Observational and Experimental Evidence of Imbalance in Destigmatization

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ABSTRACT

Stigma has broad reach – it directly affects people who are targeted, and harms even those who are merely associated with stigmatized individuals. But stigma also can change. Some groups now have better standing than in years past (e.g., HIV patients, disabled persons, gays and lesbians). After destigmatization, those who were directly stigmatized ought to no longer experience disadvantages – however, the consequences for the stigmatized by association are less obvious. We suggest that the changes in perceptions linked to destigmatization are imbalanced such that mere associates of stigmatized individuals will continue to experience the damage done by stigma. We find evidence of imbalance using data from observational and experimental studies. The observational study examines careers of film artists during and after the “Red Scare” in Hollywood. We find that in the post-blacklist period (compared to the blacklist period), blacklisted artists were more likely to work than their pre-stigma coworkers (the “mere associates”). The experimental study shows that being gay (vs. straight) had a stronger negative effect on current liking of a man’s randomly assigned college roommate than it did on the gay man himself. This suggests that the stigma associated with homosexuality has reduced while the stigma of associating with a gay person has not.
Social stigmas represent what is evil and immoral in a society. But what is stigmatized can change. Consider HIV patients, persons with disability, racial minorities, or gays and lesbians. Once ostracized, people in these social categories are no longer seen as deviant. At the same time, new stigmas have emerged, for example toward people who have trouble disposing of unwanted goods – labeled “hoarders” – or those who retain prejudice – labeled as “racists” or “bigots.”

Stigma is constructed through sociological and psychological processes. Identifying stigmatized individuals establishes society’s moral boundaries, drawing stark lines between “normals” and “deviants” (Goffman, 1963; Becker, 1963). The tainted group is seen as categorically different, and its perceived distinctiveness drives prejudicial beliefs (Fine, 2012). Stigma is also characterized by widespread suspicion of guilt (Adut, 2008) and transmits through thin social ties (Pontikes, Negro and Rao, 2010). In contrast to the perception that stigma is distinctive, indirect effects of stigma are broad and unfocused, typically encompassing a wide swath of people. This is a result of psychological reactions to stigma, which are exhibited through both explicit and implicit processes (Pryor, Reeder, Yeadon, and Hesson-McInnis, 2004; Pryor, Reeder, and Monroe, 2012). People instinctively avoid and devalue stigmatized individuals as well as anyone who is geographically or socially proximate to a stigmatized person (Link and Phelan, 2001; Hebl and Mannix, 2003).

Because social perceptions change, a critical question is how destigmatization unfolds. What happens after people stop believing in witches, when gay relationships become mainstream, or when the scare of cold war is over? When stigma is lifted, individuals who were directly stigmatized ought to experience fewer social disadvantages. But the consequences for those merely associated with stigma are less obvious.
During destigmatization a once-stigmatized group is rehabilitated such that they ought to no longer be viewed as tainted or sub-human. This develops through individual and collective processes. Collective processes can be formal, such as new laws or business policies that prohibit discrimination, or informal, such as activist groups or high-profile individuals promoting public acceptance (Soule, 2012). Indeed, hearing others’ changed views influences people to change their individual prejudicial attitudes (Zitek and Hebl, 2007). During destigmatization, people are made aware of unfair treatment and reminded that members of the stigmatized group are human. This may prompt them to override their aversion to individuals who were directly stigmatized.

It is not clear that the same process applies for those carrying stigma by association. Reactions to stigma, like stereotypes in general, involve both explicit and implicit processes (Galinsky, Mantora, and Ku, 2003; Pryor et al., 2004). Implicit processes are dominant when stigma is transferred through thin social ties, or by mere association (Pryor et al., 2012). This dual-process model of stigma transfer may have implications for destigmatization.

We suggest that destigmatization is imbalanced. It occurs through effortful cognition, and as a result it will transfer less readily to those associated with the stigmatized, as compared to those directly stigmatized. Ironically, associates will continue to experience damage – while those who were directly stigmatized will not. This means that the social effects of stigma are not entirely reversible. The discrete category of targeted individuals can recover, but the less identifiable group of mere associates will experience ongoing adverse effects.

We find evidence of such an imbalance in destigmatization using data from observational and experimental studies. The observational study investigates careers of film artists during and after the “Red Scare” in Hollywood. Using a combination of difference-in-differences estimators and pre-treatment matching, we find that blacklisted artists were more likely to work in the post-
blacklist period compared to coworkers of the blacklisted. The experimental study shows that being gay, compared to being straight, has a more-negative effect on a man’s randomly assigned college roommate than it does on the gay man himself, again suggesting an imbalance in benefits destigmatization. This discrepancy can be accounted for as a result of reliance on implicit as well deliberative sociocognitive processing.

**Direct and Associative Stigma**

Goffman (1963) described stigma as a mark that indicates a person is flawed, spoiled, and inferior. A stigmatized individual is reduced from “a whole and usual person to a tainted, discounted one” (p. 3). He or she is considered unfit for interaction and is excluded from social and economic life. Goffman identified three types of stigma marks: deformations of the body, tribal attributes or beliefs, and flaws in personal character. Bearing a mark initiates an attributional process through which other aspects of the individual are devalued (Jones et al., 1984), which leads to a perception that the essence of the person is tainted.

Recent studies emphasize that stigma develops as a social construction that identifies a distinguishing characteristic of a group regarded as a basis for devaluation (Dovidio, Major and Crocker, 2000; Leary and Schreindorfer, 1998). A person becomes stigmatized when a transgression is made public, or becomes “common knowledge” (Adut, 2008). Interactions with stigmatized individuals activate negative stereotypes and interpersonal rejection, leading to social discrimination and economic disadvantage (Becker, 1963). As such, stigma is a broad construct that encompasses prejudice, stereotyping, discrimination, and social rejection (Hebl and Dovidio, 2005).

Evolutionary accounts have been proposed to explain the origins of stigma. Most notably,
there are survival advantages to staying away from carriers of communicable pathogens. In addition, there may be benefits for people who avoided poor and unpredictable partners in dyadic exchanges, or for fostering coalitions excluding people who might not succeed in group competition (Kurzban and Leary, 2001). These adaptive processes are mediated by social factors, particularly language and culture. Designating the markers of stigma through labels and definitions is critical for stigma to have widespread social effects.

The harmful effects of stigma are not limited to marked individuals. Psychological and sociological research shows that stigma spreads beyond the bearers of the negative designation, what Goffman referred to as courtesy stigma. This is now also described as associative stigma or stigma by association (for a review, see Pryor et al., 2012). Associative stigma defines a process by which someone suffers negative consequences by virtue of association with one or more stigmatized others. Stigma by association can occur through meaningful relationships, such as family ties, but also through simple connections of mere spatial-temporal co-occurrence, such as being seen in the presence of an obese person (Hebl and Mannix, 2003; Penny and Haddock, 2007), or having worked on a project with someone who later is stigmatized (Pontikes et al., 2010).

Two processes control how direct stigma and associative stigma operate (Pryor et al., 2004). The first process involves reflexive reactions, whereby people have instinctive and spontaneous negative affective responses. The second involves more deliberate and controllable reactions. Both types of reactions result in a tendency to devalue and avoid the stigmatized and their associates. Automatic reactions are activated first and deliberate reactions follow, but deliberate reactions do not necessarily override automatic ones.

The primary and secondary reactions are distinct: both can operate simultaneously, and if
one is removed the other can persist. In one experiment, (Study 2 in Pryor et al., 2012) subjects were shown pictures of a man and an overweight woman who were either said to be relatives (meaningful relationship condition) or to have happened to be photographed together (non-meaningful relationship condition). In both conditions, effects of stigma by association were evident, such that the man was denigrated as a consequence of being pictured with an overweight woman. However, *implicit* weight-related attitudes moderated effects in both conditions, whereas *explicit* attitudes moderated effects only for meaningful relationships. This suggests that the spread of stigma through deliberative thinking occurs when subjects assume there is a meaningful relationship between the stigmatized individual and the associate. But stigma spreads through reflexive processes when two individuals are proximate – whether or not they have a meaningful relationship. Timed studies of stigma response show that distancing as a result of implicit attitudes is immediate, and the strengthening or dissipation of stigma avoidance due to explicit attitudes is observed later (Pryor et al., 2004).

