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ORIGINAL PAPER

Exploring the Effect of Exposure to Short-Term Solitary Confinement Among Violent Prison Inmates

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Abstract
Objectives This study tracked the behavior of male inmates housed in the general inmate populations of 70 different prison units from a large southern state. Each of the inmates studied engaged in violent misconduct at least once during the first 2 years of incarceration (n = 3,808). The goal of the study was to isolate the effect of exposure to short-term solitary confinement (SC) as a punishment for their initial act of violent behavior on the occurrence and timing of subsequent misconduct.
Methods This study relied upon archival longitudinal data and employed a multilevel counterfactual research design (propensity score matching) that involved tests for group differences, event history analyses, and trajectory analyses.
Results The results suggest that exposure to short-term solitary confinement as a punishment for an initial violence does not appear to play a role in increasing or decreasing the probability, timing, or development future misconduct for this particular group on inmates.
Conclusions Upon validation, these findings call for continued research and perhaps a dialog regarding the utility of solitary confinement policies under certain contexts. This unique study sets the stage for further research to more fully understand how solitary impacts post-exposure behavior.

Keywords Solitary confinement · Punitive segregation · Corrections · Inmate misconduct

Introduction

In recent decades, prison scholars have investigated the impact of solitary confinement (SC) on the psychological and physical health of prison inmates. Unfortunately, however,

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the effects of SC as an administrative control mechanism in response to specific acts of misconduct have remained relatively unexplored. SC, as defined here, is specific to what Shalev (2009) describes as "punitive segregation," wherein exposure to isolation is a temporary punishment in response to acts of misconduct. Alternatively, corrections administrators may use isolation as a form of "protective segregation" for inmates at risk of victimization or as a means of "administrative segregation" wherein highly-disruptive inmates are subject to isolation for longer-terms, perhaps transferred to a prison unit specifically designed around isolation (i.e., a supermax facility)—see Mears (2013) for a more detailed discussion. In any case, isolation involves the restriction of an inmate's movement to a cell for 23 h per day. In terms of punitive segregation, exposure may be tied to a single event, such as a particular act of violence, rather than for a potentially arbitrary decision deeming an inmate unmanageable and in need of longer-term isolation (Mears 2008; Mears and Watson 2006; Riveland 1999).[1]

Few studies have focused on post-SC behavior and even fewer have attempted to parse out any causal effect from exposure to SC on subsequent behavioral outcomes at the individual-level. Exposure to short-term punitive segregation resulting from an instance of serious misconduct has the potential to result in a beneficial (i.e., attenuating/deterring) outcome, a harmful (i.e., exacerbating) outcome, or something in-between following return to regular prison settings. In practice, SC varies in terms of rationale for use, frequency of subjugation, duration, and facility conditions. There may also be variation in terms of how any associated side effects may unfold post-exposure to SC (i.e., whether the effect, if any, is immediate, delayed, permanent, or temporary)—see Browne et al. (2011). Additionally, any SC effect should be contextualized by the prison-unit and the staff within as the probability of receiving solitary may also vary from one unit to the next and/or may change over time (Suedfeld et al. 1982). SC may also be more influential among general-population inmates early on in the prison sentence given that the first 3 years of incarceration tend to the time in which serious misconduct is most likely (Griffin and Hepburn 2006; Morris et al. 2012).

Many have considered the possibility that the impact from exposure to isolation may vary across different types of inmates and under varying circumstances (e.g., the effects of SC may be different for inmates with mental illness—see Smith 2006). Before the effects of exposing inmates to SC can be understood these contexts should be considered. Such studies should account for contexts by control or design prior to being in a position to adequately informing the debate surrounding the use of SC itself.

The goal of this study was to assess the empirical relationship between exposure to short-term SC, defined by punitive segregation, and subsequent violence/misconduct among to male inmates found guilty of a physically violent act upon entering prison. Presented immediately below is an overview of evidence specific to this study, which is presented through the lens of deterrence theory. This is followed by a section outlining the need for solitary research and its role in corrections policy, along with an overview of how counterfactual research designs are critical to this endeavor. A summary of the unique data utilized is then provided along with detailed information specific to the counterfactual

[1] At the most extreme level, supermax housing/units may expose inmates to SC on a daily basis for extended periods of time and is commonly reserved for those considered the "worst of the worst"—see Mears and Bales (2010). Though the findings presented here may not apply to supermax inmates/facilities, it is important to acknowledge previous work that has assessed the effect of solitary confinement as an administrative control mechanism in supermax settings—e.g., Briggs et al. (2003); Sundt et al. (2008). Such findings suggest a lack of support for the idea that the use of supermax has a substantive positive impact on inmate assaults.

established herein, measurement, and the varying statistical techniques used to test for a solitary effect. Attention is also given to how the data and analyses were critiqued for robustness. The study's results are subsequently presented, followed by a discussion that synthesizes the results alongside methodological limitations. The article concludes with a discussion specific to theoretical and policy implications as well as needs for further research and validation of this important topic.

Background Literature

Solitary as a Deterrent Effect

Regarding beneficial (i.e., deterring/attenuating) outcomes post-solitary, deterrence theories would posit that those directly or indirectly exposed to SC should refrain from subsequent misconduct to prevent further exposure to solitary. In turn, the inmate population in general should also be less likely to engage in similar behavior in an effort to avoid experiencing the potential punishment. While the focus here is specific to individuals, rather than as a general effect, the following section provides an overview of deterrence literature that might explain why a deterrent effect may be expected post-exposure.

Some studies have found that exposure to SC results in lethargy, apathy, and chronic fatigue (see Smith 2006). These side effects may in turn negatively impact the odds of continuity in violent behavior to some degree, ultimately reducing the odds of behaviors leading to SC exposure (see Smith 2006).

The administrative control model, initially developed by DiIulio (1990), called for attention to the role of prison administration as a determinant of prisoner misconduct. DiIulio (1990) found that prisons operating under a formal organizational structure, with clearly defined rules, routines, and a clear-cut division of labor and leadership should have less misconduct. Along the same line, Useem and Kimball (1989) argued that "administrative breakdown" is the cause of collective misconduct, but this is a complex phenomenon that does not explain misconduct at the individual level, rather it focuses on the prison-level. Huebner (2003) explored the effects of a variety of coercive punishments, including SC, on post-exposure assaultive behavior. Her study, framed around the administrative control model and based on a sample of over 4,000 inmates across 185 state (US) prisons, found that, net of other effects, increased SC rates in a prison unit were *not* predictive of subsequent violence. Unfortunately, however, the study did not account for individual SC exposure and was cross-sectional by design, thus causal implications cannot be implied from the results. Yet it should be noted that Huebner's goal was to test administrative control theory rather than exploring the isolated effects of a particular punishment. Other macro-level analyses specific to the effect of solitary confinement have been published specifically in regard to the use of supermax prisons (e.g., Sundt et al. 2008; Briggs et al. 2003). Such findings suggest a lack of support for the idea that the use of supermax has a substantively positive impact on inmate assaults; as a reminder, the current focus is on short-term solitary confinement in general prisons, not those subject to alternative forms of isolation (e.g., supermax facilities).

