

Involuntary Outpatient Commitment: Current Evidence and Options

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Contents

	<u>Page</u>
Acknowledgements	
Executive Summary	1
1. Purpose and Scope	2
2. Outpatient Commitment: Origins, Types, and Controversies	3
3. Evidence Review of Outpatient Commitment Research	7
Approach to Evidence Review	7
Hospital Outcomes	17
Medication Possession and Treatment Engagement	21
Arrests	23
Cost Savings	26
4. Options to Outpatient Commitment	33
Conditional Release	33
Competency Determinations	38
Guardianship or Conservatorship	41
Psychiatric Advance Directives	45
Summary of Alternatives	47
5. Conclusions	48
Appendix	
A. Author Biographical Sketches	50
B. Characteristics of Studies Reviewed	51
References	63

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Executive Summary

Outpatient commitment (OPC) is a civil law mandate ordering an individual to obtain psychiatric treatment against one's will or risk sanctions up to and including forced hospitalization. Our review indicates that there is moderately strong evidence about the effectiveness of OPC for persons with severe mental illness in reducing admissions to psychiatric hospitals and in engaging recipients in community-based services. However, on the basis of current evidence, there is only weak support for the idea that OPC reduces criminal justice involvements or saves states money.

These conclusions come from a review of numerous publications associated with the three major U.S. studies conducted over the past fifteen-years in North Carolina and in New York. We reviewed the published evidence about the effectiveness of OPC in comparison to usual care with regard to use of psychiatric hospitals, medication possession, arrests, treatment engagement, and cost savings for the states. We also examined published evidence from other sources on several alternatives to OPC including conditional release, guardianship, competency determinations, and advance directives.

The three research investigations of OPC that were reviewed include the Bellevue Pilot Study (Steadman, et al., 2001) conducted with individuals released from a public general hospital with psychiatric services in New York City; the Duke Mental Health Study (Swartz, et al., 1999) conducted with individuals released from a state psychiatric hospital and three general hospitals in North Carolina; and the Assisted Outpatient Treatment Study (Swartz et al., 2010) conducted with state-wide data on individuals mostly released from state or local hospitals in New York State.

OPC appears to work only in settings where there are adequate and intensive services. Whether a court order without intensive treatment has any effect cannot be answered from current research. Current research does not point to really strong alternatives to OPC with regard to conditional release, guardianship or conservatorship, competency determinations, or advance directives. Each of these options might apply to some but not all individuals who are placed on OPC. Further research is required to establish whether these alternatives work, for whom, and under what circumstances.

1. Purpose and Scope

We were asked to undertake this review on behalf of the Maryland Department of Health and Mental Hygiene as a resource document for the Continuity of Care Advisory Panel that the Department had convened in August 2013 at the direction of the Governor. The Advisory Panel was charged with exploring ways to enhance continuity of care for individuals with a serious mental illness and to make recommendations to “strengthen the public behavioral health service delivery system, improve health outcomes, and address deficiencies that lead to interruptions of care” (DHMH Press Release, July 3, 2013).

One of the mechanisms that other states have used to achieve some of these goals is outpatient commitment (OPC), a civil law procedure whereby courts order an individual to obtain treatment against their will or risk sanctions up to and including forced hospitalization. OPC is a controversial procedure and its merits have been vigorously debated over the past three decades. The actual enforcement of these mandates is a complex practical matter that varies from one jurisdiction to another. Over the years, a variety of research studies have been undertaken to address its effectiveness. The results of these studies have become a central part of the debate over OPC as both critics and proponents draw opposing conclusions from the available evidence. The Department, desiring to anchor the Advisory Panel’s deliberations on a firm evidence base, sought a team of experienced mental health services researchers who did not have a stake in the ongoing debates about OPC to take a fresh look at the research evidence and evaluate its strengths and limitations. The review was conducted over a six-week time period that was nested within the Advisory Panel’s August 2013-October 2013 meeting schedule.

The three-person team that was assembled to undertake this review includes members with over fifty years of combined experience in mental health policy and services research, mental health economics, forensic psychology and violence risk assessment (see biographical sketches in Appendix A). Team members have no competing interests relevant to this report. In the spirit of full disclosure, however, we

acknowledge our close professional associations with the principals involved in the three major studies reviewed in this report. We have worked with Drs. Steadman, Swartz, and Swanson on a number of collaborative research and training programs and have co-authored publications with them. We have not, however, participated in any OPC-related research activities. Further, we believe we have undertaken a fair and independent review of the research literature on OPC and its options.

2. Outpatient Commitment: Origins, Types, and Controversies

The origins of outpatient commitment (OPC) can be traced to the 1970s during the era of deinstitutionalization when states and communities were struggling with the downsizing, closure, and consolidation of state-operated psychiatric hospitals (Torrey, 2011). One of the unanticipated consequences of deinstitutionalization and efforts to develop treatment in the community was a high rate of readmission (Geller, 1991). Many so-called “revolving door” patients could be stabilized in hospital but upon release they did not voluntarily participate in community treatment and gradually deteriorated to the point of becoming dangerous to themselves or others and requiring hospitalization. They soon were readmitted to a state hospital via local emergency departments or law enforcement pick-ups and the cycle would begin anew.

OPC has been described as “a lightning rod issue in American psychiatry” (Geller, 2006) and the debate over its use “as polarized as any argument in our field” (Dvoskin & Spiers, 2003). Advocacy organizations have lined up on both sides of the issue with the Treatment Advocacy Center (Torrey & Zdanowicz, 2001) and the National Alliance on Mental Illness (1995) being strong supporters whereas the Bazelon Center (2013) has been among its chief opponents.

OPC is the community treatment version of traditional inpatient commitment. Both are civil law mandates whereby a judge orders an individual to obtain treatment against one’s will or risk sanctions. The main difference is that inpatient commitment results in hospital confinement whereas outpatient commitment allows an individual to live in the

community as long as they are compliant with the treatment order. The exact criteria for commitment vary by jurisdiction but inpatient commitment usually involves danger to self or others, grave disabled, or need of protection or treatment for health and safety because of a mental disorder.

Among the criteria for outpatient commitment are mental illness; capability to survive safely in the community with supports; based on psychiatric history, respondent is in need of treatment to prevent further deterioration, and current mental status negates or limits ability to voluntarily comply. In reviewing 14 cross-sectional studies, Churchill and colleagues found a remarkably consistent profile of patients placed on OPC or its equivalent across jurisdictions embedded in very different cultural and geographical settings: “typically males, around 40 years of age, with a long history of schizophrenic-like or serious affective psychoses, previous admissions, poor medication compliance, aftercare needs, the potential for violence and displaying psychotic symptoms, especially delusions” at the time of the OPC order” (Churchill, et al., 2007: 178).

There are a variety of terms in the published literature that are used to describe OPC-like procedures. In the U.S., OPC has been referred to as “assisted outpatient treatment” (Swartz et al., 2010), “involuntary outpatient commitment”, and “involuntary outpatient treatment” (Geller, 2006). In Canada, the United Kingdom, and Australia, OPC is referred to as “community treatment orders” or “compulsory treatment” (Gray & O’Reilly, 2005; O’Brien, McKenna, & Kydd, 2009). Currently, 45 states have some type of OPC statute (Treatment Advocacy Center, 2013). Three main types of OPC can be distinguished: (1) back-end, involving conditional release from hospital; (2) front-end, involving community treatment instead of inpatient hospitalization among those who meet commitment criteria; and (3) preventive outpatient commitment, ordering community treatment for those who do not yet meet civil commitment criteria in an effort to avoid further deterioration to the point where inpatient commitment is needed (Wales & Hiday, 2006).

Type 1 or conditional release is the oldest form of OPC having evolved from the trial visit practices that many state hospitals had used for decades to determine whether patients could receive treatment and function in the community prior to formal discharge. In the U.S., conditional release is also known as “extended placement” and as “conditional discharge.” In the U.K., conditional release is referred to by its Mental Health Act authorizations as either “Section 17 leave” or “Section 25a leave” (Burns et al., 2010). This type of OPC is initiated at the discretion of the hospital director under authority of an ongoing inpatient commitment order. Noncompliance can lead to hospitalization if the patient refuses treatment or if adequate services are unavailable in the community. What distinguishes conditional release from other types of OPC is the absence of a separate court-order for the outpatient phase of treatment. We review evidence regarding the effectiveness of conditional release in section four of this report.

