimate of the Ripple Effect Multiplier, Retail Trade Industry

Based on Federal Minimum Wage Increase of September 1997: $4.75 to $5.15

(1) Wage percentile	(2) Average wage before[a]	(3) Number of workers (thousands)[b]	(4) Average raise due to minimum wage increase[c]	(5) Average hours worked/ week before change[d]	(6) Average weeks worked/ year[e]	(7) Mandatory raises (millions of dollars)	(8) Ripple-effect raises (millions of dollars)
10th	$ 4.26	610	$ 0.26	26.6	39	$ 162.3	$ -
15th	$ 4.70	786	$ 0.23	24.4	39	$ 173.2	$ -
20th	$ 4.97	779	$ 0.17	26.2	39	$ 135.4	$ -
25th	$ 5.11	1,130	$ 0.08	27.4	40	$ 102.1	$ -
30th	$ 5.31	1,083	$ 0.10	28.4	40	$ -	$ 126.5
35th	$ 5.56	948	$ 0.09	29.1	43	$ -	$ 111.9
40th	$ 5.86	923	$ 0.07	31.9	43	$ -	$ 83.8
						$ 573.1	$ 322.2
						Multiplier=	$ 1.6

Source: Current Population Survey, 1997.

Notes: See notes to Table 1. Samples from the retail trade industry are too small to provide reliable estimates for the 5th wage percentile.

Table 3: Demographic Profiles of Workers in 2000

	Total	Workers expected to receive mandated raises only from hypothetical Federal minimum increase $5.15-$5.85	All workers expected to receive raises from hypothetical Federal minimum increase $5.15-$7.15
Individual Characteristics			
Average Hourly Wage	$16.27	$5.52	$6.17
	(0.03)	(0.00)	(0.00)
Student and 16-24 yrs. old	6.5%	31.1%	24.0%
	(0.06)	(0.72)	(0.37)
Teenager	6.8%	33.3%	25.4%
	(0.06)	(0.72)	(0.37)
Student or Teenager	8.6%	38.7%	30.6%
	(0.07)	(0.73)	(0.39)
Non-white	24.5%	29.7%	30.3%
	(0.00)	(0.62)	(0.35)
Female	48.2%	57.5%	58.3%
	(0.00)	(0.69)	(0.38)
No High School Diploma	13.8%	37.3%	32.0%
	(0.00)	(0.38)	(0.36)
Age	38.2	30.1	32.1
	(0.03)	(0.21)	(0.12)
Usual hours worked/week	39.3	30.6	32.7
	(0.03)	(0.18)	(0.10)
Family Characteristics			
Family Income	$66,623	$47,981	$46,348
	(139.70)	(716.07)	(373.37)
Worker's Earnings as % of Family Income	60.8%	33.9%	38.9%
	(3.60)	(0.48)	(0.30)
Severely Poor (Federal Poverty Level)	5.4%	17.1%	14.3%
	(0.06)	(0.52)	(0.27)
Low-Income (200% Federal Poverty Level)	18.5%	45.5%	44.9%
	(0.09)	(0.69)	(0.38)
Number of Workers (in millions)	134.8	4.8	14.4
% of Workforce	100%	3.6%	10.7%

Source: CPS March Annual Demographic File 2000-2002.
Notes: Dollar values are in 2000$. Standard errors are in parentheses. CPS supplemental weights were adjusted to account for differences in reporting errors between March ASEC earnings data and CPS ORG wage data.

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Creating Good Jobs in Our Communities

How Higher Wage Standards Affect Economic Development and Employment

T. William Lester and Ken Jacobs November 2010

Center for American Progress Action Fund

AMERICAN WORKER PROJECT

Creating Good Jobs in Our Communities

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Executive summary

From sports arenas to high-tech manufacturing zones and from commercial office buildings to big-box retail, local governments spend billions of dollars every year to entice private businesses to invest in their communities and create jobs. Yet these public funds often help create jobs that pay poverty-level wages with no basic benefits.

Cities across the country are working to gain greater control over these projects and help create quality jobs by attaching wage standards to their economic development subsidies. Communities are linking labor standards to public development projects in various ways, including community benefits agreements and prevailing wage laws. But the most common and comprehensive policies are business assistance living wage laws, which require businesses receiving public subsidies to pay workers wages above the poverty level.

These economic development wage standards have successfully raised pay for covered workers. Yet opponents of these standards argue that such laws prevent businesses from creating jobs and thus help some workers at the expense of employing more workers. Some business leaders and developers also claim that adding labor standards to economic development projects will scare away potential investors by sending an "antibusiness" signal.

This report examines these claims and finds that economic development wage standards have no negative effect on citywide employment levels. This casts serious doubt on arguments that standards dampen municipalities' ability to use subsidies to attract new businesses or create negative business climates where all firms avoid investment.

The study finds that the 15 cities effectively implementing business assistance living wage laws—Ann Arbor, Berkeley, Cambridge, Cleveland, Duluth, Hartford, Los Angeles, Minneapolis, Oakland, Philadelphia, Richmond, San Antonio, San Francisco, San Jose, and Santa Fe—had the same levels of employment growth

overall as a comparable group of control cities. The study also finds that these laws do not harm low-wage workers. Employment in the low-wage industries most likely affected by the living wage laws was unaffected by the change.

The study is the most methodologically sound, quantitative study conducted to date on business assistance wage standards. It uses the best available data that tracks employment by establishment and establishment movements over time in order to make accurate accounts of employment change at the city level. The study carefully selects cities that have effectively implemented business assistance living wage laws and ensures a controlled comparison that minimizes the effects of

Communities with business assistance living wage laws

unobservable variables by comparing 15 living wage cities to 16 cities with similar attributes where advocates lodged unsuccessful campaigns to pass such ordinances.

This study provides a strong test of the economic impact of wage standards because business assistance living wage laws are the type of economic development wage standard likely to have the most widespread effect on employment. Other types of economic development wage standards, such as community benefits agreements and prevailing wage laws, either affect far fewer projects or are more closely tied to market wages, and are thus are even less likely to have any effect on employment.

This report—like the groundbreaking studies that established that minimum wage laws do not kill jobs as opponents maintained—brings academically sound, empirical research to bear on a debate that for too long has been relatively uninformed by quality, comparative evidence on the laws' actual effects.

The evidence demonstrates that raising job standards does not reduce the number of jobs in a city. This means that job growth does not have to come at the expense of job quality. Local government leaders can therefore ensure that taxpayer dollars do not subsidize poverty wages by supporting economic development wage standards and feel confident that their local business climate will not be affected.

Introduction

State and local leaders enact a wide variety of economic development policies to encourage private businesses to locate, invest, and ultimately create jobs for local residents. This business attraction model is exemplified by policies—such as direct subsidies, tax exemptions, and targeted infrastructure improvements—that allocate public funds to private businesses or developers. Conservative estimates indicate that state and local governments spend more than $50 billion every year on this type of activity.[1] The logic behind such policies stems from the idea that businesses are relatively mobile and may choose to relocate or expand in low-cost areas. Yet these publicly funded projects have sometimes resulted in jobs that pay low wages and provide no benefits.

Stark increases in overall labor market inequality have led some policymakers and labor advocates to challenge the dominant business attraction strategy. Data from the past two decades suggests there is a fractured link between employment growth and raising local citizens' overall well-being. Many now view chasing jobs at all costs to be a questionable policy.

Even during the job-rich growth of the 1990s a significant portion of new jobs paid low wages and typically lacked benefits and career ladders. This trend continued in the 2000s and has led to falling real wages for most workers, increases in working poverty, and rising income inequality. Average wage growth for the bottom 80 percent of workers grew by only 0.6 percent between 2001 and 2007 while wages for those in the top quintile rose by 5.3 percent.[2]

Labor advocates, religious and community leaders, and elected officials have pushed for and passed local wage standard ordinances to address the problem of declining job quality. The push to link labor standards to public development projects has occurred through various forms, including community benefits agreements as well as prevailing wage and living wage laws.

A community benefits agreement is a project-based contract signed by community groups and a developer that requires the employers participating in the project to adhere to a negotiated set of wage standards and provide specific amenities on a particular project. CBAs are a growing phenomenon but so far have only affected a relatively small number of completed projects.

Prevailing wage laws require that covered businesses pay their employees wages at or above the typical wages in a certain industry, and thus not undercut the existing market wage structure. Prevailing wage laws have been used frequently on government contracts but only very recently have begun to be applied to a broad range of jobs created by government-supported economic development.

The most common and comprehensive economic development wage standards are business assistance living wage laws, which require businesses participating in projects receiving public subsidies to pay workers wages above the poverty level.

The living wage movement began in Baltimore in 1994 and more than 140 local jurisdictions now have some form of living wage law. The movement originally focused on ensuring that government contractors did not pay poverty wages but evolved into a broader set of urban policies that presented a clear alternative to the business attraction model of economic development. Living wage advocates in some cities have extended the basic form of living wage law to firms that receive public dollars through economic development subsidies.

These "business assistance" living wage laws directly challenge the logic of local economic development policies by placing additional requirements on firms that engage in development agreements with the public sector. Some business leaders and politicians have criticized wage standards for raising the cost of doing business. These opponents claim that raising wages would lead to job losses since employers would walk away from development deals. They also often identify economic development wage standards as an "antibusiness" signal to other firms who may not receive local subsidies but would nonetheless choose not to locate in the city.[3]

What is certain is that economic development wage standards in large U.S. cities continue to be highly controversial. The debate over Chicago's proposed "big-box" living wage law in 2006, for example, drew national media attention and resulted in Mayor Richard M. Daley's first-ever veto. New York City is currently debating whether to adopt a citywide economic development wage standard and Pittsburgh recently extended a prevailing wage law to cover workers at firms that receive financial assistance. The current debates are critical at this time, not only

because several major cities are considering business assistance living wage laws but also because the current economic crisis—with its near double-digit unemployment—increases the pressure on elected officials to increase the number of jobs, regardless of their quality.[4]

Given the public's desire for both creating jobs and raising the quality of jobs, this report assesses the question of whether or not business assistance living wage laws reduce jobs and economic development activity in the cities that choose to pass them.

