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The Gender Wage Gap and Domestic Violence

By Anna Aizer*

Three-quarters of all violence against women is perpetrated by domestic partners, with poor women disproportionately affected. The estimated costs of domestic violence in terms of medical care and declines in productivity exceed $5.8 billion annually (Centers for Disease Control 2003). In this paper I examine the impact of the gender wage gap on levels of domestic violence in the United States. An economic theory of household bargaining that incorporates violence predicts that increases in a woman's relative wage increase her bargaining power and lower levels of violence by improving her outside option. To test the predictions of this theory, I estimate the impact of the gender wage gap on violence against women by exploiting exogenous changes in the demand for labor in female dominated industries relative to male dominated ones. I find that decreases in the wage gap reduce violence against women.

This research addresses a number of limitations in existing work. First, most previous studies of the relationship between women's income and domestic violence fail to establish a causal relationship by failing to account for the potential for omitted variable bias or reverse causality. Even the handful of papers that do consider this potential endogeneity focus largely on a woman's own wage when a household bargaining model suggests both that a woman's *relative* wage matters and that *potential*, not actual, wages determine bargaining power and levels of violence. Finally, previous work is based on survey data which are prone to nonrandom underreporting and are not consistently collected over time.

To overcome these shortcomings, I employ two strategies. First, I develop a new measure of violence based on administrative data: female hospitalizations for assault. These data represent an improvement over individual survey data because they do not necessarily rely on self-reports of violence, are consistently collected over a long period of time, and include the universe of women in California (roughly 15 million individuals). Second, to overcome the endogeneity of individual wages and account for the fact that theory predicts that potential, not actual, wages affect violence, I analyze the impact of the wage gap as a function of local demand for female and male labor on domestic violence. To do so I take advantage of the fact that certain industries have traditionally been dominated by women (e.g., services) and others by men (e.g., construction) to create sex-specific measures of prevailing local wages based on the industrial structure of the county and statewide wage growth in industries dominant in each county. Constructed in this way, this measure of the gender wage gap reflects sex-specific labor demand (see Timothy Bartik 1991; Olivier J. Blanchard and Lawrence F. Katz 1992) not underlying worker characteristics in the county which could be correlated with domestic violence. I find that reductions in the gender wage gap explain nine percent of the decline in domestic violence witnessed between 1990 and 2003.

While these findings are consistent with a model of household bargaining that incorporates violence, they are inconsistent with sociocultural models of "male backlash" that predict that as

* Department of Economics, Brown University, 64 Waterman Street, Providence, RI 02912 and NBER (e-mail: aizer@brown.edu). The author thanks Janet Currie, Pedro Dal Bó, Mark Duggan, Melissa Kearney, and seminar participants at Brown University, UC Berkeley Goldman School, the University of Maryland, and the BU/Harvard/MIT joint seminar in health economics for helpful comments and suggestions. This research project was supported by NSF-SES 0648700 and NIH RO3HD051808-01A2.

women's wages increase, violence against them increases because men feel their traditional gender role threatened. They are also inconsistent with the model of exposure reduction developed by criminologists that predicts that as the labor force participation of women increases, violence against them may decline because women spend less time with their violent partners. I find that the reductions in violence occur during nonworking hours, which is inconsistent with exposure reduction. These findings shed new light on the health production process as well as observed income gradients in health and suggest that in addition to addressing concerns of equity and efficiency, pay parity can also improve the health of American women via reductions in violence.

I. Background on Domestic Violence

A. *Prevalence of Domestic Violence and Risk Factors*

Every day 14 thousand women in the United States are battered and four are killed by their intimate partners. Data on domestic violence from the 1994 National Violence Against Women survey reveal an annual prevalence of two percent, a lifetime prevalence of 25 percent and that intimate partners are responsible for three-quarters of all violence against women (Patricia Tjaden and Nancy Thoennes 1998). Disadvantaged women face much higher risks of abuse. Women with annual income below $10,000 report rates of domestic violence five times greater than those with annual income above $30,000 (Bureau of Justice Statistics 1994). Black women are also at significantly greater risk of violence (Callie M. Rennison and Sarah Welchans 2000). The National Crime Victimization Survey is the only survey that allows tracking of domestic violence over time, and these data suggest that reported rates have declined by 50 percent between 1993 and 2001, a trend that is likewise present in the California hospitalization data analyzed here.