Type 2 outpatient commitment developed in the 1960s and 1970s as a less restrictive alternative to hospitalization. Type 3 or preventive OPC is the most controversial type and it is the predominant focus of our evidence review. It is intended for those who are unwilling or unable to accept treatment voluntarily, who have a history of psychiatric hospitalizations, who are at high risk of deteriorating to the point of becoming dangerous in the absence of treatment, and who often cycle through the criminal justice system. Both North Carolina and New York have preventive outpatient commitment statutes (Allbright et al., 2002). However, the majority of participants in the three major research studies conducted in these states were placed on OPC orders directly from an involuntary inpatient hospitalization. So the research mimics a conditional release study even though the state statutes are broader and preventive-oriented. Although Maryland does have a conditional release statute, it is one of only five states without an explicit OPC statute.

Having an OPC statute is no guarantee of its use, however. Use varies a lot across jurisdictions with OPC statutes and surveys have indicated a variety of reasons for low use such as lack of services, liability concerns, attitudes of key players, lack of ability to enforce, among others (Robbins, et al., 2010). Currently, there are no good national

data available on OPC use across states. The National Research Institute affiliated with the National Association of State Mental Health Program Directors, Inc. (NASMHPD) recently conducted a survey of the 50 state mental health authorities about their use of involuntary treatment (Lutterman, personal communication). Only 15 states responded to questions about OPC, the others indicated they had no way of knowing who was placed on OPC orders by the courts due to the lack of a central registry or a reporting mechanism between the courts and the state mental health authority. There are also a number of reports in the literature to the effect that court-orders are not vigorously enforced.

A study of Assisted Outpatient Treatment (AOT or Kendra's Law) in New York found wide regional and county variations in its implementation and use even though a single statute was adopted statewide (Robbins et al., 2010). Most AOT occurred in New York City area where AOT was used essentially as a *step-down* or conditional release program from inpatient psychiatric care whereas when it was used elsewhere in the state it was often as a *step-up* procedure for people in the community who did not comply with efforts to engage them in enhanced services.

A wide-ranging set of commentaries both pro and con have developed around OPC (see Geller, 2006; Slade et al., 2013). Pro arguments hold that most refusal of or noncompliance with treatment are rooted in mental illness and “because the symptoms of mental illness abridge an individual’s autonomy, small intrusions into self-determination—“a tincture of coercion”—actually increases freedom” (Geller, 2006: 236). In a similar vein, Sharfstein (2005) has referred to the need for “caring coercion” in circumstances when sufficient coercion must be applied to provide treatment in the best interests of the patient. The alternatives are viewed as much less desirable for the individual, including being: unserved or underserved; remaining psychotic; behind locked doors in a hospital, jail, or prison; and deprived of life in the community in the least restrictive and most integrated setting. Further, OPC is seen as increasing service engagement and medication compliance leading to sustained period of psychosis-free living in the community and a better quality of life.

Con arguments hold that OPC is “another social control mechanism in the guise of benevolent coercion that will be directed to a larger cohort of persons with mental illness than would ever be in institutions in the current era of treatment” (Geller, 2006: 236). Further, it allows treatments of dubious value to be forced on marginally difficult people. OPC is also seen as increasing stigma for the individuals involved and is likely discriminatory in its application to minority persons. “Coercion, fundamental to the OPC process, undermines the therapeutic relationship, leads to alienation from treatment, and increases stigma. OPC is of dubious ethics because people are duped into complying with non-enforceable court orders” and it “does not improve quality of life” (Geller, 2006: 236).

In the remainder of this report we summarize the evidence about the effectiveness of preventive OPC in comparison to usual care (or other non-OPC control group) with regard to use of psychiatric hospitals, medication possession, engagement in community services, arrests, and cost savings for the states as reported in the North Carolina and New York studies. We then discuss the evidence base and practical issues associated with alternatives to OPC including conditional release, competency determinations, guardianship or conservatorship, and advance directives.

3. Evidence Review of Outpatient Commitment Research

In this section, we describe the scope and approach of our review and the three main studies that are examined in detail. We also highlight the design strengths and limitations of each study, summarize key findings, and rate how strong the evidence for OPC is in several outcome domains.

Approach to Evidence Review

Two generations of research studies on OPC can be distinguished based upon the sophistication and rigor of their designs (Ridgely et al. 2000). First generation studies were observational studies of patients released from psychiatric hospitals and placed on

OPC. Reviews of these studies already exist (Geller, 2006; Ridgely et al., 2000). As a group, these studies have a number of methodological limitations that undermine their validity or ability to establish the effects of OPC, including either the absence or nonequivalence of control groups, the lack of data on pre-OPC events, and selection biases such as focusing only on patients with the greatest likelihood of success in the community. As a result, it is impossible to draw clear inferences about the effectiveness of OPC from these studies. The second generation of studies employed more rigorous designs such as randomized controlled trials (RCTs) or sophisticated statistical matching and modeling strategies designed to overcome first generation limitations. These are the studies we focused upon in our review.

We followed an evidence-review strategy. Rather than simply summarizing the findings reported in the literature we undertook a critical appraisal of the literature by applying the rules of evidence to factors such as internal validity, adherence to reporting standards, conclusions, and generalizability. We stopped short of a full systematic review (Burns, 2008) using formal meta-analysis techniques because of the small number of relevant second generation studies and the time constraints of completing this report for the Maryland Continuity of Care Panel in a six-week period.

The three investigations of OPC that we review in this report are the Bellevue Pilot Study conducted with individuals released from a public general hospital with psychiatric services in New York City; the Duke Mental Health Study conducted with individuals released from a state psychiatric hospital and three general hospitals in North Carolina; and the Assisted Outpatient Treatment (AOT) Study conducted with state-wide data on individuals released from state or local hospitals in New York State or directly from the community after refusing community services. There are several Australian studies of community treatment orders (OPC) but these are excluded here so we can focus on the stronger U.S. studies (e.g., Preston, Kisely, & Xiao, 2002; Kisely, Smith, Preston, & Xiao, 2005; Kisely, Campbell, Preston, & Xiao, 2006; Kisely et al., 2013; Kisely, Xiao, & Preston, 2004; Segal, Preston, Kisely, & Xiao, 2009).

The Bellevue Pilot Study was a randomized clinical trial of involuntary outpatient commitment conducted between 1996-1998 in the psychiatry service at Bellevue Hospital, a public general hospital in New York City (Steadman et al., 2001). The Bellevue program was a pilot for an early version of assisted outpatient treatment (Telson, 2000). A total of 142 participants were randomly assigned; 78 received court-ordered treatment, which included enhanced services, and 64 received the enhanced-service package only. Between 57 and 68 percent of the subjects completed interviews at one, five, and 11 months after hospital discharge. Outcome measures included hospitalization, arrest, quality of life, symptomatology, treatment noncompliance, and perceived level of coercion.

While the Bellevue Study was a randomized trial of OPC, it suffered from a few limitations that generally resulted in null findings (summarized below). The analysis sample sizes were small due to 32%-43% attrition during the follow-up period suggesting that the analyses reported may have been under-powered. Further, despite using a random assignment procedure, the OPC and non-OPC groups differed significantly on prior homelessness, co-occurring substance abuse disorders, and proportion hospitalized in the 12 months immediately prior to study entry. Further, over the course of the study, the experimental assignment became confounded as many control subject case managers believed their patients had been placed on court-orders and no enforcement mechanism for noncompliance was put in place.

The Duke Mental Health Study was a randomized clinical trial of involuntary outpatient commitment conducted in North Carolina between 1993-1996 at a state psychiatric hospital and three local general hospitals with psychiatric inpatient services (Swartz et al., 1999). At the time of study enrollment, participants were involuntarily hospitalized patients authorized for a period of court-ordered outpatient commitment. The study essentially employed a random release from outpatient commitment design. Study subjects were randomly assigned either to continue under their court orders for an initial length of 30-60 days (n=129) or were released from their outpatient commitment order (n=135). Those in the treatment arm could have their orders

extended if deemed appropriate by the treating physician. The primary outcomes examined in the main study report were the total number of hospitalizations during the 12 month follow-up period, the number of hospital days, and a dichotomous summary measure of whether there were any hospitalizations during the study period. The authors conducted analyses of treatment by the randomized treatment arm at both the *person-level* and at the *person-month* level. Person-level observations were based on a single annual measure for each participant whereas person-month observations disaggregated the annual data into a longitudinal series of 12 monthly observations for each participant. The investigators also stratified the treatment arm by the length of treatment orders actually received during the study period (less than 180 days, or 180 days or greater).

