A criminal history is an undeniably excellent predictor of future criminal behavior (Gendreau, Little, and Goggin, 1996). Yet despite stability in offending over time, most offenders, even those with serious criminal histories, eventually desist from crime (Brame, Bushway, and Paternoster, 2003; Kurlychek, Brame, and Bushway, 2006, 2007, in press; Langan and Levin, 2002; Maruna, 2001). Employment-based reentry programs are designed and evaluated with an eye toward hastening the desistance process for individuals with criminal history records (Drake, Aos, and Miller, 2009). In other words, these programs rely on employment and training to causally lower an individual's likelihood of recidivism.

Employment is an obvious starting point in the reentry process because it is the major “routine activity” of most adults, and individuals who are exiting prison look to work as their major source of legitimate income (Bucklen and Zajac, 2009; LeBel, Burnett, Maruna, and Bushway, 2008). Yet the most recent meta-analysis of experimental evaluations of noncustodial employment programs for individuals with a criminal history finds that...
none of the programs has a causal impact on employment or rearrest (Visher, Winterfield, and Coggeshall, 2005). Ongoing, high-quality evaluations of employment-based reentry programs like the Center for Employment Opportunities and Safer Foundation provide only a slightly more optimistic conclusion. If these evaluations continue to report null or inconclusive findings, evidence-oriented policy makers may justifiably conclude that employment-based reentry programs do not merit further investment.

We support evidence-based practices, and we applaud the use of the experimental methodology to study the effectiveness of work programs at causing desistance. In this article, however, we consider an alternative justification for employment-based reentry programs. We use a well-developed theory from labor economics called signaling theory (Spence, 1973) to argue that these programs still serve an important role in the reentry process, even if they do not have a causal impact on employment and recidivism.

This idea begins with the well-known fact that graduates of employment training programs recidivate at lower rates than nongraduates, and they perform better in the labor market (Wilson, Gallagher, and MacKenzie, 2000). For example, in the experimental evaluation of the Center for Employment Opportunities (CEO) program discussed later in this article, the evaluators at the Manpower Demonstration Research Corporation (MDRC) found that CEO participants who were employed for the first four quarters and placed in an unsubsidized job had a 10% recidivism rate during that first year (arrest, conviction, or incarceration). In contrast, CEO participants who did not successfully complete the program had a 44% recidivism rate during the first year.1 The evidence-based mentality that dominates program evaluation rightly discounts this finding. Comparing graduates with nongraduates in a nonrandomized setting will lead to biased inferences if one is solely interested in the causal impact of the program on desistance. But this same comparison is a valid, and powerful, demonstration of the potential function of program completion as a desistance signal during reentry.

We are not interested, in this context, in the causal impact of CEO on recidivism. Rather, we are interested in using the information conveyed by successful completion of a challenging program to differentiate between individuals with the same level of involvement in the criminal justice system. Information that can differentiate between those with criminal histories is all the more important, now that criminal histories are regularly used to bar individuals from employment (Pager, 2003, 2007) at the same time that antidiscrimination policies are pushing employers to avoid across-the-board bans on employment for those with criminal history records (Equal Employment Opportunity Commission, 1987; Goode, 2011).

The seeds of this idea have been presented before. Bushway and Reuter (2004) recognized that nonexperimental comparisons of program participants and nonparticipants

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1. The results were provided by Cindy Redcross in a series of personal communications with the authors.
highlight the importance of motivation for desistance, given that these comparisons show that those who participate in programs are different in fundamental but unobservable ways from nonparticipants. Freeman (2008) also argued for the active use of additional information to identify low-risk individuals among the group of individuals with criminal history records. In this article, we formalize this idea by describing the mechanism by which successful completion of an employment-based reentry program can identify good employees, or even more broadly, crime desisters, in a world where these characteristics are unobservable by employers seeking to make good hires in real time.

In the case of CEO, most reentering prisoners are minority men with extensive criminal history records, as well as with limited education and employment experience. Not surprisingly, research has shown that it is impossible to predict at the time of release, with great accuracy, who among the ex-prisoners are going to be stable employees (e.g., Visher, Debus-Sherrill, and Yahner, 2011). Yet statistics from the MDRC show that CEO program completers—as identified at the end of year one—have much better employment outcomes than CEO noncompleters in years two and three of the follow-up period. For example, among individuals who did not recidivate in the first year, 42% of the CEO completers worked six or more consecutive quarters during years two and three, compared with only 17% of the CEO noncompleters. Or, in contrast, only 9% of the CEO completers had no formal employment in years two and three, whereas 45% of the CEO noncompleters had no formal employment in that period.

A very large and statistically significant difference in employment outcomes is found for a group of individuals who had access to the same reentry services and resided in the same New York City labor market. Although we cannot know for certain, it seems to us that this difference is consequential enough to inform hiring decisions by potential employers, with respect to the level of crime risk and work productivity. To the extent this assumption is true, program completion may serve as a strong signal for employers seeking to identify good employees. Although research on employer decision making is limited, it was reported in one focus group study of employers that knowing that a “candidate successfully completed a transitional program after release from prison and has built a positive employment record” would favorably impact the hiring decision for 90% of the employers (Fahey, Roberts, and Engel, 2006: 13). No other proposed intervention had a similar impact on employers’ self-report hiring decisions. Successful completion of a voluntary, employment-based program is thus a viable signal for potential employers, although in truth, any demanding treatment

2. We say “not surprisingly” because the concerns about selection bias are based, fundamentally, on the belief that evaluators cannot fully observe the differences between participants and nonparticipants. If all differences were observable, then evaluators could in fact predict success conditional on all of the relevant observables—and there would thus be no major concerns about selection bias.

3. MDRC calculated employment rates from unemployment insurance records in New York State.
program that requires overt effort on the part of participants also may be a reasonable candidate. On the surface, signaling seems very similar to “dynamic risk assessment.” Signals, like dynamic risk factors, are observable characteristics of an individual that are subject to change. However, program completion is a signal not because it changes, but because it is subject to manipulation by the individual in question (Spence, 1973). Moreover, signals are observable characteristics that reveal previously unobserved information about a person. No change in one’s underlying state is assumed or required. In contrast, dynamic risk factors are included in risk prediction tools to capture new information about the changes in a person’s risks and needs as they proceed through the criminal justice system or the reentry process (Andrews, Bonta, and Wormith, 2006). In contrast, from the standpoint of signaling, efforts to acquire the signal should impose different costs on individuals in a manner that is correlated with the unobserved information that employers (or other stakeholders) wish to know. Although we believe the concept of signaling is complementary to the concept of rehabilitation that motivates most reentry programming, the signaling model is not complementary to the risk–needs–responsivity approach. It is based on a different theoretical perspective and carries with it different policy implications. After we provide the first detailed description of the signaling model in criminology, we will return to the differences between this approach and the risk–needs–responsivity approach in the conclusion.

We start with a brief review of the literature on employment-based reentry programs, focusing on the most recent experimental evaluations. We then provide a discussion of recent studies of criminal recidivism and “redemption,” followed by studies that illustrate sorting or selection mechanisms (long used to justify randomization) in the evaluation of work programs. We use these two research strands to develop an argument for using the completion of voluntary work programs as an informative signal for employers.

