The Price of Protection: A Trajectory Analysis of Civil Remedies for Abuse and Women’s Earnings

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Abstract
We know men’s violence against women is costly. Yet, we know little about the costs—or benefits—of women’s efforts to end it. This study investigates the temporal dynamics of women’s earnings and petitioning for a Protection from Abuse (PFA) civil restraining order. Women’s earnings might rise or fall at the time of petitioning but quickly return to pre-petitioning levels, a short-term boost or shock; or, petitioning might precipitate a longer-term stall or upward shift in women’s earnings. To test for these effects, we use latent growth curve analysis and evaluate women’s earnings trajectories over six years. We find overwhelming evidence that the period around petitioning is one of exceptional earnings instability for petitioners, many of whom experience both shocks and stalls. Virtually no one experiences a boost in the quarter of petitioning or an upward shift in earnings growth in the year after petitioning for a PFA. Welfare, however, buffers negative effects of petitioning on women’s earnings growth. We also calculate lost earnings as the difference between women’s counterfactual projected and estimated earnings. Our findings inform theoretical and policy debates about interventions intended to address poverty and violence against women.

Keywords
violence against women, restraining orders, earnings, welfare

Men’s violence against women is costly. In one estimate, Max and colleagues (2004) put the aggregate annual cost of intimate partner violence in the United States between $3.9 and $7.7 billion. Some costs are to state and local governments—the expense of police, medical, and social services; other costs redound to individuals—lost productivity, medical and mental health care, loss of and damage to property, and reduced quality of life (Desmond and Valdez 2012; VAWA Reauthorization Act 2013). Financial costs to individual women are considerable when abuse obstructs employment. Farmer and Tiefenthaler (2004) estimate that domestic violence contributes to an annual aggregate loss of nearly 3 million work days and $100 million in pay. Consequently, women sub-

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jected to abuse tend to face increased food insecurity, trouble paying rent or utility bills (Brush 2004; Tolman, Danziger, and Rosen 2002), and chronic cycling between work and welfare (Bell 2003; Purvin 2003).

One way women try to end abuse and mitigate its costs is to petition for a civil restraining order against an abuser (Protection from Abuse [PFA]). Restraining orders are appeals to the state for protection against “physical violence, threats, or intimidation” (DeJong and Burgess-Proctor 2006:69). Research shows that PFAs can promote women’s safety and help women and their advocates manage the threat of abuse (Benitez, McNiel, and Binder 2010; Frantzen, San Miguel, and Kwak 2011). However, scholars and practitioners know very little about the relationship between PFAs and women’s economic circumstances. We focus on a critically important economic outcome for women—earnings from paid employment—and consider whether and how women’s earnings trajectories change when and after they petition for a PFA.

No published research assesses the work-related costs or benefits to women associated with their appeals to the state for protection from abusers. Research on abuse as an obstacle to work seldom includes information on calling the police, going to the hospital emergency department, or petitioning for a PFA (e.g., Raphael 2000; Riger, Staggs, and Schewe 2004). At the same time, studies of law-and-order remedies for the harms of abuse, such as mandatory arrest or no-contact orders, do not collect data on work-related outcomes (e.g., Carlson, Harris, and Holden 1999; Frantzen et al. 2011). A third body of research, focused on help-seeking by women subjected to abuse, rarely reports household income, let alone women’s earnings (e.g., Fischer and Rose 1995; Kothari et al. 2012). In summary, existing scholarship addresses different pieces of the puzzle, but researchers have yet to put those pieces together.

How might appealing to the law-and-order state for protection from abuse affect women’s earnings trajectories? Existing models of the relationship between abuse and work—although they address neither restraining orders nor earnings—provide a useful place to start. Broadly speaking, there are two conflicting sets of expectations about the relationship between abuse and work. On the one hand, exposure and exchange models focus on potential benefits of work. Work reduces exposure to abuse because women spend less time at home (Gelles and Straus 1989). Earned income increases women’s bargaining power in relationships, providing leverage for women to curb abuse (Farmer and Tiefenthaler 1997; Gelles and Straus 1989; Goode 1969, 1972). On the other hand, feminist backlash and dual traps models are skeptical about work as a way to reduce abuse. Men might respond to women’s increased economic independence with escalating abuse or backlash (e.g., Kimmel 2002; Riger and Krieglstein 2000). Poverty and abuse constitute mutually reinforcing traps for women (Hattery 2009; Raphael 2000; see also Bonomi et al. 2014).

Although they provide a useful starting place, exposure, exchange, backlash, and dual traps models are limited for our purposes in that they generally ignore time. That is, they do not speak to the duration of effects, the order of events, and other time-sensitive processes—all of which may be particularly important when explaining change over time. Overall, we extend existing models of abuse and work by theorizing both the effects of petitioning for a civil restraining order on women’s earnings growth and the ways time matters.

Extending the exposure and exchange frameworks, we would expect petitioning for a protective order to increase women’s earnings growth. Protections offered by the state could decrease exposure to abuse, increase women’s leverage, or both, improving women’s earnings. Gains could be short-lived: a boost. Or, petitioning could mark an upward shift in women’s earnings growth over a longer period. Alternatively, extending the dual traps and backlash frameworks, petitioning for a restraining order might decrease women’s earnings growth. The traps of poverty and abuse may be too difficult to escape, or petitioning could provoke backlash, depressing earnings growth. A decline may be
a brief shock to women’s earnings or may persist, becoming a stall in longer-term earnings growth.

Women subjected to abuse might also try to use welfare in conjunction with their appeals to the courts (Davis 1999; Riger and Staggs 2004). The effects of petitioning for a PFA on women’s earnings may be different for women who turn to the welfare system for support compared to women who do not. The difference may be negative. Because welfare substitutes for other forms of income, we may observe decreased earnings growth for welfare recipients—an income substitution effect. Then again, the difference may be positive. Welfare may foster women’s independence, laying the groundwork for increased earnings growth—an independence effect.

Taking time seriously suggests we evaluate two additional differences: (1) the relative timing of receiving welfare and petitioning and (2) persistence through the petitioning process. First, for women who receive welfare, we expect the relationship between petitioning and earnings growth to hinge on the timing of receiving welfare relative to petitioning for a PFA. For women who are on welfare before petitioning, we expect to see both income substitution and independence effects. In contrast, for women who turn to welfare after petitioning, we expect to see income substitution but not independence effects. Second, recognizing that petitioning unfolds over time suggests additional distinctions in the effects of petitioning on earnings growth. In particular, we expect women who get farther into the process to be more likely to experience longer-term changes—positive or negative.

In this study, we endeavor to answer three main questions:

1. What costs or benefits are associated with petitioning for a Protection from Abuse order (PFA), in terms of effects on individual women’s earnings trajectories?

2. How do the effects on women’s earnings of petitioning for a PFA vary over shorter and longer time periods?

3. To what extent do welfare receipt and persistence through the petitioning process alter these effects?

To answer these questions, we model individual women’s earnings trajectories across a six-year period from 1995 through 2000. We estimate earnings trajectories for the population of women complainants who petitioned for a PFA in Allegheny County, Pennsylvania. We use longitudinal, individual-level data to compare women to themselves at different points in time—in particular, before and after petitioning for a PFA. We employ latent growth curve (LGC) techniques, which are particularly well-suited for simultaneously gauging effects of short-term, episodic changes as well as longer-term shifts in trajectories (Bollen and Curran 2006). We find that, on average, women who petition for a PFA take a serious hit to both their short- and longer-term earnings. We then calculate the difference between women’s counterfactual projected and actual estimated earnings. We find that welfare receipt limits negative effects to the period immediately around petitioning, but the relative timing of petitioning and receiving welfare matters. For some women, persisting through the PFA process is associated with better outcomes.

**THE ECONOMIC COSTS OF PARTNER-PERPETRATED ABUSE**

Empirical research leaves little doubt that abuse interferes with women’s economic well-being. A substantial body of work finds significant negative effects of recent abuse on women’s labor force attachment, especially for poor women (e.g., Bell, Lohman, and Votruba-Drzal 2007; Browne, Salomon, and Bassuk 1999; Shepard and Pence 1988). Indeed, abundant empirical evidence documents the economic costs of abuse to individual women, including reduced wages, work hours, job experience, and employment stability (e.g., Lloyd and Taluc 1999; Logan
et al. 2006; O’Leary-Kelly et al. 2008). Through direct interference and the indirect effects of physical injuries and post-traumatic stress symptoms, abusive men control and coerce their current and former wives and girlfriends (Brush 2011; Raphael 2009). Abuse jeopardizes women’s education, employment, and economic as well as bodily welfare.