The Duke study has a number of methodological strengths. It was a relatively large randomized trial with a well-designed control group. The randomized design breaks the correlation between confounders and treatment assignment, meaning that observed effects of the intervention should be due to the intervention itself and not to characteristics of the patients or providers, for example.

The control group is very comparable to the intervention group in that they were discharged from a similar inpatient stay and had access to similar treatment opportunities in the community. In addition, both arms were enhanced with case management services, increasing the strength of the control treatment thus rendering any OPC effects more conservative. Compliance of the participating mental health centers to the treatment adherence protocol was described as excellent.

Although the main analysis using the randomized design is very strong, there are a number of weaknesses, primarily with generalizability and the analysis by length of treatment orders.

The authors follow up the person-level analysis with a longitudinal person-month analysis, examining whether participants used any hospital days each month, rather

than only an aggregated count at the end of the 12-month period. This design increases the number of observations available for analysis and thus increases statistical power. The longitudinal analysis used step-wise modeling to reduce the number of covariates, which could be a relatively modest flaw in the analysis. Specifically, step-wise modeling is not suggested for hypothesis testing, as was conducted here, as high correlations among predictors (multicollinearity) can lead to misleading results. It is not clear which covariates were included or excluded in the analysis, since no results are reported in tables.

The generalizability of the results may also be questioned. The treatment setting, especially with enhanced case management, may not be typical of usual care in most regions of the U.S. Having greater treatment opportunities for the control subjects is a strength of the study design, but at the cost of greater generalizability. The direction of the bias is not obvious, since it both increases the success of the control arm, but also of the treatment arm. As is true with all randomized studies, participants in this randomized trial may differ from candidates for OPC in the general population, since study participants consented to be randomized and to participate in the study assessments.

Our primary concern rests with the analyses stratifying the treatment arm by length of orders. While we agree with the authors that examining the characteristics of those who ended up receiving long versus short treatment orders may be of clinical and policy interest, we feel the analysis by the length of the treatment orders is misleading, since length itself could be considered an outcome of treatment. No comparison to the controls is possible for this group. That is, the assignment to short vs. long orders was not randomized, and is likely an outcome of how well the study participant was doing in the community. On page 1970 of the manuscript, the authors' state:

“Initial outpatient commitment orders from the index hospitalization varied in length but typically were 30 to 60 days. At the conclusion of this initial period, clinicians treating the subjects in outpatient commitment were instructed to reevaluate the

legal criteria for outpatient commitment and seek recommitment if legally appropriate.”

In a separate manuscript examining the arrests in participants from the Duke study (Swanson et al., 2001, p. 185), the authors further elaborate on the process of determining length of orders:

“Specifically, renewal of the court order required a second determination (by a psychiatrist and the court) that the respondent would predictable become dangerous (or “gravely disabled”) without treatment and predictable would not comply with treatment. At the end of the initial OPC period (up to 90 days), each case was reevaluated systematically. Prompting notices were sent to clinicians, reminding them that a participant’s OPC order was about to expire and summarizing the OPC criteria for easy reference. If at that point the psychiatrist and the court concluded that the respondent was no longer likely to become dangerous without treatment or – even if so – would comply voluntarily with treatment, then the legal criteria for OPC were not satisfied and the order could not be renewed. Beyond the legal criteria, common sense suggests that clinicians would not cull out clients assessed to be at highest risk of criminal behavior and arrest and selectively not renew their OPC orders.

Anecdotally, when clinicians were asked by research staff members to state their reasons for not renewing an OPC order, the most typical answer was that the patient had been compliant with treatment and was doing reasonable well at that time; hence, continuing a court order was not seen as legally justifiable. Empirically, respondents who had been mostly compliant with medications in the 4 months prior to hospitalization were significantly less likely to receive extended OPC after their initial court order expired, and medication compliance during follow-up was associated with lower risk of arrest in these data. In sum, if any bias affected the selection of participants for longer periods of OPC, it would seem to work against finding that extended OPC lowers risk of arrest.”

While the above statement does indicate that at the time of the decision to renew the order, persons doing well with treatment would be less likely to receive longer orders, the results from these analyses cannot be treated causally. That is, while the associations between length of treatment orders and outcomes are descriptive, they cannot imply that lengthening orders leads to the outcomes under question, since the length of orders appears itself to be an outcome. In statistical terms, this is referred to as endogeneity and results in biased estimates of outcomes. This criticism has been noted by others (e.g., Szmukler & Hotopf, 2001; Cochrane 2012). We therefore do not believe that this result should in any way imply that greater length of treatment orders will improve outcomes; the study design does not permit such a conclusion.

The New York State Assisted Outpatient Treatment (AOT) Study was a large observational study which used administrative data from the New York State Office of Mental Health that were linked to Medicaid insurance claims from 1999-2007 (Swartz et al., 2010). Administrative data included information on AOT orders and case manager reports filed at baseline and every six months thereafter on recipients of intensive service. The AOT sample consisted of 3,576 individuals who were also on Medicaid, corresponding to roughly two-thirds of all individuals who had been placed on AOT orders during this time period. Multivariate analysis was used to control for differences in baseline covariates between those receiving AOT and controls. The study used two analyses: (1) pre-post analysis, comparing person-month trends in outcomes before and after the receipt of AOT, and (2) pre-post comparison group analyses, comparing the pre-post person-month outcomes of AOT recipients to controls with similar diagnoses, who were not on AOT but received intensive outpatient treatment on a voluntary basis. The primary outcomes addressed in the main report were likelihood of psychiatric hospitalization, number of psychiatric hospital days, receipt of psychotropic medications, receipt of intensive case management services, and service engagement.

This study includes two distinct analyses: (1) an administrative data analysis of AOT participants using pre-post comparisons with individuals as their own controls, and (2)

an analysis of case management data comparing AOT recipients either alone or in combination with ACT or ICM. Because of the differences in methodological techniques and data sources, these analyses are reviewed separately below.

Administrative Data Analysis

There are a number of strengths to the administrative data analysis. The observational nature of the design allows for a larger number of participants and a longer time period than what would have been possible from a randomized trial. In particular, the authors have access to a large observational sample of Medicaid-enrolled persons receiving AOT between January 1999 and March, 2007. The use of Medicaid and State administrative data make a large number of important outpatients available, including inpatient and outpatient services, hospital days, and medication adherence. Hospitalizations were recorded both from Medicaid claims and from state psychiatric hospital stays. Medicaid claims were also used to generate measures of medication adherence (Medication Possession Ratio).

There are also important limitations to this analysis. The pre-post analysis includes data from 3,576 (63%) of the 5,634 individuals receiving AOT orders in NY during this time. The reduction in sample size is due to limiting study participants to those on Medicaid, the data source for the study's treatment events and service contacts. While advantageous for data purposes, this restriction limits the generalizability of the study with regard to uninsured populations.

While appropriate analyses were used in the form of propensity score modeling in the pre/post analysis, these techniques would have been better used on data comparing AOT recipients to non-AOT control subjects. We have a number of technical concerns about this analysis.

First, the propensity scoring, which is a tool generally used to refine a control group to make it more similar to the intervention group, was used on observations from AOT subjects only. Because three types of observations seem to be used for this analysis

(pre-AOT, during the first six months of AOT, and seven or more months of AOT), two separate logit models were conducted generating propensity scores. It is not clear from the manuscript how propensity weights from these two models were combined, a better approach would have used a polychotomous method, such as multinomial logit, to estimate the propensity weights from a single equation, thus accommodating the dependence between the two models. In addition, the covariates used in the propensity model are not well specified. They are listed as “all available demographic and clinical variables, MPR, and hospitalization history” (p. 978). Several of these variables are time invariant (demographic, clinical, and hospitalization history) which means they would balance anyway across pre-post observations since these were on the same individuals. The use of MPR as a baseline risk factor is suspect, since it is likely from the same time period as the outcome being measured.