We examine business assistance living wage laws because they are the most widespread form of economic development wage standards, which means they provide a large enough sample of cities and affected employers to allow for rigorous quantitative analysis. They also allow for more consistent comparison across cities than community benefits agreements, which tend to be unique to each deal. And living wages have been subject to previous academic study, providing a useful basis of comparison.

This study provides a hard test of the economic impact of wage standards because business assistance living wage laws are the type of economic development wage standard likely to have the most widespread effect on employment. Other types of economic development wage standards, such as community benefits agreements and prevailing wage laws, either affect far fewer projects or are more closely tied to market wages and are thus likely to have less influence on employment or business climate.

This study uses a unique, private-sector database that contains an extensive time series of observations from 1990 to 2008 to make detailed before and after calculations of how living wage laws change employment and total business establishments at the city level. We estimate these changes among a set of 31 large and economically diverse urban jurisdictions by comparing outcomes in cities that have passed (and enforce) business assistance provisions to those that attempted, but failed to pass such provisions. This research design—adopted by other living wage researchers and used widely in labor economics and policy analysis—has the benefit of controlling for underlying institutional and structural differences between cities with and without business assistance living wage laws that have the potential to confound results.

The study considers the broad set of industries and firm types most likely to be covered by business assistance living wage laws and finds no evidence that such laws reduce employment or business growth over the short or long term.

How business assistance living wage standards may affect urban economic development

Many living wage proponents argue that business assistance clauses will not cause significant job losses. Research indicates that higher minimum and living wages lead to efficiency gains for firms through reduced turnover.[5] Increasing wages for the lowest-paid workers also stimulates local economies, as low-income households typically spend more of their dollars locally.

In addition, some researchers point out that business assistance living wage laws typically apply to only a small number of firms that receive direct subsidies, and only a fraction of these firms employ workers below the mandated wage. Business assistance laws function from this perspective as a lower bound that serves to prevent localities from subsidizing low-wage jobs, but don't represent a drastic reshaping of existing local labor practices and thus could not have a significant effect on employment.

Living wage opponents suggest, on the other hand, that these laws could "kill deals." If local governments force subsidized businesses to increase wages above the level usually offered, these firms will choose not to enter into development agreements, leading to the loss of all the jobs, not just the low-wage positions. Another argument is that, even if deals do move forward, employers would cut back on staffing levels or substitute toward higher-wage, higher-skilled labor, resulting in fewer people employed. Still another argument is that these laws create a poor business climate. Opponents predict fewer jobs created overall if government enacts business assistance provisions.

These competing interpretations and multiple paths of causation make it important to distinguish the myriad ways that business assistance living wage laws could affect urban economies. We divide such potential effects into three groups—direct, direct spillover, and indirect effects—each of which may have one or more potential consequences.

Direct effects

Wage standards directly affect a narrow set of establishments that participate in subsidized development projects and operate businesses that hire a significant number of low-wage workers. Examples of "directly affected" businesses include retailers or food service operators that are part of a larger, publicly subsidized urban redevelopment project that is subject to the living wage requirement—such as Los Angeles's Staples Center sports arena development. Workers at low-wage assembly plants or back-office processing centers that are often targets for local economic development incentives may also receive direct wage increases as a result of the law.[6] Measuring only these direct consequences is nearly impossible through quantitative analysis because of the limited number of deals affected by such laws and the type of data available.

Direct spillover effects

Other low-wage employers may experience changes from higher wage rates through a direct spillover effect.[7] Such direct spillover effects accrue if the mandated wage increases at covered firms result in an overall increase in wage standards in the local economy that forces other low-wage employers to raise wages as a competitive response.[8] The textbook, neoclassical economic viewpoint explains that this increase in wages would result in a reduced labor demand. This is the same theoretical interpretation that some researchers apply to the minimum wage debates.

Indirect effects

Living wage laws may also indirectly affect the overall level of economic development activity in a city. The passage of business assistance provisions may send a strong antibusiness signal to employers seeking to locate in the enacting city or existing businesses considering local expansion. Some researchers argue that the indirect effect of living wage laws—particularly business assistance provisions since they theoretically could affect a much broader set of firms than contractor-only laws—may actually outweigh any observable direct effect on the local business climate.[9] Even firms that may not seek economic development subsidies, but nonetheless hire a significant portion of low-wage workers, may view a strong living wage law as a proxy for broader political shifts at the local

scale toward a more pro-labor stance and therefore reduce their investments in the local jurisdiction.

Business assistance provisions may also shift the practice of economic development policymaking itself and thereby reduce aggregate employment levels. If business assistance clauses are effective in acting as a minimum standard, or floor, for the type of job quality expected from economic development incentive projects, then city officials may shift their business attraction strategy away from industries that provide a larger number of lower-paid positions to sectors that pay higher wages such as manufacturing, research and development, or biotechnology. The number of economic development "deals" may decline because the chances of landing such high-value targets are lower, and higher wage industries may require fewer workers due to high productivity. Labor advocates who oppose public subsidies for low-wage industries may laud these indirect effects but policy changes may end up reducing the total number of jobs created through economic development programs.

Previous studies on wage standards

The empirical literature on how living wage ordinances affect employment primarily focuses on detecting the direct and direct spillover effects and only rarely distinguishes whether the laws explicitly apply to business assistance provisions. Two types of studies characterize this literature: individual case studies of single cities before and after passage of a given law—which tend to find no employment effect—and quantitative studies from a group of living wage and nonliving wage cities over a period of time—which tend to find significant negative consequences. The living wage literature mirrors the tension between case studies and panel studies in the broader economics literature on federal and state minimum wage changes.[10]

One of the earliest detailed case studies on Baltimore's landmark 1994 contractor-only living wage law found that the living wage did not significantly increase contract costs and that employment remained the same at covered firms.[11] Yet this study did not compare employment changes at covered firms to a control group. Researchers in a study of Los Angeles conducted two independent surveys of firms and workers that were covered and uncovered by the city's ordinance that applied to city contractors.[12] The study found that wages in covered firms increased while turnover and absenteeism dropped relative to the control group, and there was no significant difference in employment levels.[13] Another case study showed that San Francisco's living wage law that applied to workers at the SFO International Airport resulted in direct wage increases for nearly 10,000 workers but had no discernable effect on employment.[4]

These empirical case studies do not focus explicitly on business assistance provisions but they provide valuable insights into the laws' potential effects. And studies of city-level minimum wage provisions provide a further sense of the likely impact of living wage laws that extend beyond city contractors. Minimum wage ordinances cover all private-sector establishments, not just those that receive financial aid from the city. A study of Santa Fe's minimum wage law in 2003 found

only marginal cost increases for businesses and no significant effect on employment.[15] In San Francisco and Alameda County, researchers surveyed restaurants before and after San Francisco's citywide minimum wage took effect in 2004.[16] They found a significant wage increase, a reduction in labor turnover, and no negative affect on employment.

Living wage case studies have the benefit of clearly identifying covered firms and therefore accurately measuring direct effects but the results of studies that compare a single case to a control group don't allow us to generalize about the greater effects of living wage ordinances. Research designs that use observations from all or many living wage cities and make comparisons across a large number of controls generally have greater external validity—that is, they are more validly generalizable to other communities.

David Neumark, an economist at the University of California, Irvine, who is frequently cited by opponents of living wage laws, examines how state minimum wage increases and city living wage laws affect wages, employment, and poverty rates using a panel of large cities that passed ordinances between 1996 and 2002.[17] Based on data from the Current Population Survey, Neumark's research finds large wage increases and reductions in family poverty associated with the timing of living wage laws. But it also finds significant disemployment effects for younger, lower-skilled workers.

Robert Pollin, economics professor at the University of Massachusetts, Amherst, and head of the university's Political Economics Research Institute, and his colleagues at PERI, Jeannette Wicks-Lim and Mark Brenner, who have extensively studied living wage laws, critique Neumark's wage results as being vastly overstated given the fact that most living wage laws cover only a small fraction of workers, and that his dataset only identifies metropolitan areas rather than individual cities and weights Los Angeles too heavily.[18] The drawback of using broad household surveys, such as the CPS, is that there are too few cases to accurately distinguish "covered" and "uncovered" workers. Neumark cannot specifically identify a worker employed at a firm covered by the living wage.[19] Neumark also restricts his analysis to the 1996-2002 period due to data constraints, which is a relatively short time period during an economic expansion.

Summary of previous living wage studies

Individual case studies: This research has found no negative employment effects. Studies have been successful at identifying covered firms but are viewed by some as less generalizable and have not explicitly addressed business assistance laws.

Multiple-case, panel studies: This research has generally, although not always, found that living wage standards reduce employment and that business assistance laws are more harmful than contractor-only laws. Yet they are weak at identifying covered firms and most have not used appropriate datasets for examining cities.

Scott Adams, an economist at the University of Wisconsin, Milwaukee, and David Neumark more recently compare low-wage workers' income and employment levels in cities that passed living wage laws and cities that had a failed living wage campaign.[20] Using the failed cases as a control sample to attempt to hold constant the local political or institutional factors that fuel living wage campaigns—such as union density—may also affect the outcome variables such as employment. The study finds a statistically significant negative employment effect for lower-skilled workers—but only for those cities that have business assistance provisions, which they argue have the potential to affect most, if not all, low-wage workers in a given city. This is the only quantitative study that distinguishes results for business assistance living wage laws.