**Employment-Based Reentry Programs as a Cause of Desistance**

The most recent meta-analysis of experimental evaluations of noncustodial employment programs concludes that none of the programs has an impact on employment or rearrest (Visher et al., 2005). Drake et al. (2009) reached a slightly less negative conclusion in their meta-analysis when they considered employment programs encompassing broader offender populations than ex-prisoners. Relying on both experimental and nonexperimental evaluations of community-based employment and job training, they reported an average
decline of 4.6% in the likelihood of recidivism. Because these programs cost less than $500 a participant, they concluded that these policies may be worthwhile from a cost/benefit perspective. Raphael (2010) also reached a more positive conclusion in his review of programs intended for high-risk populations other than ex-prisoners, most notably, Job Corps and JOBSTART. Following his nonsystematic review, he concluded that “there is some evidence that income support, transitional employment and human capital investments in former and potential future inmates may reduce criminal behavior and recidivism” (p. 44). The programs Raphael highlighted, however, are considerably more expensive than the job search and soft skill programs reviewed by Drake and colleagues.

Visher et al. (2005) noted in their meta-analysis that most existing evaluations are quite old (the most recent was from 1994), and they do not adequately reflect current thinking about the best practices in the correctional field, including transitional employment. Transitional employment is subsidized employment made available immediately after a person exits prison, often accompanied by soft skills training and job search assistance. The exemplar for this type of program is the Center for Employment Opportunities (CEO) in New York City, a high-profile, well-respected, and relatively intensive (and expensive) transitional jobs program that serves more than 2,000 parolees annually. The CEO program begins with a 4-day preemployment life skills class, followed by a transitional job with a job coach for 4 days a week, and office-based job coaching on the 5th day, when participants are not assigned to work the transitional job. When the program participant is considered job ready, he is assigned to a job developer who tries to find him a permanent job. Then, when the parolee transitions to a permanent job, he is eligible for ongoing incentives for stable employment. Parenting services and advice about personal matters also are available.

The CEO program has been subjected to a rigorous, randomized evaluation for which the 3-year employment and recidivism results have recently been released (Redcross, 2010). CEO serves a high-need, mostly minority male population in their 30s—an age group that other experimental research shows is particularly amenable to employment intervention (see Uggen, 2000). Forty-three percent of the participant pool has neither a GED nor a high-school diploma, and they have an average of seven prior convictions, with an average prison term of 5 years. The control group, which looks very similar to the treatment group, only receives some life skills training and job search assistance from CEO, but it has access to other employment services from other agencies. The program itself was well implemented, with 80% of the treatment group completing the life skills class, 73% working in a transitional job, and 34% transitioning into an unsubsidized job found by CEO, with approximately 40% moving into unsubsidized work overall.

Despite this very intensive intervention, the 3-year follow-up found no differences in employment between the treatment and control groups after the transitional job ended. By the end of the first year, only a third of the combined treatment and control groups had any employment that is part of the unemployment insurance (UI) program (sometimes called “pay stub jobs”). This employment rate is consistent with what researchers typically find for
ex-prisoners, both before and after their prison spell (Pettit and Lyons, 2007; Sabol, 2007; Tyler and Kling, 2007).

Surprisingly, given the lackluster employment results, the evaluation finds that CEO reduces the probability of reconviction by 12% (43% vs. 49%) and reduces the probability of reincarceration by 11% (58% vs. 65%). These results are apparently driven by the 40% of the sample who enter CEO within 90 days of their release from prison. This group, which is higher risk than those who come to CEO later, experiences a 17% reduction in rearrest (49% vs. 59%), a 22% reduction in reconviction (44% vs. 57%), and a 15% difference in reincarceration (60% vs. 71%). These results are sizeable and impressive. However, they also are puzzling. As observed by Zweig, Yahner, and Redcross (2010: 14), “it is unclear how the CEO program actually works to reduce recidivism.”

A reasonable hypothesis is that crime reduction comes from the extensive coaching and positive caseworker support provided by CEO, over and above the employment assistance. For example, the CEO treatment group members are 25% more likely to report that they have someone to whom they can turn for support with personal or family matters. This conclusion—that recidivism reduction stems from caseworker support—is buttressed by the 1-year findings from the Transitional Jobs Reentry Demonstration (TJRD), an experimental evaluation of four transitional jobs programs (Redcross et al., 2010).

TJRD is targeted at a population that resembles the population in CEO and is made up entirely of offenders who are within 90 days of their release from prison—the population that the CEO results suggest benefits the most because of their higher risk. The program was implemented well, with 85% of the treatment group moving to a transitional job. Again, the control group receives substantial job search assistance and some life skills coaching. Unlike CEO, the transitional jobs are not small work crews with supervisors from CEO who can provide direct coaching. Instead, participants are employed in workplaces like Goodwill stores and a garbage recycling facility where they are supervised as if they are regular employees.

The employment results from TJRD are less positive than CEO, with 28% of the sample in unsubsidized, UI-covered work at the end of the 1st year (compared with 33% in CEO). However, as in CEO, no detectable difference in postprogram employment exists between treatment and control groups. Moreover, no difference in recidivism outcomes exists between the TJRD treatment and control groups. By the end of the 1st year, roughly 55% of both groups are arrested, convicted, or incarcerated at least once. A reasonable conclusion from the experience with TJRD and CEO, then, is that it is not transitional employment itself that leads to the recidivism decline observed in CEO. Rather, it is the on-job coaching or other services that are part of the transitional jobs program at CEO.

Why “Work Doesn’t Work”
The apparent failure of work programs to improve consistently and substantially the employment and offending experiences of their participants begs an obvious
question: “Why?” The theoretical literature is sufficiently well developed that it comes as a bit of a surprise, at least at first glance, that the work programs to date have produced such disappointing results. At least four plausible reasons exist.

First, implementation problems tend to plague field experiments of this sort. Specifically, participants often fail to comply with program conditions. Individuals assigned to a control condition can seek nonprogram assistance or training on their own, whereas individuals assigned to an experimental condition can refuse to show up for training or subsidized employment. Plenty of evidence of this sort of noncompliance in social experiments exists (see Heckman, Hohmann, Smith, and Khoo, 2000). Noncompliance is problematic for evaluation of the “treatment effect” of subsidized work, skills training, and search assistance. The problem originates because the causal estimand in these studies is the “intention to treat” (ITT), that is, the causal impact of randomized offers to participate in a work program. When substantial noncompliance with the program conditions occurs, the ITT can be attenuated—often substantially so—relative to the “average treatment effect on the treated” (ATT), or the causal impact of actual participation in a work program.