Second, no information is given on the level of balancing of covariates across treatment groups, nor on variable means across these groups, even in on-line supplements. Balancing on covariates is an important part of validation of results from propensity score analysis and is not guaranteed by its implementation

Third, the wide availability of ICM or ACT in NY, while certainly a strength of the treatment options, may be a limitation of the study, since it reduces generalizability to other settings which may not have adequate resources to provide ACT or ICM on a voluntary basis.

Case Management Data Analysis

There are a number of strengths to the case management data analysis. First, it combines the administrative data source mentioned above with case manager reports. This allows the analysis of a variable describing whether participants were engaged in outpatient treatment services, in addition to the hospitalization outcome. It does limit the sample to those with complete case management data.

Second, persons receiving ACT without AOT, or AOT in combination with either ACT or ICM were included, making a three-way comparison (ACT alone, AOT with ICM, and AOT with ACT; N=3519). This analysis thus examines an important comparison condition – voluntary ACT treatment in contrast with either ACT or ICM combined with involuntary treatment orders. The analysis used a rich set of baseline covariates, including several variables measured by case managers and not available in claims data, such as engagement in services as baseline and global assessment of functioning.

However, there are a number of limitations to the case management data analyses. Here, the authors did not use propensity-weighted techniques as the pre-post analysis did, analyzing the receipt of AOT with ICM or ACT in contrast with ACT without AOT as an exogenous treatment variable, which is potentially problematic. While a rich set of covariates were used for the multivariate analysis, there is likely some selection bias that remains, thus potentially biasing the estimates obtained from this case-control analysis. In addition, the presentation of only odds rather than the probability of service receipt make interpretation difficult, and the authors themselves fell into the trap of referring to their results inappropriately as reductions in the likelihood of outcomes such as hospitalizations.

Our decision to focus on these three studies was based on several considerations. They are the principal second generation studies that have been conducted to date. Although the Bellevue and Duke studies have been scrutinized in evidence reviews conducted by the Cochrane Collaborative (Kisely et al., 2011), the RAND Corporation (Ridgely et al., 2000), and the King's College Institute of Psychiatry (Churchill et al., 2007) there are features of these studies that have yet to be fully examined. Further, the findings of the New York State AOT study were not included in any of these earlier reviews. Both the RAND and King's College reviews were published several years before the AOT final results became available. Since a minimum bar for study consideration in a Cochrane Collaborative review is a RCT design (Higgins & Green, 2011), the New York State AOT studies will never appear in any of their reviews. The

AOT study is attracting a lot of attention among mental health stakeholders in the U.S., and as a result, it is important that the evidence base for OPC be updated with results from this study.

Our evidence-review focuses on several outcomes: hospitalization, including the probability of admission to a psychiatric hospital or psychiatric unit in a general hospital as well as the total number of days hospitalized; psychotropic medication possession; engagement in services; arrests; and costs. We will review evidence from each of the three studies under each outcome.

a. Hospital Outcomes

Bellevue Pilot Study

Findings from the Bellevue study are reported in Steadman and colleagues (2001) with regard to hospitalization, arrest, quality of life, symptomatology, treatment noncompliance, and perceived level of coercion. The abstract summarizes the findings as follows: “On all major outcome measures, no statistically significant differences were found between the two groups. No subject was arrested for a violent crime. Eighteen percent of the court-ordered group and 16 percent of the control group were arrested at least once. The percentage rehospitalized during follow-up was about the same for both groups—51 percent and 42 percent, respectively. The groups did not differ significantly in the total number of days hospitalized during the follow-up period. Participants’ perceptions of their quality of life and level of coercion were about the same. From the community service providers’ perspective, patients in the two groups were similarly adherent to their required treatments. All results must be qualified by the fact that no pick-up order procedures for noncompliant subjects in the court-ordered group were implemented during the study, which compromised the differences between the conditions for the two groups, and that persons with a history of violence were excluded from the program.”

These null findings have to be viewed through a number of the study’s methodological limitations, such as small sample size and differences in baseline

characteristics between treatment and control groups. Despite the differences, the rates at which participants experienced these outcomes was quite similar in the reported data, however, indicating that lack of power is not the only reason for a null finding. The number of limitations in the Bellevue study (e.g., small sample size, non-equivalent comparison groups, lack of enforcement of court orders), however, may have affected the findings and make it difficult to draw definitive conclusions.

Duke Mental Health Study

In the paper's abstract, the authors summarize their results as follows: "In bivariate analysis, the control and outpatient commitment groups did not differ significantly in hospital outcomes. However, subjects who underwent sustained periods of outpatient commitment beyond that of the initial court order had approximately 57% fewer readmissions and 20 fewer hospital days than control subjects. In repeated measures multivariable analysis, the outpatient commitment group had significantly better hospital outcomes, even without considering the total length of court-ordered outpatient commitments" (Swartz et al., 1999: p. 1968).

In terms of the randomized analysis of OPC as compared with no OPC, the study design would permit a strong conclusion of no difference in hospital outcomes, as described above. However, the main outcome result based on randomized assignment was not reported in tables, so it is impossible to determine whether a large standard error (e.g., if the study were under-powered) was driving the lack of difference rather than a small estimated effect. Additional analyses also interacted OPC assignment with an indicator of psychotic (as opposed to affective) disorder, and found stronger effects of OPC on hospitalization in the group with psychoses. Again, this methodology is strong assuming that the randomization effectively balanced the baseline covariates and risk factors in this stratified analysis (also not reported in tables).

Using the longitudinal data, hospital use was analyzed as a function of randomized treatment arm, demographic and clinical factors, and a time trend. Results are not provided in any tables, but the authors note "Assignment to the outpatient commitment

group was associated with a significantly lower odds of any readmission (OR=0.64, CI=0.46-0.88)” (Swartz et al., 1999: p. 1972).

It is not immediately clear why the longitudinal finding of a decrease in hospital use is not consistent with the bivariate finding of no difference in hospital outcomes between cases and controls in person-level analysis. Person-level hospital outcomes include the total number of admissions, any hospital admission in the 12-month follow-up period, and the total number of hospital days. We assume that no difference was found in the person-level comparison on any of these three measures, including the single measure (any hospital admission) that was analyzed in the longitudinal data, but again, results were not reported. Two possible explanations for the difference between the person-level and person-month (longitudinal) analyses arise. First, as the authors seem to indicate, it could be a power issue, in that the longitudinal data provides 12 observations per person, rather than one, thus increasing sample size and statistical power. Unfortunately, as noted above, the estimates from the person-level model were not provided. If they were negative but insignificant, the power explanation could be better assessed. It also could be the difference between the person-level analysis, which relies on random assignment and only compares bivariate means, and the multivariate model that controls for other covariates, such as time trends, thus allowing an actual difference to emerge.

In summary, the Duke study was a well-conducted randomized trial of persons with SMI court-ordered for an involuntary outpatient commitment. The analysis based on randomization provides **strong** evidence on the effect of OPC on hospital outcomes. Assuming the random assignment worked in balancing covariates, specifics of which are not reported in the manuscript, we are inclined to weight the longitudinal model that excludes the length of orders more heavily than the bivariate person-level model, and other criticisms notwithstanding, conclude that there is **strong** evidence that OPC did result in a lower probability of hospitalization than in control subjects. We believe the evidence for a different effect of OPC by length of treatment orders in either the person-level or person-month (longitudinal analysis) is **weak** for the reasons noted above.

New York State Assisted Outpatient Treatment Study

The main findings from this study are reported by Swartz and colleagues (Swartz et al., 2010). In the abstract they say: “Consumers who received court orders for AOT appeared to experience a number of improved outcomes: reduced hospitalization and length of stay, increased receipt of psychotropic medication and intensive case management services, and greater engagement in outpatient services” (Swartz et al., 2010, p. 976). Also: “In these more direct comparisons [the ones comparing AOT alone vs. AOT+intensive case management (ICM) or Assertive Community Treatment (ACT)], the court order reduced hospitalizations over and above the effect for ACT alone” (Swartz et al., 2010: 980). In the hospital analysis (Swartz et al., 2010), the authors create monthly summary measures for AOT recipients, which were split into four groups: prior to AOT, during first 6 months of AOT, during months 7 and greater of AOT, and post AOT.