Brenner, Wicks-Lim, and Pollin, the University of Massachusetts economists, and others suggest that the latest Adams and Neumark living wage study is also deeply flawed.[21] These authors argue that business assistance laws only directly affect a small fraction of workers in each city with a living wage ordinance. They also argue that using the CPS to identify city-level effects is highly problematic due to small sample sizes at the urban scale and the inaccurate assumption that policy changes at the city level will affect workers throughout a metropolitan area.

T. William Lester, a University of North Carolina professor and co-author of this study, seeks to address these data-quality concerns by using the National Establishment Time Series—the same dataset used for this study—to measure how living wage laws affect employment and the number of business establishments in California. The findings contradict Adams and Neumark, although there were too few cases to parse effects for business assistance from contractor-only living wage laws.[22] The study concludes that living wage laws had no negative impact on government contractors or low-wage service industries that might be indirectly affected by the living wage.

Panel studies of business assistance living wage laws are also criticized for treating all laws equally. Brenner, Wicks-Lim, and Pollin argue that governments have, in some case, only applied the standards to a very small number of firms, which could not produce a direct impact that is measurable by data sources like the CPS.[23] There is wide variation in the degree to which living wage laws are enforced at the local level.[24] Economic development officials have simply ignored business assistance provisions in some cases. And negotiations ahead of passage significantly watered down the measure in other cases such that incentive thresholds were set so high that no firms were likely to be covered upon passage.[25]

There is a tension in the empirical literature on living wage effects overall. Panel studies of the type applied by Adams and Neumark, which include all or a large sample of living wage cities, are preferable generally to comparing employment before and after passage within a single city.[26] Previous panel studies—with the exception of Lester—find a negative impact on employment, though this research has generally used inappropriate data and failed to properly select cities to study.[27] Individual case studies, including studies with detailed surveys, generally find no disemployment effect and make a more convincing case for measuring outcomes among firms and workers who are covered by the living wage.[28] But their limited scope makes it difficult to generalize the findings more broadly.

The research design proposed in the following section combines the best of both approaches in the literature. We conduct a front-end qualitative assessment of nearly all the business assistance living wage laws in the United States to construct an accurate treatment group consisting of large urban areas that have living wage laws that are binding and/or likely to be enforced. We then conduct a time-series quantitative analysis to estimate a generalizable assessment of how business assistance living wage laws will affect urban economic development. And we use a more appropriate dataset than previous research.

Research design and case selection methodology

Conceptual approach

The simplest way of measuring the effect of business assistance living wage laws is to gather information on the total number of jobs and business establishments for jurisdictions that have business assistance requirements for several years before and after each law went into effect. Yet this simple direct comparison is extremely limited due to the problem of "endogeneity"—the fact that cities that choose to pass business assistance living wage laws may experience other trends that are correlated with employment changes. Cities could be growing slower or faster as a group due to long-term trends such as deindustrialization or suburbanization, for example, masking the true effect of business assistance requirements.

To overcome the endogeneity problem, we need to identify an appropriate control group of cities without business assistance living wage laws to compare to our treatment group. This group of nontreated cities would ideally control for all relevant factors that may influence employment or establishment growth. Short of randomization, economists often look for natural experiments to analyze policy changes.[29] The benefit of this type of research design is that it compares outcomes between treatment and control groups that are in all other respects very similar, except for the difference in the policy. The estimated effect of the policy is therefore unbiased. Adams and Neumark attempt to control for endogeneity by comparing living wage cities to cities that experienced living wage campaigns, but either failed to pass a living wage or had had their law vetoed or struck down by the courts.[30] They refer to their control group as "failed or de-railed campaigns."

We adopt the same conceptual research design in this report as Adams and Neumark.[31] We compare outcomes for a treatment group that includes 15 large, urban jurisdictions that have passed business assistance living wage laws to a comparably sized set of cities that failed to pass business assistance provisions. This choice of control group minimizes differences in unobservable, confounding variables because these cities have similar institutional settings with regard to labor

regulation—many of the cities either have basic, contractor-only living wage laws or have undergone significant campaigns to pass stronger business assistance provisions, but did not ultimately enact them.

We assume that the existence of a living wage law campaign indicates that control cities have a similar set of labor advocates and progressive actors that have raised the issue of a living wage in the political spectrum. Both treatment and control cities are drawn from the overall set of cities in the United States that have at least proposed a living wage law. This group of cities is significantly different than other local U.S. governments in that they tend to be larger, older cities located on the West Coast or in the industrialized Northeast and Midwest.

What makes this study better than previous research

Careful screening of treatment cities to exclude cases where business assistance laws have weak enforcement or significant loopholes.

Better data that captures only the city where the law applies and allows for analysis of the industries most likely to be affected by business assistance living wage laws.

This design does a good job of controlling for confounding differences between the treatment and control groups but it does not rule out all possibility of endogeneity. We therefore test for structural differences between the treatment and control groups cities to ensure that they are truly comparable. We also add controls to allow for city-specific trends to further address concerns about endogeneity.

Case selection methodology

A key difference between our study and that of Adams and Neumark is our sample choice.[32] We conduct a systematic qualitative assessment of the set of U.S. cities that have passed business assistance living wage laws to narrow down the treatment group to exclude where the living wage has not been enforced or thresholds are too high to have an effect.

The first step in our case selection methodology was to determine the universe of all local jurisdictions that have passed or considered living wage laws that apply to businesses receiving any sort of financial assistance, including tax abatements, grants, direct infrastructure improvements, or below-market loans. We determined this universe by searching databases maintained by the Employment Policies Institute and Living Wage Resource Center.[33] These websites contain basic information on the type of living wage passed, coverage thresholds, mandated wage levels, and date of passage. EmPI's website also contained listings for cities that rejected living wage laws either through a failed ballot initiative or

council vote, a veto, or a repeal. This universe consisted of 50 cities, with 30 listed as successfully enacting a law and 20 as having failed living wage campaigns.

We excluded small cities with fewer than 60,000 people because small cities tend to engage in fewer economic development "deals" for which the living wage would apply, and because we wanted to focus on cities that would have significant employment volume given the high cost of acquiring NETS data.

We then undertook a deeper analysis of each city's law to determine whether it should be assigned to the treatment group, the control group, or dropped from the study altogether. Our goal in this process was to take the critiques of Brenner, Wicks-Lim, and Pollin and others into account by ensuring that cities in the treatment group have laws that directly or indirectly affect the local economy.[34]

Our analysis to ascertain the status and effectiveness of the laws and determine whether to exclude the city consisted of three components. First, we obtained written copies of each city's ordinance through web searches of city legislation. Each ordinance typically lists the exact coverage threshold, the types of financial assistance that qualify under the law, and any exclusions or loopholes.

The second step in our analysis was to make phone calls to the cities that were indicated as having enacted a business assistance living wage ordinance to determine if the law had ever been enforced. We called city staff at the agency or department listed as responsible for enforcing the living wage or monitoring performance. We also called each city's agency in charge of business attraction.

Our limited success in reaching knowledgeable staff led us to our third step. We scanned secondary sources including local newspaper listings and performance reports by local advocacy groups or foundations to look for direct evidence of an economic development incentive deal entered into with an employer where the living wage would apply. This allowed us to finalize a list of 15 treatment cities.

These cities have one or more of the following criteria: assistance level thresholds of $1 million or less; direct evidence of enforcement from primary and/or secondary sources; and evidence of strong enforcement campaigns and ongoing organizing activity after passage of the living wage.

Finally, we began the process of selecting the control cities with the list of 20 cities in the EmPI database that rejected a living wage ordinance and narrowed the list of cities to 16 to produce a balanced sample.[35] We conducted similar research

steps on the proposed control cities as we did on the treatment cities to ensure that a law was not eventually passed after the most recent update to the EmPI database—as was the case with Philadelphia, Pennsylvania. And we dropped several cities to attempt to maintain a broad regional balance across the treatment and control samples. Only one treatment case came from Texas, for example, and we thus felt it was not necessary to have both Houston and Dallas in the control. The resulting list of treatment and control cities is listed in Table 1.

We took extensive efforts to ensure that our treatment and control cities are comparable but it is possible that they may still differ in important ways. Table 2 compares the average values for a variety of demographic and economic variables.

It is reassuring to see that there are no significant differences between the treatment and control samples for the pretreatment period annual employment growth rates.[36] Treatment cities grow only 0.2 percent slower than the control. The two groups are also quite similar in terms of poverty and unemployment rates and racial and ethnic composition.

TABLE 1
List of treatment and control cities

Treatment cities	Control cities
Ann Arbor, MI	Albuquerque, NM
Berkeley, CA	Chicago, IL
Cambridge, MA	Dallas, TX
Cleveland, OH	Durham, NC
Duluth, MN	Eugene, OR
Hartford, CT	Indianapolis, IN
Los Angeles, CA	Knoxville, TN
Minneapolis, MN	Lansing, MI
Oakland, CA	Nashville, TN
Philadelphia, PA	New York, NY
Richmond, CA	Omaha, NE
San Antonio, TX	Oxnard, CA
San Francisco, CA	Pittsburgh, PA
San Jose, CA	Providence, RI
Santa Fe, NM	South Bend, IN
	St. Louis, MO

Source: Author's analysis

The only areas for which the groups differ significantly are on measures of household income and housing costs. The group of living wage treatment cities has clearly experienced significant growth at the upper end of the income spectrum, which results in higher levels of income inequality. The only distributional variable—the proportion of a city's households that earns above the 80th percentile nationally—bears this out. This upper income growth likely adds to housing pressure as measured by the significantly higher median rental rates and housing values. Income inequality seems to be higher in the treatment group but it is unclear that inequality itself would lead to lower job growth in the industries that are likely to be affected by living wage provisions.