However, research on the ways that abuse obstructs women’s work is limited in at least two ways. First, existing data are inadequate. With one known exception, datasets that include abuse have measures of labor force attachment and household or neighborhood income rather than women’s earnings. Moreover, available data are overwhelmingly cross-sectional, precluding analysis of changes in women’s economic well-being over time. The ideal study of individual-level economic effects of abuse would compare the earnings trajectories of women who have been subjected to abuse to the trajectories of otherwise-similar women who have not been subjected to abuse. Such a study is impossible with data available currently and for the foreseeable future.

A second limitation is the lack of attention to the economic effects of women’s seeking injunctive relief. The literatures on abuse per se and on the efficacy of the legal system’s response to women subjected to abuse occupy distinct research silos (Jordan 2009). In one silo, research on the effects of abuse on work does not address law-and-order remedies for abuse, such as mandatory arrest, no-drop prosecution policies, or civil restraining orders. In the other silo, research on the effects of law-and-order remedies on abuse does not address women’s employment or earnings. Separate silos mean that researchers, advocates, and policymakers know very little about how work interference might be mitigated or exacerbated—and therefore earnings augmented or diminished—when women appeal to the police or the courts for help. In the next section, we introduce one way women may try to put a stop to abuse and its economic consequences: the civil restraining order.

CIVIL RESTRAINING ORDERS AND THE WOMEN WHO PETITION FOR THEM

Civil restraining orders are “court-ordered injunctions designed to restrain an individual’s use of physical violence, threats, or intimidation against another person” (DeJong and Burgess-Proctor 2006:69). Restraining orders (PFAs) are part of broader efforts to promote safety and justice for women subjected to abuse as well as accountability for abusive men (Hart 1997; for critiques, see, e.g., Goodmark 2012; Hamby 2014). Since 1992, PFAs have been available in every U.S. state and the District of Columbia; since 1994, they have been enforceable across jurisdictions. However, as with most state-level statutes, PFAs vary considerably from place to place. In particular, states differ in the extent to which they might be considered “victim friendly,” especially in terms of accessibility of the legal system (Corrigan 2013; DeJong and Burgess-Proctor 2006; Durfee 2008) and the social acceptability of petitioning for injunctive relief from partner-perpetrated abuse (Logan et al. 2006; Postmus 2007).

In most jurisdictions, including the setting for the present study, obtaining a PFA is a multistep process (for summary and review, see, e.g., Roberts, Wolfer, and Mele 2008). A complainant starts the process by filing a petition in civil court. This step is often more-or-less simultaneous with an emergency hearing at which a judge may take one of three actions. First, the judge could deny the complainant’s petition. Second, the judge could grant a temporary restraining order (in our county, 10 days) but not take any further action. Third, the judge could grant a temporary PFA and schedule within that brief period a follow-up hearing to decide on a longer-term, renewable order (in our county, 12 to 18 months). Judicial decisions aside, the complainant may also drop out of the process at any time.

Although it is reasonable to infer that women want abuse to stop, not all women
subjected to abuse turn to shelters, the police, social services, the courts, or emergency or primary medical care for help (Goodmark 2012; Hamby 2014). Data on the proportion of women subjected to abuse who seek help are scarce, but researchers estimate that 12 to 22 percent of such women use PFAs (Kothari et al. 2012). Compared to other women subjected to abuse, petitioners tend to have “longer and more severe victimization histories and greater help-seeking behaviors” (Kothari et al. 2012:2847).

Women seeking PFAs average 32 years of age, typically have at least a high school education, are racially diverse, and vary by marital status. Full- and part-time employment rates for petitioners vary from study to study; one review documents a range from a low of 40 percent to a high of 93 percent (Logan et al. 2006). Few studies of petitioners report household income, let alone women’s individual earnings, yet it appears that women in the process of petitioning are often low-income. For example, Zoellner and colleagues (2000) report that household income was $10,000 or less for 72 percent of the participants in their study of 65 Philadelphia women in the process of requesting a restraining order, and 37 percent of the women were on welfare.

Indeed, welfare appears to be a vital resource for women subjected to abuse, including women who call the police and those who seek PFAs (Brandwein 1999; Davis 1999; Purvin 2007). A study of 10,875 PFA-petitioning women finds that 24 percent had at least one spell on welfare during the study period, a welfare receipt rate considerably higher than for the local population (Brush 2011; see also Hughes and Brush 2011). Nevertheless, existing research seldom unpacks the population of petitioning welfare recipients, treating all women in this category as the same. We suggest that important advances in our understanding spring from the possibility that women who are on welfare and petition the courts for injunctive relief differ from women who turn to welfare only during or after petitioning.

Many women do not persist from the initial petition to receive a longer-term restraining order (Fernandez, Iwamoto, and Muscat 1997; Holt et al. 2002; Malecha et al. 2003). Holt and colleagues (2002) find that 43 percent of women granted a temporary order did not receive longer-term orders. Furthermore, women who move further into the petitioning process may differ in important ways from women who do not persist. According to one study, mothers and women who are married to or have longer-term relationships with an abuser are less likely to complete the restraining order process (Fernandez et al. 1997). Similarly, researchers find that living with the perpetrator significantly reduces the likelihood that women will persist (Holt et al. 2002; Malecha et al. 2003).

For our purposes, another important consideration is the degree to which protective orders actually protect. Findings are complex and contradictory (Fleury-Steiner, Fleury-Steiner, and Miller 2011; Frantzen et al. 2011). On the one hand, a body of evidence and criticism suggests the effectiveness of protective orders is poorly measured, uneven, and “questionable at best” (Wright and Johnson 2012:675; see also reviews of important critiques in Goodmark 2012; Hamby 2014). Spitzberg’s (2002) review of 32 studies of PFAs found violation rates average 40 percent, with a range of 23 to 70 percent (see also Carlson et al. 1999; Tjaden and Thoennes 2000). On the other hand, neither the common violation of protective orders nor the lack of across-the-board harm reduction means that PFAs fail to reduce re-victimization. Research suggests PFAs are associated with a reduction of abuse, at least by some measures (Frantzen et al. 2011). For example, Holt and colleagues (2002) find an association between PFAs and a decline in police-reported abuse. Protective orders also have psychological benefits for at least some women subjected to abuse. Studies find that many PFA recipients perceive protection orders to be effective, and large proportions of PFA recipients report feeling extremely or fairly safe after obtaining an order (Logan
and Walker 2009). PFAs may not be a universal and failsafe remedy for women subjected to abuse, but they “can serve a useful role in threat management” (Benitez et al. 2010:376).

There is remarkably little evidence, however, on the relationship between petitioning for a protective order and earnings. As explained earlier, the paucity of evidence results from both a lack of data and the persistence of distinct research silos. Most reported research on restraining orders has data on labor force attachment but not earnings (e.g., Mele 2009; Shannon, Logan, and Cole 2007; for a review, see Logan et al. 2006). Even the most sophisticated studies of the effectiveness of protection orders ignore effects on women’s economic well-being (e.g., Kothari et al. 2012). Here, we bridge research on the effectiveness of petitioning, research on the relationship between abuse and work, and research on the importance of welfare for women subjected to abuse by comparing women’s earnings trajectories before and after they petition for a PFA.

MODELING PETITIONING AND EARNINGS GROWTH USING IDEAL-TYPICAL TRAJECTORIES

How might petitioning for a civil restraining order affect women’s earnings? Here, we extend models of abuse and work to generate competing hypotheses about the relationship between petitioning for a restraining order and women’s earnings growth. On the one hand, exposure and exchange models are optimistic about work as a way to reduce or end abuse. As women move into the workforce, they decrease their time at home, the presumed site of their exposure to “domestic” violence (Gelles and Straus 1989). Increasing their employment enables women to exchange greater financial resources and greater external utility for better bargains within the relationship or to leave the relationship altogether (Farmer and Tiefenthaler 1997; Gelles and Straus 1989; Goode 1969, 1972).