B. *Theories of the Relationship between Wages and Violence*

Most research on domestic violence has been conducted by criminologists and sociologists who have examined domestic violence largely through a sociocultural lens. Criminologists have developed a theory of exposure reduction that posits that the increase in employment among either men or women will reduce domestic violence by reducing the time partners spend together (Laura Dugan, Daniel Nagin, and Richard Rosenfeld 1999). The theory of "male backlash" prominent in the sociological literature predicts that as women's financial independence increases, violence against them should increase. According to Ross Macmillan and Rosemary Gartner (1999), a wife's independence "signifies a challenge to a culturally prescribed norm of male dominance and female dependence. Where a man lacks this sign of dominance, violence may be a means of reinstating his authority over his wife" (p. 949). A theory of male backlash that predicts that an increase in women's wages leads to an increase in violence is problematic because it ignores the individual rationality constraints faced by women in abusive relationships. That is, as their income increases, women are more likely to end the partnership if transfers decline and abuse continues.

Economic theories of household bargaining incorporate individual rationality constraints but generally do not incorporate violence. In the Appendix I present a Nash bargaining model in which utility is a function of consumption and violence, with the man's utility increasing in violence and the woman's decreasing in violence.[1] The main result is that increasing a woman's

[1] Amy Farmer and Jill Tiefenthaler (1997) present a particular case of a noncooperative model of domestic violence in which men have all the bargaining power.

relative wage increases her bargaining power and lowers the level of violence by affecting her outside option. This is inconsistent with the model of male backlash.

Two additional implications of the household bargaining model are worth highlighting as they inform the empirical analysis. First, relative wages matter. Second, it is the potential wage that determines one's outside option, not the actual absolute wage.[2] This suggests that one should focus on relative labor market conditions for women, not women's actual absolute wages, in the analysis. This also implies that improving labor market conditions for women will decrease violence even in households where women do not work (Robert A. Pollak 2005).

C. *Previous Empirical Work on Wages and Violence*

The pioneering study of the relationship between women's income and violence is Richard Gelles (1976), who finds that the fewer resources a woman has, the less likely she is to leave an abusive relationship. This work and many others that followed did not consider the potential endogeneity of women's income in this context. Specifically, omitted variables associated with women's wages such as education might explain the negative relationship with violence, or the relationship might simply reflect reverse causality—declines in abuse may increase a woman's productivity and earnings.

More recently, economists have employed structural methods or used panel data to overcome the problem posed by endogenous wages. Audra J. Bowlus and Shannon Seitz (2006) use structural methods to estimate a negative impact of female employment on abuse. Helen V. Tauchen, Ann D. Witte, and Sharon K. Long (1991) and Farmer and Tiefenthaler (1997) utilized panel data on victims of domestic violence to examine the impact of changes in a woman's income over time on violence. Panel data enables one to overcome the potential for bias from omitted variables if they are time invariant but does not rule out the potential for reverse causality. Also, results based on a small sample of women in shelters may not be generalizable. The only experimental evidence on the impact of women's economic status on domestic violence comes from a randomized intervention combining microfinance with violence education in South Africa. Women randomized to receive the intervention experienced a 55 percent drop in domestic violence relative to the control group (Paul Pronyk et al. 2006).[3]

But none of the existing work captures the importance of relative female labor market conditions, which theory predicts can explain a decline in domestic violence even in households where women do not work. In this paper I provide the first causal estimates of the impact of women's relative labor market conditions on domestic violence based on a large and representative sample of women that would capture effects in all households. I discuss the threats to identification and my strategies to address them in the next section.