These studies support the view that stigma by association occurs as a result of both reflexive and deliberate processes. The reflexive process is immediate, uncontrollable, and often driven by emotion or affect. Avoidance may be adjusted after people take time to consider their response, but deliberate processes do not always counteract reflexive reactions. This is seen in studies of stereotype suppression: Subjects who suppress stereotypes explicitly rated stereotyped targets as more similar to them, but also physically distanced themselves from the stereotyped individuals (Galinsky, Martona, and Ku, 2003). Reflexive responses are powerful and sometimes persist even in the face of deliberate, effortful thinking. At the same time, people can and do change their stereotypes.
**Destigmatization**

One key aspect of stigma is that it evolves with norms and values (Hebl and Dovidio 2005). What is considered stigmatizing varies over time and place (Jones et al., 1984); many groups that were once stigmatized have now been enfolded into the mainstream. The stigma against people accused of denying religious beliefs – called “heretics,” “pagans,” or “witches” – was strong centuries ago, but no longer exists today in Western societies. Stigma around race and ethnicity continues to fade. The stigma surrounding homosexuality also has receded, especially in recent years. Indeed, people who hold negative stereotypes of previously marginalized groups such as these may now be themselves stigmatized, as “racists,” “homophobes,” or “bigots.”

Destigmatization refers to the process in which social stigma is lifted. During destigmatization, people who were once discredited are rehabilitated in the public eye. Warren (1980) suggests there are three major modes of destigmatization: professional treatment, individual self-help, and political activism. The first two modes concentrate on destigmatization of the individual; the third on transforming the consensus view of the stigmatized group. The third mode is linked to what we call historical destigmatization. Fine (2001, 2012) argues that repairing negative reputations requires modifying accepted norms. Destigmatization results from significant “investments” made by outsiders, involving the mobilization of group interests, expert evaluation, and material and political resources, in reinterpreting facts, disseminating knowledge, and fostering contacts with the stigmatized.

This means that changes in social perceptions about the stigmatized ultimately depend on the support of outsiders. This is corroborated by psychological research that finds prejudicial attitudes are strongly influenced by peers. For example, when a confederate expressed opinions condoning or condemning racism, subjects followed suit (Blanchard, Crandall, Bingham, and
Vaughn, 1991). This effect depends on the greater social norm; a later study showed that people could be influenced to have less-prejudiced, but not more-prejudiced, attitudes toward Black people, which was attributed to a strong anti-prejudice norm for this group (Monteith, Deneen and Tooman, 1996). Zitek and Hebl (2007) found that in general, social influence effects depend on the clarity of the social norm either for or against prejudice for a particular group. The more ambiguous the norm, the more that social influence can sway attitudes. Conforming to perceived norms is especially evident in public, as compared to private, expressions of prejudice (Shapiro and Neuberg, 2008).

This research suggests that destigmatization occurs through a path-dependent process, where community mobilization and legislation lead to public expressions condemning prejudice, which then influences behaviors and beliefs toward people who were directly stigmatized. In the domain of public health, Phelan et al. (2000) note that the understanding of mental illness has broadened and there is more direct evidence for changes towards more positive public beliefs and attitudes.

**Direct and associative destigmatization.**

It is less clear whether the above analysis of destigmatization applies for those stigmatized by association. Because of the dual nature of stigmatization, which encompasses reflexive in addition to deliberate processes, one can expect that with destigmatization, associates may continue to experience more negative consequences as compared to those directly stigmatized. The compliance with norms against prejudice relies on deliberate cognition, which can lead people to revise or counterbalance initial negative judgments of stigmatized individuals. But to the extent that stigma is a result of reflexive processes, the correction may not follow.
This will especially be the case when the stigmatized and associates are connected by arbitrary, non-meaningful relationships (Pryor et al. 2012). The attempt to remove a stigma is the product of an organized process that increases knowledge and awareness about the stereotypes underlying the undeserved devaluation and rejection. In response to this, people may revise their attitudes toward the stigmatized and perhaps toward people connected to them by meaningful relationships. But they will unlikely revise their attitudes towards mere associates.

We think this happens for two reasons. First, people need to exert significant effort to realize how broadly stigma has transferred and to correct for their initial aversion. Second, during the stigmatization period, both the directly stigmatized and mere associates are systematically excluded, and as a result have less social and economic success (e.g. less employment). With destigmatization, people will attribute poor performance for the directly stigmatized to the now-discounted stigma. Mere associates will not benefit from this correction, and poor performance may be attributed to inferior individual quality. Here, stigma by association indirectly leads to imbalance. For mere associates, the implicit negative reactions will persist, and they will continue to experience the negative effects of stigma.

Two contextual conditions may also contribute to imbalances in destigmatization. First, reform efforts directly target the stigmatized, not their associates. For example, the congressional bill known as the Repeal Existing Policies that Encourage and Allow Legal (REPEAL) HIV Discrimination Act (H.R. 1843/S. 1790) addresses the problem of discrimination in the use of criminal and civil commitment laws against those who test positive for HIV. The stigmatized are naturally viewed as the principal beneficiaries of the efforts aimed to restore their social standing. New rules and norms prevent direct discrimination of the stigmatized, but do not directly protect associates.
Second, although many studies show that stigma transfers through association, there is no evidence that the reverse occurs. Neuberg et al. (1994) find stigma by association when a straight man is friends with a gay man, but no evidence of destigmatization for a gay man who is friends with a straight (vs. gay) man. Penny and Haddock (2007) find that a thin woman is liked less when shown with heavy women, but the opposite pattern applies for heavy women: they are liked more when they are pictured with other heavy women. Thus, destigmatization may not spread; if anything, people who are stigmatized are further denigrated when in the company of non-stigmatized individuals. This suggests that when people reduce prejudice against those who were directly stigmatized, destigmatization will not readily transfer to mere associates. These observations lead us to predict that with destigmatization, the assessments of the stigmatized will improve more than the assessments of those experiencing stigma by mere association.

**Empirical Tests**

We investigate imbalance in destigmatization in observational and experimental studies that take advantage of conditions where a stigma is rehabilitated. Using both experimental and observational data increases the internal and external validity of our claims and allows us to explore the mechanisms underlying the phenomenon. Using two different types of stigma helps establish the generalizability of the process. The first study uses observational data of film careers during and after the “Red Scare,” a prominent sigma during the mid-twentieth century where film artists that were suspected Communists were blacklisted. The Red Scare ended in 1958, when social perceptions of the legitimacy of the blacklist rapidly changed. This study provides evidence of real social effects surrounding destigmatization imbalance.

The subsequent studies are experiments that take advantage of current changes to the
stigma of homosexuality, which has dramatically receded in recent years. The heterogeneity in attitudes towards gays also raises the question whether the gay stigma continues to transfer to mere associates, and likewise whether the process of destigmatization extends to associates who acquired the stigma only by being linked to stigmatized persons. In this respect, the gay stigma today can be compared to the blacklist stigma in Hollywood in the late 1950s. People notice the change, and know that norms used to be different, but the stigmatized group maintains a distinct social identity. The first experiment shows evidence of imbalance in destigmatization, and indicates that imbalance occurs as a direct result of stigma transfer. Subsequent studies explore the mechanisms that underlie the imbalance.

**Study 1 – Film Careers in Hollywood during and after the Red Scare**

**Method**

*The Red Scare.* Between 1950 and 1957, artists suspected of political affiliations with the Communist Party and other Communist front organizations were publicly viewed as subversives (Ceplair and Englund, 1980). In 1947 an investigative committee of the United States House of Representatives, the House Committee on un-American Activities (HUAC), subpoenaed 43 Hollywood personalities. Ten refused to cooperate with the committee and were sentenced to a year in prison (Cogley, 1956). The HUAC conducted dozens of hearings in which individual witnesses were asked to testify about their own activities and also to inform on others who might have participated in Communist organizations and events. Statements from these hearings were published in government reports and received wide coverage in the media. Appearing in the annual HUAC reports or being listed in publications denouncing Communists distributed to industry executives – particularly the booklet *Red Channels*, published by the magazine
Counterattack in 1950 – made artists “controversial.” These listings operated as blacklists.