On the other hand, feminist backlash and dual traps models are pessimistic about work as a way to reduce or end abuse. According to these models, women’s employment does not necessarily reduce exposure to abuse because the effects of “domestic” violence spill over from home to work; for example, some men harass and abuse women in the parking lots and cubicles of their workplaces (Brush 2011). Some women subjected to abuse cannot exchange increased income for relief from abuse, because some abusers interfere with women’s work as a tactic to enforce compliance with norms of feminine domesticity, dependency, and sexual and emotional availability (Stark 2007). A backlash model proposes that as women take steps toward independence—for instance, by increasing their labor force participation—their partners escalate the abuse (Riger and Krieglstein 2000). By further undermining women’s earnings capacity, escalating abuse leaves some women “trapped by poverty, trapped by abuse” (Raphael and Tolman 1997; see also Hart 2008).

We extend these models in two ways: first, by theorizing the effects of petitioning for a civil restraining order on women’s earnings growth and second, by taking time seriously. Extending the exposure and exchange models, we expect petitioning for a protective order to increase women’s earnings growth. Protections offered by the state should decrease exposure to abuse and increase women’s leverage, improving women’s earnings growth.

Optimistic Hypothesis: The slope of earnings growth will increase after women petition for protection from abuse.

Alternatively, extending the backlash and dual traps models, we expect petitioning for a restraining order to decrease women’s earnings growth. Petitioning might provoke backlash, depressing earnings growth and deepening the dual traps of poverty and abuse.

Pessimistic Hypothesis: The slope of earnings growth will decrease after women petition for protection from abuse.
Taking time seriously further complicates the picture, suggesting petitioning could have short- or longer-term effects on women’s earnings growth. Short-term effects—those limited to the time periods immediately before and after petitioning—are intuitive. Longer-term effects are supported by workplace studies, which find that exceptional, episodic events can have lasting effects on employees’ attitudes and behaviors (Grunberg et al. 2008). Figure 1 presents four possible ideal-typical trajectories, including short- and longer-term effects (in the columns) for the optimistic and pessimistic scenarios (in the rows): a boost, an upward shift, a shock, and a stall.

Consider first the optimistic scenarios. Petitioning for a PFA could have short-term positive effects on earnings growth: the boost model (see Figure 1, panel A). By petitioning the court for injunctive relief, women reduce their exposure to abuse, producing abrupt improvements in their earnings growth.

Boost Hypothesis: The slope of earnings growth will increase in the time period immediately after women petition for protection from abuse.

Figure 1. Ideal-Typical Trajectories of Women’s Earnings Relative to Petitioning for a Protection Order

Note: The dashed box marks the time period directly before and after petitioning for a protection order. For all trajectories, we graph initial growth as positive; it could be flat or negative. We theorize a boost, an upward shift, a shock, and a stall relative to whatever baseline growth is observed in the first period.

Appearing in court to petition for a PFA may also have longer-term benefits: the upward shift model (see Figure 1, panel B). The threat of state intervention is a sustainable resource women can exchange for reduced abuse in the longer term, improving women’s earnings growth over time. Or, if petitioning allows
women to leave the relationship, associated gains in women’s earnings growth could last longer.

_**Upward Shift Hypothesis:***_ The slope of earnings growth will increase across a sustained time period after women petition for protection from abuse.

Now consider the pessimistic scenarios. Petitioning for a PFA could have short-term negative effects on earnings growth: the shock model (see Figure 1, panel C). Finding shock effects in the period surrounding petitioning would be consistent with a backlash against women’s efforts to increase their independence, stop the abuse, or exit the relationship. As women petition for protection, men may step up their coercive control, immediately depressing women’s earnings and slowing their earnings growth in the periods directly before and after petitioning.4

_**Shock Hypothesis:**_ The slope of earnings growth will decrease in the time periods directly surrounding women’s petitioning for protection from abuse.

Appearing in court to petition for a PFA could have longer-term negative effects on the slope of a petitioning woman’s earnings trajectory: the stall model (see Figure 1, panel D). Finding stalls in the earnings of petitioners would be consistent with the dual traps framework. Women’s efforts to reduce or end abuse and to increase their independence and economic well-being could merely deepen the dual traps of poverty and abuse.

_**Stall Hypothesis:**_ The slope of earnings growth will decrease across a sustained time period after women petition for protection from abuse.

To best understand associations between petitioning and earnings, we must also consider diversity in petitioners’ experiences with the welfare and law-and-order branches of the state to which women may appeal. The effects of petitioning for a PFA on women’s earnings may differ by welfare receipt. Because welfare substitutes for earnings—an income substitution effect—we might expect depressed earnings growth for welfare recipients. Alternatively, welfare may foster women’s independence—an independence effect. Welfare, like work, increases women’s external utility, making it more possible for them to bargain within the relationship, or to exit. Women’s independence could then lay the groundwork for increased earnings growth.

Taking time seriously also suggests we evaluate two additional differences in the costs and benefits of petitioning. The relative timing of petitioning and receiving welfare may shape women’s earnings trajectories. We compare welfare recipients whose welfare spells precede petitioning with those whose spells coincide with or follow petitioning. For women who turn to welfare before petitioning, income substitution effects will likely show up in the pre-petitioning period. After petitioning, their earnings may have nowhere to go but up. But, it is also possible that women who go on welfare prior to petitioning may experience independence effects. Earlier welfare spells may provide this group with resources that could later foster greater earnings growth. For women who turn to welfare after petitioning, we suggest income substitution effects may depress their earnings growth after petitioning. That is, these women may be using welfare to compensate for any negative effects of petitioning on earnings. At the same time, because independence effects take time to accrue, we expect positive effects of welfare receipt on earnings growth after petitioning to be less likely for this group.

Differences in the costs and benefits of petitioning may also be due to variations in the petitioning process over time. We compare women whose judges granted both a temporary PFA and a follow-up hearing within 10 days to petitioners without scheduled follow-up hearings. On the one hand, if petitioning creates backlash, persisting through the court process could make things worse for women. On the other hand,
persisting through the court process may be the only way to help women put a stop to abuse and thus mitigate negative effects of petitioning on earnings. In summary, we expect women who get farther into the process are more likely to experience longer-term changes—negative or positive.

DATA AND METHODS

Our population is 3,923 women in Allegheny County, Pennsylvania who reported any earnings between January 1995 and December 2000 and petitioned for a single protective order between January 1996 and December 1999. We use partial Social Security numbers and first and last names to match (1) records on PFA petitioning from the Allegheny County Prothonotary; (2) AFDC/TANF records for petitioning women from the Pennsylvania Department of Public Welfare (DPW); and (3) quarterly data on earnings of petitioning women and welfare recipients as reported by employers to the Pennsylvania Department of Labor and Industry’s Bureau of Unemployment Compensation and Benefits Administration (BUCBA). Using these data, we first analyze the full population of 3,923 women; we then split the population into the 1,568 women who received welfare assistance during the six-year study period and the 2,355 women without an observed welfare spell. Using Prothonotary data, we are able to consider additional differences in women’s trajectories related to petitioning, such as whether a Family Court judge granted a hearing for a longer-term restraining order, and whether women were only plaintiffs or were also defendants in counter-suits.

Our dependent variable is logged quarterly earnings in 1995 first-quarter constant dollars. We analyze data on earnings in the quarter a woman petitioned for a protective order, each of the four quarters prior to petitioning, and each of the four quarters after petitioning. Data for the fourth quarter of 1996 were inexplicably excluded from the BUCBA data. To generate estimates when data are missing completely at random, we use maximum likelihood (FIML), one of the two best approaches (Allison 2002). In analyses of all petitioning earners, we work from 33,066 person-quarters of data within a six-year time span.

We center our analysis on the quarter of petitioning. That is, we evaluate earnings one year before and one year after petitioning for all women regardless of when they petitioned for a PFA in calendar time. For example, if one woman petitioned in the first quarter of 1997 and another petitioned in the first quarter of 1999, we evaluate the first woman’s earnings growth during 1996 and 1997 (ending in the first quarter of 1998), and the second woman’s earnings growth two years later. We transform our dependent variable to constant dollars to ensure that earnings measured in different quarters are still comparable. In an auxiliary analysis, we control for the timing of PFA filing; these results suggest our findings are robust.

We estimate changes in women’s logged earnings using latent growth curve (LGC) models (Bollen and Curran 2006). LGC models analyze change over time by focusing on intra-individual change, estimating both starting positions (intercepts) and trends (slopes) for each individual’s growth trajectory over time.