II. Identification of the Impact of the Wage Gap on Domestic Violence

There are two main threats to identification of the impact of the gender wage gap on domestic violence. The first is the lack of objective measures of domestic violence collected consistently

[2] This is due to the fact that a woman's earnings at her threat point determine her bargaining power, and earnings at the bargaining equilibrium do not necessarily equal earnings at the threat point. Pollak (2005) provides an example of a married woman who does not work (zero wages) at the cooperative equilibrium but who would work in the event of the dissolution of the marriage.

[3] Other related work on domestic violence more generally but not the relationship between violence and income include Dugan, Nagin, and Rosenfeld (1999), Francis Bloch and Vijayendra Rao (2002), Betsey Stevenson and Justin Wolfers (2006), Thomas Dee (2003), Angela Fertig, Irwin Garfinkel, and Sarah McLanahan (2004), and Jennifer Nou and Christopher Timmins (2005).

over time. Previous work has found that self-reported measures of domestic violence are underestimates, and that the degree of misreporting is nonrandom (Mary Ellsberg et al. 2001). Even if one could accurately model the degree of underreporting, there exists no panel of self-reported domestic violence that would enable one to estimate the impact of changes in labor market conditions. Utilizing a cross-section of data is problematic because of the difficulty controlling for multiple differences (in addition to labor market conditions), across geographic regions that might bias estimates.

The second threat to identification is the difficulty constructing measures of relative labor market conditions that do not reflect the underlying characteristics of male and female workers which could be a function of underlying violence (abused women are less productive) or unobservables that might be correlated with violence (e.g., education). Thus, for purposes of identification, one ought to construct a measure of prevailing female (male) wages that reflects only the exogenous demand for female (male) labor.

To address these two threats to identification I construct new measures of both violence against women and relative wages. The measure of violence against women is derived from administrative data on female hospitalizations for assault for the state of California. This measure is collected consistently over a long period of time (1990–2003) and contains detailed geographic identifiers that enable one to characterize the local labor market and include local market (county) fixed effects. In addition, this measure does not rely on self-reports of domestic violence. I include all hospitalizations for assault based on physician classification of injury. As such, the measure is not a function of self-reported battery. However, this measure will also reflect nonintimate violence. To the extent that three-quarters of violence against women is intimate and I can control for trends in nonintimate violent crime in the regressions, any potential bias from this measurement error is limited.[4]

The measure of relative wages is constructed so as to reflect exogenous demand for female and male labor and is based on the index of labor demand originally proposed by Bartik (1991) and subsequently used by Blanchard and Katz (1992), John Bound and Harry J. Holzer (2000), Hilary W. Hoynes (2000), and David H. Autor and Mark G. Duggan (2003). This strategy takes advantage of a history of sex and race segregation by industry that is well established (Kimberly Bayard et al. 1999) to construct measures of local labor market wages of women (men) that are based on wage changes in industries dominated by women (men). For example, data for California reveal that 72 percent of service industry employees are women, while 90 percent of those employed in the construction industry are men.

Average annual wages are calculated by gender and race in each county as follows:

$$\overline{w}_{grcy} = \sum_j \gamma_{grcj}\, w_{-cyj} \tag{1}$$

where g indexes gender, r race, c county, y year, and j industry. γ_{grjc} is the proportion of female (or male) workers with no more than a high school diploma of a given race working in industry j in county c (from the 1990 Census). I focus on low-skilled workers because violence is much more prevalent among this group (see Table 1). This proportion (γ) is fixed over this period so that changes in the wage do not reflect selective sorting across industries over this period. W_{-cyj} is the annual wage in industry j in the state except for county c in year y from the Bureau of Economic

[4] This measure will also capture only severe violence. To the extent that there is less discretion in the use of hospitalization in the case of severe violence, we limit measurement error by focusing on hospitalizations. However, one might be concerned that this captures violence against those women who have no other source of medical care. In later regressions I focus on hospitalization for assault during the weekend, when there are clearly very few, if any, other sources of medical care, and the results remain.