Film studios suspended the 10 jailed artists in 1950, and sought to avoid anyone with alleged Communist affiliations or sympathies. Pontikes et al. (2010) document that the fear of Communism in Hollywood turned into a moral panic, and mere associates of the blacklisted artists also suffered in terms of their employment prospects. Specifically, artists who had worked in the same film projects with other artists who were subsequently blacklisted had, on average, a 13 percent lower chance of working, compared to artists who had not worked on the same projects as blacklisted artists.

In the analysis that follows, we define as “blacklisted” the artists named in the hearings or listed in Red Channels, and as “associates” the artists who (1) were not blacklisted themselves and (2) who worked in one or more of films with a blacklisted artist before she or he was blacklisted (Pontikes et al., 2010). Our data (described below) indicate that the correlation between having documented (but alleged) Communist affiliations and being blacklisted was 0.63, significantly positive, whereas the correlation between having Communist affiliations and being an associate is 0.01. This suggests that many artists who suffered stigma by association had little reason to be avoided.

Destigmatization. Over time, public hostility toward the blacklisted artists began to wane. Senator Joseph McCarthy, not a member of HUAC but widely recognized as the strongest supporter and enforcer of political repression against Communists, died in 1957. The Senate had voted to censure McCarthy in December 1954, but it was after 1957 that the HUAC lost significant credibility (Smith, 1989). In 1958 the government had stopped defending the blacklist. We set to this year the time of destigmatization, in which blacklisted artists ceased to
be viewed as “un-American.” In the analysis, we expected that, after the end of the Red Scare, artists directly blacklisted would experience improved rates of obtaining work, as compared to mere associates. Associates also suffered from the effects of the blacklist, but we do not expect their careers to rebound.

In early 1959, former President Harry S. Truman denounced HUAC as the “most un-American thing in the country today,” and congressmen called for its abolition (Whitfield, 1996). And in 1960, Dalton Trumbo, a blacklisted writer, received full screen credit for two films, *Exodus* and *Spartacus* (written about one year before their release) (Ceplair and Englund, 1980; Smith, 1989).

**Data.** We identify the blacklisted artists, associates, and their employment in film using multiple data sources. The list of blacklisted artists (267 individuals) comprises the witnesses and targets named in the congressional hearings and reported in the Committee’s annual reports for 1951-56 (U.S. Congress HUAC, 1951-55), and the names included in *Red Channels* (American Business Consultants, 1950). The list of associates (N = 10,960) includes artists who worked in the same films as the blacklisted, but to limit self-selection bias, we exclude 1) artists who were themselves already blacklisted, and 2) artists in films with artists who had already been blacklisted. Following Pontikes et al. (2010), an artist A is an associate of a blacklisted artist B at time t if A and B had worked in (at least) one film together until time t_{i-1} and B is blacklisted at time t_i.

To measure employment in film and artists who worked together, we use data in the entries compiled by the American Film Institute Catalog of Motion Pictures (hereafter, “AFI”), which report cast members, production and distribution companies, release date, length, and

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1 Because there is some debate about the exact timing of the end of the blacklist, we also include
genre for all motion pictures produced and released in the United States. Our analysis focuses on the four main creative roles in film: actor, director, writer, and producer (Baker and Faulkner, 1991). We exclude non-commercial films and shorts. We supplement the AFI entries with data on Academy Award nominations for individual artists (Shale, 1993).

**Design.** The analysis compares work in film in the destigmatization period, from 1958 onwards, with work during the stigma period, from 1950 to 1957. We observe outcomes for individual artists from two groups in two periods. One group, the blacklisted, was directly affected by changes in social perceptions after the end of the blacklist; we expect that the other group, the associates, was not. We measure outcome variables and covariates for the same set of artists in the two periods. We estimate the changes in work using a difference-in-differences (DD) estimator, which subtracts the average change in work of the associates from the average change for the blacklisted. This estimation approach can be used to analyze data with time and cohort dimensions, and it removes biases due to both (1) unobserved but fixed omitted variables for individual artists and (2) biases from comparisons between the two groups that could be the result of temporal trends.

The DD approach relies on a counterfactual framework that compares two groups: a “treatment” group and a “comparison” group. In our case, having been blacklisted is the treatment, and having a mere association with a blacklisted artist – or an artist who worked with someone who was subsequently blacklisted – is the comparison group. The DD models compare the average change of an artist’s employment in film in two time periods: the stigma period (the duration of the blacklist) and the destigmatization period (after the blacklist). We implement the reduced form regression of DD, which facilitates the construction of DD estimates and standard errors as well as tests of the identification assumptions (Card, 1992).
Given two groups, \( D = 1 \) for the blacklisted artists and \( D = 0 \) for the associate artists; two periods, \( T = 0 \) for years 1950-57 and \( T = 1 \) for years 1958-61; and the potential outcomes \( Y_{i(t)} \), where \( Y_{i(1)}(t) \) is the potential outcome a blacklisted artist \( i \) attains in period \( t \), and \( Y_{i(0)}(t) \) is the potential outcome an associate artist \( i \) attains in period \( t \), the estimator can be written as:

\[
\Delta Y_i = \delta + \alpha \cdot D_i + u_i,
\]

where \( \Delta Y_i = Y_{i(1)} - Y_{i(0)} \), and \( u_i = \Delta \varepsilon_i \).

We examine differences for three outcomes: the average number of films an artist made during the time period, the average number of years an artist worked in at least one film, and, for actors, the average rank from 1 to \( n \) in the screen credits of the films made.\(^2\) The first two analyses include all artists and the third is limited to artists who were only actors. For an actor, the rank indicates the status of his or her individual performance in the film projects in which he or she worked. Actors listed first in screen credits are more prominent than those listed later, so the lower the rank, the higher the status. We expect the coefficient for blacklisted \( \alpha \) to be positive for the first two outcomes, and negative for the third outcome.

The standard DD estimator uses time-series data for treatment and comparison groups, and controls for individual fixed effects and time-related trends. However, this estimator remains subject to several validity threats. Here we discuss one main limitation and how we modified our research design to address it. The remaining issues are discussed in a methodological note in the appendix.

One important validity threat for DD stems from functional form dependence, the fact that the magnitude, or even the sign, of the DD effect may be sensitive to the average outcomes

\(^2\) For actors in more than one film in a year, we use the median rank for the year.
for “treated” and comparison units being very different at baseline. To minimize this concern, we apply a matching method that balances the distribution of covariates and makes the two groups being compared more similar. We implement “coarsened exact matching” (CEM), a nonparametric method that reduces data covariate imbalance and increases the comparability of the units in the sample (Iacus, King and Porro, 2012). We estimate the DD regression in the matched dataset.

**Matching.** The start of the blacklist period is 1950. Therefore we implement the matching process using values of the covariates measured at the end of 1949. From the group of 267 blacklisted artists in the data, we keep 257 who had at least one film role after 1945 to avoid including inactive artists. Of these, 7 artists are excluded because they worked in their first feature film after 1957. Of the remaining 250, we retain 216 who worked in one of the four main roles that we focus on (actor, director, producer, and writer). We wish to compare artists alive at the end of the post-blacklist period, and so we exclude artists who died by the end of 1962. Death dates for the blacklisted artist were determined from online and print resources. We also create mutually exclusive groups and so exclude those who entered the data as associates and became blacklisted (the associate group is defined in such a way that prevents the transition from blacklisted to associate). We implement CEM on the remaining set of 129 artists. The reduction from 216 to 129 is substantial, but our primary concern is to enhance internal validity by limiting the bias from self-selection into the two groups.

To implement CEM, we match on covariates that are related to the group assignment and the outcome variable. We select covariates based on previous research: gender, professional roles, tenure in the industry, previous work in the more common film genres, and prominence from artistic awards (Baker and Faulkner, 1991; Bielby and Bielby, 1999; Zuckerman et al.,
2003). Table 1 provides more details about these covariates.