LGC analysis is flexible enough to estimate a variety of nonlinear patterns of change over time (e.g., quadratic or cubic), a necessity here given that women who are in the process of petitioning for a protective order are unlikely to experience stable, linear earnings growth. Of particular use here is the piecewise, or spline, LGC model, which estimates more than one linear slope over the growth trajectory. When using splines, the model estimates the average rate of change over each “piece” of time, allowing for widely divergent individual-level growth from one period to the next. These models also allow us to assess whether earnings grew differently in the periods before and after women petitioned for PFAs, and whether the intervals closest to petitioning are different from those more distant from petitioning. We model four splines,
estimating growth in two-quarter intervals, which allows us to test for both short-term and longer-term changes in women’s earnings trajectories.¹¹ We estimate unconditional models, which include no predictors of growth trajectories.

An alternative to our event-centered approach would be to model petitioning as a covariate in a more traditional trajectory analysis using calendar time. Modeling petitioning as a time-varying covariate would allow us to test short-term effects on women’s earnings trajectories, but it complicates analysis of longer-term changes. And because women petition at different points in time, modeling the effect of petitioning on the latent slope of women’s earnings—a way to get at longer-term changes—is far from straightforward. By event-centering trajectories and analyzing changes in slopes over the four splines we model here, we are able simultaneously to estimate short- and longer-term effects within a single model, regardless of when in calendar time women petitioned the courts for protection.

For a piecewise (spline) growth model with four time periods, the individual equation is as follows:

\[ y_{it} = \alpha_i + \beta_1 \lambda_{1t} + \beta_2 \lambda_{2t} + \beta_3 \lambda_{3t} + \beta_4 \lambda_{4t} + \epsilon_{it} \]  

(1)

where \( y_{it} \) represents logged quarterly earnings for the \( i \)th individual at time \( t \); \( \alpha_i \) is the intercept for individual \( i \); \( \beta_1 \) and \( \beta_2 \) are each slopes for individual \( i \) prior to petitioning; \( \beta_3 \) and \( \beta_4 \) are each slopes measuring the two-quarter intervals following petitioning; \( \lambda_{1t} \) to \( \lambda_{4t} \) are constants manipulated to capture linear change over the four time periods; and \( \epsilon_{it} \) is an error term for each individual \( i \) at time \( t \). In this model, each individual, \( i \), has her own intercept and slope.

In addition to analyzing individual-level change, LGC models also estimate the mean intercept and slope for all individuals. Models estimate average initial earnings across all women in the fourth quarter of 1995, and the average rate of change in earnings across each of the four time periods of interest. For the four-spline piecewise model, this leads to five additional equations:

\[ \alpha_i = \mu_\alpha + \zeta_{\alpha i} \]  

(2)

\[ \beta_{1\ldots4i} = \mu_{\beta1\ldots4} + \zeta_{\beta1\ldots4i} \]  

(3 through 6)

where \( \mu_\alpha \) is the mean intercept across all individuals, and \( \mu_{\beta1} \) through \( \mu_{\beta4} \) are the mean slopes across all individuals in each of the four time periods. The first equation represents a person’s individual intercept \( (\alpha_i) \) as a function of the average intercept \( (\mu_\alpha) \) over all individuals and a disturbance term \( (\zeta_{\alpha i}) \). The subsequent four equations represent the individual slopes pre-petitioning \( (\beta_1 \) and \( \beta_2 \)) and post-petitioning \( (\beta_3 \) and \( \beta_4 \)) as a function of the average slopes \( (\mu_{\beta1} \) through \( \mu_{\beta4} \)) and disturbance terms \( (\zeta_{\beta1} \) through \( \zeta_{\beta4} \)) over the four time periods.

We use results from the four-spline LGC models to calculate the costs or benefits in real dollars associated with petitioning for a protective order. Our approach is to consider a counterfactual: if women’s pre-petitioning growth trajectories were to have remained the same after petitioning for a PFA (i.e., the slopes remained constant across the two time periods), how much would women have earned in the year after petitioning? We then compare women’s projected earnings under these counterfactual conditions to our calculated estimates from the fitted models. We provide estimates in 1995 dollars along with their 2014 dollar equivalents based on the Consumer Price Index.

Another advantage of LGC analysis is the ability to test—and account for—group differences in a single model. That is, we are able to consider whether groups’ underlying earnings trajectories differ systematically. We can go beyond testing for substantial variation in average initial earnings and earnings growth (differences in mean intercepts and slopes) to permit more radical dissimilarities in growth trajectories (differences in the factor variances and covariances, time-specific error variances, and functional form). In multi-group models, we estimate all
parameters independently for each group and then evaluate whether constraining parameters to be equal significantly affects model fit (Bollen and Curran 2006).

We consider differences in women’s earnings growth across groups by welfare receipt, by the timing of welfare spell relative to petitioning, and by hearing event. First, we compare the earnings trajectories of women who received public assistance (AFDC or TANF) to women who did not receive welfare through either program during the six-year study period. Second, among welfare recipients, we compare women whose first observed welfare spell preceded petitioning with those for whom it coincided with or followed petitioning. Third, among petitioners, we compare women with a follow-up hearing date to women who either did not ask for or were not granted a follow-up hearing. Although we are not able to say definitively that all women with a scheduled follow-up hearing were granted a 12- to 18-month restraining order, we know that women without hearing dates did not obtain such an order. We compare women’s trajectories by hearing event initially for all petitioners and then for women with and without an observed welfare spell.

We also estimate several auxiliary models to further explore the relationship between PFA petitioning and earnings. First, we consider differences between the 3,759 women who were plaintiffs only and the 164 women also involved in counter-suits. Second, we investigate the trajectories of 181 women who petitioned for multiple PFAs between 1995 and 2000. Third, for women who received welfare at any time during the study period (for whom we have data on racial categorization), we compare the trajectories of Black and White petitioners. We also proxy changes in women’s labor force attachment by including time-varying dummy variables measuring the presence of zero earnings in a given quarter.12 We discuss notable findings in the next section; full results are available from the authors upon request.

Finally, we look at the earnings growth of 24,731 welfare recipients who did not petition for a PFA during the study period (results are reported in Table A1 and Figure A1 in the Appendix; see also Hughes and Brush 2011). To evaluate their trajectories relative to petitioners, we randomly assign each welfare recipient a “petitioning quarter.” This analysis is our only comparison involving women who did not petition for a PFA.

We estimate all models in AMOS 22. We use three different statistics to assess model fit: the chi-square test statistic, the Incremental Fit Index (IFI) (Bollen 1989), and the root mean squared error of approximation (RMSEA) (Steiger and Lind 1981). A non-significant chi-square test statistic indicates excellent fit. For the IFI, values closer to 1.0 indicate better model fit. Typically, values above .90 are considered acceptable, and .95 is considered optimal. In contrast, the RMSEA is scaled so that the closer values are to 0, the better the fit of the model. Values below .05 are typically considered to indicate optimal fit (Browne and Cudeck 1993). Using several measures compensate for the limitations of any single fit statistic (Chen et al. 2008; Tanaka 1993).

Although we have full population data, we still report statistical significance. Because we are studying a period of tremendous variability, our results will likely come with large standard errors, resulting in a relatively high bar for reaching statistical significance. That is, in the presence of high variation, effects and group differences must be quite large to reach conventional levels of statistical significance. Using markers of significance therefore helps establish when effects are strong enough that we might expect replicability, even under highly unstable conditions. Still, because we have full population data, we sometimes discuss substantively meaningful changes that are not significant at conventional levels, because effects may still be consequential for our population of women.