TABLE 1—MEASURES OF VIOLENCE OVER TIME AND BY SOCIOECONOMIC STATUS

	1990	2003	Percent change
Panel A. All violence			
Female assaults per 100,000	39.3	12.1	−69
Intimate partner homicide per 100,000	1.6	1.1	−31
Non–intimate partner homicide per 100,000	18.6	14.0	−25
Assaults adjusted for declines in hospital use			−36
Panel B. Assaults per 100,000 pregnant women			
All	31		
Medicaid	59		
Private pay	12		
< HS	41		
College	1.3		

Analysis annual survey of employers. By measuring prices over all counties in the state except the focal county, I remove from the measure any changes in industry wages that might be caused by changes in the underlying characteristics of workers in the county. With the wage constructed in this way, identification comes from the fact that counties with many workers in industries characterized by large, statewide wage growth will experience larger increases in average wages than counties with many workers in low–wage growth industries.[5]

This inflation-adjusted measure of the female/male wage ratio increases 3.6 percentage points between 1990 and 2003 from 0.945 to 0.981. Over this period, the true wage ratio increased from 70 to 75.5 percent (5.5 percentage points) among low skilled workers in California. We can think of the true wage gap as composed of a between-industry and a within-industry component, with the wage gap measured according to (1) representing the between-industry wage gap. It is interesting to note that even though within-industry differences in wages (between men and women) explain more of the total wage gap than between-industry differences *in levels*, between-industry differences explain more of the *change* in the wage gap over the period 1990–2003.

III. Empirical Results

A. *Descriptive Analysis—Prevalence/Trends in Domestic Violence*

Descriptive analysis of the prevalence and trends in domestic violence over this period yields a number of interesting results (Table 1). The rate of female hospitalization for assaults (per 100,000 women) declined nearly 70 percent from 39 to 12. But this downward trend reflects both declines in underlying violence and declines in hospital utilization more generally. To control for the latter, I also present the decline in assaults regression-adjusted for secular trends in hospitalization in the fourth row of Table 1. This adjusted measure of female hospitalization for assaults still declines markedly over this period, but less so, by 36 percent.

[5] I remove military workers from this analysis because they are unlikely to be represented in the hospitalization data (since it excludes military and VA facilities), nor are they likely to be represented in the arrest data, which exclude the military police.

As an external validity check, I compare this with the decline in intimate partner homicides which criminologists consider a well-defined, well-measured, if imperfect, estimate of domestic violence. Intimate partner homicides in California decline by 31 percent over this period (row 2, Table 1) which is very similar to the decline in (adjusted) female hospitalizations for assault. It is important to note that violent crime more generally also declined over this period, though not as much as domestic violence. For example, nonintimate homicides in California declined 25 percent over this same period, which is very similar to the 20 percent decline in male hospitalizations for assault (not shown). In the regression analyses that follow, I control for both secular trends in violent crime and hospital utilization more generally to help identify the impact of the wage gap on domestic violence.

There are significant differences in these measures of violence among women—with poor and less educated women disproportionately affected. Panel B of Table 1 shows differences in rates of assaults by insurance status (a proxy for income) and education for pregnant women in 1990/1991 for whom, because the data are matched with birth certificate data, we have additional information. Women on Medicaid, who have income at or below 200 percent of the federal poverty line (FPL), are nearly six times more likely to have been admitted to the hospital for an assault while pregnant than private pay patients. And while 41 (per 100,000) pregnant women without a high school diploma are admitted for an assault, only 1.3 pregnant women with a college degree are.

B. *Regression Estimates of the Impact of the Wage Gap on Domestic Violence—Main Specification*

To estimate the impact of the wage gap on domestic violence, the following equation is estimated using panel data for the period 1990–2003:

$$(2)\quad DV_{cry} = \alpha + \beta_1 WAGERATIO_{cry} + \beta_2 UNEMP_{cy} + \beta_3 INC_{cy} + \beta_4 RACE_r + \beta_5 POP_{cry} + \beta_6 VIOLENT\ CRIME_{cry} + \beta_7 DV_{cry-1} + \gamma YEAR_y + \theta COUNTY_c + \pi COUNTY \times YEAR_{cy} + \lambda RACE_r \times YEAR_{ry} + \varepsilon_{cry}.$$