Solitary as an Exacerbating Effect

Sherman's (1993) theory of defiance would suggest that inmates exposed to solitary confinement might be more inclined to continue misbehavior upon return to general prison

settings. The theory posits that if an inmate perceives correctional officials as abusing discretion (i.e., as being unfair) in the decision to punish via solitary, then we might expect continuity in misbehavior post-solitary. Defiance theory has not been applied to solitary confinement as defined here in the past, however several studies have nonetheless reported findings suggesting the potential for an exacerbating effect post-exposure to SC.

The majority of the research that has explored the effects of SC has primarily originated from the disciplines of psychology and/or physiology (e.g., Grassian 1983; Haney 2003; O'Keefe et al. 2013) and often suggests the possibility of an exacerbating effect. Within such examples, the most common finding is that inmates exposed to varying doses of SC exhibit increased levels of psychological distress post-exposure (for a review, see Haney and Lynch 1997; Smith 2006). This body of literature is sometimes referred to as the "medical model" of the SC effect. The topics within this realm are very broad and not all relate to inmate behavior post-exposure. In fact, no study could be found that explored subsequent violence as an outcome among *individual* subjects exposed to SC—but see the macro-level work of Sundt et al. (2008) and Briggs et al. (2003).

This body of literature has contributed much of what is understood about health related outcomes post-exposure to isolation though many studies have relied on cross-sectional data, do not account for a well-defined control group, tend to utilize small sample sizes, have often relied upon college student samples rather than on actual inmates, and vary in how SC is defined (see Mears and Watson 2006; Pizarro and Narag 2008; Smith 2006). Despite the tendency for limitations, there seems to be more evidence to suggest that exposure to solitary may have an exacerbating effect on continued misconduct. In terms of heath related outcomes, Smith (2006: 503) goes so far as to state that "…solitary confinement—regardless of specific conditions and regardless of time and place—causes serious health problems for a significant number of inmates."

Solitary Confinement as a Null Effect

During the late 1970s, Suedfeld and colleagues conducted a study relaying on a sample of inmates from five American and Canadian prison units. Their work offered modest support for a link between SC and subsequent violence. Inmates were surveyed and interviewed regarding the effects of SC on a range of psycho-social measures (Suedfeld et al. 1982). In general, the study found no differences between inmates who experienced SC and those who did not. However, several differences were found within particular institutions. Inmates exposed to more (frequency or duration) SC tended to display higher levels of antisocial and/or mental illness characteristics (e.g., anxiety, depression, and hostility). Suedfeld et al. also found that among their sample, inmates experiencing SC tended to be represented by higher levels of social immaturity, distrustfulness, manipulative behaviors, and self-centeredness.

The Need for Further Study of the Effects of Solitary

Little is known about the return on investment from the use of SC in and of itself. Indeed there are those who argue vehemently in favor of the abolition of SC in the prison setting and much effort is focused on critically evaluating the use of SC throughout the nation (for example, the Vera Institute's Segregation Reduction Project)—see also Gibbons and Katzenbach (2006). However, prior to concluding this debate it is critical to empirically assess questions about whether and how specific types of SC work in terms of mitigating or exacerbating the probability of future behavior. If for example, exposure SC by itself

actually decreases the odds of an inmate's subsequent level of dangerousness to others then perhaps there is merit in the use of SC despite evidence of psychological harm. Others may argue that the use of SC might be warranted even if the SC effect is null simply for the reason that a raging inmate is incapacitated, or in other emergency situations. Should the tool be of no benefit any more than for the immediate removal of an inmate from a heated situation then perhaps the use of SC should be restricted to that of short-term harm prevention and nothing more (i.e., time-out). Regardless, any position on the use of SC should be driven by empirical evidence specific to SC effects on behavioral outcomes and corrections policies should be built around this evidence following validation.

Several questions surrounding the utility of SC have yet to receive the attention they deserve. Does SC actually accomplish a goal other than removing a perceived dangerous inmate from the general prison environment? More specifically, does SC, in and of itself, have a deterrent or exacerbating individual-level effect on future misconduct (violent and otherwise)? Also, does SC play a role in the timing to subsequent misconduct, or to the development of future behavior patterns? These are but a few important questions worthy of attention. The goal of this study was to empirically assess the individual-level effects of SC on subsequent violence and misconduct in general, the timing to subsequent misconduct, and post initial-violence developmental patterns among male inmates. This study provides a unique glimpse into whether SC exposure may play a causal role in subsequent inmate behavior and not intended as a dedicated test of the theories mentioned above.

To achieve this goal data were culled from the prison population of a large southern state (US). Inmate behavior was tracked over time via disciplinary referrals for violent and other forms of misconduct across the first 3 years of each inmate's prison sentence.[2] A counterfactual was developed that matched inmates who received solitary confinement as a punishment in response to an initial act of violence (the treatment group) to similarly situated inmates who also engaged in violence, but did not receive SC as a punishment (the matched-control group).

The initial act of violence was focused upon here for several reasons. First, violent behavior is the form of misconduct most likely to result in punitive segregation, thus providing for greatest amount of usable data. Second, and perhaps most important, was for use as a partial control for confounding via methodological design. An argument can be made that the onset of violent behavior upon incarceration may be a key to exchangeability between inmates with respect to subsequent violence. In other words, the event of violence onset helps to control for confounding from unmeasured biases on the effect exposure to solitary, and its potential downstream impact on behavior (see Greenland et al. 1999). Additionally, the PSM approach follows the recommendation of Smith (2006) to assess the causal implications of SC using both a treatment and a control group of actual prison inmates.

This study does not address the use of SC as a means of protective or administrative segregation and this reality must be considered when interpreting the findings presented

[2] A reviewer appropriately questioned the rationale behind focusing on the initial act of violent misconduct as a queue to SC. The reason behind this approach was to maximize the amount of available data for this exploratory study. The prevalence of violent misconduct, which is the type of misconduct most likely to result in SC, diminishes rapidly through the initial 3 years of incapacitation. Focus on the initial act during the first year maximized the amount of available candidates (for matching purposes) while simultaneously allowing for an adequate amount of time in observation post-SC where violence and misconduct are still relatively likely. The intention here is not to downplay the potential importance of secondary and tertiary experiences with SC, but understanding the downstream effects of initial exposure to SC is here argued as worthy of a dedicated study.

herein. The rationale for excluding cases involving other forms of solitary (i.e., protective and administrative segregation) lies in the idea that the rationale behind the use of each form of isolation is qualitatively different. Punitive segregation is used in response to specific acts of misconduct and is relatively short-term in duration. This is a practice that is commonly used by corrections administrators in general prison settings and is a practice that is amendable to modification driven by validated empirical evidence. Exploring punitive segregation exclusively applies more to general security status prison settings than to specialized prison, such as supermax facilities. Existing evidence, along with relevant theory, is used as a guide for developing hypotheses and for explaining findings specific to an effect of SC on two sorts of outcomes: future violence and its timing, leading to the following hypotheses:

H_1 Inmates found guilty of an initial act of violent misconduct whose punishments include solitary confinement (SC) will be more likely to engage in subsequent violent behavior within 12-months than those not punished with SC.

H_2 Inmates found guilty of an initial act of violent misconduct who are punished with solitary confinement (SC) will engage in subsequent violent behavior sooner than those not punished with SC.

H_3 Inmates found guilty of an initial act of violent misconduct who are punished with solitary confinement (SC) will display increased antisocial development post-initial violence than those not punished with SC.