[Insert Table 1 about here]

To improve the covariate balance, we identified cut-points for each covariate. The CEM procedure matches units in the two groups that are within the cut-points for every covariate, and so ensures that matched units have similar values. Implementing CEM finds matches for 121 of the 129 blacklisted artists in the final set. From the initial set of 11,960 associates, CEM searched for matches among artists who worked in at least one feature film after 1945 in one of the four roles that we examine, and with available biographical information culled from imdb.com.

[Insert Table 2 about here]

The final number of associates matched to blacklisted artists is 3,096. We conduct the main analyses on 3,127 observations, 121 blacklisted and 3,096 associates. For the rank analysis (using actors only), the number of observations is 2,492. To substantiate the quality of the matching process, Table 2 presents the summary statistics for the two groups of the covariates used in the matching procedure before implementing the matching (Table 2a) and after the matching (Table 2b). Visual inspection indicates that the distance between the two groups for the covariates decreases after the matching. T-tests comparing the means of each covariate before the matching indicate statistically significant differences in all cases except one, while the differences are not statistically significant after the matching.

The matching process ought to also achieve balance between the units for unobservables. To examine whether the estimates remain sensitive to bias from unobservable confounders we inspect the summary statistics for covariates not included in the CEM. In the lower part of Table 2a and 2b we report the post-matching summary statistics and t-test of the mean differences of two additional covariates, a dichotomous indicator of having worked for a major film studio and
the concentration index of film jobs across all genres. Although they were not used in implementing CEM, the data ranges and t-tests for these covariates suggest that the matched blacklisted and associate artists are similar.

**Results**

We first inspect employment trends graphically. Figure 1 depicts the average number of yearly films for the blacklisted artists, associates, and others. The vertical dashed lines indicate the start and end of the blacklist period as measured in our analyses; the blue line measures the average number of films for the blacklisted group and the red line for the associates, and the green line for others (not blacklisted nor associates). The graph shows that employment prospects for both groups decreased during the stigma period, but rebounded only for blacklisted in the post-stigma period, in line with our hypothesis.

[Insert Figure 1 about here]

Next we test our hypothesis using difference-in-differences regression. Table 3 presents the main estimates of the DD regression on the matched samples of blacklisted artists and associates. The first outcome variable we consider is the difference between the average number of films in which the artist worked during the period post-blacklist (1958-61) and the period during the blacklist (1950-57). The estimates are obtained using ordinary least squares. The regression model is estimated including as the covariate the “treatment,” a dichotomous variable equal to 1 for blacklisted artists, and 0 otherwise. This way, associate artists are the omitted category. This is equivalent to testing an interaction term between blacklisted and the post-period in a regression that includes levels of the outcome variable in the two periods, rather than the difference, as the dependent variable. Using first differences at the individual level also controls for fixed effects, observed and unobserved, including stable skills or alleged affiliations with
Communism, which typically do not vary with time after the start of the blacklist.

[Insert Table 3 about here]

In Model 1 the coefficient of blacklisted is positive and statistically significant. In the period between 1958 and 1961 an artist who had been blacklisted during the earlier part of the 1950s worked in more films per year than a comparable artist who was merely associated with blacklisted artists. In case the difference in number of films may be sensitive to a few outlier cases, and Model 2 estimates replaces the outcome with the difference in the dichotomous indicator of working in film, and blacklisted shows a positive and significant coefficient also.

In Model 3 we present estimates using the difference in the median annual rank obtained by the artist in the screen credits for the films in which s/he worked in the two periods. Lower ranks indicate more significant roles in the films and suggest greater prominence of the actor. With this outcome we can explore whether the blacklisted artists gained or lost professional status. We find that the blacklisted coefficient is positive but not statistically significant. Blacklisted artists work more in the post-stigma period, but there is no evidence that they also regain more status.

We designed the estimation approach, which aggregates yearly observations within each period into an average value and include units surviving in the second period, to reduce bias due to serial correlation and attrition. The use of matched data also helps makes the two groups being compared more similar. In the appendix, we test the robustness of the estimates to the other internal validity threats mentioned above.

**Discussion**

Study 1 provides evidence of imbalance in destigmatization in a labor market setting.
These results show evidence of imbalance, which could result because mere associates continue to suffer from courtesy stigma, while people revise their stigma for those directly targeted. It could also result indirectly, where people attribute unemployment in the stigma period to individual quality for associates, and to the stigma for those directly targeted. To identify the effect, in our second study, we design an experiment where participants do not have information on performance during the stigma period. In addition, despite the bias correction approach just described, the observational data present limitations. We highlight three: the non-random assignment into the treatment condition; the threat to external validity because the stigma of the Hollywood blacklist might represent a unique institutional and geographic setting; the lack of evidence about the causal mechanism underlying the reported effects. The next set of studies use experiments that do not suffer from these limitations, and that can explore the causal mechanisms of the effect.

**Study 2 – Likeability of Men whose College Roommates Were Gay versus Straight**

In the experimental studies we consider the stigma of homosexuality, which has undergone noticeable change over time. Historically, gay men and lesbian women have been stigmatized in the United States and around the world, experiencing personal and social rejection, and discrimination in access to everything from health care to housing (Herek, 1992). In the labor market, an audit study of hiring discrimination towards gay men found that the probability of a gay applicant receiving a callback from an employer for a job was 3.7 percent lower than for an equally qualified heterosexual applicant (Tilcsik, 2011). For simplicity, in the rest of the paper we refer to both gay men and lesbians as gay, as attitudes towards lesbians closely resemble attitudes toward gay men (Herek and Capitanio, 1996).
Neuberg et al. (1994) found stigma-by-association effects in the evaluation of individuals linked to gay men, such that participants rated a straight man as less likeable when he was paired with a gay rather than a straight person. This study not only shows evidence of stigma by association with gays, but also rejects the competing hypothesis of transfer of positive traits to the gay man from his association with the heterosexual partner (i.e., destigmatization).³

In recent years, attitudes towards gays have become more positive, pointing to a process of destigmatization. This shift may be traced to several factors (Bartos, Berger and Hegarty, 2014). First, more gays have disclosed their sexual orientation, which can lead to greater liking among heterosexuals with whom they have social contact (Herek and Capitanio, 1996). Public and media education also have effectively reduced some sexual prejudice. Finally, social activism and gay people’s entry into mainstream industries have encouraged policy intervention, which has resulted in more than 200 U.S. counties introducing local laws banning discrimination based on sexual orientation from public and/or private employment, credit, and housing (Negro, Perretti, and Carroll, 2013). Thirty-six U.S. states now allow gay couples to marry legally.

Yet although changes in norms, recent policy initiatives, and court decisions have reduced stigmatization against gays, both legal protections and social acceptance are far from comprehensive. Tilcsik (2011), for instance, finds that discrimination of gay job applicants has noticeable geographical variation. In some situations, and perhaps implicitly, people may still hold negative attitudes toward this group (Doan, Loehr, and Miller, 2014).

The goal of this experiment was to test our hypothesis that destigmatization of gays will unevenly affect the assessment of a gay man and his associate. As described above, we expected

³ Note that all participants in this study were men.
that individuals’ awareness of changing social attitudes toward gay people would increase liking of a gay (relative to a straight) man, but would leave unaltered the negative effects of stigma by association on an individual who was randomly assigned to be roommates with a gay (relative to a straight) man. In other words, we predicted a 2 (target sexual orientation: gay versus straight) x 2 (stigma target vs. mere associate) interaction, such that the target individual’s sexual orientation would have a more negative effect on the mere associate than on the target himself.

Two aspects of the experimental design are of note here. First, we explicitly communicated that the relationship between the target and the associate (his roommate) was randomly determined rather than intentionally chosen. As discussed previously, we hypothesize that the imbalance in destigmatization processes, where destigmatization improves outcomes more quickly for stigmatized individuals than for their mere associates, is particularly likely for the kinds of arbitrary, non-meaningful relationships in which stigma transfer is implicit rather than explicit (Pryor et al., 2012).