The authors used appropriate statistical tools to analyze the observational data. This is the first study of involuntary outpatient commitment to include propensity score modeling to better balance the risk factors among the treatment groups (see limitations, below). The authors also used repeated measures logit analysis to accommodate the monthly observations for the sample.

The authors report that the odds of hospital admission were reduced by 23% during the first 180 days and by 41% during subsequent renewal (181+ days) of AOT orders; days hospitalized (20%/16%) were also reduced while medication possession (47%/88%) and receipt of ACT/intensive case management (242%/282%) increased.

In summary, the AOT-only analysis used propensity score weighting, the lack of details and the use of this technique only on those receiving AOT renders this analysis suspect. The paper reports reductions in hospitalizations, increases in medication possession, and increases in the receipt of ACT or ICM, but given the methodological concerns, the evidence from this analysis on AOT is **weak**.

The findings from the multivariate model comparing ACT alone to AOT with ICM, and AOT with ACT, indicate a substantially reduced odds of being hospitalized for those receiving AOT in conjunction with ICM or ACT, than in receiving ACT alone. Odds of engagement in services were also higher for AOT combination treatment than ACT alone.

In summary, the New York State AOT study is one of the largest investigations of involuntary outpatient treatment conducted to date. It is an observational study using administrative data sources and case management reports to examine outcomes such as hospitalizations, engagement in services, and medication possession. The authors use both pre-post and case/control techniques. The authors of the main study create an interesting and appropriate comparison group of persons voluntarily receiving ACT to compare with persons receiving AOT in combination with ACT or ICM. It uses appropriate statistical techniques, such as propensity score weighting and multivariate analysis, somewhat inconsistently. Because pre-post is not a strong design, the study suffers from several threats to validity from these analyses and therefore results are characterized as **weak**. The case/control design is stronger, especially due to the use of a large number of baseline control variables, but assumes selection into AOT is exogenous. These findings are therefore characterized as **moderately strong**.

b. Medication Possession and Treatment Adherence

Bellevue Pilot Study

Medication possession or use was not measured as an outcome in the Bellevue Study. The study did employ a self-reported treatment noncompliance measure and found no statistically significant differences between OPC and non-OPC groups.

Duke Mental Health Study

Swartz and colleagues (2001) examined medication adherence by participants in the Duke study. Study subjects include both those in the randomized treatment and control

arms (n=100 and 113, respectively), as well as a group of violent subjects who were not randomized (n=48). Treatment adherence was quantified using subject, family, and case manager reports on medication adherence and attendance at scheduled appointments. A binary measure was created to indicate participants who were always or usually compliant with treatment. The key independent variable measured was a binary indicator of whether the participant was under treatment orders for more or less than 6 months during the year. As indicated above, this variable likely reflects an outcome of treatment, rather than treatment itself, and its estimates cannot be interpreted as a causal relationship. The intensity of outpatient services received was included as an additional control variable, again with similar concerns about endogeneity biasing causal estimates. This study included a rich set of baseline control variables, including demographics, social supports, homelessness, and crime victimization. Using the randomized sample only, the authors find no difference in treatment adherence between the two study arms.

In further analyses of OPC recipients including both those randomized to receive OPC and those not randomized due to a history of violence, the authors find that participants receiving OPC for at least half of the year were more likely to be adherence to treatment, and this effect was larger in those receiving frequent outpatient services. For the same reasons outlined above, we do not think these estimates can be interpreted as causal, and inclusion of intensity of outpatient treatment, which is a component of the dependent variable (attendance at treatment) further complicates these biases.

In an AOT companion report, Busch and colleagues (2010) use the NY AOT study framework to examine a measure of adherence to guideline recommended medications derived from Medicaid claims data. The authors use the Medication Possession Ratio (MPR), which sums up the days' supply of medication across medications in the relevant class, thus potentially overstating adherence in the case of polypharmacy. The authors use data from 1999-2007 and stratify the sample into three groups: those who

received AOT, those who were under voluntary treatment orders but never received AOT, and those who were not under either type of order.

Similar to other AOT studies described above, treatment received was assumed to be exogenous, using only a minimal set of covariates (demographics, Medicaid eligibility category, diagnostic category), and psychiatric hospitalization in a given month. The year of the first observed treatment episode for each type of treatment was included as an additional control variable, as was use of AOT or ACT/ICM in a given month. Separate GEE logit models were run for each region/treatment combination (n=9). Sensitivity analyses controlled for the proportion of the year the study participant was enrolled in Medicaid. These models were used to generate and compare predicted probabilities of high Medicaid possession from baseline to the end of the study window. Predicted probabilities were substantially larger at the end of the study period for all three treatment groups and generally largest for the AOT group within each region; no statistical test was conducted to formally compare the differences.

c. Arrests

Bellevue Pilot Study

No participants in the Bellevue Study were arrested for a violent offense, and relatively few were arrested overall—16% in the control group and 18% in the OPC group (Steadman et al., 2001). No statistically significant between-group differences were found on indicators for any arrest, multiple arrests, or most serious charge. Due to sample size issues and the other differences between-groups at baseline as mentioned above, the strength of the evidence for these null findings is **weak**.

Duke Mental Health Study

Findings from the Duke Mental Health Study on the effects of OPC on arrests are reported in a paper authored by Swanson and colleagues (Swanson et al., 2001) that focuses on the 12-month post-discharge study window in the North Carolina OPC sample. Arrests are compared between OPC and non-OPC groups in the randomized sample. A separate analysis is also conducted including a nonrandomized group (n= 46) of individuals who the court deemed as too dangerous to be randomized in the original study.

In the sample randomized to OPC or released from orders (slightly reduced from the original sample, presumably because of missing arrest data), the authors find no difference in the rates of arrest between study arms (18.6% arrested, n=102 for OPC arm vs. 19.3% arrested, n=114 for controls). Assuming there is no further selection bias into the reduced 216 sample from the original n=264 reported above), we view this as **strong** evidence of no difference in arrests from OPC treatment.

In an analysis of 262 participants (the 216 randomized plus 46 persons who were not randomized), the authors use logit models with stepwise selection of variables to examine whether long versus short orders are associated with arrests during the study period. They generally find results suggestive that persons receiving longer orders have lower odds of arrests. This seems particularly so in a subsample of persons who have histories of both psychiatric hospitalizations and criminal histories. As described above, while these results certainly indicate an association between OPC and arrests, because of significant concerns with the methodologies used, we similarly classify these results as weak evidence that assignment to OPC affects arrests.

New York State Assisted Outpatient Treatment Study

In order to track the association between New York's AOT and arrests, Gilbert and colleagues conducted interviews between 2007-2008 with 211 adults who qualified for AOT who also had either an affective or a schizophrenic spectrum disorder (Gilbert et al., 2010). Of the 211, 139 (66%) persons received AOT at some point, 42 (20%) signed

voluntary service agreements, while 30 (14%) had neither type of contract and were excluded. Many of the 181 persons in the final analysis sample had switched between voluntary and AOT orders during the study period and were thus exposed to both conditions.

The authors compared monthly measures of arrests across five types of observations: those prior to any AOT/voluntary agreements, during AOT only, during voluntary agreements only, post-AOT, and post-voluntary agreement. Since most of the switching went from voluntary agreements to AOT, some of the post-AOT months were also post voluntary agreement as well. A binary logit model was run on monthly arrests, including four of the five monthly measures as covariates. No attempt was made to use statistical analyses to accommodate differences in characteristics prior to AOT/voluntary agreement status, and only a minimal set of covariates were used (region, race/ethnicity, gender, primary diagnosis, and high school education). The results indicate a substantially lower odds of arrest for months with active AOT as compared with pre-AOT/pre-voluntary arrest months (OR=0.39; $p<0.01$); no other statistically significant differences were noted although all the odds ratios are less than 1. The authors appropriately calculate the predicted probability of arrests for these cohorts, and find that despite a large difference in odds, the difference in predicted probability was small, at 3.7% probability of arrests pre-AOT/voluntary orders, as compared to 1.9% for those on active AOT.