Second, individuals with a criminal history have well-documented employment problems. The brutal truth is that many such individuals have difficulty holding onto jobs; providing them with a job probably does little to improve their “employability” (Bushway and Reuter, 2004). Bloom (2006: 3) cogently observed that “many people enter the criminal justice system hard to employ and leave it even harder to employ.” Among the individuals with a criminal history in the National Supported Work Demonstration, for example, 33% were fired from their program job, and another 20% were terminated for other negative reasons such as reinstitutionalization (MDRC, 1980). Although almost two thirds (65%) of the individuals with a criminal history in the Returning Home study were employed at some point during the eight months after their release from prison, less than half (45%) were currently employed (Visher, Debus, and Yahner, 2008). And the employment problems experienced by these individuals tend to be longstanding. For example, Apel and Sweeten (2010) demonstrated that young people who experience their first incarceration spell exhibit unstable work histories well before they are convicted, compared with young people who also are convicted but are not incarcerated. For example, they are less likely to have been employed at any point in the year prior to their conviction (60% vs. 67%), and they work fewer weeks when they are employed (29 weeks vs. 33 weeks). To-be-incarcerated youth also exhibit weaker attachment to legal work, as indicated by their higher probability of labor force nonparticipation at any point during the year prior to the conviction that led to their confinement (76% vs. 69%).

Third, individuals with criminal history records face inequality in the job search process. This discrimination is, in part, because they tend to be unskilled and poorly educated—qualities that make them unattractive to potential employers. For example, slightly more than one quarter (27%) of the individuals in the Supported Work evaluation
had at least 12 years of schooling (MDRC, 1980). Nationally, just 35% of jail and prison inmates have at least a high-school diploma, compared with 82% of the general population (Harlow, 2003). In this respect, then, individuals with criminal history records resemble other hard-to-employ populations—among them, welfare recipients and high-school dropouts—who lack some of the credentials valued by potential employers. In addition to their severe human capital deficiencies, however, evidence is mounting that a criminal history stigmatizes individuals in the marketplace. Pager (2003), for example, reported 50% lower callback rates among job applicants who report a prison sentence on their application (see also Holzer, Raphael, and Stoll, 2004; Pager, 2007; Raphael, 2010; Stoll and Bushway, 2008). She concluded that “criminal records close doors in employment situations” (p. 956). Many individuals with criminal history records invariably discover this from their own job search experience, reporting overwhelmingly that they feel their criminal record has hindered their ability to find a job (Visher et al., 2008). This finding does not bode well for modern prisoner reentry, as the use of criminal history records as part of the hiring process has increased substantially over the last 20 years (SEARCH, 2005).

Fourth, the level of improvement observed in intermediate outcomes like work may be insufficient to lead to observable reductions in recidivism. Following Lattimore, Steffey, and Visher (2010), suppose that an employment program can increase employment by 20%, such that the treated population has an employment rate of 60% compared with a 50% baseline among the untreated. Now suppose that employment reduces recidivism by 20%, again a sizeable treatment effect, such that the recidivism rate for the employed is 40% (compared with a 50% baseline among the unemployed). In a sample of 100 treated offenders, the employment program will only result in one fewer rearrest, and the overall impact of the program, which can increase employment by a substantial 20%, will be a 2.2% reduction in recidivism. The fundamental challenge of decreasing recidivism indirectly through these types of programs also is highlighted by Raphael and Weiman (2007).

To summarize, the most defensible conclusion from experimental evaluations of work programs is that the programs have a dismal record of jointly improving employment outcomes and of lowering recidivism probabilities among the individuals who participate in them (for similar conclusions, see the meta-analysis of Visher et al., 2005). Some programs have exhibited modest success for some groups of individuals with criminal history records, but these seem to be the exception rather than the rule. Yet the fact that “work doesn’t work” as a policy lever (in a causal sense) does not mean that work programs for individuals with criminal history records should be abandoned. Instead, individuals who successfully complete work programs might be sending a pronounced “signal” that can be exploited during the reentry process, even if the program itself has no causal impact on recidivism.
Criminal Records and Redemption

It is a criminological fact that most individuals who commit crime will desist or stop committing crime (Barnett, Blumstein, and Farrington, 1989; Laub and Sampson, 2001; Maruna, 1999). Although there is some debate about whether everyone ultimately desists (Blokland, Nagin, and Nieuwbeerta, 2005; Sampson and Laub, 2003), there is no doubt that most people who engage in crime eventually do so. In a random collection of recently convicted individuals, a sizeable subset—between 20% and 30%—of these “offenders” will never be arrested again (Brame et al., 2003; Kurlychek et al., in press; Schmidt and Witte, 1988). Comparisons of hazard models have repeatedly shown that split-population models, which allow for “instantaneous desistance,” substantially outperform models that assume that individuals with a criminal history continue to offend at a constant rate (Kurlychek et al., in press; Schmidt and Witte, 1988). Ethnographic work similarly shows that even serious, serial offenders will choose to exit offending (Baskin and Sommers, 1998; Maruna, 1999).

Although there is no debate about the existence of desisters, there is a considerable amount of debate about the ability to distinguish prospectively between desisters and persisters. For example, John Laub stated in his Sutherland address to the American Society of Criminology that “human agency induces an apparent instability or random component into life-course turning points making neat prediction—even from adult factors—inherently a difficult if not impossible endeavor” (2006: 244).

Recent empirical research on long-term recidivism hazards has begun to test the limits of this claim (Blumstein and Nakamura, 2009; Bushway, Nieuwbeerta, and Blokland, 2011; Kurlychek et al., 2006, 2007; Soothill and Francis, 2009). Using hazard models, this research has shown definitively that individuals who have not offended for a very long time—between 7 and 10 years—have a very small probability of offending in the next year. In fact, they often seem to have the same level of risk as individuals without a criminal history. This line of research has thus established that it is possible to distinguish risk levels

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5. There are two basic approaches to modeling hazard rates. The first approach is to assume that all offenders have a constant probability of recidivism or “failure” in any given period—an assumption that characterizes traditional criminal career models (see Barnett et al., 1989). The logic underlying this approach is that all offenders will eventually recidivate, if only they can be followed for a long period of time. In practice, many failure times are “censored” because of death or insufficient follow-up length. The second approach is to assume a “split population” comprising one group of offenders who will eventually recidivate, and a second group of offenders who have effectively desisted and will never recidivate, and are thus never at risk of experiencing failure. The latter are considered instantaneous desisters.

6. Most of this work has been done with first-time offenders, which does not accurately characterize people exiting prison. Bushway et al. (2011) used individuals with serious criminal histories and found similarly dramatic reductions in risk, although the risk may not decline to that of nonoffenders.
among individuals with criminal histories, although in this case it takes at least 7 years of waiting before the desisters reveal themselves.

Some scholars in criminology are very comfortable with the idea of prospectively identifying risk levels and of using these predictions in policy (see Andrews et al., 2006). However, these more contemporaneous risk tools have a limited ability to make risk distinctions (Rhodes, 2011). An individual’s criminal propensity can only be known within fairly wide confidence limits (Bushway, Sweeten, and Nieuwbeerta, 2009), and any risk prediction exercise will necessarily involve considerable error (Gottfredson and Moriarity, 2006). At the same time, we do know that there are individuals, even in high-risk categories, who have desisted from crime.