Second, we built into the experimental design an apparent passage of time (30 years) to allow for destigmatization. That is, we assumed that participants are aware of both the current social norms surrounding gays and lesbians (that expressing homophobia is generally, but perhaps not universally, unacceptable) and also the past norms surrounding gays and lesbians (that homophobia and outright discrimination were more acceptable in previous years). Thus, we set up a context in which both the target’s sexual orientation and the onset of his relationship with the associate occurred in a period of stigmatization (1984). At the same time, participants were asked to evaluate the target and his associate in the present day, during a period of relative destigmatization (2014). As such, the present experiment sought to establish a parallel situation to the stigma of suspected Communist affiliation in the 1950s, such that there was a clear period
of stigma (the blacklist period) and also a period of destigmatization in which the target’s and the
associates’ outcomes were imbalanced. Participants in the present study were therefore asked to
consider the target’s and the associate’s relationship in the 1980s but to evaluate the two
individuals in the present day.

Method

Participants and Design. A total of 308 adult participants (52% men) completed the
experiment online using the crowdsourcing tool Amazon Mechanical Turk (hereafter mTurk),
and were paid $1. The reliability of data obtained from participant samples in mTurk is
comparable to that of traditional methods, and is discussed elsewhere (Buhrmester, Kwang, and
Gosling, 2011; for vignette experiments, Weinberg, Freese and McElhatten, 2014). mTurk
participants are more demographically diverse than other standard Internet samples and
American college samples, although younger than population-based surveys. The average age of
the respondents was 32.3 (range 18-68); 47% had a college degree or higher. The respondents
were 73% White, 9% Black, 7% Asian, 6% Latino, and 5% of another ethnic background.

Roughly 10% (n = 32) of participants identified as gay or bisexual. These participants
were excluded because gay respondents’ attitudes towards gays are likely to be systematically
different (more positive, Bartos et al., 2014) relative to the general population, and because our
theoretical framework focuses on the mechanisms of stigma as perceived by the non-stigmatized.
However, the reported results are not sensitive to the inclusion versus exclusion of gay and
bisexual respondents.

The experiment involved a 2 (target sexual orientation, between subjects) x 2 (target
versus associate, within subjects) design. Each participant was asked to assess a target individual,
“John,” and an associate, “Phil,” who were described as having been randomly assigned to have been roommates in college in the 1980s. The target individual was described as either gay (stigma condition) or straight (control condition).

**Materials and Procedure.** Participants were recruited for a study on “Predicting Life Outcomes,” which was ostensibly investigating their ability to predict the life outcomes of various individuals based on information gathered from the individuals’ past. All participants, after providing demographic information, were told that the selection of two individuals for them to evaluate (John, the target, and Phil, the associate) was random. In fact, all participants rated the same two individuals, and varied only in the information they received about John’s sexual orientation.

Participants were given a picture of two young men in a somewhat dated-appearing photograph (ostensibly taken in their dorm room at the University of Maryland in 1984), some general background information about each man, and some quotes described as coming from a third individual who knew them at that time. The third individual described an incident during college in which John, the target, was arrested for having sex in a public park, either with his boyfriend (stigma condition) or his girlfriend (control condition). Our intent with this incident was to draw participants’ attention to the legal context of homosexuality in 1984 (sodomy was then a felony in Maryland) and the precarious position of a gay man at that time. Complete text of the stimulus materials can be found in the Appendix.

After reading about the target and the associate, participants responded to four questions testing their comprehension of the material. It was particularly important to ensure that participants recalled that the target and associate’s relationship was random (rather than intentionally chosen) and that the target was gay (stigma condition) or straight (control
Dependent Variables. Following the demographic information, stimulus materials, and comprehension-check items, participants rated both the target and the associate on 6 dimensions. The dependent variables of primary interest were participants’ liking of and desired closeness with the target and the associate, as these are the dimensions that are most directly affected by social stigma (Kurzban and Leary, 2001; Walther, 2002). For thoroughness, and to reinforce the cover story about predicting life outcomes, participants also rated the target and the associate on competence, morality, masculinity, and success in life in the present day. Order of items was randomized within each set.

Liking was measured with 4 items: likeable, friendly, warm, and disagreeable (reverse scored). Items were rated on a Likert scale ranging from 1 (describes him not at all well) to 5 (describes him very well). Cronbach’s alphas were .83 (John) and .74 (Phil).

Desired closeness was measured with 8 items, as follows: “If I met John today, we would probably get along well,” “John seems like my type of guy,” “I would enjoy having John as a co-worker,” “I wouldn’t mind sitting next to John on an airplane flight,” “If John were dating a close member of my family, I would be OK with it,” “I would enjoy having a beer or cup of coffee with John,” “I don’t think John and I would have much in common,” and “I would be unhappy if John were my next-door neighbor (the latter 2 items were reverse scored). Items measuring desired closeness with the associate were identical except that they referred to Phil rather than John. Items were rated on a Likert scale ranging from 1 (strongly disagree) to 5 (strongly agree). Cronbach’s alphas were .82 (John) and .77 (Phil).

Competence was measured with 4 items: competent, successful, skilled, and loser (reverse scored). Items were rated on a Likert scale ranging from 1 (describes him not at all well)
to 5 (describes him very well). Cronbach’s alphas were .87 (John) and .77 (Phil).

Morality was measured with 7 items: moral, honorable, clean-living, respectable, repulsive, immoral, and sleazy (the latter 3 items were reverse scored). Items were rated on a Likert scale ranging from 1 (describes him not at all well) to 5 (describes him very well). Cronbach’s alphas were .91 (John) and .78 (Phil).

Masculinity was measured with 4 items: masculine, macho, feminine, and girly (the latter 2 items were reverse scored). Items were rated on a Likert scale ranging from 1 (describes him not at all well) to 5 (describes him very well). Cronbach’s alphas were .79 (John) and .55 (Phil).

Finally, success in life was measured with 7 items: “John has a family (spouse and children),” “John earns a high income,” “John has achieved professional success,” “John is respected in his community,” “John has a lot of friends,” “John could be considered a failure,” and “John has trouble with interpersonal relationships” (the latter 2 items were reverse scored). Items measuring the associate’s success in life were identical except that they referred to Phil rather than John. Items were rated on a Likert scale ranging from 1 (very unlikely) to 5 (very likely). Cronbach’s alphas were .81 (John) and .77 (Phil).

Results

Our central prediction was an interaction effect, such that the negative effect of stigma condition on liking and closeness would be stronger for the associate than for the target himself. A 2-way analysis of variance (ANOVA) with the liking dependent variable revealed a significant main effect of the individual being rated, $F(1, 275) = 61.88, p < .0005$, such that John was liked less than Phil; no main effect of experimental condition, $F(1, 275) < 1$; and, as hypothesized, a significant interaction effect, $F(1, 275) = 1.33, p = .035$. The pattern of means (see Figure 2)
shows that the effect of John’s sexual orientation hurt liking of Phil, his randomly assigned roommate, more than it did John himself. Follow-up tests of simple effects showed that John was liked equally well whether he was gay \((M = 3.75, SD = 0.80)\) or straight \((M = 3.66, SD = 0.74)\), \(F < 1\). In contrast, Phil was liked less to a marginally significant degree when his roommate John was gay \((M = 4.02, SD = 0.55)\) versus straight \((M = 4.13, SD = 0.58)\), \(F = 2.99, p = .085\).

A similar pattern of means, albeit a slightly weaker effect, emerged for the desired closeness variable. A two-way ANOVA revealed again a significant main effect of the individual being rated, \(F(1, 275) = 59.83, p < .0005\), such that participants desired less closeness with John than with Phil; and no main effect of experimental condition, \(F(1, 275) = 2.71, ns\). The predicted interaction effect did not achieve significance, \(F(1, 275) = 1.78, p = .183\). However, the pattern of means was in the predicted direction. Moreover, follow-up tests of simple effects showed that participants desired equal closeness with John whether he was gay \((M = 3.47, SD = 0.81)\) or straight \((M = 3.43, SD = 0.78)\), \(F < 1\). In contrast, participants desired significantly less closeness with Phil when his roommate John was gay \((M = 3.71, SD = 0.54)\) than when John was straight \((M = 3.89, SD = 0.43)\), \(F = 9.23, p = .003\).