There are a number of methodological concerns with this analysis. It is not clear how the 211 members were selected for this study, nor is it clear why the study did not make use of the full set of administrative data on arrests and Medicaid treatments as in the main AOT analyses (Swartz, et al., 2010). The only covariate included that could not be derived from administrative data sources was high school education. The small sample size, however, underscores the statistical significance of the difference. However, the assumption that monthly treatment status is not itself an outcome variable (exogeneity) coupled with a thin set of covariates, none of which measured prior arrest or treatment history, make this evidence of lower arrests **weak**.

d. Cost Savings

New York State Assisted Outpatient Treatment Study

The New York State AOT cost analysis is reported in a recent paper by Swanson and colleagues (Swanson et al., 2013). This report uses data on 634 individuals who started an Assisted Outpatient Treatment order within 30 days of discharge from an index hospitalization between January 2004 and December 2005, and thus represents a small subset of the full AOT data (18% of the AOT sample and 11% of those in New York State who received outpatient orders during the 1999-2007 original study period). Analyses were stratified by whether the respondent lived in New York City, or one of five other counties in New York State. Study participation was limited to those on Medicaid for at least some of the study period. Administrative data used for these analyses reflect Medicaid or state-funded mental health services use. Medicare data were not used for the analysis. Control subjects were identified in the administrative data as those receiving ICM or ACT within 30 days of a hospital discharge but not on AOT (n=255). Medical and mental health costs were derived from administrative sources while unit costs weights were used for criminal justice costs.

The authors state in the abstract: “Assisted outpatient treatment requires a substantial investment of state resources but can reduce overall service costs for persons with serious mental illness. For those who do not qualify for assisted outpatient treatment, voluntary participation in intensive community-based services may also reduce overall service costs over time, depending on characteristics of the target population and local service system.” (Swanson et al., 2013: xxx). However, our review of these analyses (see below) suggests the strength of the evidence for this assertion is **weak**.

The AOT cost study has a number of methodological strengths. The authors created an appropriate set of cost variables both from actual Medicaid expenditures and from per diems for state hospitals, jails, and prisons. They use a national estimate of criminal justice costs related to arrests. The authors create a person-month panel of data which

would enable sophisticated cost analysis. The authors selected a reasonable control group of persons who had Medicaid claims for ACT or intensive case management (ICM) and had a discharge from a psychiatric inpatient stay, similar to those in the AOT intervention group. The authors are upfront about the limitations of this control group having potentially different characteristics from the AOT intervention group.

However, the AOT cost study also has a number of methodological limitations. Rather than directly testing differences between groups in statistical analyses, the authors ran analyses separately on the AOT participations from the ICM participants, allowing them only to examine the rate of cost decline separately for each sample. No head-to-head comparisons were done, leaving open the question of why they bothered with a comparison group.

A better modeling strategy would have first compared the 12-month baseline characteristics and costs between the two groups to establish similarity of populations and trends. No comparison was conducted in the current analysis, despite potential concerns (see below). Second, the authors could have pooled the data, using the ICM recipients as a control group, using techniques such as person-level fixed effects to control for time-invariant (e.g., severity, motivation) differences between the two groups or even propensity score analysis, if the set of baseline risk factors was rich enough. No pooled analysis was conducted.

For their main results (Tables 2 & 3), the authors used only a pre-post comparison of AOT participants; the ACT/ICM group is not incorporated. Pre-post comparisons have well-known threats to validity, including regression to the mean and the lack of ability to tease out external factors that occurred disproportionately in the post period. Regression to the mean is especially acute, since all subjects by inclusion criteria, had an index hospitalization in the pre-period, and thus disproportionately high costs.

A fundamental difference may be apparent between the AOT and ACT/ICM groups. Since the ACT/ICM group was defined according to the presence of claims for ACT or

ICM, 100% of this group must have been Medicaid enrolled. However, the ACT group has no such requirement and on page 2, the authors state that 87% of the AOT group is Medicaid enrolled. This has important implications for the cost models, since many of the costs (medical, outpatient psychiatry, and pharmaceutical, among others) are based only on Medicaid claims, meaning that their values are 0 for those not on Medicaid. This criticism does not apply to state hospital or criminal justice costs, which would be observed regardless of Medicaid enrollment. Having 13% of the AOT sample with partially unobservable costs means that reported costs are lower than actual costs and that this is more likely to occur in the post-period, which is not defined by a hospitalization. This will lead the post period results to be lower than they should be.

It is difficult to interpret the separate analyses of data from the multivariate models. The authors report only beta coefficients from a non-linear GEE model. Coefficients from non-linear models are notoriously hard to interpret. Presentation of the marginal effects of key covariates would have led to easier interpretation and provided the magnitude of the cost effects, which are absent from the present multivariate analysis. If the estimates are of a negative binomial model, the sign can be determined from the coefficients, but not the magnitude. The discussion is not consistent with this interpretation, however (e.g., the text states “significant cost-reducing effects associated with AOT were found...” when all the coefficients reported are positive and significant).

A final methodological concern involves the omission of Medicare claims data. Although the authors do not claim to have comprehensive medical and mental health data, they also do not note the lack of Medicare data as a limitation. A recent study estimated from the Medical Expenditure Panel Survey data that approximately 50% of community-based persons with schizophrenia were covered at least in part by Medicare (Khaykin et al., 2010); many of these individuals are dually enrolled in Medicaid. This means that although most of the AOT sample is enrolled in Medicaid, a large component of their costs are paid for through Medicare, including pharmaceuticals for both duals and Medicare-only enrolled individuals beginning in 2006. The reported costs likely understate the true medical and mental health costs in the system. This may not

be a concern in comparing AOT with ACT/ICM if the rate of Medicare enrollment was similar between the two samples.

In summary, while Swanson and colleagues (2013) concludes that AOT can save states money (hence, the article title), there is little evidence to conclude that this is true. Thus, we rate the evidence here as **low**. This is a missed opportunity, since their data would largely support such an analysis (missing Medicare data not withstanding).

Duke Mental Health Study

A set of cost analyses were recently reported by Swanson & Swartz (2013) using data from the original Duke Mental Health Study. This report uses cost measures developed for the New York State AOT Study and applies them to the North Carolina data, essentially monetizing the findings reported in the original study. The report multiplies units of services received and recorded in the Duke Study data by unit costs based on type of service (e.g., outpatient mental health service, arrests, hospital use). A wide variety of service types were included and appropriate unit costs were derived from a variety of sources. Spending was expressed in 2008 dollars in order to be comparable to the New York AOT study.

Statistical analyses were not conducted, but mean total and component costs were provided for participants randomized to OPC and controls, as well as for those with long versus short orders (using the terms “renewed” and “non-renewed”). The cost differences between the OPC participants (\$46,509.88) and controls (\$43,515.98) is relatively small, at just under \$3,000 or less than 7% of the cost of the control subjects. This estimate would likely not be considered a statistically significant difference, thus we would agree with the authors’ suggestion that this indicates that OPC was essentially cost-neutral, and as above, would view these as methodologically **strong** results.

Costs were much larger for the non-renewed OPC subjects than for the renewed subjects (\$54,857.87 vs. \$25,921.72). This was due primarily to the substantially greater use of psychiatric hospital days by the non-renewed participants. Again, because of

substantial concerns about the validity of the long v. short treatment orders analysis, this estimate cannot be considered causal. So our conclusion, as before, is that evidence based upon these renewed/not-renewed (long vs. short) analyses is **weak**.

Overall Evidence Summary

Table 1 summarizes the current evidence for OPC on the basis of the research studies we have reviewed above. The Duke and NYS-AOT studies have two columns in this table corresponding to the two distinct analyses reported in the cited publications, as noted above. The Duke study had both a randomized comparison (RCT column) and a non-randomized analysis (short vs. long column) of OPC orders with different findings/evidence. The NYS-AOT was not a randomized study. Instead, two sets of analyses are reported; one for a series of pre-post (no control group) comparisons (pre-post column) and another for comparisons between those individuals who received AOT with ACT/ICM services vs. those who received ACT/ICM services only (last column on right).

The symbols in the cells of the table summarize the evidence across the three studies (Bellevue, Duke Mental Health, and NYS-AOT). Two distinct dimensions are needed to summarize the strength of the evidence: (1) the direction of effects (symbolized as -/0/+ indicating: reduction, no-difference, and increase) and (2) the strength of evidence (symbolized by +, ++, +++ to indicate weak, moderate, and strong increases; 0, 00, 000 to indicate weak, moderate, and strong evidence for null or no effect; and by -, --, --- to indicate weak, moderate, and strong reductions).