Each observation is a county-race-year cell with c indexing county, r race, and y year. *DV* refers to the natural log of female assaults derived from administrative hospitalization data. Natural logs are used so that estimates across multiple specifications are comparable and because significant variation in the levels of violence within the population suggests that estimating proportional effects is more suitable.[6] *WAGERATIO* is the ratio of female to male wages constructed according to equation (1).[7] *UNEMP* is the annual unemployment rate in the county, and *INC* is the natural log of per capita income in the county and year. These are included so that the impact of relative income can be identified separately from the impact of general economic conditions in the county. *RACE* is a vector of race dummies (black, Asian and Hispanic—white is excluded). *POP* is the natural log of the number of women between the ages of 15 and 44 in the county of a given race in year y. *VIOLENT CRIME* is the natural log of nonintimate homicides by county, race and year and is included to control for secular trends in violent crime. County fixed effects and county and race

[6] The results are robust to a linear specification.

[7] Examining the impact of relative wages within racial groups is justified given that interracial relationships are still relatively rare over this period: 14 percent for 18–19-year-olds, 12 percent for 20–21, and 7 percent for 34–35-year-olds (Kara Joyner and Grace Kao 2005).

specific linear time trends are included to control for any unobserved fixed differences between counties and any county and race-specific linear time trends in domestic violence, respectively. The year fixed effects will control for all statewide policy changes such as welfare reform, expansions in the *EITC*, changes in Medicaid eligibility, or state laws regarding the prosecution of domestic violence as well as the federal Violence Against Women Act of 1994 that may affect rates of domestic violence. I also include the natural log of female admissions for nonassault injuries and the natural log of male assaults to control for secular trends in hospital utilization. The latter also likely captures secular changes in violent crime not captured by homicides. Finally, in some specifications I also include lags of the dependent variable (DV_{cry-1}) to control for any other omitted time varying characteristics.[8] These would include any unmeasured changes in the underlying composition of women (or men) in the state that are correlated with domestic violence in the recent past. All regressions are limited to county-race-year cells with female population of at least 10,000 to increase the precision of measures of violence based on moderate to low frequency events. I also weight all observations by the female population in the cell.

The results from the main specification are presented in Table 2. For purposes of comparison, in the first column I present estimates from a regression that includes only minimal controls (fixed effects in levels and trends and the natural log of the female population). The relationship between the wage ratio and female assaults is very large, negative, and significant when only few controls are included. In column 2, I include most of the controls listed in equation (2) with the exception of lagged domestic violence. The estimate of β_1 (-0.831) implies that an increase in the ratio of female to male wages significantly reduces the number of women admitted to the hospital for an assault. However, the estimate declined by 40 percent from column 1 to column 2, underscoring the importance of including proper controls to reduce bias. In column 3 I include the lag of the dependent variable to control for any other time varying unobservables not captured in the extensive set of controls that may bias the results. The estimated impact declines only slightly (0.831 to 0.813), suggesting that the controls included are fairly comprehensive. The wage gap has no impact on admissions for substance abuse (column 4), which is included here as a falsification test.[9]

To gauge the size of these effects, I calculate how much of the decline in violence witnessed over the period 1990–2003 is explained by closing the wage gap by 3.6 percentage points (the actual decline for this measure over this period). The narrowing of the wage gap over this period explains nine percent of the decline in hospital admissions for assault (controlling for secular trends in hospitalization). In column 5, I present estimates of the impact of the wage gap on the natural log of male assaults. The coefficient estimate (-0.257) is only 30 percent of that for female assaults and is statistically insignificant. While we would expect this estimate to be smaller, we would not necessarily expect it to be zero. Work on domestic violence conducted by criminologists has found that interventions aimed at reducing domestic violence often lead to significant declines in men assaulted by their partners in self-defense (Dugan, Nagin, and Rosenfeld 1999).[10]

Because an increase in women's wages is likely to be accompanied by an increase in female employment, finding that domestic violence falls as female wages rise (relative to men's) may be

[8] It's well established that fixed effects models with lagged dependent variables are biased for small ("fixed") T (Stephen Nickell 1981), and this bias can be approximated by $-(1 + \beta_7)/(T - 1)$. However, the purpose of including lagged domestic violence is only to show that the coefficient on the wage ratio is unchanged, thereby providing additional evidence that the estimate of the impact of relative wages on violence does not suffer from omitted variable bias.