Exploratory ANOVAs with the other 4 dependent variables (competence, morality, masculinity, and success in life) revealed a significant interaction effect only for masculinity, \(F(1, 275) = 86.06, p < .005\). Specifically, John was seen as significantly less masculine when he was gay \((M = 3.01, SD = 0.80)\) than when he was straight \((M = 3.95, SD = 0.55)\), whereas Phil’s masculinity was unaffected by whether his roommate John was gay \((M = 3.80, SD = 0.57)\) versus straight \((M = 3.84, SD = 0.47)\).

**Discussion**

In the present experiment, we demonstrate that when evaluating a target whose identity as
a gay man was stigmatized in the past more than it is today, participants do not appear to continue to stigmatize him, as evidenced by the lack of an effect of experimental condition on liking of the target, John. We suggest that this is because participants are aware of current norms of destigmatization of homosexuality (and, arguably, stigmatization of homophobic expression). That is, we suspect that participants were correcting or suppressing any initial negative response they may have had to John’s sexuality in order to be consistent with social norms. In contrast, participants do continue to stigmatize the mere associates of a target whose identity has been destigmatized. They showed less liking of, and less desire to be close with, a man who had been randomly assigned (30 years prior) to a gay roommate, compared to when the roommate was straight. We hypothesize that participants’ conscious efforts to suppress or correct any homophobic responses focused only on the gay man himself, rather than his seemingly irrelevant roommate. Because participants are unlikely to be consciously aware that stigma by association has occurred, they are also unlikely to be able to consciously correct for it during a period of destigmatization.

Another finding of note in the current experiment was the interaction effect with the masculinity dependent variable, where, unlike with the liking and desired closeness dependent variables, John’s sexual orientation affected his own perceived masculinity, but not that of his roommate. This differential pattern suggests indirectly that while participants may have liked Phil less when he had a gay roommate, this is not because they assumed Phil to be gay or less masculine himself. This supports our theorizing that the stigma Phil acquired by association – as well as his failure to be destigmatized in the present day – occurred via nonconscious, implicit process rather than via an explicit inference about Phil’s actual traits or sexuality.

The use of experimental methods in the present study, as a supplement to the archival
methods of Study 1, provides support for our causal claim about the role of stigma by association in predicting liking and desired closeness. That is, it is not that associates of stigmatized individuals (such as co-workers of the blacklisted artists in Study 1) are somehow different from non-associates, but rather that their association with a stigmatized individual has a direct causal impact on how they are viewed by others. These two studies leave open, however, the question of the exact mechanism of the imbalanced processes of destigmatization that we have demonstrated. We have proposed that this imbalance occurs because stigma by association occurs outside of conscious awareness, and perceivers, unaware that their stigma has spread to others, fail to correct for it during periods of destigmatization. We test this proposed mechanism more directly in Study 3.

**Study 3 – Likeability of Men whose Association with a Gay Roommate was Random versus Voluntary**

*In progress*

Study 3 investigates whether the imbalance in destigmatization can be explained by way of automatic and deliberate psychological processes. We suggest that destigmatization operates through deliberate processes, where a person consciously corrects their aversion. If this is the case, we should expect the strongest imbalance in destigmatization when stigma spreads through automatic, and not deliberate processes. To test this, we are running a second experiment, similar to the one described above, where we compare evaluations of the roommate Phil, depending on whether he was randomly assigned to room with John, or if he was close friends with John and thus chose to room with him. Pryor et al. (2012) finds that for meaningful relationships, stigma spreads through both explicit and implicit processes; whereas stigma spreads only through implicit processes for non-meaningful relationships. If destigmatization unfolds through explicit
processes, we expect that when John and Phil choose to be roommates because they are close friends, Phil will experience more benefits of destigmatization, as compared to when John and Phil are randomly assigned to be roommates.

**Discussion and Conclusions**

Effects of stigma – and how it spreads – have been well studied in psychological and sociological literatures. We also know that stigma is socially constructed and changes over time. But very little attention has been paid to how destigmatization unfolds. We suggest that destigmatization results when people make a conscious effort to correct their prejudices, influenced by changing social norms. This means that people directly targeted with stigma will be rehabilitated. However, it suggests that destigmatization may not reach those who have been indirectly affected by stigma, those with mere association. We present observational and experimental evidence that supports this hypothesis. After the Red Scare in Hollywood, the careers of the blacklisted rebounded; but the associates continued to suffer. A laboratory experiment shows a similar imbalance in destigmatization, based on ratings of liking and social closeness of someone who happened to room with a gay man in college. These findings suggest that the social harm created by stigma is difficult to correct; the wide reach of stigma is not counteracted by a similar breadth when it comes to reversing the social stigma.
REFERENCES


Table 1. Description of Covariates Included in the Matching Process, Study 1

<table>
<thead>
<tr>
<th>Covariate</th>
<th>Definition</th>
<th>Source</th>
</tr>
</thead>
<tbody>
<tr>
<td>Gender</td>
<td>Dichotomous indicator equal to 0 for female and 1 for male.</td>
<td>Online dictionaries of baby names, and manual verification by independent coder using artist searches on imdb.com.</td>
</tr>
<tr>
<td>Professional role</td>
<td>Count of jobs as actor, writer, director, and/or producer and their hyphenates.</td>
<td>Film entries in American Film Institute catalog (AFI)</td>
</tr>
<tr>
<td>Previous work in film genres</td>
<td>Count of films in the 4 more common genres: Comedy; Crime; Drama; Western.</td>
<td>AFI</td>
</tr>
<tr>
<td>Tenure in the industry</td>
<td>Count of years since first film job.</td>
<td>AFI</td>
</tr>
<tr>
<td>Artistic track record</td>
<td>Count of Academy Award nominations (including wins)</td>
<td>Shale (1993)</td>
</tr>
</tbody>
</table>
Table 2. Descriptive Statistics of Covariates Included or Compared in Matching Process, Study 1

2A. Before matching

<table>
<thead>
<tr>
<th></th>
<th>Blacklisted</th>
<th>SBA Blacklisted</th>
<th>Mean ≠ 0 (p-value)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Gender (Female = 0)</td>
<td>0.240</td>
<td>0.429</td>
<td>0.351</td>
</tr>
<tr>
<td>Role</td>
<td>3.419</td>
<td>3.235</td>
<td>1.881</td>
</tr>
<tr>
<td>Tenure</td>
<td>8.419</td>
<td>5.771</td>
<td>7.347</td>
</tr>
<tr>
<td>Oscar nominations</td>
<td>0.225</td>
<td>0.534</td>
<td>0.031</td>
</tr>
<tr>
<td>Films in comedy</td>
<td>0.550</td>
<td>1.145</td>
<td>0.783</td>
</tr>
<tr>
<td>Films in crime</td>
<td>0.519</td>
<td>0.993</td>
<td>0.747</td>
</tr>
<tr>
<td>Films in drama</td>
<td>0.612</td>
<td>1.181</td>
<td>0.731</td>
</tr>
<tr>
<td>Films in western</td>
<td>0.419</td>
<td>1.261</td>
<td>1.479</td>
</tr>
<tr>
<td>Genre specialization</td>
<td>0.256</td>
<td>0.299</td>
<td>0.264</td>
</tr>
<tr>
<td>Work for major studio</td>
<td>0.736</td>
<td>0.442</td>
<td>0.637</td>
</tr>
</tbody>
</table>
### 2B. After matching