Table 1. Strength of Research Evidence for Outpatient Commitment (OPC)

Outcome	Bellevue Study	Duke Mental Health Study		NYS-AOT Study	
		RCT	Long vs. Short orders	Pre-Post	AOT+ICM/ACT vs. ICM/ACT alone
Any hospitalization	-	0 0 0 (person) - - - (person-month)	0	-	- - -
Days in hospital	0	0 0 0	-	-	
Medication possession		0 0 0	+	+	
Service engagement	0				+ +
Arrests	0	0 0 0	-	-	
Costs		0 0 0	-	-	

Legend: -/0/+ = direction of effect; number of signs = strength of evidence (e.g., +=weak, ++=moderate, +++=strong)

So with these notations, Table 1 can be read as follows starting with the first row in the table:

The Bellevue study showed there was a reduction in the probability of psychiatric hospitalization for the OPC group in comparison to the non-OPC group. However, the small sample size of the study and other complications lead us to rate the strength of the evidence as **weak** (-).

There is **strong** evidence (000) in the Duke RCT (person-level analysis) that there was no difference between the OPC and non-OPC groups in the probability of psychiatric hospitalizations. However, when analyzed at the person-month level (which increases the number of observations for each participant by a factor of 12, one for

each month) there is **strong** evidence (- - -) that the OPC group had a significantly lower probability of admission to a psychiatric hospital each month than did the non-OPC group.

The Duke study's analysis of long vs. short OPC orders was not a randomized comparison. The finding reported was that OPC participants had a lower probability of psychiatric hospitalization. However, this comparison is susceptible to selection biases and therefore the direction of the effect is difficult to determine; it is rated here as a **null** (0) finding.

Both of the analyses reported for the NYS-AOT study found that the OPC group experienced a lower probability of admission than did the comparison group. In the pre-post analyses the comparison was with the same individuals prior to their entry into AOT; this comparison does not have an external control group and is susceptible to selection biases and has other technical concerns; therefore, the evidence is rated as **weak** (-). However, the comparison of patients on AOT+ACT/ICM vs. those on ACT/ICM alone, using person-month analyses, indicated that AOT+ACT/ICM group had a significantly lower probability of hospitalization; here that finding is rated as **strong** (- - -). Further information about the comparability of this control group would solidify the strength of this evidence.

Looking across rows in the table, there appears to be generally **strong** evidence that OPC reduces the likelihood of hospital use, but has **no effect** on the total number of hospital days, weak evidence (+) that it increases medication possession, moderately strong evidence (++) that it enhances service engagement, and little or no evidence (0/-) that it reduces arrests. While the findings of these studies are suggestive of lower costs, the limitations in these studies have created only a weak (-) evidence base on costs.

4. Options to Outpatient Commitment

There are several other legal mechanisms that might serve as potential alternatives for the community supervision of individuals who otherwise would be candidates for outpatient commitment. These mechanisms include conditional release from hospital, involuntary commitment based on incompetency rather than dangerousness standards, guardianship of individuals found to be gravely disabled or incompetent to make decisions about need for treatment due to mental illness, and psychiatric advance directives that specify future treatment preferences when an individual experiences an incapacitating crisis. In the sections that follow we consider available research evidence and practical issues associated with these mechanisms. Other alternative approaches to OPC have been discussed in the literature such as peer engagement and mental health outreach (Rowe, 2013) or assertive community treatment (Ridgely et al., 2000).

We take a different approach to reviewing the evidence for these alternative mechanisms than we did for OPC in the last section of this report. Our goal has been to produce a timely report for use by the Continuity of Care Panel. The timing for that goal precluded a detailed critical review of each study referenced in this section of the report. Rather, we highlight findings, strengths, and weaknesses in a summary manner to sort out the relevance of each of these mechanisms as possible alternatives to OPC.

Conditional Release

Conditional release involves early release from hospital on an involuntary inpatient commitment order. So, practically speaking, it only applies to the segment of the OPC population at the time of hospitalization. In the United States, in addition to the term “conditional release,” this mechanism is referred to as “conditional discharge” or “extended (community) placement.” In other countries, such as the United Kingdom, conditional release is known as a “leave of absence” (aka Section 17 leave) or “supervised discharge” (aka Section 25A leave).

Though the specific legal mechanisms may differ, there are several key components of conditional release common across jurisdictions. First, conditional release occurs prior to the expiration of an initial inpatient commitment order. Second, the patient retains involuntary status and is typically subject to inpatient commitment conditions in the community, such as compliance with treatment. (However, like outpatient commitment, forced administration of medications is not permitted in most jurisdictions.) Third, a patient may be returned to hospital if clinically indicated for reasons including failure to comply with conditions, lack of services in the community, mental health decompensation, or concerns regarding dangerousness to self or others. Fourth, though it may vary widely across jurisdictions (from three months to a year), the period of community supervision is set at the time of release and may be renewed or extended at the discretion of the treating psychiatrist, hospital director, or other clinical decision-maker.

Conditional release has often been used as part of a hospital discharge plan to reassure a community residence that the patient can be readily returned to hospital if they decompensate or fail to comply with treatment expectations. Hospitals, in turn, favor conditional release because they can accept the return of a patient without having to go through formal admission procedures, the most labor intensive of hospital work.

Most of the recent research on conditional release has been conducted in Australia or the U.K. (see profiles in Appendix B.) The majority of these studies report positive findings regarding reduced rehospitalization favoring conditional release. However, one set of no difference findings has been reported by Vaughan and colleagues (2000) from a study comparing the readmission rates of all patients ($n = 123$) who were conditionally released in New South Wales, Australia between July 1994 and July 1998 with 123 patients discharged without conditions, who were matched on sex, age within five years, number of prior hospitalizations, and admission within 12 months of the index hospitalization. The overall rate of readmission did not significantly differ between groups (48% and 37%, respectively); however, there were significantly more involuntary readmissions in the conditionally released group (61%) than in the comparison group

(31%). Though the groups were well-matched on demographic variables, they differed significantly on the length of their index admissions, suggesting that patients who were conditionally released had more severe conditions than those in the comparison group. As a result, it is difficult to accept the findings at face value.

Much more positive findings supporting conditional release can be found in a series of retrospective case-control studies conducted by Segal, Burgess and colleagues in Victoria, Australia drawing upon data from the Victorian Psychiatric Register. Among the strengths of these studies are the large number of cases, multiple years of follow-up, and the use of statistical adjustments to match comparison groups on baseline characteristics. Segal and Burgess (2006a) examined outcomes for 8,879 patients given conditional release between 1990 and 2000,¹ comparing them to 16,094 patients who were hospitalized but not conditionally released. Controls were divided into two subsamples: one matched on age, sex, and diagnosis and the other randomly drawn from those patients who were released without conditions; episodes of care (continuous period of outpatient care without a break in service of 90 days or greater) were the unit of analysis. Despite poorer clinical status and more extensive psychiatric history among conditional released patients compared to controls, results showed that conditional release was associated with fewer inpatient treatment days, shorter inpatient episodes and fewer inpatient days per 100 days in care.

In a second study using the same sample, Segal and Burgess (2006b) examined the effect of conditional release on mortality risk, linking registry data with the Australia National Death Index. Propensity score matching based on social and premorbid characteristics was used to create two balanced groups of patients: those released with conditions and those released without conditions. Relative risk of death was found to be significantly lower (RR = .75) in the conditional release group compared to the usual

¹ Though the authors initially characterize this sample as 8,879 patients *who received a conditional release during the 10-year period* (Segal & Burgess, 2006a, 2006b), later descriptions and analyses of the sample suggest that a portion (87%) received a hospital-initiated order that is consistent with our definition of conditional release (Segal & Burgess, 2008). The remainder, however, received community-initiated orders (5%)—more akin to the American definition of (preventive) outpatient commitment—or combinations of hospital- and community-initiated orders (8%).

release group over the 13.5 years of the study. Further, multivariable logistic regression models showed that for each day on conditional release per 30 days at risk there was a 24% decrease in risk of injury-related death and a 4% decrease in risk of noninjury-related death. Overall, results of these two studies provide strong evidence of the effectiveness of conditional release in reducing inpatient service utilization and mortality risk compared to release without conditions.