The question of how to identify the desisters is particularly salient within the context of prisoner reentry. By any measure, the individuals studied in recent evaluations of employment-based reentry programs are high risk. For example, the CEO participants have on average seven prior convictions and have spent 5 years in prison, with very low levels of education and prior work experience. Nonetheless, the research seems to show that a sizeable minority ceases offending as soon as they are released from prison. For example, Kurlychek et al. (in press) used 18 years of follow-up data on individuals convicted of felonies in New Jersey to estimate that 25% of the population of convicted felons desists, more or less instantaneously. Can individuals exiting prison who have nonetheless desisted be identified as low risk without waiting 7–10 years or more?

The Signaling Framework
This section develops the concept of signaling and explores ways in which the reentry discussion could benefit from a focus on allowing individuals to identify themselves credibly as desisters, rather than on trying to “cause” desistance explicitly. This approach is not risk prediction in the traditional sense. Risk prediction uses observable characteristics that are correlated with recidivism to assign offenders to risk categories. A large quantity, even the majority, of the available information about risk is unobservable, meaning that risk predictions based on observables will be made with considerable error. For a signaling model, on the other hand, we are looking for behaviors, in real time, that can flag individuals who possess characteristics that are, fundamentally, unobservable.

A good point of departure to explain signaling is to recast the problem as one of asymmetric information. The person reentering the community from prison may know that he or she has desisted from crime or that he or she is a good employee—but no one else does. Moreover, she now belongs to an observable group (ex-prisoners) that is known to have poor employment outcomes and high recidivism rates. Are there ways that the desister can signal to the employer her true identity?

7. The relative costs of type I and type II errors, which are context specific, drive whether a prediction exercise is justifiable from a policy perspective (Berk, 2011; Bushway, 2011).
The notion of signaling has been formalized in labor economics. Because potential employers make hiring decisions and wage offers in the absence of perfect information about applicants’ true productivity, they may resort to what they regard as reliable indicators of that productivity. Among these indicators are job market “signals,” which are observable attributes that convey information about an individual’s productivity (see Spence, 1973). Importantly, the acquisition of signals should be a matter of individual choice, and good signals must impose opportunity costs on the signal sender. Costless signals would easily be adopted by any rational individual who wishes to achieve the benefit of the signal, such as a well-paying job, whether or not he is truly a productive worker.

A good signal is also one for which the costs of acquiring it vary inversely with the unobserved variable (e.g., crime desistance and work productivity) about which the observers wish to learn (Spence, 1973). In the labor market context, education is a very good signal. Setting aside monetary costs, obtaining a college degree is not as costly for an overachieving student who does not need to devote much time and effort to studying, and at any rate has the ability and motivation to fulfill the requirements for the degree. Coincidently, by virtue of the same underlying qualities, employers have learned that such individuals are likely to be “productive workers” when they are hired. On the other hand, obtaining a college degree is costly for an underachieving student who has to make more sacrifices with respect to time and effort to complete the degree requirements and, consequently, has a lower probability of going to college (or matriculating if he or she does go to college). Employers have therefore learned that a college degree sends a strong signal about how productive and successful a potential employee will be if hired, even if college has absolutely no causal impact on work productivity.

A simple empirical example, inspired by Spence (1973), can make this idea clearer. Suppose we have two groups of workers: Group A and Group B. Individuals in Group A produce 10 units for every period in the labor market, and individuals in Group B produce 20 units. Group B is therefore more productive. However, although these two groups of workers have differential underlying productivity, this is not visible to the potential employer. An employer who hires randomly from this pool will obtain a very mixed overall level of productivity. Clearly, employers would prefer to hire as many individuals from Group B as possible, and Group B workers would like to be able to reliably signal their heightened productivity to employers.

Furthermore, suppose the (opportunity) cost of acquiring a signal, such as completing high school or college, is 10 units for members of Group A and 5 units for Group B. In other words, gaining the signal is twice as costly for members of Group A. An employer, who cannot otherwise distinguish between Groups A and B, can nonetheless allow Group B to signal its membership by paying 10 units to applicants without the signal and 20 units to those with the signal. Members of Group A will earn 10 units either way (they get paid 10 without the signal and 20 with the signal, for which they have to pay 10), and they are therefore better off not acquiring the signal at all. Members of Group B, however, are better off investing in the signal because they will get 10 units without the signal and
15 units (20–5) when they invest in the signal. In this case, even though the signal adds absolutely no productivity to society—it is not causally related to productivity, or at least it need not be—it serves a useful function by allowing Group B members to self-identify as high-productivity workers. Note that this example hinges on the fact that acquiring the signal costs the high-productivity group less than the low-productivity group. The intuition no longer holds if the cost is the same for both groups.

This example also can support some other useful insights, which are relevant for prisoner reentry. First, in the absence of a signal, employers would just pay the two groups the same wage that reflects their average productivity. That average will depend on the relative size of the two groups. (The unconditional wage gets higher as Group B gets proportionally larger.) That means members of Group A are worse off when signaling occurs. The only case where this is not true is if the productivity of Group A is lower than the minimum wage, and the productivity of Group B is not high enough to bring the average over that level. Second, although we will not do the math here, Spence (1973) showed that Group B will ultimately do better in a world with signaling, as long as Group B is a comparatively small group. Although the precise meaning of “small” depends on the relative magnitude of the productivity difference, the key insight here is that signaling benefits people the most when the size of the high-productivity group is relatively small.

Third, there is no one equilibrium signal cost in this case (but see Riley, 2001). Group B will attain the signal for any signal cost that is greater than 10 units and less than 15 units. Once the signal costs 15 units, Group B will be better off not acquiring the signal and accepting a wage offer of 10 units. Neither the employer nor the worker benefits from having a signal that costs more than 10 units. More generally, it makes sense to try and find the least costly signal that will allow members of Group B to self-identify as high-productivity workers. Fourth, this system will create feedback loops in equilibrium. The employer starts out with conditional beliefs about productivity and offers a wage schedule as a function of the signals. Potential employees get to make signaling decisions that will maximize their return, net of signal costs. Employers then get to hire and see the relationship between the productivity of the worker and the signal in real time, with the chance to modify the wage schedule as necessary.

Fifth and finally, there is no indication in this system that the signal does anything to increase productivity. In the economics of education, the human capital effect is sometimes called the “sheepskin” effect. One of the major challenges to empirically verifying the signaling effect is that the human capital theory of education and the signaling theory of education make very similar predictions—they both predict that wages will increase with education (Bedard, 2001). Empirical attempts to verify signaling have in part been focused on identifying situations where the two theories generate different predictions. 8 For

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8. Tyler et al. (2000) provided supporting evidence for this last proposition by finding situations where two people with the same test score do not get the same certificate. They arguably have the same human capital.
example, Lang and Kropp (1986) observed that making education mandatory will eliminate the signaling value of a high-school diploma. Although human capital theory predicts that changes to the mandatory nature of high-school completion should not affect the behavior of high-school completers, signaling theory predicts that high-school completers will invest in college to further differentiate themselves.