[9] I also estimate the impact of the wage ratio on female hospitalizations for car crashes and suicide attempts—both estimates are small and statistically insignificant.

[10] Anna Aizer and Pedro Dal Bó (2009) also provide evidence that strengthening the prosecution of batterers results in a decline in violence against men killed by their partners.

Table 2—Impact of Wages on Domestic Violence—Main Specification

	ln(female assaults) (1)	ln(female assaults) (2)	ln(female assaults) (3)	ln(drug admissions) (4)	ln(male assaults) (5)	ln(female assaults) (6)
Panel A. Ratio of wages						
Female/male wage	−1.469	−0.831	−0.813	−0.023	−0.257	0.119
	[0.673]	[0.313]	[0.317]	[0.072]	[0.284]	[0.562]
Female/male wage × weekend						−1.15
						[0.444]
Observations	984	982	982	887	982	616
R^2	0.91	0.95	0.96	0.99	0.99	0.96
Panel B. Linear difference in wages						
Male wage–female wage	0.0047	0.0024	0.0024	0	0.0009	−0.0003
	[0.0020]	[0.0009]	[0.0009]	[0.000]	[0.0008]	[0.0017]
(Male-female wage) × weekend						0.0031
						[0.0015]
Observations	984	982	982	887	982	616
R^2	0.91	0.95	0.96	0.99	0.99	0.96
County, year, race fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
County and race specific linear time trends	Yes	Yes	Yes	Yes	Yes	Yes
ln(population), ln(nonintimate homicides)		Yes	Yes	Yes	Yes	Yes
Unemployment rate and ln(per capita income)		Yes	Yes	Yes	Yes	Yes
Lagged dependent variable			Yes	Yes	Yes	
ln(nonassault injuries)		Yes	Yes		Yes	Yes
ln(assaults opposite sex)		Yes	Yes		Yes	Yes

Notes: Robust standard errors clustered on county in brackets. Column 4 includes data from 1992–2003; column 6 includes data for years 1990, 1992–1994, and 1996 and also includes the main effect of weekend and interactions between weekend and unemployment and per capita income.

evidence of either a bargaining story or exposure reduction.[11] To test whether exposure reduction is responsible for these findings, I estimate the impact of changes in the wage ratio on assaults that occurred during the weekday versus the weekend. To do so I interact the wage ratio with a dummy indicating whether the assault occurred during the weekend for a subset of the data that includes information on day of the week.[12] The estimates presented in the last column of Table 2 suggest that all the decline in domestic violence as a result of the falling wage gap occurs during the weekend, which is inconsistent with the exposure reduction hypothesis. This result is also reassuring if one were concerned that hospitalization rates were disproportionately capturing women with no other source of medical care since during the weekend there are few, if any, alternatives to hospital care available.

[11] This assumes that the substitution effect exceeds the income effect.

[12] Hospital data with information on the day of the week of admission is available only for years 1990–1996 excluding 1991. These data indicate that there are more hospitalizations for assault (and other injuries) during the week compared with the weekend, but that assaults represent a greater share of injuries during the weekend (0.07) versus the week (0.05).

The results are not sensitive to how the wage gap is defined. In panel B of Table 2, I redefine the wage gap to be the linear difference between male and female wages (male wages–female wages). The coefficient estimates are smaller because of the scale of the wage gap defined this way, but the implied effects are similar to the effects estimated based on the ratio of wages.