Our analyses proceed in three stages. First, we explore women’s average earnings before and after petitioning for a PFA using descriptive statistics. Second, we begin using latent growth curve analysis. We present results from a simple linear model, considering the
Table 1. Descriptive Statistics for Population of Women Who Petitioned for a Single Protection Order between January 1995 and December 2000

<table>
<thead>
<tr>
<th>Distribution by Welfare Receipt and Hearing Event</th>
<th>N</th>
<th>Percent</th>
</tr>
</thead>
<tbody>
<tr>
<td>Total Population</td>
<td>3,923</td>
<td>100</td>
</tr>
<tr>
<td>Welfare Observed</td>
<td>1,568</td>
<td>40</td>
</tr>
<tr>
<td>Welfare Spell Before Petitioning</td>
<td>1,228</td>
<td>78</td>
</tr>
<tr>
<td>No Welfare Spell Before Petitioning</td>
<td>340</td>
<td>22</td>
</tr>
<tr>
<td>Hearing</td>
<td>708</td>
<td>45</td>
</tr>
<tr>
<td>No Hearing</td>
<td>860</td>
<td>55</td>
</tr>
<tr>
<td>Welfare Not Observed</td>
<td>2,355</td>
<td>60</td>
</tr>
<tr>
<td>Hearing</td>
<td>1,060</td>
<td>45</td>
</tr>
<tr>
<td>No Hearing</td>
<td>1,295</td>
<td>55</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Earnings (in Constant 1995 $s) Before and After Petitioning</th>
<th>Pre</th>
<th>Post</th>
<th>Post – Pre</th>
</tr>
</thead>
<tbody>
<tr>
<td>Median Yearly Earnings</td>
<td>$4,285</td>
<td>$5,220</td>
<td>$935</td>
</tr>
<tr>
<td>Standard Deviation</td>
<td>2931</td>
<td>3165</td>
<td>234</td>
</tr>
<tr>
<td>Percent Quarters with Zeros</td>
<td>37</td>
<td>35</td>
<td>-2</td>
</tr>
</tbody>
</table>

picture when we do not allow women’s earnings to grow at different rates before and after petitioning for a PFA. We also introduce a more dynamic four-spline model that captures changes in women’s earnings before and after, and more proximate to and more distant from, petitioning for a PFA. Using these four-spline results and the counterfactual approach discussed earlier, we estimate the dollar costs or benefits to women’s earnings that come with petitioning for a protective order. Third, we use multiple-group techniques to investigate how women’s trajectories differ by welfare receipt, relative timing of welfare spell, and hearing event.

RESULTS
A Descriptive Assessment of Petitioners

Table 1 summarizes our study population by welfare receipt, relative timing of welfare spell, and hearing event. Our full study population includes 3,923 women living in Allegheny County who petitioned once for a Protection from Abuse (PFA) order with any reported earnings between January 1995 and December 2000. Of women petitioning for a single PFA, 40 percent had at least one observed welfare spell during the six-year study period. We observe welfare spells prior to petitioning for 78 percent of recipients. For both welfare recipients and women without an observed welfare spell, a little fewer than half (45 percent of each group) have a scheduled follow-up hearing.

The lower portion of Table 1 compares median yearly earnings, standard deviations, and quarters with zero earnings before and after women petition for a PFA. These aggregated data tell a story of extremely low earnings along with financial gains for petitioning women. In the year prior to petitioning, women’s median earnings are $4,285 ($6,577 in 2014 dollars), whereas in the year following petitioning, their median earnings are almost a thousand dollars greater, $5,220 ($8,013 in 2014 dollars). At least some of these gains appear to come from women’s (re)turning to work. In the four quarters before they petition, the share of women with zero earnings...
in a given quarter averages 37 percent, a figure that declines slightly to 35 percent in the post-petitioning period. This aggregate description suggests relatively uniform earnings growth, consistent with the optimistic ideal-typical trajectories in Figure 1. However, these aggregated measures may be masking important intra- and inter-person differences that could plausibly cast a shadow across this relatively rosy picture.

Estimating Average Earnings Growth Trajectories: Linear and Four-Spline Models

To begin our examination of the trajectories of women’s earnings in the time surrounding petitioning for a PFA, we first fit a linear latent growth curve model, represented as a path diagram in Figure 2. Path diagrams represent relations between observed (measured) and unobserved (latent) variables. Latent variables are enclosed in ovals; observed variables are represented with boxes. Straight arrows indicate direction of influence; the curved two-headed arrow indicates covariance between two variables that is unexplained in the model. Measurement error is indicated by δ. The factor loadings for the measures of women’s earnings on the latent intercept are fixed to 1.0 to represent the initial starting point of the growth trajectory, and the loadings on the latent slope begin at 0 four quarters prior to petitioning, increasing by 1 each quarter to indicate linear growth. The latent intercept and slope are freely correlated.

Examining the means and variances of the latent intercept and slope terms provides a general picture of the average growth of women’s earnings when we do not allow changes in earnings growth over the observed period. Focusing first on the mean latent slope, we see that, on average, women experience slow but significant earnings growth

Figure 2. Path Diagram for Linear Specification of Unconditional Latent Growth Model Predicting Growth in Logged Quarterly Earnings (1995 Constant Dollars) of Single PFA Petitioning Women in Allegheny County, PA, 1995 to 2000

Note: *p < .05; **p < .01; ***p < .001 (two-tailed tests).
over the period surrounding petitioning. The significant variances of the intercept and slope terms also suggest there is meaningful individual-level variation around women’s initial earnings and earnings growth. The significant negative correlation between the intercept and slope indicates that women with higher initial earnings experienced slower earnings growth over the study period.

The linear LGC model does not fit the data well ($\chi^2 = 3334.99, df = 40, p = .000; \text{IFI} = .88; \text{RMSEA} = .15$). The significant chi-square statistic, coupled with an IFI below .90 and an RMSEA far above .10, suggest there is substantial room for improvement, but this is expected when we assume linear growth during the time periods surrounding women’s petitioning. Thus, we next allow for changes in women’s earnings pre- and post-petitioning by fitting a four-spline piecewise LGC model.

Table 2 summarizes results from our four-spline LGC models. We begin with a model for the full population of petitioning women. Examining the means of the latent slopes suggests that, on average, women experience significant earnings growth in the year before petitioning for a protective order. However, women’s earnings growth in the period just before petitioning is nearly one half the size of growth in the first time period (.17 to .09, $p < .05$), providing some support for the Shock Hypothesis. The final year of the study reveals a second shift in women’s trajectories: after petitioning, women, on average, experience no significant earnings growth. That women’s earnings trajectories pivot in this direction around the petitioning quarter provides support for the Stall Hypothesis. With an IFI of .99 and an RMSEA of .02, the unconditional four-spline model fits the data quite well; yet, the significant chi-square indicates there is still room for improvement ($\chi^2 = 54.42, df = 25, p = .001$).

**Calculating Lost Earnings: A Counterfactual Approach**

We use a counterfactual approach to calculate the dollar costs that come with petitioning for a protective order. Figure 3 uses results from our first estimated four-spline model to graph women’s predicted earnings over the four time periods (the solid lines) alongside two plausible counterfactual growth trajectories (the dashed and dotted lines). These counterfactual trajectories help us generate a range of potential earnings losses associated with women’s appeals to the police and courts to protect them from abuse. First, as shown in panel A, we generate a conservative projection of women’s average earnings losses in the year after petitioning by setting the slope of the post-petitioning growth trajectory to equal the slope in the two quarters immediately prior to petitioning. The difference between this conservative counterfactual trajectory and results from the fitted model indicates that women could be losing an average of $203 in 1995 dollars (equivalent to $312 in 2014 dollars) in the year after petitioning for a PFA.

A less conservative but also plausible counterfactual, shown in panel B, carries forward the first observed slope and calculates earnings assuming linear growth over the entire trajectory. Under these less conservative assumptions, women stand to lose an average of $663 in 1995 dollars (equivalent to $1,018 in 2014 dollars) in the period surrounding and after petitioning. Overall, these counterfactual results suggest that, on average, women experience substantial earnings losses in the year after they petition for a protective order. For a population of women already economically disadvantaged, such losses are especially likely to be consequential.