**Signaling in the Reentry Process**

The situation for the recently released prisoner is similar to the problem posed by Spence (1973). There is information asymmetry—desisters may know who they are, but no one else does.9 Employers are only truly going to learn about individuals with criminal history records after making the decision to hire them. This is not a repeat game, so individuals do not have the ability or incentive to acquire a “reputation” for crime desistance or work productivity. Yet individuals who have desisted pose less cost than individuals who remain embedded in crime. Therefore, at least in principle, this is a context where a signaling process could be beneficial.

We are suggesting that policy makers identify explicit mechanisms that will allow the 20% to 30% of returning prisoners who have desisted from crime to self-identify to employers or other decision makers as soon as they step beyond the prison walls, or very shortly after release from prison. Such mechanisms would substantially shorten the 7–10 years that are currently required to identify them reliably. In hypothesizing about potential “desistance signals,” recall that good signals must be voluntary, they must be attainable by a comparatively small proportion of the population of interest, and they must have opportunity costs for the individual that vary inversely with desistance probabilities or work productivity. In light of these requirements, voluntary enrollment, active participation, and successful completion of prison- and community-based employment programs—at a minimum—are excellent candidates for desistance signals.10

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9. An anonymous reviewer expressed concern about the assumption that individuals “know” they are desisters. Indeed, research has shown that many returning prisoners are overly optimistic about their ability to refrain from crime (LeBel et al., 2008). However, signaling has value even if individuals are not consciously aware of their status, provided they know their costs for engaging in signaling behavior. Therefore, we will assume that individuals with criminal history records know the costs of enrolling in employment-based programs, even if they do not consciously “know” whether they are desisters.

10. An anonymous reviewer helpfully brought to our attention some conceptual parallels with the health services literature, with its concepts of initiation (voluntary enrollment), engagement (active participation), and retention (successful completion).
The signaling model actually exploits the fact that individuals with criminal history records sort themselves into different institutional arrangements and recidivism likelihoods. To elaborate on the model’s utility for prisoner reentry, we restrict our attention to employment-based programs. Specifically, individuals who voluntarily enroll in, actively participate in, and successfully complete employment training programs differ from those who refuse to participate or drop out, mostly in ways that are difficult to measure and are independent of program effects on behavior. Such individuals possess unobservable characteristics—motivation and ability, among other intangibles—which can result in overly optimistic assessments of the benefits of work programs on employment and crime. Highly motivated individuals will experience better employment and recidivism outcomes for reasons that have nothing to do with the program itself. Individuals therefore sort themselves into recidivism risk levels, inadvertently, on the basis of their successful completion of voluntary work programs. This well-known “selection problem,” in fact, is the impetus for conducting experimental evaluations of the causal impact of work programs on employment and recidivism.

From the perspective of program evaluation, differential sorting or selection is truly problematic. Consider an example from the Returning Home study. Visher et al. (2008) reported that individuals with a criminal history who held a job while in prison, or who participated in job training classes, had substantially lower rearrest and reincarceration probabilities by the 8th month after release from prison. If our interest were strictly limited to program evaluation—“does the program work?”—we would regard this study with justifiable skepticism about the capacity of vocational programs and prison work to positively change inmates’ “dynamic needs,” or otherwise causally reduce their propensity to recidivate. After all, Returning Home subjects were allowed to self-select into the “treatment regimen” of training and work that they underwent while in prison. Thus, whatever the “it” is that compelled these inmates to enroll in prison-based training and work, “it” also is probably responsible for their much lower recidivism risk after their return to the community.

From a signaling perspective, however, the differential sorting of individuals into program participants (or completers) versus nonparticipants (or dropouts) is essential. In order for program participation and completion to be good signals, we must verify that the requirements are less costly for desisters to meet than for nondesisters. Research has shown that former prisoners who admit a willingness to continue crime or drug use before release are, not surprisingly, less likely to desist, as well as less likely to work or complete work-related programs successfully (Lattimore et al., 2010). Individuals who are cynical about the legal system are more likely to continue crime and are less likely to work (Sampson and Bartusch, 1998). Drug abuse and mental health problems are correlated with both desistance and employment (Greifinger, 2006). A reasonable assertion is that the requirements of work programs, and employment more generally, are simply more costly to individuals who are not yet ready to give up criminal offending or to earn income.
legitimately. Note that this assertion does not refer to monetary cost, but to an opportunity cost, or a psychic or emotional cost. Showing up on time and following the rules might be costly to a legally cynical person who possesses an oppositional view toward the legitimate world, or adopts an identity as a “gang banger” or street tough (LeBel et al., 2008). However, these same program requirements should be less costly to someone who has become committed to life as a “family man” or other more conventional identity (Bucklen and Zajac, 2009).

The Returning Home example provides evidence that participants in work programs do indeed differ from nonparticipants in a way that can make participation a reliable desistance signal. Additionally, among individuals who participate in work programs, those who remain actively involved in and complete work programs differ from those who have irregular attendance or who drop out. In the reanalysis of Supported Work by Uggen (2000), a “participation” model yielded much stronger evidence for the desistance potential of subsidized work than an “assignment” model (see his Table 2, Models 1 and 3, p. 540). For the purpose of this discussion, we ignore the models in which Supported Work is interacted with age. On the one hand, individuals who are randomly assigned to subsidized work have an odds of arrest that is only 3% lower than the controls (Model 1: \( \exp(-0.03) - 1 \)), an “intention to treat” effect that is not statistically significant. On the other hand, the subset of the assigned individuals who are actively employed in a subsidized job, in any given month, have an odds of arrest that is 26% lower than their counterparts who are not so employed (Model 3: \( \exp(-0.30) - 1 \)), which includes the controls as well as the experimental subjects who have dropped out of the subsidized job. If one jointly considers participation and program completion—specifically, an individual who is actively employed in a subsidized job for the duration of the program—the odds of rearrest in any given month are 34% lower (Model 3: \( \exp(-0.30 - 0.11) - 1 \)). In this case, the contrast comprises the controls, the experimental subjects who have already dropped out of the subsidized job, and the experimental subjects who will drop out of the subsidized job before completing the program.

Although the program effects calculated from Uggen’s (2000) participation and completion models are clearly biased by selection—recall MDRC’s (1980) finding that one third of the individuals with a criminal history in Supported Work were fired from their program job, arguably for reasons that are correlated with recidivism—they nicely illustrate the signaling function that program participation and completion have during the reentry period. A 34% difference in recidivism risk is substantively large, and it might translate

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11. Consider the following anecdote from Project RIO. When asked what happens to referred parolees who do not participate in the program, a parole officer related the following (Finn, 1998: 9): “While some of these clients get jobs on their own, most of them are incorrigible releasees from the get-go—they won’t take courses, go to substance abuse treatment, or find work. So sooner or later they violate the conditions of release and are returned to court.”
into meaningful differences in performance for employers willing to hire from the pool of individuals with criminal history records.

We therefore seek to turn the problem of selection onto its head, and to identify program completion as a signal to be exploited by employers trying to differentiate individuals with criminal history records who are “good bets” from those who are not. Consistent with our argument, completion of the Supported Work program is correlated with lower arrest risk, not necessarily because Supported Work causes lower risk among individuals with a criminal history record—it might, although probably only for older individuals (see Uggen, 2000)—but because program completion serves as a meaningful signal of which individuals with a criminal history have lower recidivism risk in the first place.