C. *Additional Specifications*

In this section I present the results of a number of alternative specifications that corroborate a causal interpretation of the main results. First, I present empirical evidence that the measure of the wage ratio does not reflect changes in underlying characteristics of the work force that might occur if there were selective in-migration to high female wage–growth counties (e.g., those with a growing service sector). I do so by comparing the above estimates of equation (2) with estimates that include controls for compositional changes in the county to capture any selective in-migration. In the Appendix I also present estimates of the determinants of female (male) wages and find that changes in underlying composition of the population do not affect these measures. Second, I present the results of regressions in which I enter male and female wages separately to test the hypothesis that it is the relative wage that matters and that the relative wage measure does not simply reflect changes in average wages. Third, I instrument for relative wages in the county using statewide employment growth in industries dominant in the county. This instrument is very similar to the measure of relative demand used in the main specification (in fact, the identifying source of variation is the same), but it has the advantage of being the exact measure of labor demand used previously, though in different settings. Finally, I present estimates in which I identify the impact of changes in the wage ratio based on an alternative source of variation: changes over time in the industrial composition of the county.

Additional Controls.—I include additional controls to address two potential concerns. The first is that changes in the characteristics of men and women in the county correlated with both violence and the wage gap might bias the estimates. This would occur if areas with a declining wage gap were characterized by selective in-migration of people with a lower propensity for domestic violence. Though I previously included the lag of the dependent variable (Table 2) which likely captures any changes in the underlying characteristics of the county that could be correlated with both wages and violence, to further address this concern I include additional controls. These controls include education, specifically female and male college enrollment in all public colleges in California by race, county and year,[13] foreign immigration by county and year, and incarceration flows (number released–number detained) defined by county, race, and year. These three measures (education, immigration, and incarceration) were selected because they represent the most significant determinants of individual wages that are also likely to be correlated with violence: more educated women earn more and are less likely to be the victims of violence, immigrant women earn less and are less likely to avail themselves of law enforcement and domestic violence services, and men with a criminal background earn less and are more violent. In addition, these three measures are available at the county-race-year level (with the exception of immigration which does not include race) unlike data from the Census which would require interpolations for the intercensal years, adding measurement error and attenuation bias.

The second concern relates to the possibility that changes in the wage gap might be correlated with changes in access to nonhospital medical care which might reduce reliance on the hospital. This could occur if the closing of the wage gap were correlated with increases in female political

[13] This includes all community colleges, California State, and University of California campuses.

Table 3—Impact of Wages on Domestic Violence—Controls for Labor Supply

	Ln(female assaults)
Female/male wage	−0.871
	[0.381]
Black	−21.76
	[21.378]
White	−44.852
	[27.240]
Hispanic	−36.008
	[18.274]
Unemployment rate	0.878
	[2.600]
ln(per capita income)	0.296
	[0.478]
ln(nonintimate homicides)	0.017
	[0.028]
Incarceration flows per 10,000 males	0
	[0.001]
ln(immigration)	−0.015
	[0.076]
ln (female students)	0.099
	[0.208]
ln (male students)	−0.146
	[0.275]
ln(female population)	0.252
	[0.180]
ln(primary care clinics)	0
	[0.119]
Lagged dependent variable	0.016
	[0.037]
ln(male assaults)	0.355
	[0.066]
ln(female nonassault injuries)	0.453
	[0.112]
Observations	930
R^2	0.96

Notes: Robust standard errors clustered on county in brackets. County, year, and race fixed effects as well as county and race specific linear time trends also included.

power which might lead to allocation of more public resources to women. To address this I control for the number of primary care clinics per 1,000 women in the county.

The inclusion of these controls in Table 3 does not change the main result. In fact, inclusion of the controls slightly increases the point estimates of the impact of the wage ratio on assaults. In addition, none of the additional controls has a significant impact on domestic violence, which is likely due to the extensive set of controls included in the main specification.

Female and Male Wages Entered Separately.—As a further test of the theory that relative wages affect the level of domestic violence, I enter male and female wages separately in the regression (column 1, Table 4). The results are consistent with previous results and with the theory: a rise in female wages holding male wages constant reduces domestic violence, while a rise

TABLE 4—IMPACT OF RELATIVE WAGES ON DOMESTIC VIOLENCE: ALTERNATIVE SPECIFICATIONS

	ln(female assaults) (1)	ln(female assaults) (2)	ln(female assaults) (3)	ln(drug admissions) (4)
ln (female wage)	−0.781			
	[0.559]			
ln (male wage)	0.956			
	[0.516]			
Female/male wage		−0.697	−0.964	0.019
		[0.351]	[0.355]	[0.196]
Observations	982	955	804	776
R^2	0.96	0.96	0.96	0.99
Test that female and male wages are equal and opposite in value				
F (1, 37)	0.06			
p-value	0.81			