Regional balance of the samples also explains some of these differences. We attempted to produce balance in selected control cases but we are still left with a treatment sample that is overweighted toward California (seven cases in the West). These differences are not enough to conclude that the samples are systematically biased but it does provide a motivation for including the type of city-specific trend controls discussed later.

TABLE 2

Comparative statistics between control and treatment group

Variable	Treatment group (mean)	Control group (mean)
Total population	665,149	1,000,709
% African American	18.8%	22.2%
% Hispanic	22.1%	17.4%
% Non-Hispanic White	45.3%	54.1%
% With BA or higher	34.9%	26.5%
% Foreign born	20.2%	13.5%
% Poverty	17.9%	17.8%
% Unemployed	7.5%	7.4%
Median household income	$41,003	$35,943
Median rent	$700	$578
Median housing value	$203,460	$111,131
% Of households in top US income quintile	21.0%	15.2%
% Employed in FIRE or professional/tech. services	20.1%	18.1%
% Employed in manufacturing	10.0%	11.6%
% Renters	52.4%	49.1%
Housing vacancy rate	5.2%	7.4%
Average annual growth rate 1990-1997	2.3%	2.5%
Average three-year growth rate 1990-1997	7.1%	6.6%
Total number of cities	15	16
Frequency by region		
Northeast	3	3
Midwest	4	6
South	1	4
West	7	3

Source: US Census Data, 2000 obtained from the State of the Nation's Cities

Database construction

Another key innovation in our research design is the primary data source used to measure the outcome variables. We use the National Establishment Time Series database as our primary data source to construct a city-level panel data set using annual observations.

Background on the National Establishment Time Series dataset

The NETS data is a proprietary database developed by Dr. Donald Walls of Walls and Associates in conjunction with the Dun and Bradstreet business listings information service. D&B gathers data each year from extensive phone surveys of businesses for the purposes of establishing credit ratings for businesses of all sizes. NETS is different from the typical D&B files that are sold to business and credit issuing entities in that it is a longitudinal database created by taking 19 annual snapshots of the D&B file and linking establishments across years using a unique identifier assigned by Dun and Bradstreet. This identifier is called the DUNS number. NETS contains establishment-level data on employment; estimated sales; industry, as tracked by the eight-digit Standard Industrial Classification code; ownership structure; and address for 1990-2008. NETS tracks establishment moves over time, which allows us to accurately account of total employment in each local jurisdiction in each year.

NETS is unlike household surveys such as the Current Population Survey in that it attempts to capture the entire universe of establishments operating in a given year. Once D&B assigns a DUNS number to an establishment, they contact that establishment each year by telephone to update information on their location, ownership structure, industry, employment, and sales figures.[37]

The NETS database does a reasonably good job in capturing the level of economic activity and in measuring employment levels. A careful academic review of the NETS file argues that D&B has "an economic incentive" to ensure that its infor-

mation is up-to-date and accurate, and that it covers all existing establishments.[38] It is valuable to use NETS for a study of the living wage because it offers consistent long-term information on employment and the number of establishments at the local level rather than the county, metropolitan, or state level. NETS also offers detailed industry information on each record, which allows us to focus on the specific low-wage industry groups that are most likely to be affected by business assistance provisions, but also measure industries that are often targets for local business attraction strategies even if they are not low-wage industries in particular.

Using the National Establishment Time Series

The first limiting step in our analysis was on establishment size. We only use NETS records for establishments that had more than four employees at any point in their life cycle between 1990 and 2008. This limiting step was done to reduce the cost of our data purchase and to maintain comparability with other data sources. This limiting step is not likely to have a major effect on this research since very small firms do not typically receive local financial assistance, and they make up a small portion of overall employment in each city.[39]

NETS is a dynamic database in that it tracks each establishment's location overtime. Most establishments do not move but approximately 14 percent of the NETS records in our sample have changed location at some point in time. The address information listed in the NETS is only for a firm's current location, so if a given establishment started in New York in 1994 but moved to Boston in 2000, its current geographical identifiers would reflect a location in Boston. But we would want to count this firm in New York in order to make an accurate employment total for New York in 1994-2000. We are able to overcome this problem since NETS contains information not only on current geographic location but also on the origin, time, and destination of each establishment move. We build our city-level database by combining the information on the origin zip code and current zip code of each establishment to construct a set of variables that track the zip code location of each establishment in each year.[40]

Once each establishment was assigned to a city for each year that it was in existence, we then aggregated the NETS database to the city level by summing employment and the number of establishments in each city for various industry sectors and firm types of interest to our analysis.

Measuring employment and establishments

The primary objective of this report is to test for the various ways a business assistance living wage law could affect fundamental measures of economic activity in the cities that choose to pass them. The richness of the NETS database enabled us to produce outcome variables to test the hypothesis that business assistance provisions reduce jobs through direct, direct spillover, and indirect means. We calculate employment and establishment count variables for 14 separate industry sectors and firm types, organized into three broad categories. The first category, which we argue best approximates the set of employers most likely to be affected by the living wage through direct or direct spillover effects, consists of low-wage service sector industries. We calculate five outcome variables for this category: broad low-wage services; narrow low-wage services; retail; restaurants; and hotels.

The first variable in this category, broad low-wage services, captures a broad set of low-wage industries likely to be affected by large-scale urban redevelopment projects. However, this variable is of a broad cross section of industries and may be combining some higher-wage industries with low-wage ones. We therefore also break down this variable using the more refined industry data to produce a variable that captures only the low-wage industries from within the broad category, such as building security and parking services. We furthermore include the three largest employers of low-wage workers in most urban economies: retail; restaurants; and hotels. These industries are often targets for local business attraction and urban redevelopment projects and represent the group of employers who are potentially most affected by direct spillover effects of higher wages.

The second major category of outcome variables comprise what we term "common economic development targets," which can be thought of as capturing both direct and indirect effects. Workers in this category are not necessarily low-wage but it includes those industries that are often targets of business attraction efforts. We define employment and business establishment totals by city for the following groups: manufacturing; nondurable manufacturing; back-office; wholesale; big-box retail; and finance insurance and real estate.

Most U.S. cities have experienced some form of deindustrialization and industrial job losses, and manufacturing establishments have long been the target of local economic development initiatives. Nondurable manufacturing industries tend to be less capital intensive and less unionized, and therefore have the potential to pay lower wages. At least some portion of this sector may be affected by the

living wage mandate. Economic development deals have also focused on the highly mobile back office activities of corporate services such as call centers and credit processing services, and so we construct an outcome variable that attempts to capture this activity. Wholesale distribution centers are also targets for local economic development, especially for jurisdictions that have former industrial land in need of redevelopment or that have large, undeveloped tracts. We include so-called big-box retail stores because they are often targets of local development deals, especially in jurisdictions heavily dependent on local sales taxes. Finally, we include finance insurance and real estate as an additional test because we would not expect that this high-wage industry would be affected by a living wage law.

We also generate two outcome variables that are defined by an establishment's place in the firm structure. We measure establishments that are the headquarters of a firm that has at least two other establishments at different locations—as well as branch plants, which are nonheadquarter establishments in firms with multiple establishments. These two variables do not include single location firms or small businesses. We characterize these establishment types as those that may be more susceptible to the indirect or signaling effects described above. Decisions about where to locate them are based to some degree on the region's business climate, and they typically provide jobs above the living wage threshold although they are somewhat less likely to be targets for development subsidies.

We also provide results for total private-sector employment and establishments as a summary measure.

Using the database to measure the effects of wage standards

We use our panel data set to measure how passing a business assistance living wage law affects a city's level of employment and its total number of establishments. These are the basic outcomes of economic development. We conduct this analysis by using a panel regression model that is now standard in the empirical literature on the economics of minimum and living wage increases.

The first step is to gather data on the timing of each treatment city's passage of a business assistance living wage. We measure changes in employment in the years after passage relative to changes in employment in the years leading up to passage. We then compare this difference in employment change to the same employment changes in the control sample. This technique is referred to as "difference-in-differences."

We also use statistical techniques to control for confounding factors such as the fact that different cities passed laws at different times and that there are significant differences between the cities in terms of economic structure, historical growth patterns, size, and demographics. Our model includes a control for population size based on the Census' annual estimates as well as controls for each year in the panel. It also includes dummy variables for each year in the controls for macroeconomic effects that are common to all cities in the analysis. The U.S. economy was in recession in 2001, for example, and most local economies experienced job losses. Failing to control for such effects could lead us to erroneously conclude that living wage laws passed in 2000 resulted in significant job losses, which were in fact caused by a cyclical trend that was unrelated to passage.

We also include controls for each city itself and controls for city-specific linear trends. We include these controls to adjust for any idiosyncratic differences between the cities both within the treatment sample itself and between the treatment and control samples. For instance, Santa Fe is included in the treatment group based on its passage of a citywide minimum wage that includes all firms. Yet the entire Southwest region of the United States grew at a faster rate than other areas of the country for the full panel period of 1990-2008. Failing to control for these regional differences in growth could lead us to understate the living wage effects.

The study finally measures the effect of passing a business assistance living wage law over a four-year period, including estimates for two years prior and two years after passage. This allows us to control for any prepassage spike or fall in the outcome variable and also allows us to examine if the living wage has any delayed effect. If the impact on a city's business climate is real, it may take several years to have a detectable influence of overall employment or employment in a specific low-wage industry.