A nonexperimental evaluation of a prison work release program (Adult Transition Center, or ATC) in Cook County, IL, similarly illustrates the sorting and selection processes at work during the reentry period. Jung (2010) reported that participation in ATC was positively correlated with the probability of UI-covered employment for up to 8 years after release from prison, compared with male ex-prisoners released from minimum security prisons. However, individuals who completed the program (40% of the men who participated) experienced far better employment and recidivism outcomes than those who dropped out (as a result of rule violation). Their employment probabilities were approximately 10 percentage points higher, and their recidivism probabilities were almost 10 percentage points lower (see his Figures 3 and 4). This same pattern can be found in the more recent evaluation of CEO discussed in the introduction. Less than half of the subjects in the treatment group who make it through life skills training are placed in an unsubsidized job, in part because of failure to abide by the rules of the program. Of those placed in an unsubsidized job, just one third are employed for all four quarters of the 1st year (14% of the total sample of CEO participants). The recidivism rates of these individuals after the 1st year are less than one quarter of the recidivism rates of those individuals who do not complete the program. Moreover, the formal employment rate of the CEO graduates remains much higher compared with those who did not complete the requirements of the program. Independent of the causal impact of CEO on recidivism, then, people who fully complete CEO are sending a signal that they are lower risk than those who do not, and they have much better employment prospects.

Indeed, the improved employment prospects of the CEO graduates might be a reflection of the fact that employers are already using CEO graduation as a signal of better employability. Although very little direct study of this question has taken place, we are aware of one study (Fahey et al., 2006) that asked employers in a focus group if successful participation in a transitional jobs program would help an individual who had recently exited prison find employment. Ninety percent of the employers felt that it would, which made this the most effective program of any of the 12 interventions tested by the researchers. However, the sample size was very small (N = 20), and these results were not statistically significant.
Anecdotally, it is not hard to find human resource professionals who advise employers that are willing to hire ex-offenders to look not only at the nature of the criminal record but also to look at “what these individuals accomplished behind bars or since they’ve been released—did they obtain a GED, enroll in college or complete anger management, drug rehabilitation, correspondence or vocational training program?” (Patton, 2008: 86). Holzer, Raphael, and Stoll (2002) reported that employers often are willing to hire individuals with criminal history records who come from trusted intermediaries—that is, reentry workforce developers—who have prescreened and prepared qualified applicants. However, the reality is that very little direct evidence exists that employers use program completion as a way to identify productive employees once they have decided to hire individuals with criminal history records.

Some limited evidence is available that program providers recognize that their primary job is to identify good employees, rather than solely to develop new skills. Reviewers such as Holzer et al. (2002) emphasized the screening role of intermediary agencies. More anecdotally, the director of Project RIO in Texas justified the decision not to colocate Project RIO’s programs in the same office as parole for the following reason: “[W]hile we lose some clients because they won’t travel from the parole office to a RIO office, forcing them to make the trip helps us screen them for motivation” (Finn, 1998: 12). A recent story on the National Public Radio broadcast, This American Life, described the main activity of an employment program in Rochester, NY, for individuals with a criminal history record as “weeding people out.” A staff member stated that, “if you can’t behave correctly in this class, we won’t recommend you for a job” (Davidson, 2011). Only 45 of the 170 people (26%) who started the program were placed in a subsidized job.

On the policy front, there is a small and growing movement toward “Certificates of Relief” or “Certificates of Good Conduct.” Unlike sealing or expungement, these certificates provide a positive statement of rehabilitation based on behavior that carries with it a removal of restrictions that are created by the criminal history record. The rehabilitation certificates in New York State are statutorily among the strongest, carrying with them the elimination of any statutory exclusions based on the criminal history record. In addition to a 3- or 5-year waiting period, the parole board offering the certificate requires that individuals offer information on employment history and means of support, proof of payment of income taxes for the last 3 years, and proof of payment of any fines or restitution. In effect, the parole board is using these factors, together with a mandatory waiting period, to certify desisters and eliminate explicit barriers to employment.

Rehabilitation certificates are consistent with the idea of desistance signaling developed in this article. Although no statistical evaluation is provided of the risk associated with them, it will be very hard for a nondesister to acquire the signals required by these standards, and the standards actually speed up the process over and above what would be required by simply waiting for an extended period of nonoffending. These types of certificates also are consistent with Freeman’s (2008) assertion that the government can marshal evidence
about desistance probabilities, which can then be used by other stakeholders to differentiate effectively among individuals with criminal histories. From this perspective, these certificates do not cause “desistance” but function purely as signals.

Conclusions

Summary
There has been a growing focus on employment-based reentry programs, especially transitional job programs. Also, there has been an exciting trend toward rigorous evaluations of these programs after almost a 15-year period with no experimental evaluations of these types of programs. We began this article with a review of these evaluations. Visher et al.’s meta-analysis from 2005 suggests that there is no evidence these programs work. The recent evaluations of CEO are slightly more optimistic (Redcross, 2010) because they find evidence of substantial drops in recidivism for CEO participants who enroll at CEO within 3 months of exiting prison. However, these results also are puzzling because the recidivism gains seem to occur without the gains in employment that are thought to “cause” a decline in recidivism. A reasonable hypothesis is that the recidivism decline is not caused by employment per se, but rather by the extensive wrap-around services delivered by CEO through work crew supervisors and office-based support. This hypothesis is partially supported by first-year findings from the Transitional Jobs Reentry Demonstration (Redcross et al., 2010), where the transitional jobs more closely resemble traditional jobs with standard supervision. The evaluation of TJRD after 1 year found neither an increase in employment nor a decrease in recidivism.

The idea that transitional jobs provide a useful platform for the delivery of needed services that lower recidivism risk, even if employment is unaffected, is an intriguing idea that deserves further exploration. Ongoing evaluations will continue to explore this idea, and we encourage continued efforts to find ways to both increase employment and decrease recidivism in a causal way among this hard-to-serve population.

However, we also lay out some reasons why both policy makers and researchers should maintain healthy skepticism about the potential for these programs to have a large, causal impact on recidivism. As suggested by the ongoing evaluations, the mechanism linking changes in employment to declines in recidivism has become increasingly questioned. The reduction in recidivism is indirectly caused by an increase in employment. Therefore, even a sizeable 20% increase in employment will cause a small, indirect decrease in recidivism (Lattimore et al., 2010; Raphael and Weiman, 2007). More generally, this population has many needs, and many of these needs, like substance abuse and mental health concerns, might be antecedents to employment. As suggested by Edward Latessa (2011: 2), “work is much more than just getting someone a job—it involves how work is viewed, the satisfaction one derives from work, how one gets along with co-workers and supervisors, and other work related aspects linked to attitudes and skills.” Other work by Bucklen and Zajac (2009) has found that the major contributors to failure among recent parolees was not difficulty in
getting a job, but rather antisocial attitudes, unrealistic expectations, and poor coping and problem-solving skills. Employment programs that do not address these other problems will likely have little impact.