**Trajectory Differences by Welfare Receipt, Welfare Spell Timing, and Hearing Event**

In the next stage of our analysis, we explore group differences in women’s earnings trajectories. Table 2 reports the estimated coefficients and fit statistics, and Figure 4 displays predicted earnings for each of the three group comparisons. We begin by considering
Table 2. Results from Latent Growth Models Estimating Earning Trajectories of Women Who Petition for a PFA

<table>
<thead>
<tr>
<th></th>
<th>Initial Earnings</th>
<th>Pre-petitioning Growth</th>
<th>Post-petitioning Growth</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Intercept (α)</td>
<td>Slope (β₁)</td>
<td>Slope (β₂)</td>
</tr>
<tr>
<td>1. All Petitioners</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Population (N = 3,923)</td>
<td>4.604***</td>
<td>.172***</td>
<td>.086***</td>
</tr>
<tr>
<td>Goodness-of-fit statistics:</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>$\chi^2 = 54.423$, df = 25,</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>$p = .001$; IFI = .999; RMSEA =</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>.017</td>
<td></td>
<td></td>
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<tr>
<td>2. By Welfare Receipt</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Observed Spell (N = 1,568)</td>
<td>3.293***</td>
<td>.144***</td>
<td>.105*</td>
</tr>
<tr>
<td>Goodness-of-fit statistics:</td>
<td>−2.182***</td>
<td>−.048</td>
<td>.034</td>
</tr>
<tr>
<td>$\chi^2 = 70.265$, df = 50,</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>$p = .031$; IFI = .999; RMSEA =</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>.010</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>3. By Timing of Welfare Spell</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Spell Before (N = 1,228)</td>
<td>3.154***</td>
<td>.120*</td>
<td>.191***</td>
</tr>
<tr>
<td>No Spell Before (N = 340)</td>
<td>3.791***</td>
<td>.237*</td>
<td>−.212*</td>
</tr>
<tr>
<td>Before – Not Before</td>
<td>−.637***</td>
<td>−.117</td>
<td>.403***</td>
</tr>
<tr>
<td>Goodness-of-fit statistics:</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>$\chi^2 = 52.780$, df = 50,</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>$p = .367$; IFI = .999; RMSEA =</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>.006</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>4. Welfare Recipients by Hearing</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>With Hearing (N = 708)</td>
<td>3.355***</td>
<td>.122</td>
<td>.199**</td>
</tr>
<tr>
<td>No Hearing (N = 860)</td>
<td>3.243***</td>
<td>.161**</td>
<td>.029</td>
</tr>
<tr>
<td>Hearing – No</td>
<td>.112</td>
<td>−.039</td>
<td>.170*</td>
</tr>
<tr>
<td>Goodness-of-fit statistics:</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>$\chi^2 = 53.103$, df = 50,</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>$p = .356$; IFI = .999; RMSEA =</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>.006</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>5. No Welfare Spell by Hearing</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>With Hearing (N = 1,060)</td>
<td>5.701***</td>
<td>.128**</td>
<td>.059</td>
</tr>
<tr>
<td>No Hearing (N = 1,295)</td>
<td>5.292***</td>
<td>.244***</td>
<td>.081</td>
</tr>
<tr>
<td>Hearing – No</td>
<td>.409*</td>
<td>−.116</td>
<td>−.022</td>
</tr>
<tr>
<td>Goodness-of-fit statistics:</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>$\chi^2 = 74.75$, df = 50,</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>$p = .013$; IFI = .999; RMSEA =</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>.015</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Note: Standard errors reported in parentheses; group difference results report fully unconstrained models (with unconstrained intercept and slope variances, covariances, and error variances).  
*p < .05; **p < .01; ***p < .001 (two-tailed tests).
**Figure 4.** Predicted Earnings Comparisons among Groups of Women before and after Petitioning for a Protective Order

*Note:* The slopes from pre2 to post2 are short-term effects. The slopes from petition to post4 are longer-term effects.

whether women who did and did not receive welfare during our six-year study period follow distinct earnings trajectories in the years surrounding petitioning for a PFA. As shown in the second panel of Table 2 and panel A of Figure 4, these two groups of women have substantially different earnings trajectories. Of course, women without a welfare spell start out with significantly higher earnings than women who used welfare during our study period. Yet, the differences between these groups of women go far beyond initial earnings. For the group of women with at least one observed welfare spell, earnings growth is similar across the time periods before and after petitioning. (All slope differences across periods are statistically insignificant.) In contrast, for women without an observed welfare spell, earnings growth drops significantly during the immediate pre-petitioning period (from .19 to .07, \( p < .01 \)) and drops further to become significant negative growth in the first post-petitioning period (–.11, \( p < .001 \)).

Whereas women with an observed welfare spell do not experience changes in earnings growth that reach conventional levels of statistical significance, they do experience substantively meaningful changes in their average earnings growth. In the period just
after petitioning, welfare recipients’ average earnings growth is between 44 and 65 percent lower than during any other observed period. As discussed earlier, the high degree of variability (especially surrounding and after petitioning) means very large effect sizes are required for reaching statistical significance. Although such a high bar is helpful for assessing the external validity of our findings, the substantive declines in earnings growth we observe in the first post-petitioning period are associated with real financial costs for the women in our study.

Statistical evidence suggests that accounting for underlying differences in the trajectories of women with and without an observed welfare spell improves model fit. Modeling welfare recipients together with women who did not use welfare during our study period also masks important material and theoretical findings. Materially, it is important to recognize that women who do not appeal to the welfare state experience a hit to their earnings growth in the years surrounding petitioning so great that welfare recipients close the initial earnings gap by 42 percent. The separate group analyses are also informative. For women who use welfare, petitioning for a PFA is not associated with a statistically significant shock or stall in earnings growth; welfare appears to buffer recipients from some of the worst economic effects of the instability that is clearly associated with abuse and women’s appeals to the courts to end it. Yet, for women who did not use welfare during our study period, petitioning for a PFA is associated with not only a shock to, but also a stall in, their earnings growth.

Next, we assess the importance of the relative timing of petitioning and experiencing a welfare spell (the third panel of Table 2 and panel B of Figure 4). Women whose welfare spells precede petitioning are clearly different from women who go on welfare only after petitioning; their earnings in the initial pre-petitioning period are significantly lower, and their trajectories are dissimilar. When women petition without having been on welfare, we observe a severe shock effect in the immediate pre-petitioning period. We also observe a stall, although the declines after petitioning are not as steep. In the post-petitioning periods, women who go on welfare only after petitioning experience no significant earnings growth. In sharp contrast, women whose welfare spells begin before they petition have positive growth throughout the observed periods. Compared with roughly similarly situated petitioners, women whose welfare spells precede petitioning do not experience negative growth in either the short or longer runs. With an IFI of .99, an RMSEA of .01, and a non-significant chi-square statistic ($\chi^2 = 52.78, df = 50, p = .367$), Model 3 produces optimal fit.

Second, we split petitioning women by welfare receipt and hearing event, finding several noteworthy differences. Results from these comparisons are displayed in the fourth and fifth panels of Table 2 and panel C of Figure 4. Consider first the trajectories of welfare recipients. Women with and without hearings start off in the same place—their intercepts are almost indistinguishable—and initial growth for the two groups is also similar. Yet, in the run-up to petitioning, these two groups of women diverge. For petitioners with hearings, earnings growth continues at a similar rate; for petitioners without hearings, there is a substantial shock to earnings. Although growth in the year after petitioning is again similar for the two groups, the shock in earnings for women without hearings creates a gap between the two groups that remains across the following year. With an IFI of .99, an RMSEA of .01, and a non-significant chi-square statistic, model fit remains excellent.

Continuing on, we reexamine the gap between petitioners with and without PFA hearings for women who did not receive welfare over the study period. Again, the model continues to fit these data quite well. Among women with no observed welfare spell, there is a difference in initial earnings. Women with a subsequent PFA hearing begin the observation period with higher earnings than women who petition without a hearing. That is, at the outset, women who persist from initial
petitioning to the hearing stage appear different from women who do not persist to the hearing stage. However, we find no other significant differences in the earnings slopes of the two groups before or after petitioning. Visualy, both petitioning groups—women with and without hearings—follow similar trajectories to all women without an observed welfare spell in panel A.

In auxiliary analyses, we consider additional ways the petitioning process might vary for different groups of women. We assess differences between women who are plaintiffs only \((N = 3,759)\) versus women also involved in counter-suits \((N = 164)\), and we find their earnings trajectories to be statistically indistinguishable. We also evaluate the trajectories of 181 women who petitioned for multiple PFAs during our study period, estimating earnings growth before and after their first petition. Although our methods are not well suited to evaluating the trajectories of multiple petitioners, our analyses suggest that multiple petitioners are different from single petitioners—they have lower initial earnings and experience distinct changes in their earnings.

Other auxiliary analyses show that among petitioning welfare recipients, racial differences in earnings trajectories are negligible. Such results are perhaps contrary to the intersectional expectations of feminist theorists, who catalog the diverse ways racially marked and unmarked women (along with immigrants and religious minority women) might approach and experience abuse, work, welfare, and the police and courts (e.g., Choo and Ferree 2010; Crenshaw 1991; Ellison et al. 2007; Goodmark 2012; Hamby 2014; Hattery 2009). Our findings are nevertheless understandable given that we have race data for welfare recipients only. The null findings from comparing Black and White welfare recipients do not preclude meaningful differences in earnings trajectories by race among women who do not receive welfare assistance.