Main findings

Employment effects

The study examines how living wage standards affect 14 distinct employment variables: total citywide; broad low-wage services; narrow low-wage services; retail; restaurants; hotels; manufacturing; nondurable manufacturing; back-office; wholesale; big-box retail; finance insurance and real estate; headquarters; and branch plants.

Together these provide a comprehensive examination of the potential combined direct, direct spillover, and indirect effects that business assistance living wage laws can have on local employment. Figure 1 presents these 14 variables as the possible range of employment change expected after passage, allowing up to two years for lagged effects. None of the 14 outcome variables show a statistically significant negative consequence of passing a business assistance living wage standard. Statistically significant outcomes would mean that we are 90 percent confident that the estimate is different from zero. But this is not the case for any of the variables, which means we can conclude that there is no employment effect. (More detailed results are presented in the technical appendix.)[41]

Our estimates indicate that passage of a business assistance living wage law has no measurable effect on citywide employment. Employment levels are unaffected in low-wage industries as is employment in industries likely to be targets of economic development subsidies and in firms that are sensitive to the perceived business climate of a city. This suggests that business assistance living wage laws are unlikely to have direct, direct spillover, or indirect effects on employment levels. These findings discredit the primary arguments used by opponents of business assistance living wage laws that these laws are harmful to employment in direct and indirect ways.

It is important to note that the results are based on nearly 20 years of data—a timeframe that contained years of recessions and expansions—which suggests that business assistance living wage laws are unlikely to have an effect on employment levels even during hard economic times.

These results are also quite robust. For example, the inclusion or exclusion of any particular city from the treatment group has no meaningful effect on the results.

Figure 1 visually represents the 90 percent confidence interval of our point estimates. Any number line in Figure 1 that includes zero in the shaded area indicates that the estimated effect is not different than zero. This means that there is no estimated employment effect, which is the case for all the variables tested. The estimated impact of employment in low-wage industries—the sectors where we can expect the living wage to have the largest bite—bears some additional discussion. Our estimates for the five low-wage sectors we measure are all nearly zero, or slightly positive. These results strongly contrast with the findings of Adams and Neumark, who find significant negative employment effects for low-wage workers overall. For retail and restaurants our estimates are precise enough to reject the point estimates of their study.[42]

Effects on establishments

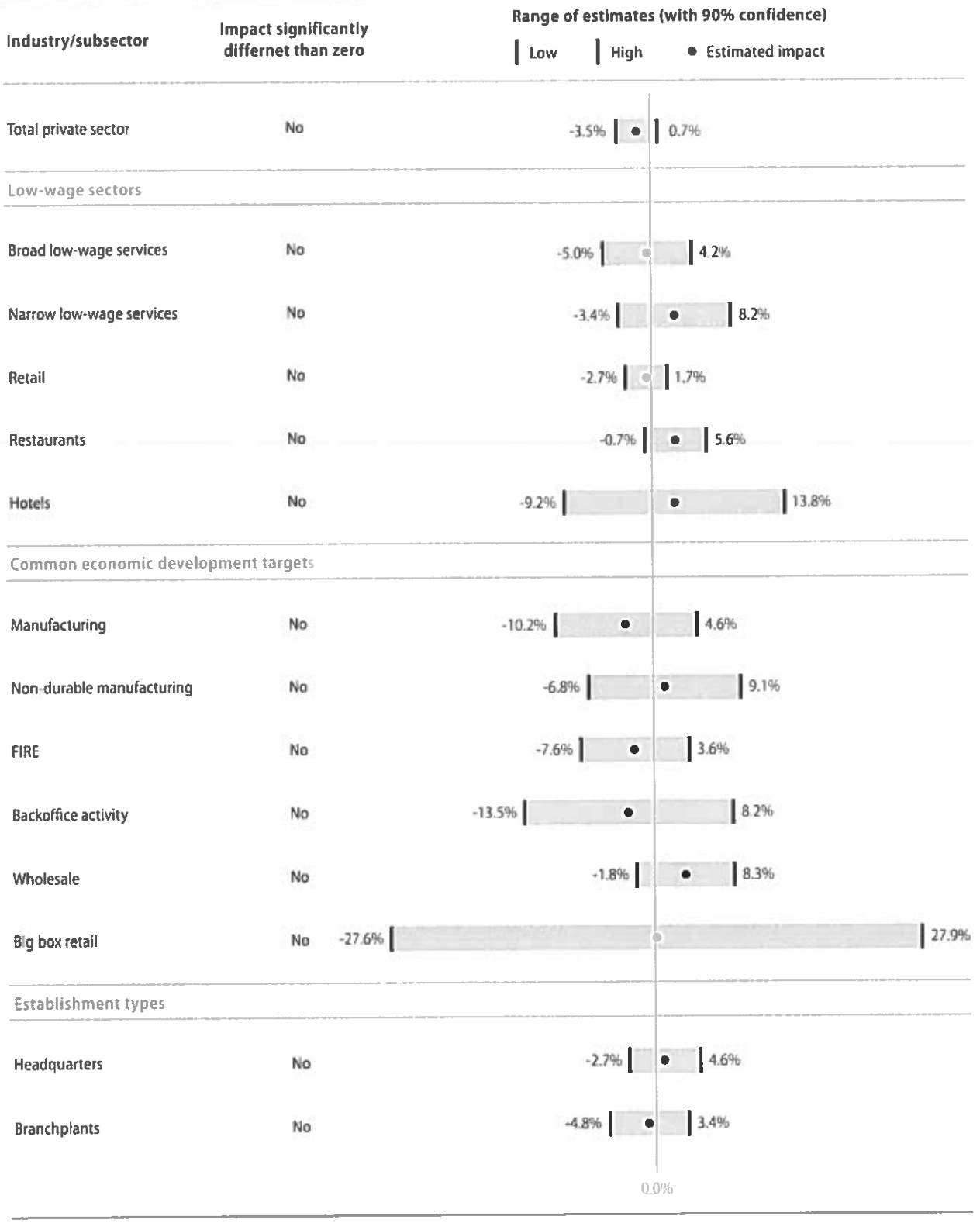
We also present our results for the number of business establishments in each outcome category to provide an additional measure of economic development activity. Even if business assistance laws do not affect aggregate employment levels in these sectors in a detectable manner, it is still possible that the overall number of businesses established in a living wage city would decrease due to negative signaling effects or because fewer businesses are attracted through local development initiatives.

Figure 2 presents the results in a parallel manner to Figure 1, with the dependent variable changed to establishment counts, rather than employment. Figure 2 visually represents the 90 percent confidence interval of our point estimates. Any number line that includes zero in the shaded area indicates that the estimated effect is not different than zero—meaning there is no employment effect, which is the case for all the variables tested, with the exception of the number of non-durable manufacturing establishments, which are estimated to slightly increase because of a business assistance living wage law.

The results presented in Figure 2 indicate that none of the 14 variables show any discernable—or statistically significant—negative effect on the number of business establishments. These results provide additional confirmation that the passage of a business assistance living wage law is unlikely to have a harmful effect citywide or in any particular industry.

FIGURE 1

Living wage employment impacts

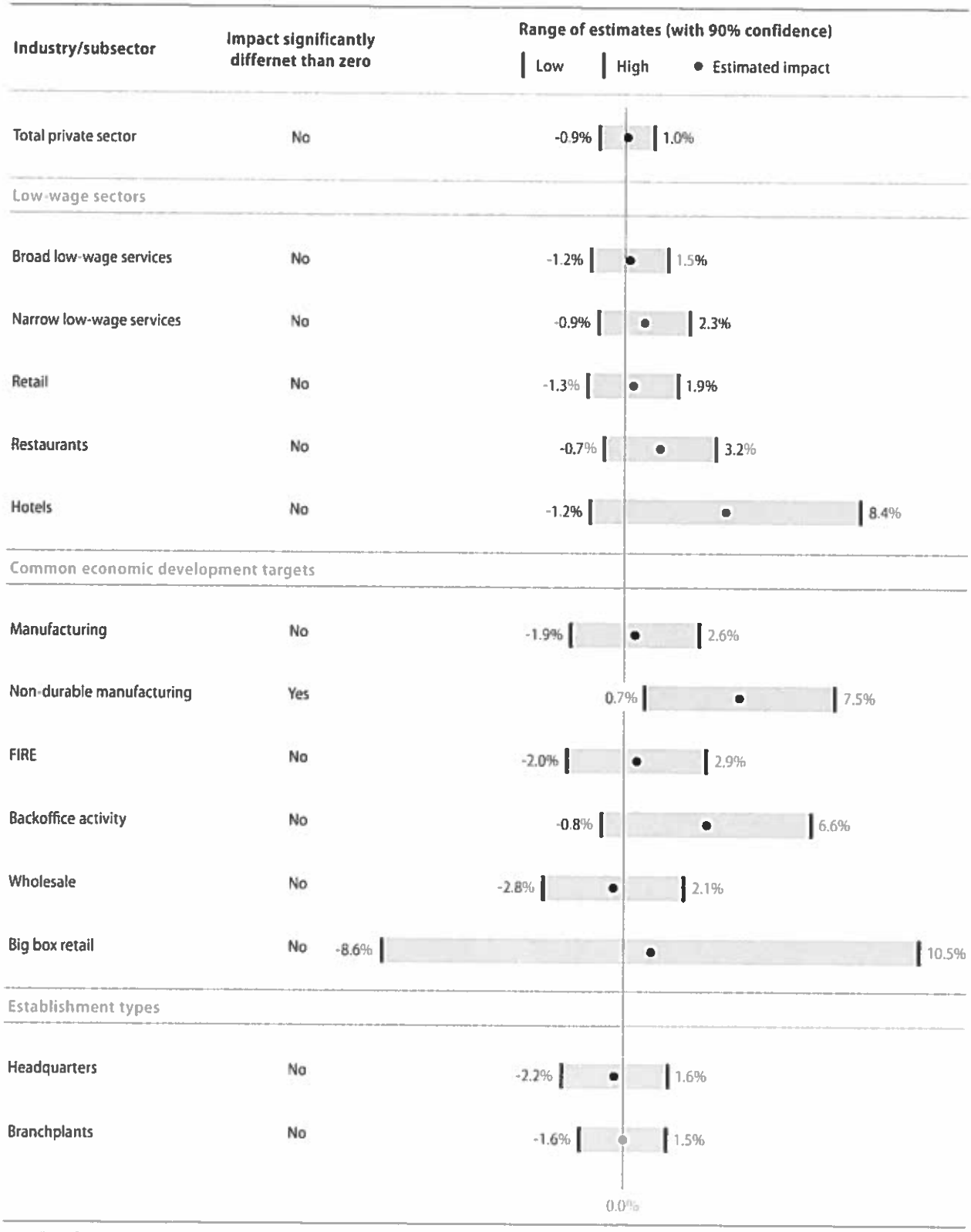


Note: All specifications include controls for the natural log of population, city linear trends, and city and year fixed effects.