Yet the implementation of multidimensional programs is difficult, particularly in correctional settings. Service providers have the potential to be confused with “the system,” an outcome that is likely to discourage the positive attitudes needed for success (Buck, 2000). The problem is compounded by the ever growing use of criminal history record screening by employers (Pager, 2007; SEARCH, 2005). Individuals with criminal history records have become a marginalized class that faces heightened discrimination. Although some evidence exists that this screening is becoming more nuanced as a result of legal pressures linked to concerns about racial discrimination (Equal Employment Opportunity Commission, 1987; SEARCH, 2005), it is undoubtedly the case that individuals with criminal history records face more difficulty finding jobs now than they did even 10 years ago.

Several reviewers found our conclusions about the rehabilitation prospects for this population to be needlessly pessimistic. We believe we have been realistic about the available evidence; yet we also make no claim that we are providing a complete review of rehabilitation in general. We also strongly encourage continued rigorous experimental evaluation of carefully planned employment-based reentry programs, which are faithful to modern ideas about “what works” in correctional rehabilitation.

That being said, we believe that we have articulated an additional function for employment-based reentry programs, in addition to rehabilitation and recidivism reduction. Not all individuals with criminal history records are equally risky—and in fact, evidence is growing that a sizeable minority of this population desists soon after their latest conviction (Kurlychek et al., in press). The problem is that it is difficult for employers to reliably identify these individuals. One avenue to take toward this problem is actuarial, which would involve the use of observable characteristics to predict levels of risk, and to study employment outcomes for different types of individuals with criminal history records (Visher et al., 2011). Although clearly possible, this kind of exercise is limited, especially for a relatively homogenous population like returning ex-prisoners. The very characteristics (like motivation) that are likely to determine success in this respect are, by definition, hard to observe. As noted by employer John Shegerian, in a panel discussion about hiring individuals with criminal history records:

The other thing is, you can’t really tell what’s in a man or woman’s heart until they actually start working for you. You can use other indicators, but once they start working for you, you’ll know where they really want their future to be based on the effort that they give you. If it’s the typical 130% effort, which people who have a chip on their shoulder, who want to turn their life around, typically do, then they’re going to make it. Others who really don’t have an interest in turning their life around or really just showed up for the job.
because family members, their mother, loved ones pushed them into it, will self-terminate on their own. (2010: 7)

This problem is a classic example of information asymmetry. The necessary information for effective risk assessment is simply not available. Economists have proposed that potential employees in this situation can nevertheless emit signals that can be used to identify the substantial minority of people who will be the most productive workers. This article has presented the first formal argument that employment-based reentry programs can provide useful signals about crime desistance and work productivity for those who have recently been released from prison. Employers and other decision makers may be able to reliably identify individuals who have very low risk of recidivism, based on their successful completion of voluntary employment training programs. The key requirement of a successful signal is that the cost of acquiring it must be negatively correlated with the characteristic of interest—in this case, crime desistance or work productivity.

Our position represents a departure from the traditional view that employment and employment-based programs must help “cause” desistance in order to have a useful function during the reentry process. Our view is that a signaling perspective is a complement to, rather than a substitute for, this notion. Indeed, on the one hand, it is not problematic if employment training programs cause desistance; it is an added bonus if they do. On the other hand, it also is not necessary for these programs to have a rehabilitative impact for signaling to have value. Nonexperimental comparisons of program participants (graduates) to nonparticipants (dropouts), which routinely produce sizeable differences in employment and crime outcomes, are considered a major faux pas for causal analysis of the impact of work programs on employment and crime. We cannot overemphasize the point that signaling is not about cause and effect. It is about showing that people with the signal (program graduates) are different than those without the signal (program nongraduates), both now and in the future. This fact, which is what leads to selection bias in causal analysis, is also what generates value as a signal. Signaling therefore exploits the sorting and selection processes that are at work.

Limitations
This article builds on ideas that were mentioned briefly in two previous papers (Bushway and Reuter, 2004; Freeman, 2008) by explicitly introducing and developing the theory of signaling in the context of prisoner reentry.12 As such, this study represents a major advance over what was essentially speculation. However, although we believe that we have provided systematic evidence that employment programs meet the criteria for effective signals, we do not have systematic evidence that these programs are being used in this manner. In

12. Signaling in the face of asymmetric information is a well-developed idea in economics, and it has been recognized with the 2001 Nobel Prize, awarded to George Akerlof, Michael Spence, and Joseph Stiglitz.
the words of one anonymous reviewer, our treatment of desistance signaling has reached the level of “informed speculation.” It therefore remains the case that these ideas require additional development, both conceptually and empirically. We will present some ideas for how this development can proceed in the next section.

The boundaries of desistance signaling also deserve further exploration. As explained in the beginning of the article, we believe that signaling is different from risk assessment. On the one hand, risk assessment concerns the use of observable characteristics to predict future behavior, some of which are static and others of which are dynamic (i.e., changeable). Signaling, on the other hand, is focused on conveying information about unobservable characteristics by exploiting correlations between observable characteristics and the opportunity costs imposed by acquisition of the signal. In this context, once we have identified the relevant class of people, such as individuals with criminal history records, only information about the signal(s) is needed to make good decisions.

In a very real sense, signaling and risk assessment are related, but competing, perspectives. Risk assessments, although not costless, are cheaper than government-funded employment programs that exist simply to provide signals. Although we are convinced that the signaling model has both theoretical and empirical merit, especially in the employment context, we cannot say that justifying programs for no other reason than to provide signals is “better” from a cost/benefit perspective than using all available information to generate risk assessments.

Another potential concern with the idea of signaling is that it will redistribute who benefits—desisters “win” because they can be reliably identified, whereas nondesisters “lose” for the same reason—but it will not actually increase the overall benefit to society. In the standard signaling model, individuals in a world without screening get the “default,” unconditional value of their marginal product and as a result signaling leads to redistribution and no net benefit. But what if that default value is zero or negative because of the high costs related to the risk to hiring individuals who subsequently offend? The default condition for a group of people with serious criminal history records may be that no one gets hired and individuals exit the labor market (Apel and Sweeten, 2010). A more efficient signaling mechanism has the potential to put substantial numbers of these ex-prisoners back “on the grid” with respect to the labor market. In a world where only 30% of individuals exiting prison have pay stub jobs in any given year, even an increase of 10 percentage points in employment would be a substantial increase in overall welfare.

The fact that there is a net benefit to society still does not fully resolve the problem about what to do with the majority of individuals with criminal history records who have not in fact desisted. Although we are confident that a full embrace of the power of signals would increase the overall well-being to society, we do not have an effective solution to the

13. We thank Greg DeAngelo and Don Vitaliano for this observation.
problem of what to do about the nondesisters who are still embedded in crime, beyond continued efforts to explore ways to foster change. As discussed, rigorous evaluation and exploration of ways to encourage rehabilitation is in its infancy, and further investment is easy to justify.

The good news, however, is that the signaling equilibrium is dynamic, and the groups who benefit need not be fixed. Signaling implies dynamic feedback between signal sender and signal receiver, and the equilibrium conditions allow for change over time in the value of the signal. If offenders learn that program enrollment, participation, and completion are being used to identify instantaneous desisters, offenders who otherwise would not participate might now choose to do so. Conversely, if these desistance signals are ignored by correctional officials, the “good bets” who otherwise would acquire them might instead opt out.