Relying on Matched Samples to Estimate the Solitary Confinement Effect

The present study employed a technique known as propensity score matching (PSM) to establish a counterfactual [see Rosenbaum and Rubin (1983, 1984, 1985)]. This strategy was developed in tandem with traditional regression-based approaches, event history analyses, and with a trajectory analysis. PSM approximates the conditions of an experiment by establishing "synthetic" treatment and control groups, which are balanced across all available variables known or believed to potentially confound the effect of exposure to SC. The propensity score itself is simply "the conditional probability of assignment to a particular treatment given a vector of observed covariates" (Rosenbaum and Rubin 1983: 41). PSM is appropriate for the study of the SC effect because it is well suited for situations where randomization not feasible, but where the process of assignment to a treatment can be modeled via observed variables that are likely to confound the probability of exposure to the treatment (Holmes 2014). Through this process, the assumption of conditional independence must be considered. This assumption is satisfied only when a reasonable degree of confidence can be established that the assignment process to a treatment is ignorable upon conditioning via the propensity for treatment. This assumption allows for the use of the propensity for treatment (i.e., the estimated probability of having received SC, in this case) to match "treated" inmates to similarly situated inmates who were equally likely to have received SC, but did not.

The matching process involved the identification of an equally likely counterpart (that is, in propensity for treatment) to each treated inmate in terms of the likelihood of having received the treatment. Through this process, there is the possibility that some treated inmates will fail to be matched with a non-treated counterpart, and vice versa. In the present analysis, the 1-to-1 nearest neighbor matching algorithm was employed, without

replacement (see Gau and Fraser 2009 for an overview).[3] The caliper (i.e., the maximum difference between propensity scores among matched pairs) was set to .05 (but findings were indifferent at more restrictive caliper settings), which was one-quarter of the standard deviation of the estimated propensity scores, though the findings did not change with more conservative caliper settings.

In the context of SC, the PSM approach is useful due to the transparent way in which problems surrounding the counterfactual are handled though it is not a substitute for an experimental research design. PSM is used more for the generation of reasonably comparable and balanced samples for comparison provided that the matching is based on strong predictor variables. An advantage of PSM over regression-based approaches lies in the fact that the goal is to produce a strong prediction of the propensity for exposure to a "treatment" and no so much for developing an unbiased model specific to the variables used to estimate propensity. In other words, if there are a reasonable number of predictor variables that are highly correlated with excluded predictors, strong matches are still possible.

By framing the counterfactual in an appropriate way, the experimental design can be approximated, but not duplicated, in terms of the requirement of equal probability of selection to a treatment or control group (i.e., balance/exchangeability). The balancing of propensity for treatment serves to make those who received SC similar to those represented by the counterfactual, thereby approximately isolating the SC effect away from potentially confounding influences, though again, PSM is no substitute for an experiment. Indeed, unobserved confounding may impact the results presented herein since PSM assumes unmeasured confounders are ignorable (discussed further below).

The present study is unique in the fact that: a) longitudinal records allow for the control of the behavioral history of each inmate prior to, and after, the potential for treatment, b) data provided detailed information in terms of inmate characteristics, and c) data specific to the prison unit to which an inmate was assigned was included and allowed for a multilevel matching approach, and d) few if any study have addressed the SC effect in this manner specific to post-exposure behavior. The following section provides an overview of the research methodology used to make this contribution to the literature.

Method

Data

Data for the present study were culled from archived records collected and maintained by the primary corrections agency of a large southern (US) state. Within these records were data on each inmate's demographic characteristics, criminal history, official disciplinary histories specific to each act of prison misconduct as well as the corresponding administrative response (i.e., penalties received), inmate custody classification data, and information specific to the prison unit to which an inmate was assigned. These data were limited to account only for adult males entering a state prison, a state jail, or a state contracted private prison facility between May 2004 and May 2006. Data were further limited to those sentenced to at least 3 years *and* who were incarcerated for those first 3 years consecutively (N = 24,331). It is worth noting that in this particular state, prison units are not

[3] Matching was also carried out using the Mahalanobis distance approach as well as using multiple neighbors. The results of the study did not change by doing so.

security-level specific (i.e., each prison unit houses minimum through maximum custody inmates) and each prison may utilize SC in response to serious misconduct. Inmates sentenced to capital punishment or life-without-the-possibility-of-parole were excluded. Inmates officially confirmed as prison-gang members were also excluded as were those housed in administrative segregation (aka *ad seg*). Gang members were excluded for two reasons: a) the data did not indicate *when* the inmate was officially classified as a gang member and b) confirmed gang members are often housed in administrative segregation, not in the general population. Administrative segregation inmates were excluded because their opportunity for misconduct (particularly violent misconduct) is limited since they are not exposed to the general population. Beyond these criteria, data were further limited to inmates written up for a violent infraction at any point during the first 2 years of their prison sentence and who had not been previously subjected to SC (the rationale for this strategy is discussed in more detail below). This strategy resulted in a final pool of 3,808 inmates nested within 70 prison units who were found guilty of a violent infraction during the risk set, and thus eligible for observation via the above outlined criteria. The range of eligible inmates housed in the prison units was between 15 and 276 inmates.

Measurement

Treatment Variable

The treatment variable was exposure to solitary confinement immediately following the initial act of violent misconduct (as a punishment), occurring at any point during the first 2 years of incarceration. The first 2 years of imprisonment was chosen as the risk set for experiencing SC to allow for ample observation (1 year) post-exposure. As noted, it is the first 3 years of incarceration when violence is most likely. Officially, the state defines SC as a segregated custody status resulting from a major disciplinary infraction and hearing. According to policy, SC should become available when all other forms of discipline have been attempted, where safety is a concern, and/or when misconduct is so serious as to warrant SC. Inmates in SC are allowed out of their cell to shower once per day. Inmates may only be housed in solitary for up to 15 days, and if the punishment calls for extended time, 72 h must separate each 15-day block (i.e., the inmate is returned to the general population in the interim). In addition, inmates in SC are not allowed visitors, though there are some exceptions (e.g., visits from a chaplain for religious purposes). These inmates are responsible for notifying visitors on his "visitors list" of the situation and that visitation will not be allowed for the duration of the punishment. According to state records, the majority of SC placements were subsequent to an act of violent misconduct, however, SC is sometimes used in response to non-violent misbehavior such as disobeying orders, being disruptive, making threats, and other minor incidents.[4] The analyses here were limited to inmates found guilty of violent behavior for the reason that violent behavior is more likely to be responded to via SC as a punishment and for the reason that inmates engaging in violence are arguably more exchangeable in terms of establishing a counterfactual (discussed further below). Here, 1,076 of 3,808 (28.3 %) inmates received SC following their first reported act of violent misconduct; these individuals represent the treatment group. The control group did not receive SC in response to their initial violent infraction, but may

[4] The data available accounted only for whether and when a penalty of SC was applied. Details on the duration of specific instances (i.e., number of days) of SC were not available.

have received an alternative form of punishment (e.g., forfeiture or suspension of good conduct time credit).[5]

Outcome Measures

The outcome variables of the present study consisted of: (a) the *re*-occurrence of a violent infraction at any time during the 12-month period *following the initial violent infraction,* (b) the number of days until the second violent infraction (if occurred), and c) the developmental trajectory profile of misconduct for 180 days post-initial violent act—each conditional on the above noted criteria. Specifically, a violent infraction was defined by an official report of physically assaultive behavior that targeted another inmate or a staff member and resulted in at least a minor injury (threats of violence were excluded). Of the 3,808 inmates reported for an initial act of violence, 1,099 (28.9 %) were also reported for violence occurring at some point during the 12-months *post*-initial violence. The secondary outcome variable was represented by the number of days between the first and second violent infractions and was limited only to those inmates who were reported to have engaged in violence for a second time during the observation period (mean = 287 days; SD = 259 days)—but see below. The establishment of post-initial violence misconduct trajectories and other subsequent analyses (e.g., for non-violent misconduct) are discussed below in more detail.