Notes: Robust standard errors clustered on county in brackets. Column 1 is based on an OLS fixed effect regression; in column 2, I instrument for the wage ratio using statewide growth in employment by industry weighted by the county-specific shares in these industries; in columns 3 and 4 the wage ratio is derived from changes in the industrial composition of the county over time; in column 4 are results of a falsification exercise.

in male wages, holding female wages constant, increases domestic violence (−0.781 and 0.956, respectively). The male and female coefficient estimates are statistically equal in absolute value and opposite in sign, with F statistics and p-values presented at the bottom of Table 4; however, the estimate of female wages is imprecise. These results also rule out the possibility that the relative wage measure simply captures increases in average wages.

Instrumental Variable (*IV*) *Estimates.*—While I have argued and presented evidence that county level wages measured according to equation (1) represent exogenous measures of female and male demand for labor, I also instrument for the county-level wage ratio using state growth rates in employment for each industry weighted by county specific shares in those industries.[14] The advantage of this alternative measure is that it is equivalent to the exogenous measure of labor demand widely used in the labor economics literature (see Timothy J. Bartik 1991; Blanchard and Katz 1992; Bound and Holzer 2000; Autor and Dugan 2003). But the variation in this measure essentially derives from the same source as the measure of wages defined according to equation (1)—statewide changes in demand for workers in a given industry. As such, we would expect similar estimates based on the two measures.

The IV results presented in column 2 of Table 4 are similar, though slightly smaller and less precise than the estimates from the main analysis. The first stage, not presented here, is strong: statewide employment growth in female dominated industries has a positive and significant impact on the wage ratio in the county, while statewide employment growth in male dominated industries reduces the county wage ratio (F statistic 15.56). I argue that these results, along with those that include additional controls for labor supply, support the exogeneity of the wage measures used in this analysis and corroborate the main findings.

Redefining the Wage Gap based on Changes in Industrial Composition of the County.—Finally, I reconstruct the wage ratio to take advantage of an alternative source of identifying

[14] In these regressions I measure the relative wage ratio in the county using county wages (not statewide wage) and instrument for it using industry-level statewide employment growth measured over all counties except the focal county.

variation: changes in the industrial composition of the county over time. For this measure I create time-varying measures of γ (the proportion of women/men working in a given industry) based on linear interpolations between 1990 and 2000 Census data and holding industry wages fixed at 1990 levels. The result presented in the third column of Table 3 is slightly larger for female assaults than those based on the main specification and presented in Table 2.[15] However, this measure is much less arguably exogenous. Appendix Table 1 shows that female and male college enrollment are more predictive of this measure of wages than wages that hold industrial structure fixed, which may indicate endogenous shifts in industrial composition. This potential endogeneity may explain why the point estimate is higher. Finally in column 4, I present the results of the falsification exercise based on this alternative measure of wages: changes in the wage ratio are unpredictive of substance abuse admissions for treatment.

IV. Conclusion

Over the past 15 years, violence against women has declined as their employment and earnings have increased. A model of household bargaining presented in the Appendix that incorporates violence is consistent with these trends. I provide empirical support for a causal relationship between relative labor market conditions for women and violence. Using new sources of administrative data that overcome many of the shortcomings of previous data on domestic violence, I find that the decline in the wage gap witnessed over the past 13 years can explain nine percent of the reduction in violence against women. These findings suggest that in addition to more equitable redistribution of resources, policies that serve to narrow the male-female wage gap also reduce violence and the costs associated with it. Given existing evidence that domestic violence negatively affects child outcomes, reductions in domestic violence are likely to improve child outcomes as well. As such, in addition to addressing concerns of equity and efficiency, improved pay parity may also generate important intergenerational effects.

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[15] Because of difficulty matching counties in the 1990 and 2000 Census, 20 percent of the sample is lost.

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