N: 465, 16 controls and 15 treatment cities.

FIGURE 2

Living wage establishment impacts



Note: All specifications include controls for the natural log of population, city linear trends, and city and year fixed effects
N: 465, 16 controls and 15 treatment cities.

Conclusion and policy implications

Business assistance living wage laws are promoted as a way to maximize a city or county's economic development subsidies by supporting the creation of family-supporting jobs. Critics argue that an improvement in job quality comes at the expense of a reduction in the quantity of jobs. This study presents strong evidence that these claims are unfounded.

Previous empirical research on the impact of business assistance living wage laws has detected significant decreases in employment.[43] Yet experts have questioned this past research on the grounds that the data sources could not detect urban-level impacts and that they did not adequately control for whether cities actually enforce their business assistance provisions. This study uses a more robust dataset than the previous research and includes background archival research into each treatment city's law, and we find no evidence of negative employment effects from business assistance living wage laws. Our research design is conceptually identical to that of Adams and Neumark, yet we can rule out negative consequences of the scope they report.

One caveat is important here. Our dataset does allow for the detailed consideration of direct and indirect effects across a wide array of potential industries but we cannot use it to measure the effect on local wages. We cannot show that workers directly received wage increases due solely to the application of a business assistance living wage. This finding would be crucial in evaluating how effective living wage laws are on the main problems they attempt to address, such as poverty and inequality. Yet many other studies in the living wage literature have shown that workers and their families do receive wage increases.[44] It is important to consider these findings in conjunction with the type of detailed case studies that can gather direct observations of wages and employment at covered firms.

Our results—which indicate no significant impact on economic development outcomes—are far from an extreme finding. In fact, it is consistent with recent research on the economic impact of minimum wage laws.[45] These general findings that labor

standards such as the minimum and living wage do not result in the type of negative economic consequences predicted by either orthodox economic theory or critics of the laws stand to offer a strong alternative interpretation for policymakers.

Accurate information on business assistance laws is critical at this time, as the current economic crisis has increased pressure on local leaders to create jobs. Local governments are increasingly being asked by businesses to lower labor standards in exchange for investment. This study suggests that such calls to lower labor standards in exchange for jobs are not based in fact.

Economic development wage standards are one tool that a city can use to create jobs of greater quality. We have compared two sets of cities in order to assess the effectiveness of such laws—those with enforced business assistance living wage laws and those without—and found that there is no loss in the number of jobs due to the living wage requirement. It appears that, even during hard times, economic development wage standards are an effective tool for increasing wages in a city without sacrificing the number of jobs.

Technical appendix

Employment and establishment variables listed by Standard Industry Classification codes

Broad low-wage services: Personal Services (72), Business Services (73), Automotive Repair, Services and Parking (75), Miscellaneous Repair Services (76), and Amusement and Recreation Services (79).

Narrow low-wage services: Miscellaneous Personal Services (729), Mailing, Reproduction, Stenographic (733), Services to Buildings (734), Misc. Equipment Rental & Leasing (735), Personnel Supply Services (736), Guard services (738101), Automobile Parking (752), Automotive Repair Shops (753), Carwashes (7542), Commercial Sports (794), and Misc. Amusement, Recreation Services (799).

Retail: All establishments in SIC 51–59, with the exception of SIC 58, Eating and Drinking Places.

Restaurants: SIC 58, Eating and Drinking Places, including cafeterias.

Hotels: All establishments in SIC 701, Hotels and Motels.

Manufacturing: We include all establishments in SIC 20 through SIC 39 in this group.

Nondurable manufacturing: This variable includes establishments in SICs 20–29.

Back office: This variable includes establishments in the following SIC codes: Adjustment and collection services (7322), Direct mail advertising services (7331), Photocopying and duplicating services (7334), Computer and Data Processing Services (737), and Telephone services (738910).

Wholesale: This industry includes establishments in SICs 50 and 51.

Big-box retail: We approximate the big-box category by only including retail establishments that are branches of firms with at least 10 other locations and with sales volume in the top 75 percent of the other retailers in the city.

Finance insurance and real estate: This industry includes establishments in SICs 60–67.

Identification strategy

We use our panel dataset to estimate the following regression that measures how living wage laws effect employment and establishments for the industry groups described above.

$$\mathrm{Ln}(E_{it}^{k}) = a + \sum_{l=t-2}^{l=t+2} (\beta_l \cdot LW_{it(l)}) + \ln(\rho o \rho_{it}) + \delta_i + \gamma_t + \tau_{it} + \varepsilon_{it}$$

The dependent variable $\mathrm{Ln}(E_{it}^{k})$ is the natural log of the outcome variable (either employment or the count of establishments) in city i in year t. The model is estimated separately in the same for each of 14 industry groups or establishment types (k) such as retail or manufacturing or headquarters. Equation (1) predicts $\mathrm{Ln}(E_{it}^{k})$ as a function of a living wage indicator variable LW_{it}, which is coded 1 for each year that a business assistance living wage provision is in effect for an entire year in a given city. $LW_{(it)}$ is therefore zero for all years in the control sample and 1 for all years beginning in the calendar year after passage for the treatment group. The set of coefficients (β_l)that measure the effects of living wage passage are entered in distributed lag structure beginning two years before the living wage and continuing two years postpassage. The inclusion of lead terms on the LW variable captures what is happening to the outcome variable just before the law takes effect. This is important and has become a standard procedure in panel studies of causal effects because a spike (or dip) in employment just before the treatment can result in an erroneous treatment effect.[46] The inclusion of lag terms of LW (for example, postpassage) similarly accounts for long-term effects. The coefficient on the final lagged term $(\beta_{l=t+2})$ represents the cumulative effect not only in the second year after passage but in all years in the sample after passage. This is therefore the primary coefficient of interest for policy implications.

For controls, Equation (1) includes a term that measures the natural log of each city's annual population, fixed effects for each city δ_i—which control for and idiosyncratic differences between cities that do not vary with time—as well as year fixed effects, γ_t, which adjusts for common time effects such as changes in the macroeconomic environment. We also include a city-specific time trend, τ_{it}, that controls for differential trends in the outcome variable across the group of cities that vary over the entire time period. This is critical for the set of cities in our sample, which are drawn from various regions of the United States. If some cities are facing long-term declines in manufacturing employment and others are located in growing industrial regions, for example, we want to isolate the impact of living wage passage by removing the overall (time-varying) trend from each city. We also estimated Equation (1) with a time trend for each group (the treatment and control groups as a whole), as well as regional (for example, West, South, Northeast) trends to test that adding a city-specific trend potentially over controlled for differences between each city, and that the trend itself might be capturing some variation in the outcome variable that is attributable to the true living wage impact. Changing the scale of the time trend made no substantive difference in the results and, as such, is not reported here. Equation (1) also includes a constant term (a) and a random error term ε_{it}.

We only present results for ($\beta_l = t-2$) two years prior to and ($\beta_l = t+2$) two years after the passage of the living wage. The lagged term can be interpreted as the "long-term" impact of passing a business assistance living wage. The coefficients reported can be interpreted as the semielasticity of employment (or establishments) in response to changing living wage status. In other words, the percent change in the outcome variable that one can expect from passing a business assistance living wage law.

TABLE A

Results of employment regression

Industry/subsector	(1)	(2)	(3)	90% confidence interval on long-term effect	
	Pre-trend	Immediate effect	Long-term effect	Lower bound	Upper bound
Total private sector	-0.012	0.02	-0.014	-0.035	0.007
	(0.011)	(0.016)	(0.013)		
Retail	-0.023	-0.008	-0.005	-0.027	0.017
	(0.014)	(0.017)	(0.013)		
Broad low-wage services	0.025	0.012	-0.004	-0.05	0.042
	(0.024)	(0.037)	(0.028)		
Narrow low-wage services	0.053*	0.023	0.024	-0.034	0.082
	(0.030)	(0.032)	(0.035)		
Wholesale	-0.032	-0.002	0.032	-0.018	0.083
	(0.032)	(0.042)	(0.030)		
Restaurants	-0.002	-0.019	0.024	-0.007	0.056
	(0.024)	(0.032)	(0.019)		
Manufacturing	0.008	-0.046	-0.028	-0.102	0.046
	(0.041)	(0.050)	(0.044)		
Non-durable manufacturing	0.062	-0.119*	0.012	-0.068	0.091
	(0.045)	(0.070)	(0.047)		
FIRE	-0.007	0.071**	-0.02	-0.076	0.036
	(0.033)	(0.035)	(0.034)		
Hotels	0.166**	0.006	0.023	-0.092	0.138
	(0.083)	(0.062)	(0.069)		
Backoffice activity	-0.063	0.023	-0.026	-0.135	0.082
	(0.093)	(0.077)	(0.065)		
Big box retail	0.046	0.075	0.002	-0.276	0.279
	(0.082)	(0.118)	(0.166)		
Headquarters	-0.018	0.037	0.01	-0.027	0.046
	(0.025)	(0.035)	(0.022)		
Branchplants	0.001	0.011	-0.007	-0.048	0.034
	(0.023)	(0.031)	(0.025)		

Note: All specifications include controls for the natural log of population, city linear trends, and city and year fixed effects.