Inmate-Level Covariates

The data provided information on a large array of inmate characteristics and demographics recorded upon entry to the facility (discussed below). Included as covariates for the matching procedure were the inmate's age at entry to prison (Entry Age; in years); race (dummy variables for Black, Hispanic, White, and other—with White/other serving as the reference category); marital status (Married: yes = 1; no = 0); having been born in the US (US Native: yes = 1; no = 0); the inmate's intelligence score (IQ; WAIS-R); educational attainment based on a grade-level equivalency examination (Education; ranged from 0 to 13 where 0 refers to less than first grade equivalence and 13 refers to more than a high school degree); body mass index (BMI); sentence length (Sentence Length; in years—log transformed to correct for skew); prior incarceration history (Prior Incarceration: yes = 1; no = 0); the number of days an inmate was incarcerated prior to the initial violent act (Days to Violence; right censored); misconduct history—the frequency of a range of non-violent acts of misconduct occurring prior to the initial violent act;[6] and, the offense of record type (i.e., the offense by which the inmate was imprisoned—dummy variables for Violent, Property, Drug, or Other offense—with Violent/Other offenses serving as the reference category). Finally, the number of pre-initial-violence punishments received, short of solitary confinement, was also controlled for.

[5] The loss of good-time credit was accounted for in separate analyses (not shown), but had no impact on the findings presented below. However, the loss of good time may have its own effect on the probability of continued violence and should be the topic of a separate study exploring causal factors of continuity in prison violence.

[6] Regarding preceding misconduct, the frequency of property, accountability, security, sexual, contraband, and substance-use-related infractions were taken into account. The specific infractions underlying each of these infraction categories were based on the protocol presented in Camp et al. (2003: 532).

Prison-Level Covariates

At the prison-unit level (n = 70), covariates included the average IQ score of inmates within the unit (Unit IQ; see Diamond et al. 2012); the physical age (in years) of the prison unit itself (Unit Age); the rate at which solitary is imposed for acts of violent misconduct (Unit Solitary Rate); the mean age of inmates assigned to the unit (Avg. Inmate Age); the mean sentence length of inmates assigned to the unit (log transformed to correct for skew; Avg. Sentence Length); and, the ratio of prison staff to inmates (Staff Ratio). Descriptive statistics are presented in Table 1.

Table 1 Descriptive statistics

Variable	Full sample (n = 3,808)				Exposed to solitary (n = 1,372)			
	Mean	SD	Min.	Max.	Mean	SD	Min.	Max.
Social demographics								
Education level	6.64	3.84	0	13	6.77	3.81	0	13
Age at entry	27.37	8.51	15.26	76.76	27.94	8.97	15.52	76.76
Black	0.37	0.48	0	1	0.35	0.48	0	1
Hispanic	0.4	0.49	0	1	0.43	0.49	0	1
Married	0.11	0.31	0	1	0.11	0.31	0	1
IQ	88.42	13.33	49	142	85.95	12.93	54	130
BMI	27.36	4.89	16.36	81.37	27.4	4.73	16.36	51.01
Criminal history								
Days to violence	301.8	202.14	0	730	285.65	194.88	0	729
Prior incarceration	0.25	0.43	0	1	0.25	0.43	0	1
US native	0.92	0.28	0	1	0.91	0.28	0	1
Sentence length (log)	8.23	1.43	6.59	16.12	8.28	1.52	6.59	16.12
Drug conviction	0.11	0.32	0	1	0.1	0.3	0	1
Property conviction	0.13	0.34	0	1	0.13	0.34	0	1
Misconduct history								
Property	0.26	0.85	0	10	0.21	0.7	0	6
Accountability	5.6	9.2	0	90	4.45	7.34	0	72
Security	1.75	3.94	0	52	0.96	2.39	0	24
Sexual	1.3	5.06	0	95	0.6	2.59	0	45
Contraband	2.75	5.55	0	54	2.39	5.02	0	42
Substance use	0.32	1.73	0	28	0.22	1.53	0	28
Threat	0.14	0.54	0	11	0.03	0.2	0	3
Pre-violence punishments	5.96	8.88	0	57	4.34	6.37	0	56
Unit demographics								
Avg. unit IQ	86.13	3.96	61.96	91.7	86.7	2.35	81.18	91.7
Unit age	22.69	25.95	9	157	23.37	28.13	9	157
Unit solitary rate	0.1	0.07	0	0.46	0.13	0.08	0.01	0.46
Avg. inmate age	34.58	4.01	25.07	48.85	34.81	4.09	25.07	48.85
Avg. sentence length	14.01	23.82	0.09	179.38	12.87	22.98	0.09	179.38
Staff-to-inmate ratio	0.18	0.08	0.09	0.83	0.17	0.06	0.09	0.52

Observation Strategy

In terms of isolating an SC effect on the subsequent violence outcome, observation of inmate behavior began immediately following the initial violent infraction. This starting point, or entry to the risk-set, could commence at any time during the first 2 years of the prison sentence. Upon entry to the risk-set, regardless of whether a penalty of SC was experienced, each inmate was observed for the following 12 months with the possibility of observation extending no further than the 36th month of incarceration (e.g., in the event that the initial act of violence occurred in the 24th month). During this 12-month post-infraction observation period, each inmate either committed at least one additional violent infraction, or he did not. This strategy is illustrated in Fig. 1 based on five hypothetical inmate observational scenarios and applies equally to the non-violent misconduct outcomes addressed herein. The rationale for the 12 months of observation post-initial violence is two-fold. First, the 12-month window standardized the observation period so all inmates sampled could potentially be observed for 12 months following the initial act of violence—regardless of when the first violent act occurred during the initial 24-month observation period—maximizing the sample size for the situation and time frame. Second, violent misconduct prevalence declines monotonically throughout the first 3 years of incarceration among male inmates and is considerably rare during and after the third year of incarceration (e.g., Griffin and Hepburn 2006; Morris et al. 2012). Figure 1 graphically illustrates the protocol for entry to the risk-set and subsequent observation.

Establishing the Counterfactual and the Diagnosis of Comparability

The effect of exposure to SC on the presence and timing of subsequent misconduct was assessed via a quasi-experimental counterfactual design referred to as propensity score matching (PSM). Here, both the treatment and control groups are balanced on a vector of attributes (i.e., confounders) making the expected probability of receiving the treatment balanced—assuming conditional independence. This assignment mechanism ends up being a statistically random process that renders differences between treatment and control groups ignorable (i.e., a counterfactual)—see Rosenbaum and Rubin (1983, 1984, 1985).