<table>
<thead>
<tr>
<th></th>
<th>CEM Matched</th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th>Mean ≠ 0 (p-value)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Blacklisted</td>
<td>SBA Blacklisted</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>Mean</td>
<td>SD</td>
<td>Min</td>
<td>Max</td>
<td>Mean</td>
<td>SD</td>
<td>Min</td>
<td>Max</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Gender (Female = 0)</td>
<td>0.265</td>
<td>0.443</td>
<td>0</td>
<td>1</td>
<td>0.265</td>
<td>0.443</td>
<td>0</td>
<td>1</td>
<td></td>
<td>(1.00)</td>
</tr>
<tr>
<td>Role (A/D/W/P/Hyphenates)</td>
<td>3.368</td>
<td>3.169</td>
<td>1</td>
<td>15</td>
<td>3.350</td>
<td>3.027</td>
<td>1</td>
<td>15</td>
<td></td>
<td>(0.97)</td>
</tr>
<tr>
<td>Tenure</td>
<td>8.214</td>
<td>5.693</td>
<td>0</td>
<td>22</td>
<td>8.769</td>
<td>6.445</td>
<td>0</td>
<td>23</td>
<td></td>
<td>(0.49)</td>
</tr>
<tr>
<td>Oscar nominations</td>
<td>0.205</td>
<td>0.501</td>
<td>0</td>
<td>3</td>
<td>0.137</td>
<td>0.571</td>
<td>0</td>
<td>5</td>
<td></td>
<td>(0.33)</td>
</tr>
<tr>
<td>Films in comedy</td>
<td>0.581</td>
<td>1.169</td>
<td>0</td>
<td>6</td>
<td>0.641</td>
<td>1.228</td>
<td>0</td>
<td>8</td>
<td></td>
<td>(0.70)</td>
</tr>
<tr>
<td>Films in crime</td>
<td>0.453</td>
<td>0.793</td>
<td>0</td>
<td>4</td>
<td>0.470</td>
<td>0.805</td>
<td>0</td>
<td>4</td>
<td></td>
<td>(0.87)</td>
</tr>
<tr>
<td>Films in drama</td>
<td>0.573</td>
<td>1.162</td>
<td>0</td>
<td>6</td>
<td>0.615</td>
<td>1.258</td>
<td>0</td>
<td>7</td>
<td></td>
<td>(0.79)</td>
</tr>
<tr>
<td>Films in western</td>
<td>0.410</td>
<td>1.267</td>
<td>0</td>
<td>11</td>
<td>0.821</td>
<td>2.066</td>
<td>0</td>
<td>12</td>
<td></td>
<td>(0.07)</td>
</tr>
<tr>
<td></td>
<td>Mean</td>
<td>SD</td>
<td>Median</td>
<td>Mean</td>
<td>SD</td>
<td>Median</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Genre specialization</td>
<td>0.260</td>
<td>0.302</td>
<td>0</td>
<td>1</td>
<td>0.269</td>
<td>0.292</td>
<td>0</td>
<td>1</td>
<td></td>
<td>(0.81)</td>
</tr>
<tr>
<td>Work for major studio</td>
<td>0.735</td>
<td>0.443</td>
<td>0</td>
<td>1</td>
<td>0.795</td>
<td>0.406</td>
<td>0</td>
<td>1</td>
<td></td>
<td>(0.29)</td>
</tr>
</tbody>
</table>
Table 3. Difference-in-Difference OLS Regression of Average Number of Films Worked, Working in Film and Median Rank in Film Credits, Study 1

<table>
<thead>
<tr>
<th>Dependent Variable Difference in:</th>
<th>Avg. Number of Films</th>
<th>In Film</th>
<th>Median Rank In Credits</th>
</tr>
</thead>
<tbody>
<tr>
<td>Blacklisted</td>
<td>0.381* (0.043)</td>
<td>0.080* (0.023)</td>
<td>0.088 (0.054)</td>
</tr>
<tr>
<td>Constant</td>
<td>-0.527* (0.018)</td>
<td>-0.148 (0.005)</td>
<td>-0.156* (0.006)</td>
</tr>
</tbody>
</table>

Notes: Standard errors in parentheses. * p < 0.05. N = 3217 for Avg. # Films and In Film, and 2492 for Median Rank. Periods are: 1958-1961 vs. 1950-1957.
Figure 1. Average Number of Films during the Analysis Period for the Three Artist Groups, Study 1

![Graph showing the average number of films per year for different groups, with years from 1949 to 1962 and a y-axis ranging from 0 to 1.25.]

Figure 2. Ratings of Target and Associate, Study 2.

![Bar chart showing liking scores for John (target) and Phil (associate). The chart includes two bars for each individual, one representing Stigma and the other Control. The liking scores range from 3.4 to 4.2.]
Appendix

Methodological Note, Study 1

In this section we discuss how Study 1 addresses the main validity threats to DD estimators. Recent work shows that when data are yearly cross-sections of outcome variables and covariates, the DD estimator is subject to severe serial correlation problems that lead to understatement of the standard errors of treatment coefficients (Bertrand, Duflo and Mullainathan, 2004). Bertrand et al. (2004) demonstrate that a solution that corrects for serial correlation requires removing the time-series dimension of the data, and aggregating the data into two periods. Accordingly, we calculate a single average value, for each artist in each time period, for all outcome variables and covariates and compare the values in the two periods. This aggregation performs well with small and large numbers of units and does not lead to overestimation of t-statistics and significance levels.

Second, one key identifying assumption of the DD estimator is that of parallel trends by which, in the absence of the treatment, there would be no difference in the mean of the units in the two groups (Meyer, 1995). Indeed, treatments are often targeted based on pre-existing differences in outcomes. We deal with this validity threat in multiple ways. We inspect pre-existing differences in trends in the outcome variable for the two groups, and then estimate another DD of the average number of films worked during the blacklist period relative to a pre-blacklist period (1945-1949). In addition, we conduct a simple falsification test for an alternative comparison group – those who were neither blacklisted nor associates. Finally, we also examine the heterogeneity of the effects among blacklisted artists. Differences in the intensity of the stigmatization within the “treated” group allow us to study if the changes in outcomes differ in the expected direction.

A third validity threat concerns compositional differences, the fact that the two groups
can lose units during the study period due to attrition. We address this issue by coding available biographical information and including in the analysis only artists who died after 1962.

The fourth validity threat we consider involves the tradeoff between the reliability of the estimates and their long-term effects. In particular, we examine the sensitivity of the estimates to alternative definitions of the pre- and post-blacklist periods, and the duration of the effects in the destigmatization period.

We first investigate the parallel trend assumption. An inspection of Figure 1 shows that, consistent with the assumption, both blacklisted artists and associates had fewer jobs during the blacklist period, but only blacklisted artists experience rebounded after the blacklist period, while the decline continued for associates. These differences between the two groups are not consistent with an alternative explanation for the DD estimates, mean reversion (Ashenfelter, 1978).
### Table 4

**Table 4. Difference-in-Difference OLS Regression of Average Number of Films Worked – Falsification Tests, Study 1**

<table>
<thead>
<tr>
<th>Matched Sample: Blacklisted + SBA, pre vs. stigma</th>
<th>Matched Sample: Blacklisted + SBA + Rest</th>
<th>Matched Sample: Blacklisted + SBA</th>
<th>Matched Sample: Non-Writers Blacklisted + SBA</th>
<th>Matched Sample: Blacklisted + SBA</th>
<th>Matched Sample: Blacklisted + SBA</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Dependent Variable</strong></td>
<td><strong>Difference in Avg. Number of Films</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Blacklisted</td>
<td>-0.212</td>
<td>0.405*</td>
<td>0.300*</td>
<td>0.331*</td>
<td>0.334*</td>
</tr>
<tr>
<td></td>
<td>(0.191)</td>
<td>(0.044)</td>
<td>(0.043)</td>
<td>(0.067)</td>
<td>(0.070)</td>
</tr>
<tr>
<td>Others</td>
<td>0.469*</td>
<td></td>
<td></td>
<td>0.300*</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.029)</td>
<td></td>
<td></td>
<td>(0.043)</td>
<td></td>
</tr>
<tr>
<td>In Anti-Communist Films</td>
<td>-0.949*</td>
<td>0.405*</td>
<td>0.300*</td>
<td>0.331*</td>
<td>0.334*</td>
</tr>
<tr>
<td></td>
<td>(0.083)</td>
<td>(0.044)</td>
<td>(0.043)</td>
<td>(0.067)</td>
<td>(0.070)</td>
</tr>
<tr>
<td>Communist Affiliation</td>
<td>-0.046</td>
<td></td>
<td></td>
<td>(0.111)</td>
<td></td>
</tr>
<tr>
<td>Blacklisted X Communist Affiliation</td>
<td>0.123</td>
<td></td>
<td></td>
<td>(0.138)</td>
<td></td>
</tr>
<tr>
<td>Blacklisted Unfriendly</td>
<td>-0.519*</td>
<td>-0.555*</td>
<td>-0.422*</td>
<td>-0.539*</td>
<td>-0.527*</td>
</tr>
<tr>
<td></td>
<td>(0.130)</td>
<td>(0.020)</td>
<td>(0.016)</td>
<td>(0.019)</td>
<td>(0.018)</td>
</tr>
<tr>
<td>Blacklisted Friendly</td>
<td>-0.063</td>
<td></td>
<td></td>
<td>0.409*</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.172)</td>
<td></td>
<td></td>
<td>(0.042)</td>
<td></td>
</tr>
<tr>
<td>Constant</td>
<td>-0.519*</td>
<td>-0.555*</td>
<td>-0.422*</td>
<td>-0.539*</td>
<td>-0.527*</td>
</tr>
<tr>
<td></td>
<td>(0.130)</td>
<td>(0.020)</td>
<td>(0.016)</td>
<td>(0.019)</td>
<td>(0.018)</td>
</tr>
</tbody>
</table>

Notes: Standard errors in parentheses. * p < 0.05. N=1112 (column 1); N=3286 (column 2); N=3217 (column 3); N=2989 (column 4); N=3217 (column 5); N=3217 (column 6).