Column (1) lists the coefficient on the 2-year lead of LW treatment, Column (2) lists the contemporeneous effect, and Column(3) lists the long-term impact of LW treatment. Robust standard errors are clustered at the city level and are listed in parenthesis under each coefficient.

N: 465, 16 controls and 15 treatment cities.

*significant at .1 level, ** significant at .05 level, *** significant at .01 level

TABLE B

Results of establishments regression

Industry/subsector	(1)	(2)	(3)	90% confidence interval on long-term effect	
	Pre-trend	Immediate effect	Long-term effect	Lower bound	Upper bound
Total private sector	-0.01	0.001	0.001	-0.009	0.01
	(0.007)	(0.008)	(0.006)		
Retail	-0.021*	-0.005	0.003	-0.013	0.019
	(0.011)	(0.010)	(0.010)		
Broad low-wage services	-0.003	0.002	0.002	-0.012	0.015
	(0.009)	(0.008)	(0.008)		
Narrow low-wage services	-0.012	-0.002	0.007	-0.009	0.023
	(0.012)	(0.012)	(0.010)		
Wholesale	-0.006	-0.004	-0.004	-0.028	0.021
	(0.015)	(0.018)	(0.015)		
Restaurants	-0.023*	-0.006	0.0126	-0.007	0.032
	(0.013)	(0.012)	(0.012)		
Manufacturing	0.005	0.002	0.004	-0.019	0.026
	(0.013)	(0.013)	(0.014)		
Non-durable manufacturing	0.005	0.015	0.041**	0.007	0.075
	(0.014)	(0.027)	(0.020)		
FIRE	-0.012	0.005	0.00432	-0.02	0.029
	(0.012)	(0.017)	(0.015)		
Hotels	0.052	-0.01	0.036	-0.012	0.084
	(0.036)	(0.034)	(0.029)		
Backoffice activity	0.005	0.034	0.029	-0.008	0.066
	(0.031)	(0.031)	(0.022)		
Big box retail	-0.022	0.169**	0.0096	-0.086	0.105
	(0.063)	(0.080)	(0.057)		
Headquarters	0	-0.005	-0.003	-0.022	0.016
	(0.011)	(0.010)	(0.011)		
Branchplants	-0.021	0.002	-0.0003	-0.016	0.015
	(0.014)	(0.014)	(0.009)		

Note: All specifications include controls for the natural log of population, city linear trends, and city and year fixed effects.

Column (1) lists the coefficient on the 2-year lead of LW treatment, Column (2) lists the contemporaneous effect, and Column(3) lists the long term impact of LW treatment. Robust standard errors are clustered at the city level and are listed in parenthesis under each coefficient.

N 465, 16 controls and 15 treatment cities

*significant at .1 level, ** significant at .05 level, *** significant at .01 level

TABLE C
Description of living wage laws: Treatment sample

City	Passage date	Description
Ann Arbor, MI	3/5/2001	The legislation applies to employers holding city service contracts valued at $10,000 or more. Companies with fewer than five employees and nonprofits with fewer than 10 employees are exempt. The living wage was $11.71/hour in 2009 if the company provided health care insurance or $13.06/hour if it provided no insurance.
Berkeley, CA	6/1/2000	The ordinance applies to municipal workers, employers who are awarded city contracts, businesses receiving financial assistance, nonprofit organizations, and municipal leaseholders. The living wage in 2010 is $12.41/hour with health benefits or $14.47/hour if no insurance is provided.
Cambridge, MA	5/9/1999	The ordinance applies to municipal employees, city contractors and subcontractors who have contracts worth more than $10,000, and businesses who have received at least $10,000 in financial assistance. The living wage was $13.69/hour in 2009.
Cleveland, OH	6/19/2000	The ordinance applies to companies with 20 or more employees and nonprofits with 50 or more employees that receive at least $75,000 in financial assistance from the city, as well as tenants of recipients of financial assistance, and companies holding a contract with the city worth $25,000 or more. The ordinance also applies to subcontractors of companies who receive assistance or city contracts. The living wage in 2009 was $11.71/hour when health insurance was provided and $13.06/hour if health care was not provided.
Duluth, MN	7/14/1997	The legislation applies to employers and subcontractors who receive at least $25,000 of financial assistance in the form of business loans or grants, enterprise zone credits, tax increment financing, industrial land write-downs, and lease abatements.
Hartford, CT	10/12/1999	The ordinance applies to service contracts of $50,000 or more, development projects with $100,000 or more in city assistance, and real estate developments costing more than $25,000 on city-owned land. The living wage was $11.66/hour in 2009 if health insurance was provided and $17.78/hour if no insurance was provided.
Los Angeles, CA	5/5/1997	The ordinance applies to employers who are awarded assistance of $1,000,000 or more in one year or service contracts of $25,000 or more. It also applies to subcontractors and employers with public leases or licenses. The living wage is $10.30/hour with health insurance and $11.55/hour with no insurance in 2010. The living wage is subject to annual cost of living adjustments.
Minneapolis, MN	11/4/2005	The ordinance applies to employers with service contracts or subcontracts of $100,000 or more. Employers must attempt to create one living wage job for every $25,000 that they receive. The living wage in 2009 was $11.66/hr (110 percent of the federal poverty rate) with health insurance, or 13.78/hr (130 percent of federal poverty rate) without insurance.
Oakland, CA	4/1/1998	The ordinance applies to employers awarded $100,000 or more in assistance, city contractors receiving $25,000 or more, and leaseholders of recipients of assistance who occupy property that is improved through the assistance and employ 20 or more people. The living wage in 2009 was $9.13/hour with health insurance or $10.50/hour if no insurance is provided.
Philadelphia, PA	5/26/2005	The ordinance applies to city contractors with contracts worth more than $10,000 and recipients of city financial aid in excess of $100,000, as well as lessees of city property. It sets the living wage at 150 percent of the federal minimum wage. It includes a clause on health benefits, which states that an employer must provide health insurance if it provides benefits to some full-time employees elsewhere in the firm. The ordinances mandates a living wage advisory commission to oversee enforcement, of which businesses may represent no more than 4/9 of the members.
Richmond, CA	10/1/2001	The ordinance applies to all city contractors with a contract worth more than $25,000, and recipients of any local economic development aid of $100,000 or more. It also applies to lessees of public property that employ 25 full-time employees or more and generate $350,000 or more in annual gross receipts. And it includes subcontractors of contractors, economic development recipients, and lessees. The living wage was $11.42/hour if employer paid at least $1.50/hour in health benefits, or $12.92/hour without insurance at the time of the law's adoption.
San Antonio, TX	7/1/1998	The ordinance applies to businesses receiving tax abatements requiring they pay 70 percent of employees in new jobs $9.27/hour, and 70 percent of durable goods workers $10.13/hour. Businesses may be eligible for tax abatement if they fill 25 percent of new jobs with economically disadvantaged individuals.
San Francisco, CA	11/1/2000	The ordinance applies to employers who are awarded city contracts, businesses receiving financial assistance, nonprofit organizations, and municipal leaseholders at the San Francisco International Airport. It set wages at $10.00/hour in 2002 with 2.5 percent increases expected annually.
San Jose, CA	6/8/1999	The ordinance applies to employers who are awarded a service or labor contract of $20,000 or more, or assistance of $100,000 or more. The living wage was $11.61/hour in 2005 for employers who provided health insurance and $12.86/hour when employers provided no insurance.
Santa Fe, NM	2/27/2002	The ordinance applies to full-time municipal employees, city contractors who have contracts worth more than $30,000 and that have more than 10 employees, recipients of financial assistance worth $25,000 or more, and businesses requiring a license from the city. The living wage was $10.50/hour in 2009.

Endnotes

1 Alan Peters and Peter Fisher, "The Failures of Economic Development Incentives," *Journal of the American Planning Association* 70 (1) (2004).

2 Author's reanalysis of CPS wage data presented in: Lawrence Mishel, Jared Bernstein, and Heidi Shierholz, *The State of Working America 2008/2009* (Ithaca: Cornell University Press, 2009), Table 3.5.

3 Timothy J. Bartik, "Thinking About Local Living Wage Requirements," *Urban Affairs Review* 40 (2) (2004): 269–299.

4 For example, for the first time a significant proportion of North Carolina's state-level economic development incentives have gone to companies that pay wages below the county's average wage. See: David Bracken, "State settles for luring low-wage jobs," *News & Observer*, September 19, 2010, available at http://www.newsobserver.com/2010/09/19/690381/incentive-deals-settle-for-low.html.

5 Arindrajit Dube, T. William Lester, and Michael Reich, "Minimum Wage Effects Across State Borders: Estimates Using Contiguous Counties," *Review of Economics and Statistics* 92 (4) (2010).