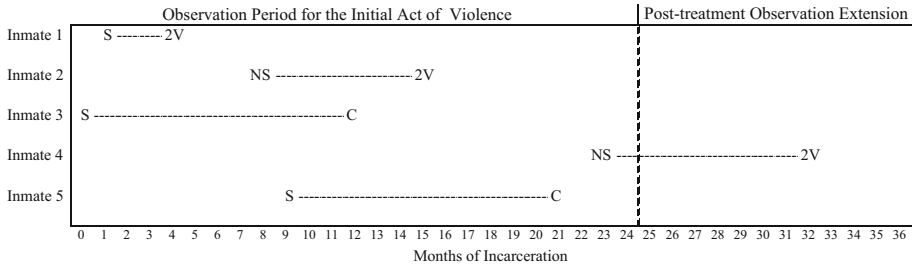


Fig. 1 Example of observational scenarios for five hypothetical violent inmates. The post-treatment observation extension applies only to those inmates whose initial act of violence occurred between months 13 and 24

Table 2 Balancing statistics

	Difference between violent inmates subject to solitary (n = 1,072) and those not (n = 2,736)			Difference between violent inmates subject to solitary (n = 917) and those not (n = 917)			Multilevel logit model for propensity score estimates	
	Before matching			After matching			Odds ratio	SE
	Absolute Difference	*t*	*D*	Absolute Difference	*t*	*D*		
Social demographics								
Educational level	0.18	1.47	4.70	0.14	0.87	3.60	0.98	.045
Age at entry	0.79**	2.89	9.10	−0.37	−1.03	−4.30	1.03	.006
Black	−0.03*	−2.15	−6.90	−0.02	−0.78	−3.20	1.03	.118
Hispanic	0.04*	2.28	7.30	0.01	0.29	1.20	1.09	.060
Days to violence	−22.55**	−3.50	−11.30	−10.43	−1.26	−5.20	0.92^{+}	.000
Married	0.00	0.00	0.00	0.01	0.54	2.30	0.93^{+}	.035
IQ	1.04*	2.15	7.80	0.33	−0.85	−4.00	0.99	.047
BMI	0.05	0.34	1.10	−0.19	−0.95	0.20	0.92*	.038
US native	−0.01	−0.70	−2.20	0.01	0.81	3.40	1.05	.045
Criminal history/sentencing								
Sentence length (log)	0.07	1.62	5.10	−0.01	−0.12	−0.50	1.02	.045
Prior incarceration	0.01	0.37	1.20	−0.02	−1.20	−5.00	1.00	.045
Drug conviction	−0.02	−1.54	−5.00	0.00	−0.20	−0.80	0.95	.040
Property conviction	0.00	0.10	0.30	0.01	1.04	−4.40	1.02	.051
Property misconduct	−0.08**	−2.80	−9.40	−0.01	−0.18	−0.60	1.04	.064
Accountability misconduct	−1.60**	−5.46	−18.50	−0.27	−0.78	−3.10	0.99	.007
Security misconduct	−1.10**	−8.84	−31.30	−0.09	−0.85	−2.60	0.82**	.061
Sexual misconduct	−0.98**	−6.09	−22.00	−0.03	−0.24	−0.60	0.71**	.064
Contraband	−0.51**	−2.90	−9.50	−0.07	−0.34	−1.30	0.98	.049
Drug misconduct	−0.14**	−2.62	−8.60	−0.01	−0.18	−0.70	0.97	.042
Threat misconduct	−0.15**	−9.06	−33.60	−0.01	−1.00	−2.10	0.57**	.063
Pre-violence punishments	−0.11**	−7.24	−30.40	−0.01	−0.12	−0.20	1.06	.065
Prison unit demographics								
Avg. unit IQ	0.21**	6.40	22.70	0.00	0.01	0.00	1.19	.177
Unit age	−0.09**	−2.83	−8.80	0.00	−0.06	−0.30	0.75**	.072
Unit solitary rate	0.61**	19.99	62.40	−0.08	−1.85	−7.80	1.84**	.181
Avg. inmate age	0.08**	2.53	8.00	−0.07	−1.75	−7.20	1.09	.110
Avg. sentence length	−0.02	−2.11	−5.00	−0.01	−0.77	−2.20	1.21^{+}	.132

Table 2 continued

	Difference between violent inmates subject to solitary (n = 1,072) and those not (n = 2,736)			Difference between violent inmates subject to solitary (n = 917) and those not (n = 917)			Multilevel logit model for propensity score estimates	
	Before matching			After matching			Odds ratio	SE
	Absolute Difference	*t*	*D*	Absolute Difference	*t*	*D*		
Staff-to-inmate ratio	−0.13**	−4.02	−13.70	−0.03	−0.89	−3.10	0.73*	.118

Variance component for multilevel *logit* model was .53 with a standard error of .13. AUC = .77

$^{+}$ $p \leq .10$; * $p \leq .05$; ** $p \leq .01$

Here, the propensity scores were estimated using a multilevel logistic regression model that predicted the probability (or propensity) of having received SC as a direct result of engaging in violent behavior for the first time upon incarceration (results presented in Table 2; ROC presented in Fig. 2; AUC = .77). It is well known that considerable levels of between-prison variation in the odds of inmate misconduct exist and should be accounted for (e.g., Wooldredge et al. 2001). After assessing an empty multilevel model (an intercept-as-outcome model) to determine whether sufficient variation in treatment probability significantly varied across prison units (intraclass correlation for binary outcome = .280), exposure to the treatment was modeled via a host of predictor/confounder variables (outlined above). The multilevel propensity score estimation model is represented via Eq. 1. Each predictor was grand-mean-centered prior to analysis. It is also important to reiterate that all predictors of the treatment represent a time period antecedent to the initial act of violence and the possibility of receiving the treatment.

$$
\begin{aligned}
\Pr(\text{Solitary}) = {} & \beta_0 + \beta_1(\text{Education}) + \beta_2(\text{Age}) + \beta_3(\text{Black}) + \beta_4(\text{Hispanic}) \\
& + \beta_5(\text{Days to Violence}) + \beta_6(\text{Married}) + \beta_7(\text{IQ}) + \beta_8(\text{BMI}) \\
& + \beta_9(\text{US Native}) + \beta_{10}(\text{Sentence Length}) + \beta_{11}(\text{Prior Incarceration}) \\
& + \beta_{12}(\text{Drug Conviction}) + \beta_{13}(\text{Property Conviction}) \\
& + \beta_{14}(\text{Property Miscond.}) + \beta_{15}(\text{Acct. Miscond.}) + \beta_{16}(\text{Security Miscond.}) \\
& + \beta_{17}(\text{Sexual Miscond.}) + \beta_{18}(\text{Contraband}) + \beta_{19}(\text{Drug Miscond.}) \\
& + \beta_{20}(\text{Threat Miscond.}) + \beta_{21}(\text{Pre} - \text{violence Punishments}) + r \\
\beta_0 = {} & \lambda_{00} + \lambda_{01}(\text{Avg. Unit IQ}) + \lambda_{02}(\text{Unit Age}) + \lambda_{03}(\text{Unit Solitary Rate}) \\
& + \lambda_{04}(\text{Avg. Inmate Age}) + \lambda_{05}(\text{Avg. Sent. Length}) + \lambda_{06}(\text{Staff Ratio}) \\
\beta_{\text{k}} = {} & \text{fixed}; \quad \text{for k} = 1 \text{ through } 20
\end{aligned}
\tag{1}
$$

The distribution of estimated propensity scores between inmates who received SC for the initial violent act compared to those who did not demonstrated ample overlap across the full range of propensity scores between the treated group and potential matched controls (see also Fig. 2).