In our final auxiliary analysis, we compare the earnings growth of welfare recipients who petitioned to those who did not petition (see Table A1 and Figure A1 in the Appendix). Welfare recipients who petition have higher initial earnings than do non-petitioning welfare recipients. This gap closes, however, in the periods surrounding petitioning. Although both groups grow at similar rates in all other periods, petitioners experience a shock to their earnings growth in the first period after petitioning. Remember that as a group, welfare recipients’ earnings growth appears to be buffered from shocks and stalls. But, even these women experience significantly lower growth than non-petitioners (a difference of .11 in the first post-petitioning period, \(p < .05\)).

**DISCUSSION AND CONCLUSIONS**

Women who petition for a civil restraining order are extremely disadvantaged, struggling to stop abuse and mitigate or compensate for its effects on their lives and livelihoods. In the process, they appeal to the law-and-order state—and often the welfare state as well—for help. By design, petitioning for protection from abuse (a PFA) should help stop the abuse and thus put an end to the many ways abuse obstructs women’s work. Theoretically, petitioning for a PFA could thus help women grow their earnings. In reality, however, we know very little about how petitioning for a PFA relates to women’s earnings. In fact, to our knowledge, ours is the first empirical study of the work-related costs or benefits to women associated with their appeals to the police or courts for protection from abuse.

To investigate potential effects of PFA petitioning on women’s earnings trajectories, we draw from two sets of models of abuse and work. Extending exposure and exchange models, we are optimistic that petitioning for a PFA could increase women’s earnings growth in the short-term as boosts or in the longer-term as upward shifts. However, appealing to the state for protection may come at a price. Extending backlash and dual traps models of abuse leads us to be more pessimistic. Petitioning for a PFA could decrease...
women’s earnings growth in shocks or stalls. We assess these hypotheses using longitudinal individual-level data and latent growth curve techniques. By centering our analysis on petitioning, we are able to consider changes in women’s earnings growth before and after the event, regardless of when in calendar time women petitioned the courts for protection.

Our findings constitute overwhelming evidence that the period around petitioning for a PFA is almost universally one of tremendous earnings instability and financial costs for PFA petitioners, as the Shock and Stall Hypotheses predict. We estimate that women lose between $312 and $1,018 in 2014 dollars through the year after petitioning alone, and additional analyses suggest women are not recouping these losses later. These findings contrast sharply with the relatively rosy picture drawn from comparing aggregated median earnings in the year-long periods before and after petitioning and refute the Boost and Upward Shift Hypotheses.

Our finding a shock effect is hardly surprising. A short-term fall in earnings is to be expected given the upheaval and disruption that likely come with appealing to the state for protection from abuse. Finding longer-term stalls is more surprising. However, workplace studies find that “dramatic and disruptive” episodic life-events can have lasting effects on employment and related outcomes (Grunberg et al. 2008:217). For scholars of work, our results suggest that effects of petitioning for a protective order—and women’s experiences during this period of their lives—might linger. For employers, our results suggest that petitioners face both immediate and longer-term obstacles to their presence and productivity at work (O’Leary-Kelly et al. 2008); they may need more than time off to go to court. For policymakers and welfare administrators, our results underscore the need for exemptions from time limits on welfare receipt.

Our results are consistent with backlash models of the relationship between abuse and work, which have amassed a wealth of empirical support over the past two decades (for a review, see, e.g., Brush 2011). In the first observed period, petitioning women’s earnings rose. But, in the run-up to petitioning—a period that likely includes abuse for most of the study population—abuse might obstruct work, causing declines in earnings growth. Then, when women appeal to the state for protection, men’s abuse could escalate, further obstructing work and diminishing women’s earnings growth.

Certainly, backlash is not the only explanation for our empirical results. The growth in women’s earnings in the first observed period could reflect women’s efforts to increase their financial resources as a response to abuse; knowing they need resources to exit or to strike better bargains within their relationships, women subjected to abuse might work more and earn more. The related growth in women’s earnings may level off naturally or come to an abrupt end when women use PFAs to help them end the abuse. Although empirical research does not support the notion that abuse is an incentive to work (e.g., Brush 2011), our data do not allow us to dismiss this alternative. Moreover, other explanations may account for the growth in women’s earnings in the pre-petitioning period and losses in the post-petitioning period. Whatever the explanation, our study shows convincingly that women’s petitioning for a PFA does not come with either short- or longer-term increases in earnings growth.

Beyond population averages, group analyses reveal important differences in the earnings trajectories of PFA petitioners. Women without an observed welfare spell during the study period took a tremendous hit to their earnings growth, presumably when the trauma and instability of the petitioning period reduced their hours worked or their wage rates (especially if they were fired or changed jobs because of abuse or because they moved to escape abuse). Welfare receipt, however, appears to cushion even very low-earning women from the worst shock and stall effects of petitioning for a PFA. Our results are consistent with advocates’ insistence that welfare
provides a vital resource for women subjected to abuse (see, e.g., Brandwein 1999; Davis 1999; Raphael 2009).

Our analysis of differences among welfare recipients extends to the relative timing of petitioning and receiving welfare. We find that earnings trajectories of women with a welfare spell that precedes petitioning are clearly different from the trajectories of women who go on welfare after petitioning. When women petition without having been on welfare, we observe a severe shock effect and a stall, again supporting the pessimistic scenario. However, these effects may be partly attributable to income substitution. Receiving welfare may partially compensate for lost earnings, providing women material support during a time of tremendous instability. In contrast, women whose welfare spells begin before they petition do not experience either a shock or a stall in earnings growth. One explanation may be the independence effects of welfare receipt. Welfare may help the lowest-earning women establish independence from abusers, laying the groundwork for sustained earnings growth. These findings represent an important advance in our understanding of the complex relationships among receiving welfare, petitioning for a PFA, and women’s earnings.

Not all women who petition for a PFA persist through the process; 55 percent of petitioners in our study did not request or were not granted a hearing for a longer-term restraining order. We consider whether persisting through the process leads to differences in women’s earnings trajectories. On the one hand, if petitioning creates a backlash, persisting through the court process could exacerbate negative effects. On the other hand, persisting could be the only way to help women put a stop to abuse and thus mitigate the negative effects of petitioning on earnings. We find that persisting neither exacerbates nor mitigates effects of petitioning for women without an observed welfare spell. The earnings of women with and without a scheduled hearing differ only at the outset. In contrast, we find that persisting affects the way petitioning impacts earnings for welfare recipients. There is a mediating effect of persisting on earnings loss; welfare recipients with hearings are the only group to experience no shock to their earnings. Persisting through the petitioning process—when combined with welfare—can be a critical resource for some women during a turbulent and difficult period in their lives.

Our study draws attention to the need for additional data. There are a host of measures—for example, education level, marital status, presence and number of children, and occupation—we would like to include in our analysis but cannot. Given these data limitations, we cannot dismiss possible complications of omitted variable bias. In addition, our data may not be representative of all petitioning women in the United States, limiting the generalizability of our results. Our research takes only a first step toward unpacking the costs of women’s efforts to end abuse. Additional steps require investments in nationally representative data that combine repeated measures of abuse, earnings, appeals to the state for protection from abuse, welfare receipt, and other key covariates.

For women subjected to abuse seeking a lifeline, the disruptions of leaving their homes, moving and changing jobs, petitioning for protection through the courts, and applying for stigmatized income support through welfare are considerable. Our findings provide quantitative confirmation that this period is also one of tremendous economic instability. Moreover, our results suggest that not only the costs of abuse, but also the price of protection, contribute to earnings inequality and women’s economic insecurity. The economic losses women almost universally experience when petitioning for a civil restraining order call out for researchers, advocates, and policymakers to develop strategies to enhance gender equality and women’s safety, solvency, and economic stability.
APPENDIX

Table A1. Results from Latent Growth Models Estimating Earning Trajectories of Welfare Recipients by Petitioning

<table>
<thead>
<tr>
<th></th>
<th>Initial Earnings</th>
<th>Pre-petitioning Growth</th>
<th>Post-petitioning Growth</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Intercept ($\alpha$)</td>
<td>Slope ($\beta_1$)</td>
<td>Slope ($\beta_2$)</td>
</tr>
<tr>
<td>PFA ($N = 3,923$)</td>
<td>3.293*** (.092)</td>
<td>.144*** (.043)</td>
<td>.105* (.043)</td>
</tr>
<tr>
<td>No PFA ($N = 24,731$)</td>
<td>2.956*** (.022)</td>
<td>.160*** (.011)</td>
<td>.163*** (.011)</td>
</tr>
<tr>
<td>PFA – No PFA</td>
<td>.337*** –.016 –.058 –.106*</td>
<td>.024</td>
<td></td>
</tr>
</tbody>
</table>

Goodness-of-fit statistics: $\chi^2 = 414.148$, df = 50, $p = .000$; IFI = .998; RMSEA = .016

Note: Standard errors reported in parentheses; group difference results report fully unconstrained models (with unconstrained intercept and slope variances, covariances, and error variances). *$p < .05$; **$p < .01$; ***$p < .001$ (two-tailed tests).