6 For example, the City of San Antonio used financial incentives in 2005 to attract World Savings Bank to expand its mortgage processing facility in the area, creating approximately 2,000 jobs. In addition, the City of San Antonio also attracted an auto parts supplier in 2005, which agreed to raise wages from $10 per hour to $11.03 per hour in exchange for a 10-year tax abatement. See: David Hendricks, "Enticements for Toyota suppliers should pay dividends for S.A.," *San Antonio Express-News*, July 9, 2005; Elizabeth Allen, "World Savings banking on tax breaks," *San Antonio Express-News*, November 17, 2005.

7 While many economists refer to spillover effects as an indirect rather than direct policy outcome, we use the term "direct spillover" to distinguish between those effects that result in clear upward wage pressure on firms—either through a mandated wage floor (narrow direct) or through competitive effects (direct spillover)—from those that have indirect effects on the local political context for economic development.

8 Such responses to wage floors are consistent with the monopsonistic model of the labor market. See: Alan Manning, *Monopsony in Motion* (Princeton, NJ: Princeton University Press, 2003).

9 Bartik, "Thinking About Local Living Wage Requirements."

10 David Card and Alan B. Krueger, "Minimum Wages and Employment: A Case Study of the Fast-Food Industry in New Jersey and Pennsylvania," *The American Economic Review* 84 (4) (1994): 772–793; Dube, Lester, and Reich, "Minimum Wage Effects Across State Borders"; David Neumark and William Wascher, "Employment Effects of Minimum and Subminimum Wages: Panel Data on State Minimum Wage Laws," *Industrial and Labor Relations Review* 46 (1) (1992): 55–81.

11 Christopher Niedt and others, "The Effects of the Living Wage in Baltimore." Working Paper 119 (Economic Policy Institute, 1999).

12 In its most basic form, the difference-in-differences, or DD method simply calculates employment changes before and after passage of a law in both the treatment and control groups and then subtracts the employment difference in the treatment group from the difference in the control.

13 David Fairris, "The Impact of Living Wages on Employers: A Control Group Analysis of the Los Angeles Ordinance," *Industrial Relations* 44 (1) (2005): 84–105. Card and Krueger's method included in: Card and Krueger, "Minimum Wages and Employment."

14 Michael Reich, Peter Hall, and Ken Jacobs, "Living Wage Policies at the San Francisco Airport: Impacts on Workers and Businesses," *Industrial Relations* 44 (1) (2005): 106–138.

15 Robert Pollin and Jeannette Wicks-Lim, "Comments on Aaron Yelowitz, 'Santa Fe's Living Wage Ordinance and the Labor Market.'" Working Paper WP108 (Political Economy Research Institute, 2005).

16 Arindrajit Dube, Suresh Naidu, and Michael Reich, "The Economic Impacts of a Citywide Minimum Wage." Working Paper 111-05 (Institute of Industrial Relations, 2006).

17 David Neumark, "How Living Wage Laws Affect Low-Wage Workers and Low-Income Families" (San Francisco: Public Policy Institute of California, 2002).

18 Mark D. Brenner, Jeannette Wicks-Lim, and Robert Pollin, "Measuring the Impact of Living Wage Laws: A Critical Appraisal of David Neumark's 'How Living Wage Laws Affect Low-Wage Workers and Low-Income Families.'" Working Paper 43 (Political Economy Research Institute, 2002).

19 David Fairris, "The Impact of Living Wages on Employers: A Control Group Analysis of the Los Angeles Ordinance," *Industrial Relations* 44 (1) (2005): 84–105.

20 Scott Adams and David Neumark, "The Effects of Living Wage Laws: Evidence from Failed and Derailed Living Wage Campaigns." Working Paper 11342 (National Bureau of Economic Research, 2005).

21 Mark D. Brenner, Jeannette Wicks-Lim, and Robert Pollin, "Detecting the Effects of Living Wage Laws." In Robert Pollin and others, eds., *A Measure of Fairness: The Economics of Living Wages and Minimum Wages in the United States* (Ithaca: ILR Press, 2008).

22 Adams and Neumark, "The Effects of Living Wage Laws."

23 Brenner, Wicks-Lim, and Pollin, "Detecting the Effects of Living Wage Laws."

24 Stephanie Luce, *Fighting for a Living Wage* (Ithaca: Cornell University Press, 2004).

25 For example, St. Louis technically has a business assistance law but it only applies when a firm receives at least $20 million in local incentives. As discussed below, in this paper we treat St. Louis as a control city since its business assistance provision is weak.

26 Neumark, "How Living Wage Laws Affect Low-Wage Workers and Low-Income Families"; Adams and Neumark, "The Effects of Living Wage Laws."

27 T. William Lester, "The Impact of Living Wage Laws on Urban Economic Development Patterns and the Local Business Climate: Evidence from California Cities," *Economic Development Quarterly* (forthcoming).

28 Fairris, "The Impact of Living Wages on Employers."

29 Examples of such natural experiments in labor economics include spatial discontinuities such as comparing employment on either side of a state line after a minimum wage increase (see: Card and Krueger, "Minimum Wages and Employment"; Dube, Lester, and Reich, "Wage Effects Across State Borders") as well as regression discontinuity approaches (see: John DiNardo and David S. Lee, "Economic Impacts of New Unionization on Private Sector Employers: 1984-2001," *The Quarterly Journal of Economics* 119 (4) (2004): 1383–1441).

30 Adams and Neumark, "The Effects of Living Wage Laws."

31 Ibid.

32 Ibid.

33 "Employment Policies Institute," available at http://www.epionline.org (last accessed April 30, 2010). The Living Wage Resource Center was a website maintained by the now-defunct Association of Community Organizations for Reform Now, or ACORN, which was very active in supporting and passing living wage laws across the country. This web-based listing contained information on living wage type, wage level, and date of passage, and was accessed in 2008 during the author's dissertation research, which is published as Lester, "The Impact of Living Wage Laws on Urban Economic Development Patterns and the Local Business Climate: Evidence from California Cities," (forthcoming).

34 Brenner, Wicks-Lim, and Pollin, "Detecting the Effects of Living Wage Laws."

35 Chicago, Illinois, did not reject an explicit business assistance form of living wage but we included it since the City Council passed a living wage law that applied to "big-box" retailers in 2006 that the mayor immediately vetoed. Throughout the late 1990s and early 2000s, Chicago used Tax Increment Financing, or TIF, to help bring in a host of big-box retailers including Target and Home Depot. Although this legislation was not tied to the receipt of financial assistance, since it targeted a segment of the retail market that had become accustomed to receiving development assistance, it would have been functionally equivalent to a business assistance provision. In addition, during the ongoing debates over the merits of the law, opponents frequently aired the negative "business climate" argument, which suggests that the law would have had a similar indirect effect.

36 Note that the earliest living wage law in our sample occurred in 1998.

37 To ensure that new businesses are captured by their telephone surveys, D&B reviews each state's database of fictitious name filings and business incorporation listings. While D&B makes multiple attempts to reach each establishment, there are cases in which a DUNS number appears for several years, then disappears, and then reappears at the same address. In such cases, Walls and Associates imputes employment figures for each missing year based on the previous available records.

38 David Neumark, Junfu Zhang, and Brandon Wall, "Employment Dynamics and Business Relocation: New Evidence from the National Establishment Time Series." Working Paper W11647 (National Bureau of Economic Research, 2005).

39 Since NETS has a higher capture rate for very small firms, including self-employed persons, it is less comparable with other publically available data sources such as the QCEW or County Business Patterns. Previous research indicates that for establishments with five or more employees there is a high correlation between employment measurements in NETS and other sources.

40 To match zip codes to the political jurisdictions we used a geographical association based on the population-weighted centroid of each zip code in 2000. We obtained the zip-to-place match from the MABLE/Geocorr2K: Geographic Correspondence Engine v1.3.3 (August 2010), published by the Missouri Census Data Center, available at http://mcdc2.missouri.edu/websas/geocorr2k.html. While we understand that zip code boundaries shift over time, and that new zip codes are created that would perhaps not be recognized by the 2000 Census, this turned out not to be a significant issue for our sample of large core urban counties in the NETS. In our sample of more than 1 million establishments from the counties that contained our treatment and control cities, 95.1 percent of records were matched to a city (i.e. census place) using this method. For the remaining 4.9 percent we geocoded each record based on their reported current latitude and longitude in the NETS database. To be fair, among this group of geocoded records (4.9 percent) we are not able to capture the effect of moves since the latitude and longitude information is only available for the last year the establishment was active in the database. However, of this group only 9 percent ever moved, resulting in an overall capture rate of firm moves of 99.9 percent for the entire sample.

41 Please note that the variable for big box retail, due to how it was narrowly defined, doesn't allow us to include all the treatment and control cities in the analysis. This leads the standard errors to be much bigger than all the other estimates.

42 Adams and Neumark, "The Effects of Living Wage Laws."

43 Ibid.

44 Michael Reich, Peter Hall, and Ken Jacobs, "Living Wage Policies at the San Francisco Airport: Impacts on Workers and Businesses," *Industrial Relations* 44 (1) (2005); David Fairris and others, "Examining the Evidence: The Impact of the Los Angeles Living Wage Ordinance on Workers and Businesses" (Los Angeles: Los Angeles Alliance for a New Economy, 2005); Mark Brenner, "The Economic Impact of the Boston Living Wage Ordinance," *Industrial Relations* 44 (1) (2005).

45 Dube, Lester, and Reich, "Wage Effects Across State Borders"; Card and Krueger, "Minimum Wages and Employment."

46 Orley Ashenfelter and David Card, "Using the Longitudinal Structure of Earnings to Estimate the Effect of Training Programs," *The Review of Economics and Statistics* 67 (4) (1985): 648–660.

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