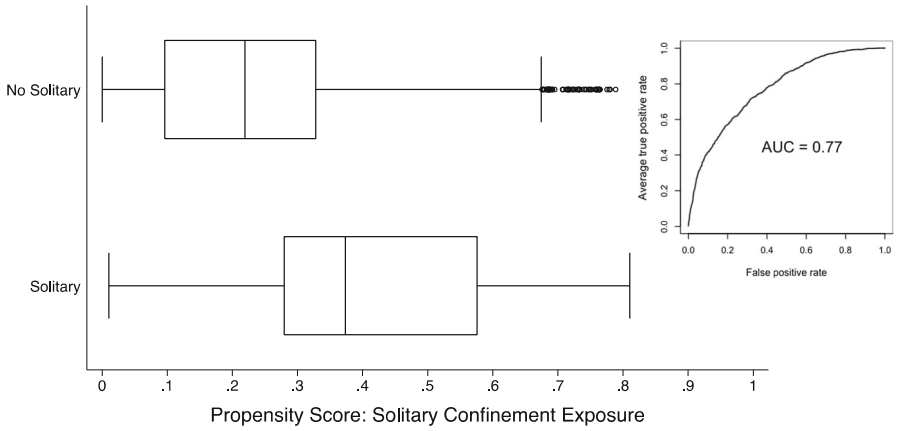


Fig. 2 Predicted probability (propensity) for receiving solitary post-initial violence between treatment and control groups with ROC Curve

The next step involved the assessment of balance across the set of covariates between treated cases and their matched controls. This assessment was carried out by exploring independent samples *t* tests for each covariate both before and after the matching procedure was carried out along with standardized difference (*D*) statistics (Rosenbaum and Rubin 1985). Balance is achieved if *t* statistics among the matched sample are smaller than 1.96 in absolute value and less than 20 for *D* statistics –see Table 2. As shown, the matching procedure was successful in balancing the data for a counterfactual assessment of the SC effect.

Empirically Assessing the Solitary Effect

Independent samples *t* tests were calculated in order to determine whether the SC effect on subsequent violence/misconduct (i.e., the difference in prevalence) was statistically significant. Regarding whether SC impacted the timing to subsequent violence/misconduct, a *t* test was again employed to test whether exposure to SC affected the number of days to subsequent violence/misconduct over the 12-month post-treatment/control observation period. This specific procedure was limited to those inmates who were subsequently reported for a violent act or for any act of misconduct, respectively, 12-months *post*-initial violence (n = 2,957)—but see below discussion of supplemental analyses. Also estimated were a series of Cox regression models assessing the solitary effect on the timing to subsequent violence/misconduct, if any, as well as an assessment of the SC effect across post-initial violence misconduct trajectories (also discussed below).

Results

Table 3 presents the results for the main analysis, namely the test results for the difference between the outcomes for treated versus non-treated individuals from both matched and unmatched samples. Shown here, the unconditioned probability of secondary violence suggested a stronger likelihood of violence for inmates not penalized via SC, which

Table 3 Solitary confinement difference test results

	n	Mean (Treated)	Mean (Controls)	Difference	Standard Error	*t* statistic	95 % CI of ATT
Subsequent violence outcome							
Unmatched	3,808	0.254	0.302	−0.048	0.016	−2.95**	–
Matched	1,830	0.267	0.254	−0.012	0.021	0.59	−.043, .039
Days to subsequent violence outcome among those who were subsequently violent							
Unmatched	1,609	314.231	284.002	30.228	14.887	2.03*	–
Matched	728	305.750	303.574	2.176	19.077	0.11	−57.9, 15.5

Results presented for matched differences were based on bootstrap replicated analyses (1,000 repetitions)
* $p < .05$; ** $p < .01$ (two-tailed)

counters extant literature. Post-matching, however, the direction of the conditional SC effect turned to the expected direction but was not statistically significant.[7] To further assess the robustness of this finding, confidence intervals for the difference between treated versus control outcomes were calculated via bootstrap replication of the PSM models and an appraisal of such helps to illustrate the SC effect. Of 1,000 bootstrapped assessments of the subsequent violence PSM model, only 53 (or 5.3 percent) of runs resulted in what we would consider a statistically significant difference in mean subsequent violence between treated and control groups (based on doubling the standard error of the bootstrapped ATT, which is ±.04 in magnitude). In spite of the rarity of reaching this level of statistical significance, the practical significance of the effect was arguably non-substantive. Plainly, the bootstrapping approach suggested that in 5.3 percent of 1,000 repeated analyses would we find that exposure to solitary had an effect in either direction assumed not due to chance. In such cases, the proportion of inmates that would experience an effect from solitary was quite small (less than 4 percent; i.e., the treatment effect size [ATT]). Therefore, these results suggest that exposure to solitary confinement for acts of violence tends to have no substantive effect on continuity in violent behavior among inmates engaging in initial violence, but for approximately 2 percent of exposed inmates we would expect that exposure to solitary may have increased their tendency for continued violence and for an equivalent proportion, the opposite might be expected, for a total of 4 percent being affected by the solitary experience on average.

The same analysis protocol was extended to explore the SC effect on the time lapse (in days) between initial and secondary violence among those who *did engage in subsequent violence* within 12-months post-initial violence. These findings also indicated no substantive difference in the timing to subsequent violence between inmates exposed to SC and those who were not.

Within the framework of counterfactual modeling, it is important to consider the possibility for influence from hidden (unmeasured) bias as potentially confounding test results (see Becker and Caliendo 2007; Greenland et al. 1999; Gau and Fraser 2009; Rosenbaum 2002). In response to this concern, the sensitivity assessment protocol suggested by Rosenbaum (2002) was carried out following each analysis. This approach, referred to as

[7] It is also worth noting that a regular multilevel regression model was estimated that simply accounted for the solitary effect as a predictor of subsequent violence among all inmates in the sample. The parameter estimate for exposure to solitary was not significant. Results from this model can be made available upon request.

"Rosenbaum bounds", assesses how powerful an unmeasured covariate would need to be in order to render the PSM results invalid (adjusted for binary outcomes where applicable). In each case here, the coefficient gamma was a modest 1.3, suggesting that an unmeasured covariate, in the worst-case scenario (see Becker and Caliendo), would need to be highly correlated with the outcome, and have an odds ratio of 1.3 or greater to invalidate the results (note that only one predictor in the regression model—the prison unit's solitary rate—exceeded 1.3 in odds ratio). While bounds statistics are not a cure for unavailable covariates, the design and results here suggest a reasonable level of robustness against the influence of unmeasured covariates or hidden/unmeasured bias, provided they are highly correlated with those unmeasured (additional limitations discussed below).

Regarding the time to event assessment, a series of Cox regression (i.e., survival) models were estimated across varying time-at-risk (i.e., post initial violence) thresholds (30, 60, 90, 120, 150, and 365 days, respectively) using SC as a sole covariate. This was done to assess temporal variation in a potential solitary effect (e.g., whether there may be an effect limited to the period soon after the initial act, etc.) and was carried out to provide another way of looking at this issue, not necessarily a better one. The results indicated a null effect for exposure to SC across the board for both the complete sample and for those who did commit subsequent violence, respectively.[8]

Supplemental analyses were also carried out extending the final outcome measure to a count of all types of infractions (i.e., including non-violent misconduct) reported post-initial violent act (results omitted to save space). Regarding all forms of subsequent misconduct, no differences were found between those exposed to SC for their initial violent act and their matched controls.