Figure A1. Predicted Earnings Comparisons for Welfare Recipients by Petitioning

Note: We randomly assign welfare recipients who did not petition for a Protection from Abuse order a “petitioning quarter” around which we evaluate their earnings growth.
Acknowledgments
The second author collected the data. We thank the University of Pittsburgh Center for Social and Urban Research Steve Manners Program for funding this research. We thank Eileen Kopchik, who assisted with dataset construction. Meghan Haureen Tighe confirmed the details of PFA petitioning in Allegheny County. We also thank Alesha Durfee; Amelia Haviland; Pamela Paxton; Claire Renzetti; participants of the Workshop on Power, Resistance, and Social Change at the University of Pittsburgh; participants at the roundtable on Work and Occupations at the 2013 annual meetings of the American Sociological Association; and the editors and five anonymous reviewers for providing feedback on earlier drafts of this manuscript. Data collection and analysis conducted under IRB# 0806004 obtained by the second author.

Notes
1. Intimate partner violence (IPV) is one term for the force and force-threats used by abusive partners to attack, belittle, coerce, or control their spouses, dates, or lovers (Brush 2011). In this study, we instead refer to abuse, which includes but does not emphasize physical violence and is named in the intervention of interest in this study: Protection from Abuse (PFA) orders. We also follow Goodmark (2012) in referring to women subjected to abuse rather than victims, survivors, or battered or abused women.
2. These data span from 1995 to 2000 because the second author originally collected the data in 2000, and the Institutional Review Board required elimination of all individual identifiers after the dataset was assembled. Further extending the longitudinal study is impossible.
3. The National Survey of Families and Households (NSFH) includes both women’s earnings and select measures of abuse (Sweet, Bumpass, and Call 1988; see also, e.g., Anderson 2007). However, the three waves of data (1987 to 1988, 1992 to 1994, and 2001 to 2003) are too spread out to evaluate effects of abuse on women’s earnings growth.
4. The absolute level of earnings at any time point shapes surrounding earnings growth—including the period directly prior. For example, if petitioning reduces earnings levels only at the time that women initiate the petition, depressed growth would show up only in the time period directly preceding petitioning (after which growth would bounce back). Here, we theorize shocks as affecting women’s earnings at the time of and immediately following petitioning. Thus, we expect to see reduced growth in the periods directly before and after petitioning. This is distinct from boosts, which we theorize as affecting women’s earnings only after petitioning.
5. In addition to subject identifiers, PFA records include the dates of preliminary petitions, dates of follow-up hearings (if granted), and dates of repeat petitions.
6. Cash assistance for low-income mothers, called Aid to Dependent Children in the original New Deal legislation, was called Aid to Families with Dependent Children from the 1960s until 1996, when Congress rescinded entitlements and renamed the program Temporary Assistance to Needy Families. The welfare records include time-varying monthly data on welfare receipt and the number of individuals in a household on welfare as well as subject-varying measures of race/ethnicity, age, and sex.
7. The second author obtained these administrative data with the approval of her university’s Institutional Review Board, which exempted her from obtaining consent from individual subjects on the understanding that contacting PFA petitioners individually could put them at risk. Approval was conditional on stripping the data of individual identifiers after matching across the three sets of administrative records. The second author negotiated separate Data Release Agreements with DPW, BUCBA, and a Family Court judge.
8. The earnings of the women in our data are highly skewed; two-thirds of PFA petitioners have at least one quarter during which they have zero earnings. We therefore log quarterly earnings and analyze growth in logged earnings in all models.
9. Changing the way time is coded can affect the estimation of model parameters (Blozis and Cho 2008). To assess potential effects of manipulating calendar time, we conducted auxiliary analyses controlling for the timing of petitioning. Results suggest timing of petitioning has no effect, except for the latest group of petitioners. In particular, women who petitioned during the final five quarters of our study experienced significantly slower growth at the end of their earnings trajectories. Changes in welfare policy during our study period may help explain these differences (Hughes and Brush 2011).
10. We have a choice between multilevel modeling and structural equation modeling. Although multilevel models are superior in some situations, such as with unbalanced or clustered data, the two approaches produce the same results in most circumstances. Our data do not have any of the characteristics that would make multilevel modeling preferable. Furthermore, given our focus on trajectories of change, growth curves have intuitive appeal.
11. We present and discuss estimates from linear and four-spline models in the body of this article. Additional results using two- and three-spline models are available from the authors upon request. Notably, polynomials could efficiently estimate even complex shifts in women’s trajectories over time. But, they would not allow us to estimate differences in earnings slopes between periods and between groups simultaneously.
12. Undoubtedly, some of the estimated changes in women’s earnings growth during the periods surrounding PFA petitioning result from women’s going to work or leaving the labor force. However, even controlling for zero earnings in a given quarter, we continue to observe periods of significant earnings growth (not attributable to women’s transition from zero to non-zero earnings) and significant changes in women’s earnings trajectories over time.

13. Petitioning welfare recipients are not representative of welfare recipients as a whole. Petitioning women have higher initial earnings, are more likely to be White, and spend more time on welfare than do their non-petitioning counterparts (see Hughes and Brush 2011).

14. In auxiliary analyses, we investigate variation in pre- and post-petitioning trajectories by an additional year in each direction. For example, we evaluate longer post-petitioning trajectories for women who had at least eight quarters of earnings data post-filing (and two quarters of data pre-filing). Across the total population and across subgroups of women, we find that earnings growth more than a year out from petitioning differed little from the short- and longer-term growth periods included in reported results. These results provide further evidence that petitioning for a protective order coincides with a stall in women’s average earnings trajectories.

15. In perspective, the $1,018 lost in the year after petitioning is roughly equal to a month’s rent in Allegheny County ($950).

16. Fit is very good in the presence of equality constraints across groups, but tests of nested models show significant improvements in fit when we allow maximum differences in the underlying trajectories between women who did and did not receive welfare assistance during the six-year study period.

17. The consistent upward trajectory of women with an early welfare spell—who have the lowest initial earnings of all groups—might appear to mark a “floor” effect. However, the differences in the trajectories in panel B of Figure 4—in particular, the fact that they cross in the petitioning period—suggests that women who received welfare before petitioning do have somewhere to go but up. That we observe neither shocks nor stalls associated with petitioning for this group is not an artifact of regression to the mean or a floor effect.

18. All analyses that break down the population of welfare recipients into smaller groups produce statistically insignificant chi-square tests, indicating the apparent improvement in model fit may, in fact, be a result of reduced sample size.

19. Although not a statistically significant difference, PFA petitioners without hearings experience notably steeper earnings growth in the first pre-petitioning period than do women with hearings (.24 and .13, respectively), closing the gap in initial earnings just slightly. However, in the first six months after petitioning, women without hearings experience steeper earnings declines than do petitioners with hearings (−.15 and −.06, respectively), reestablishing the gap between the two groups.

20. There are important conceptual differences: for multiple petitioners, the post-petitioning period is also the pre-petitioning period for a future petition. We also face important methodological challenges: the timing of the second petition and the total number of observed petitions varies across women in ways that might influence their trajectories but are not easily modeled, especially given the small number of multiple petitioners in our study.

21. As noted earlier, there is considerable variation in the procedural and substantive details of PFA petitioning across states and localities. Similarly, public assistance programs vary by state and often by county, especially since Congress restructured welfare in 1996. Labor markets, too, are local. Then again, the process for petitioning for a PFA in Allegheny County, Pennsylvania, is not unusual. Many features of the study location are typical for the United States as a whole; for example, the cost of living in Allegheny County is near the national average.

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