Table 4 presents models that further examine the parallel trend assumption. In Model 1 we estimate a DD regression analogous to that in Table 3 but compare the differences between the average number of films worked each year during the blacklist period and during the period before the blacklist. Here, we find that the coefficient for blacklisted has no statistical significance, so the difference in the number of jobs for blacklisted artists and associates did not increase or decrease during the blacklist.

It is possible that the blacklisted artists experienced different trends in jobs compared to
another group, perhaps all other artists in the industry. The green line in Figure 1 depicts the average number of films made by the artists who were not blacklisted or associates, labeled “others.” The figure hints that the trajectory of work in film for the blacklisted moves closer to that of the others group.

In Model 2 we estimate a DD regression using a modified matched sample to address this form of omitted interaction bias (Meyer, 1995). We implemented a CEM procedure analogous to that described above, but included associates and others in the group to match with the blacklisted. Model 2 then compares the difference in films worked by the blacklisted, the associates, and others by adding to the blacklisted covariate another for the others group. The estimates show a positive effect for blacklisted as well as others. An F-test of the equality of blacklisted and others finds that we cannot reject the null hypothesis that the two coefficients are equal ($F = 2.10, p = 0.15$). This suggests that the rise in films worked by the blacklisted artists between the post-blacklist and the blacklist period indeed occurred as compared to the associates. Interestingly, the careers of the blacklisted artists do not show a higher or lower rebound than the trend followed by other artists in the industry.

Next, we examine a different form of interactional bias. It could be that the effect of destigmatization is due not to changes in public perceptions but to some artists pursuing distinctive strategies to improve their reputation and distance themselves from the blacklist and its stigma. One way to achieve that would be to seek jobs during the blacklist period in projects whose content includes anti-Communist messages. Two lists of anti-Communist films that were released in this time period are documented in Cogley (1956) and Combs (1990). In the DD regression of Model 3 we add a dichotomous variable for whether the artists were in one or more anti-Communist films, and the effect of this variable is negative and significant. Perhaps anti-
Communist films were propaganda vehicles with limited box-office appeal and starring in them could hurt rather than help careers. In these estimates, the effect of blacklisted holds even after accounting for the anti-Communist film dummy. Their interaction does not have a statistical significant effect (model available upon request).

Another kind of interactional bias from behavioral differences could depend on artists working using aliases or fronts. Some writers worked in a “black market” for film scripts. For example, Dalton Trumbo worked for Monogram during the blacklist years, writing films (including *Gun Crazy*) that were credited to Millard Kaufman. Note that AFI provides information for screen credits obtained through pseudonyms, which we counted in the original data, so the estimates obtain net of these corrections. Furthermore, working in the black market was a strategy only available to writers, as the other roles are visible to the audience. Model 4 in Table 4 contains the DD regression estimated by excluding writers from the two groups. The blacklisted effect remains positive and statistically significant.

Model 5 examines interactional bias from another potential confounder. Increased work could be linked not to changes in social perception of blacklisted artists, but because of changing perceptions of Communists, at least in the film industry. Using data about the affiliations of artists to Communist organizations culled from copies of the original FBI reports compiled before the HUAC hearings we included a dichotomous indicator equal to one for the artists with Communist affiliations (at least one). In these estimates, the effect of Communist affiliations is not significant while blacklisted remains positive and significant (the Communist covariate overlaps in part with blacklisted and associates, and when we include it in the DD regression we can interpret the coefficient of blacklisted as the effect for artists without Communist affiliations).
Next we test the validity of the DD estimates by examining heterogeneity within the blacklisted group. This can indicate bounds for the possible effects. In the aftermath of the Red Scare, blacklisted artists were viewed as victims of political propaganda. In this context, friendly witnesses to the HUAC could be viewed as less deserving of rehabilitation because they collaborated with a morally questionable cause (even if to protect their careers and lives). Unfriendly witnesses who did not cooperate ought to deserve greater recognition after the end of the blacklist period. In Model 6 we separate the effect of blacklisted for artists who were friendly to the HUAC and artists who were unfriendly (or were listed in *Red Channels*). The estimates suggest that the significant effects of destigmatization benefited the unfriendly blacklisted artists.

Table 5. Difference-in-Difference OLS Regression of Average Number of Films Worked and Median Rank in Film Credits, Study 1

<table>
<thead>
<tr>
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</thead>
<tbody>
<tr>
<td>Blacklisted</td>
<td>0.372* (0.044)</td>
<td>0.400* (0.041)</td>
<td>0.398* (0.046)</td>
<td>0.401* (0.045)</td>
<td>0.421* (0.042)</td>
</tr>
<tr>
<td>Constant</td>
<td>-0.492* (0.017)</td>
<td>-0.549* (0.018)</td>
<td>-0.540* (0.018)</td>
<td>-0.570* (0.019)</td>
<td>-0.587* (0.019)</td>
</tr>
</tbody>
</table>

Notes: Standard errors in parentheses. * p < 0.05. N = 3217

Finally, we examine the stability of the results to alternative definitions of the post-blacklist period. In Table 5 we move the start of the post-blacklist period to 1959 for the outcome variable average number of films and find patterns similar to Table 3. In separate seemingly
unrelated regressions, the coefficients for blacklisted in the additional estimates are not significantly different from that in Table 3 except for 1959-62. This is also the case if we measure work for one less or more year in the post-blacklist period.
Stimulus Materials, Study 2

Phil and John had both enrolled in the University of Maryland that semester [October 1984] as freshmen. They did not previously know each other, but were randomly assigned to room together by the university.

**John (#130)**

- Hometown: Silver Spring, MD
- Math major
- Worked at one of the computer labs on campus
- Worked at Dining Services

The following is a segment of an interview with another resident of the dorm where John lived as a freshman. He is recalling his memories of John. The interview took place in 2012.

#290, 11/8/2012: “Well what I do remember about John was one time when he told me that he had been arrested. He and his boyfriend (girlfriend) were at the park for an outdoor concert or something. And he was actually arrested for having sex with the boyfriend (girlfriend) in the park. At that time there weren’t a lot of people who were open about being gay. But he was out about it. There was something about how it could have been a felony charge ‘cause you know it was illegal then, between men, and he could have actually gone to jail. But I think it ended up being a lesser charge, indecent exposure or whatever, because of some technicality. (I think it
ended up being an indecent exposure charge or whatever, because some people saw them.) He joked about it a lot afterward – he had a sense of humor about the whole thing.”

**Phil (#131)**

- Hometown: Stuarts Draft, VA
- History major
- Intramural lacrosse
- Worked for one year as a research assistant for a history professor who was writing a book on the Civil War.

The following is a segment of an interview with another resident of the dorm where Phil lived as a freshman. He is recalling his memories of Phil. The interview took place in 2012.

#457, 11/5/2012: “I do remember him actually, I don’t remember where he was from but it was like a small town or something. So when he got to college in the city it was kind of eye-opening I guess. He sort of dived into college life when he got here and in that first year he was always the one at parties until 4, 5 in the morning and trying everything. By the next year I think he sort of settled down. I remember him as always friendly to everyone.”