Each of the above noted analyses were also re-estimated based on propensity scores that excluded the prison-level covariate *solitary rate*. This was done in order to gauge whether the prison-unit rate for SC was so powerful as to confound any isolated individual-level effect from SC that may be driven by a handful of specific prison units rather than prisons at-large. In doing so, the solitary effect did not change. In response to a reviewer's suggestion, the models were further limited to only those with increased numbers (greater than median and then above the 75th percentile) of non-solitary punishments for misconduct prior to the initial act of violence. In doing so, the results did not change (not shown in tables).

Developmental Approach

While the results presented to this point are comprehensive, they do not assess the solitary effect via a within-individual (i.e., change over time) framework. Accordingly, the solitary effect was explored via a trajectory modeling approach wherein latent trajectory profiles of behavior post-initial violent act of misconduct (i.e., all misconduct occurring post- initial act) were established. Observation for this approach consisted of 180 days post-initial violence, demarcated by 30-day increments (a total of 6 time periods) for both the treatment and matched control groups.

As shown in Fig. 3a, assuming no within-group heterogeneity in development post-violence (i.e., 1-group), the trajectories are quite similar suggesting that in general the development of misconduct post-initial violence is similar between those experiencing solitary for the initial violent act, and those not. Indeed, modeling the treatment variable in

[8] See Morris et al. (2013) for a discussion of why survival analyses involving re-offending should be explored between contexts of survivorship.

a growth-curve model (i.e., multilevel model for change) found no statistically significant impact from SC on misconduct across the post-violence time frame.

The analysis was further extended to account for potential underlying between-group (i.e., latent) heterogeneity post-violence using finite-mixture-modeling (see Nagin 2005). Here, the zero-inflated Poisson link function was assumed. This approach was carried out to provide for a within- and between-individual assessment of the solitary effect on latent developmental profiles of misconduct post-initial violence. Posterior probabilities and Bayesian information criterion (BIC) statistics suggested retaining a 3-class solution where each group's functional form was specified as quadratic. Upon identifying the 3-class solution, the treatment variable was modeled on group membership in a multinomial logit model (results omitted). SC was not found to predict group membership within any of the three trajectory classifications—see Fig. 3b.

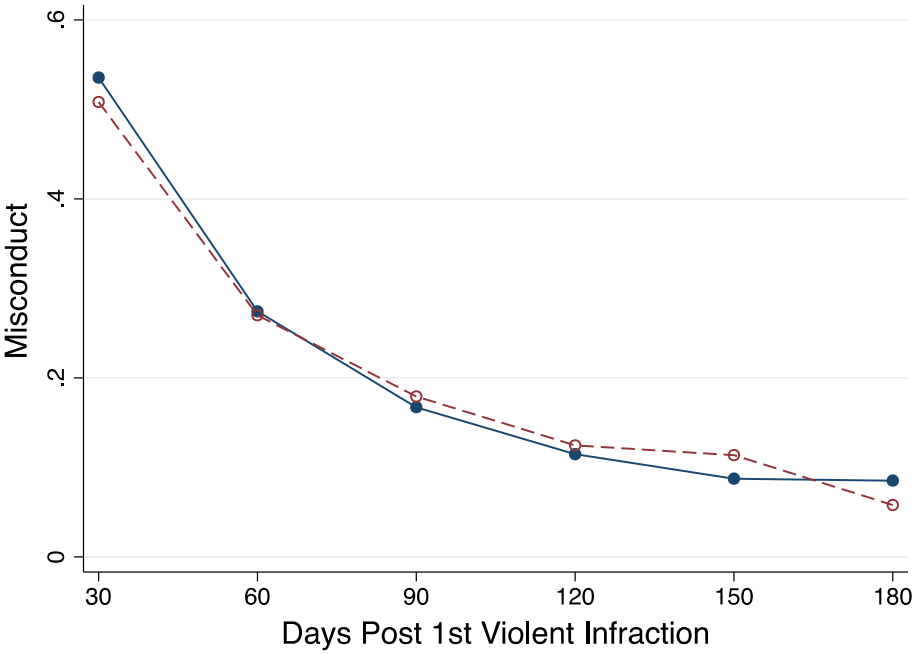
Discussion

The results from this study stemmed from a multilevel counterfactual modeling approach (propensity score matching) that was used in an attempt to assess the effect of SC on subsequent violence/misconduct, the timing to subsequent violence/misconduct, and the developmental pattern of subsequent violence/misconduct. The analyses here were specific to one large state's population of male inmates who engaged in violence not long after being incarcerated. Nearly all treated inmates (i.e., those exposed to SC) were successfully matched to similarly situated non-treated inmates. Several hypotheses were established based on expectations of positive effects (i.e., aggravating) on subsequent violence post-SC. The findings are relatively robust in suggesting that on average, the initial experience with SC alone (i.e., as a direct and independent effect) may not play a causal role in subsequent physical violence, its timing, or its downstream effect on misconduct development. In tandem these findings suggest neither a positive nor a negative relationship between SC and subsequent violent behavior, and for misconduct in general (post initial-violence). It is important to consider that these findings may not apply to all inmates who demonstrate violent behavior upon incarceration. For example, supermax and administrative segregation inmates are exposed to solitary confinement for much longer durations, making them contextually unique compared to SC as defined here.

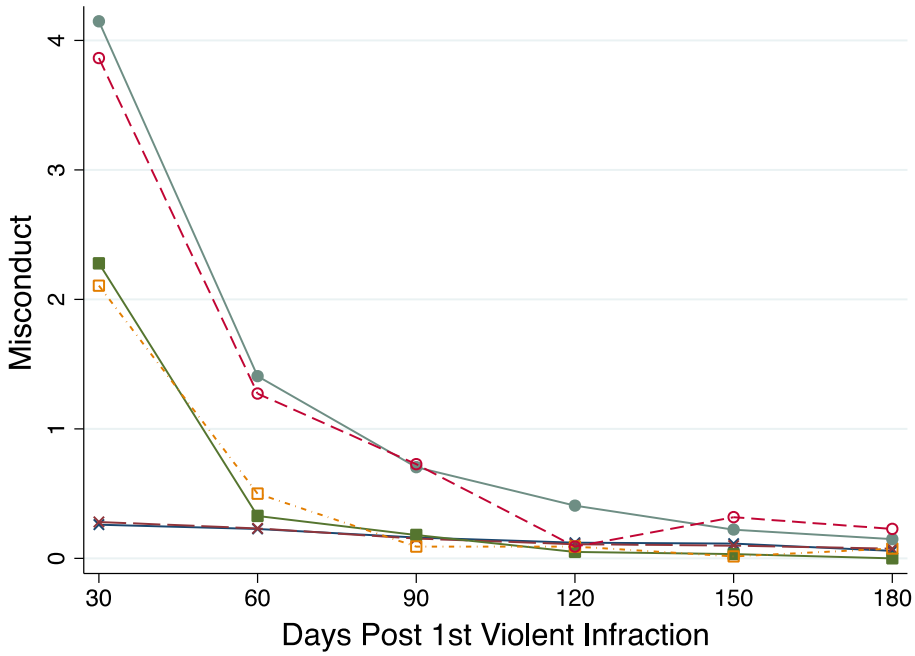
Limitations notwithstanding, this study is unique in the fact that it employed a relatively robust method that involved a large sample, longitudinal data, controls for selection bias, a treatment group and a control group and several formats of the outcome (i.e., prevalence, count, timing, and development). By modeling the SC effect through a counterfactual, the potential for biased estimates became increasingly ignorable, though not to the degree of a randomized experiment. In doing so, this study fills an important gap in the empirical literature given the few studies specific published to date.