Some empirical justification for this claim exists. For example, Kuziemko (2007) found that program participation in prison dropped after determinate sentencing was adopted. This finding suggests that program participation was being used as a signal by the parole board, and some offenders were only participating to impress the board. Yet if the programs are indeed beneficial from an evaluation standpoint (i.e., if they cause desistance), then added incentives to participate are hardly a bad thing. Kuziemko (2007) also showed an increase in recidivism after this change—indirect evidence that the programs did have a positive impact on behavior over and above their value as signals.

This discussion highlights another potential limitation with our development of desistance signaling. We argue that signaling complements rehabilitation. Yet signals must be voluntary to be of any use as such. And, indeed, many transitional employment programs are voluntary. At the same time, there is empirical evidence to support “mandatory” rehabilitation programs such as Project HOPE, where individuals are forced to participate as a requirement of their supervision (Kleiman, 2009). By definition, these programs will not be effective signals (see also Tyler et al., 2000). Not all rehabilitation programs will provide valuable signals, and the signaling value of certain programs might actually be undermined by forcing compliance.

Research Implications
Research needs to develop along four separate tracks. First, formal models of the information asymmetry problem in the context of prisoner reentry must be developed. A formal model will be able to accommodate the unique features of the problem for individuals with criminal history records, and it can then be parameterized to provide concrete insight into the limits and potential of desistance signaling. Key factors include the returns to employers, the size of the “productive” group, and the costs (and benefits) of the signal to both individuals and society.

More attention also needs to be devoted to clarifying the parallels between crime desistance and work productivity. As used in this article, as a simplifying assumption,
they are essentially the same thing—desisters are productive workers, and nondesisters are unproductive workers. Although some justification for this description exists, it does not necessarily reflect reality. Research should focus on the degree to which these two concepts are interrelated. The weaker the correlation between crime desistance and work productivity, the greater the need to specify more clearly the relevant signals that can be of use to different stakeholders.

Second, empirical testing of the signaling power of program completion needs to be conducted. The classic way to test these ideas is to find people who are otherwise similar in terms of skills, but who are different only in terms of their acquisition of the signal (see Tyler et al., 2000). It is somewhat problematic in labor economics, where signals like a GED and high-school diploma contain at least some human capital value. It might be less problematic in the context of criminal history records, particularly if the finding that transitional work programs have no pronounced causal impact on labor market outcomes continues to hold in future evaluations. In this environment, the task will be to compare labor market outcomes for members of the control group who have similar attributes to program graduates but who did not have access to the “program completion signal.”

Third, so-called certificates of rehabilitation, which are created explicitly as signals, also might be worthwhile areas of research. Individuals who qualify for certificates but have not applied for them for exogenous reasons might be compared with those who do have the certificates. Or outcomes for those with certificates, before and after the awarding of the certificate, also might be compared. Other important empirical efforts could involve more detailed surveys of employer attitudes toward program completion, among the subset of employers who engage with individuals with criminal history records. More explicit consideration of the signaling function within the job development community also might be beneficial.14

Finally, elaboration of the signaling framework beyond employment programming might yield important new insights. Several anonymous reviewers of this article immediately jumped to possible extensions. We were most comfortable with the idea within the context of employment, given signaling theory’s origins within labor economics. But it is nonetheless true that the signaling model is quite general, has broad potential, and is transferable to any setting in which there is information asymmetry between two or more parties. For

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14. “Signaling” is sometimes confused with “creaming” among treatment providers. Creaming is a pejorative term in this context that implies that the provider is only providing services to the best and easiest-to-serve group. The notion of creaming, and the hostility it sometimes generates, makes a productive discussion about signaling as a positive function of reentry programming difficult to manage. We believe the hostility emanates from the idea that one intends to help some but not others. But what if, as the experimental evidence suggests, no one is actually being helped directly by the program? Instead, the value added comes from the ability of the program to distinguish, in a constructive way that does not discriminate on the basis of past actions or stable characteristics, between those who have changed and those who have not. The fundamental assertion of this article is that this information is valuable.
example, possible extensions of the model that incorporate other signals that have relevance for the reentry process include educational certification and substance abuse treatment, among other forms of correctional programming. Similar to the graduates of employment programs, graduates of these types of programs are likely to differ from their counterparts in ways that reveal information about risks to the community or the workplace. Additionally, although we focused on community-based employment programs, the signaling model can possibly be extended even further to incorporate prison-based employment programs that, when coupled with the signals acquired from participation in community-based programs, can jointly serve as a powerful signal about underlying risk.

**Policy Recommendations**

The assertion in this article is that not every reentry program needs to explicitly cause change in order to be cost effective. Some programs in the criminal justice system may be beneficial simply because they provide information about participants that is otherwise unavailable. “Old-fashioned” parole was based on this idea, albeit it in a clinical and not in a particularly systematic way. Concern about the arbitrary nature of parole decisions was one reason behind the determinate sentencing movement (Travis, 2005). But the appeal underlying the problem of information asymmetry and the value of signaling is that allowing people to sort themselves, through their own actions, need not be arbitrary or clinical, particularly if opportunities are universally available and are not limited by class or race.

Such an exercise also need not fall within the confines of risk prediction, which is inevitably driven by a few static factors like age, gender, and criminal history, which are not changeable. Although these factors define the problem, they are not grounded in the reality, highlighted by John Laub (2006), that change is possible, even among the “riskiest” groups of offenders. Acquiring unobservable information about who has changed, even if we cannot directly manipulate that change, is a valuable government function and it is worthy of further investment. A correctional system in which individuals spend a large amount of time incarcerated, without learning more about their potential after release, seems wasteful. A careful reconsideration of how program enrollment, participation, and completion can generate useful and actionable information about crime desistance and work productivity is an important task for criminal justice policy makers and other stakeholders.

The idea of signaling also might be a useful concept for policy makers currently struggling to make intelligent decisions about the employment eligibility of individuals with criminal history records. Currently, statutory bans on such things as employment focus almost exclusively on the nature of the offense and, to a lesser extent, on the time since the last offense. Such a decision framework throws away any and all information that can be learned from the actions of the individual since the time of the last offense. Yet, although we can acknowledge that individuals who committed a certain offense have a certain level of risk, we also can acknowledge that this is not a homogenous group. Concrete actions by individuals to fulfill the obligations of a sentence, obtain employment, pay taxes,
and “graduate” from reentry programs provides real information about their current (and prospective) level of risk. On the one hand, employers and others may simply not have enough information to differentiate between individuals with criminal history records. On the other hand, government policy makers may have this information—but it needs to be organized in such a way that employers can use it.

Policies such as certificates of rehabilitation, like those offered by New York State, have the potential to improve life outcomes drastically for a growing class of individuals at little cost to society. The fact that such certificates exist is encouraging; the lack of systematic research supporting their development is not. This article supports a serious, concerted effort to better understand and expand on these programs, if and when empirical evidence confirms their potential signaling value.

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