A review of these findings warrants discussion of potential explanations of the results as to why they run counter to the study hypotheses. More specifically, why would SC be found to have a generally null effect on subsequent violent behavior and its timing? Correctional officer discretion may play a role in the lack of an SC effect on subsequent violence. Prison security staff may use discretion in the determination as to whether an inmate will receive a penalty of SC for an act of violence. That is, certain staff may either over- or under- subject inmates to SC in response to early violent acts; in addition, this effect may depend on factors such as the work shift, administrative tone, etc. Some research suggests that factors such as race may play a role in decisions to formally respond

(a)


Misconduct
0
.2
.4
.6
30
60
90
120
150
180
Days Post 1st Violent Infraction

(b)


Misconduct
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180
Days Post 1st Violent Infraction

◀**Fig. 3** Trajectory analysis of post-initial violent act misconduct between matched samples. **a** 1-Group model, **b** 3-group model. *Solid lines* are the SC (i.e., treated) groups

to misconduct (Irwin 2005; Mears and Bales 2009), though race was not explicitly addressed in the present study. Indeed, violent acts committed by prisoners will vary in context and minor injuries might mean something different from one guard or prison unit compared to another. Here it was assumed that this effect, if any, is a random process between prison units (after matching) and therefore does not impact the null findings presented herein at least as it applies to a SC response to initial violence. Between prison-unit differences were explicitly accounted for via the multilevel model used to calculate the propensity scores.

Considering the possibility that SC is used in an "emergency room" kind of response to events could also explain the lack of findings here. In such cases it is unclear whether short bouts of SC would be expected to have any impact at all on subsequent behavior. Here it must be assumed that SC in general among this population of inmates for emergency room situations is also a random process. In any case, this is a reality that must be considered and it would have been worthwhile to assess misconduct while physically undergoing isolation, unfortunately those data were not available here.

In sum, these findings suggest that subjecting inmates to short-term SC in response to initial acts of violence tends not appear to have direct consequences on the likelihood or timing of subsequent violence and other maladaptive behavior, or on misconduct development during the 6-months following initial violence. In the end, these results seem credible for the reasons that: (a) they are based on matched samples from an entire state's inmate population, (b) stem from a counterfactual research design, (c) rely on varying and complementary analyses, and (c) account for selection bias as far as the data allow. However, it is equally important to address why these results may be questionable.

A potential problem within counterfactual research designs, including some experimental designs, is the issue of unobserved confounding (i.e., unmeasured covariates) and to what extent unmeasured features of propensity for a treatment may influence the matching process, and ultimately the end results (Greenland et al. 1999). In the case of PSM, the propensity score (for treatment) contains error due to either having excluded potentially important features or due to measurement error in those features included (see Stürmer et al. 2005). In the present study, several potential confounders were unavailable (e.g., mental health features, substance use history, duration of exposure, etc.), perhaps calling into question these results due to imbalance between those exposed to solitary and those not. However, it is unclear whether unmeasured features would have actually confounded the PSM protocol. There are currently no known validation studies that have assessed confounding influences of exposure to solitary confinement in prison settings. The lack of information on mental health is particularly important as mental health disorders may impact both the likelihood for violent behavior, as well as the likelihood of being sentenced to solitary confinement (Smith 2006).

Another aspect to consider is that some evidence suggests that disciplinary referrals may not reflect the actual amount of violence that occurs in prisons (e.g., Minke 2012). This dark figure of violence is also a limitation of the data available, which were drawn from official records. In any case, this study presents a rare look into the individual level effects of short-term SC on subsequent behavior while incarcerated among violent inmates, though alternative matching scenarios and the unavailability of other potentially confounding influences are caveats that readers must consider.

Regardless of these findings, the medical model of SC has produced a considerable body of empirical evidence that suggests SC exposure is harmful. In this case it appears that a single dose of SC may not have short- or long-term effects on subsequent misconduct, its timing, or subsequent development other than the obvious: temporary removal of the inmate from the general prison environment. From a corrections administration standpoint, one might consider these findings in support of the use of short-term SC for the reason that: (a) it removes the inmate from the general population and staff when danger is more probable, and (b) it does not tend to increase or decrease the odds of subsequent violent and misconduct in general. From a corrections expenditures perspective, these findings call for a dialog regarding considerations for blanket policies specific to SC tailored around the idea that short-term SC should be limited to being a last resort response and perhaps limited to shorter rather than longer intervals, particularly for initial violent acts. In this case, the findings suggest that reductions in the use of SC may result in no loss to any deterrent effect from SC and the cost-savings from such reductions could be redirected to alternative efforts that show more promise toward reducing prison violence (see also Kleck et al. 2005).

Policy makers might also consider solitary as potentially having context-based effects depending on exposure time. For example, scholars have yet to explore how and whether SC exposure duration may impact future behaviors other than violence. If solitary must be used as an immediate tool for temporary isolation, short of supermax transfer or administrative segregation, then it would be worth understanding whether exposure to SC for a single day is comparable to that of 15 days, and so on. If there is no difference, then there is no reason to keep an inmate in solitary for any more than 1 day, or until tempers have settled. It is also worth considering and evaluating whether SC aggravates or mitigates other punishments in terms of influencing future behavior (e.g., a combined penalty), including criminal behavior post-release from prison.

Scholars should continue to explore the effect of SC on a variety of outcomes specific to prison life and beyond. Field interviews and self-report surveys with inmates who have experienced SC will be useful in this regard by providing a better understanding of the sociological, structural, and psychological facets of how SC operates in terms of influencing future behavior. Alternative outcomes are also important to consider including recidivism, mental health, and perhaps its effect on future disciplinary hearing outcomes.[9] Exploring varying pathways to SC exposure are also important to consider since SC can result from non-violent misconduct. It would also be necessary to include contextual analyses similar to those presented here by considering important factors including race, gender (see Blackburn and Trulson 2010), mental illness, substance abuse, intelligence, etc., and to account for between-prison-level effects in the likelihood of receiving SC, as well as what that experience may actually entail in practice.

Conclusions

Limitations notwithstanding, this study found that exposure to short-term solitary confinement, following an initial act of prison violence, did not tend to impact the likelihood of future violence and/or misbehavior among male inmates from a large state's general

[9] An important study by Mears and Bales (2009) found that exposure to supermax incarceration alone (but not the number of times/duration, or whether it occurred close to release) may increase the odds of violence recidivism among Florida inmates.

inmate population. From a policy standpoint, these findings call into question the utility of punitive segregation policies that might allow for arbitrary discretion in subjecting inmates to solitary in terms of the frequency and/or duration of exposure, though validation is needed. Equally, these findings question policies founded on the idea that exposure to short-term solitary confinement deters future violence and/or maladaptive behavior as well as those surmising that exposure to solitary confinement exacerbates maladaptive behavior in the short term. It is hoped that this study will add to the debate on the utility of punitive segregation and that health related aspects of solitary confinement are also considered.

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