In a third analysis using the same sample, Segal and Burgess (2006c) created three comparisons groups: (1) patients given conditional release within 30 days of hospitalization (“early intervention”, $n = 1,008$), (2) patients eligible for conditional release within 30 days, but who were not released (“early eligible”, $n = 1,821$), and (3) patients who were not eligible for conditional release within 30 days, but were conditionally released more than 30 days after hospital admission (“late intervention”, $n = 1,821$). Controlling for sociodemographic characteristics, diagnoses and legal status at index admission, ordinary least-squares regressions showed that the early-eligible and late-intervention groups had significantly more inpatient episodes, total number of inpatient days, number of inpatient days per episode and number of inpatient days per 30 days in the follow-up period compared to the early-intervention group.

These last findings suggest a long-term benefit of conditional release if it occurs earlier rather than later in a hospital stay, reminiscent of the findings reported in the Duke Mental Health Study (Swartz et al., 1999) for long vs. short OPC orders. However, the same selection problem arises here as in the Duke study. The two groups appear to differ on severity and functional status and, in the absence of randomization or proper statistical adjustments for these dimensions, the purported advantages of conditional release sooner vs. later are not supported.

Nonetheless, the two earlier Australian studies provide strong evidence of the effectiveness of conditional release in reducing hospitalizations and mortality risk compared to release from hospital without conditions, though patients may initially be returned to hospital more quickly. The latter finding suggests that the supervision

associated with conditional release affords the opportunity for earlier detection and intervention if a patient is becoming destabilized, preventing serious mental health decompensation and, as a result, reducing inpatient utilization in the longer run. However, there have been no careful studies of the nature and amount of supervision that occurs under conditional release so the whys and hows of the favorable results achieved remained to be clearly documented.

Does conditional release achieve any better or worse results than OPC? A recent U.K. study published in the April 2013 issue of the British medical journal *Lancet* addresses this question by comparing community treatment orders (CTOs) with Section 17 leaves, the British equivalents of OPC and conditional release, respectively (Burns et al., 2013). The Oxford Community Treatment Evaluation Trial (OCTET) was a non-blinded, parallel-arm randomized controlled trial of patients assigned either to CTO (n= 167) or Section 17 leave (n= 169). Eligible patients were those involuntarily admitted to hospital with a diagnosis of psychosis, aged 18-65 years, who were deemed suitable for supervised outpatient care by their clinicians. The primary outcome measure was whether or not the patient was admitted to hospital during the 12-month follow-up period, analyzed with a log-binomial regression model adjusted for sex, schizophrenic status, and duration of illness.

Results showed no difference in the rate of readmission between patients on CTO (36%) and Section 17 leave (36%). Total duration of psychiatric hospitalizations, number of readmissions, number of multiple readmissions, days in the community until first readmission during 12-month follow-up, as well as clinical or social functioning 12 months after randomization, also did not differ between groups. The authors additionally compared variables associated with the “treatment process”. Though number of service contacts per month did not differ significantly between groups, analyses showed that patients on outpatient commitment were under legal compulsion during follow-up for longer (median 255 days vs. 102 days) and had more days in the community under legal compulsion (median 183 days vs. 8 days).

The OCTET study is one of the most rigorous studies conducted to date in this area. It compares two types of mandated community supervision so it does not address the question of what would happen if patients were randomly assigned to voluntary care (no supervision). It does present strong evidence that conditional release can produce equivalent results as OPC with less legal compulsion. These findings, however, contrast with Segal and Burgess's (2008) finding from their Australian research indicating that total number of days hospitalized was lower for those on community treatment orders compared to patients conditionally released from hospital. One implication of these contrasting findings is that it is difficult to generalize findings from one country to another given varying legal, clinical, and cultural contexts as well as marked differences in the availability and intensity of community services. Further, the relevance of the OCTET study for the U.S. has been questioned in part on the grounds that it focused on a group of patients that would not meet OPC criteria in the U.S. (Treatment Advocacy Center, 2013).

Further research is needed in the U.S. to establish the equivalency of outcomes for conditional release and OPC. With the exception of the OCTET study, randomization was not employed in any of the studies reviewed above. Of course, there are many legal and ethical issues that make the ideal RCT design impractical or challenging to implement faithfully. Yet, the studies reviewed above clearly demonstrate that, without randomization or careful statistical controls, selection bias is likely to occur as patients assigned to conditional release, OPC, or other conditions are likely to differ on clinical history and functional status.

The use of conditional release as an alternative to outpatient commitment raises important legal and practical concerns. From a legal perspective, inpatient status is retained while on conditional release; whether patients continue to meet criteria for involuntary hospitalization throughout the duration of their conditional release is infrequently assessed. Thus, though conditional release appears to hold promise as a less restrictive alternative to hospitalization, it is unclear whether it is indeed least

restrictive in all cases as highlighted by the OCTET study or whether it is a better alternative to OPC.

On the practical side, there is the question of monitoring mechanisms and how revocations would be enforced if the case mix on conditional release was expanded to include individuals who otherwise would be candidates for OPC. New York State, for example, invested millions of dollars annually to develop enrollment and monitoring mechanisms to support its Assisted Outpatient Treatment (AOT) program. Current, conditional release options in most states do not have that type of infrastructure in place.

Competency Determinations

There have been various discussions in the literature regarding the use of incompetency rather than dangerousness as the standard for civil commitment, both inpatient and outpatient (e.g., Bloom & Faulkner, 1987; Geller et al., 1997; Lawlor et al., 2007). For example, the argument has been made that involuntary commitment decisions that center on dangerousness are too heavily focused on estimates of the likelihood of *future* dangerous behaviors. Indeed, the high rates of false positives (those identified as dangerous who are not) identified in early research on the accuracy of clinical predictions of violence risk led many to question the dangerousness standard for both civil and criminal commitment (Monahan & Shah, 1989). Though more recent meta-analytic research has shown much higher rates of accuracy and, accordingly, lower rates of false positives (Campbell, French & Gendreau, 2009; Guy, 2008; Singh, Grann, & Fazel, 2011), there remains debate regarding the appropriateness of making commitment decisions on the basis of estimates of risk for future behaviors versus evaluations of current functioning.

Additionally, while dangerousness may be an appropriate criterion for inpatient civil commitment, insofar as the threat of dangerousness to self or others may be mitigated through the high levels of structure and supervision afforded by the hospital setting, the

same mitigating conditions are not available in the community, and thus, the logic of using outpatient commitment to mitigate dangerousness has been questioned (Appelbaum, 1984; Monahan & Shah, 1989). Finally, it also has been argued that to ensure the protection of civil rights, to the extent possible, there should additionally be an evaluation of whether the individual is competent to participate in the civil commitment hearing itself (Bloom & Faulkner, 1987).

Many different models have been proposed to incorporate competency determinations into the civil commitment process. For instance, competency to make informed treatment decisions is central to the Model Commitment Statute of the American Psychiatric Association (Stromberg & Stone, 1983), though the final criteria still require that the individual is likely to harm him/herself or others, or to experience substantial mental or physical decompensation without intervention. In their proposed 4-step model of civil commitment, Bloom and Faulkner (1987) built upon the model statute and not only eliminated the dangerousness criteria, but also suggested that prior to a determination regarding competency to make informed treatment decisions, there should be an evaluation of the individual's ability to understand the commitment process itself (i.e., competency to be committed, akin to the criminal justice standard of competency to stand trial). More recently, Lawlor and colleagues (2007) suggested a competency-based approach to managing violence with involuntary outpatient commitment, suggesting that once the role of mental illness as a contributing factor to violent behavior has been established, there should be a clinical determination of competency to make treatment decisions at the time of the index violence. Thus, under this model, competency may be relevant to civil commitment for some, but not all, patients.

Though competency may be viewed as a complementary or alternative civil commitment criterion, the focus in civil commitment hearings in the U.S. remains on dangerousness, a standard affirmed by the U.S. Supreme Court in the 1972 *Lessard v. Schmidt* decision (Appelbaum, 1984). Indeed, research suggests that the vast majority of individuals who are civilly committed in the U.S. meet dangerousness criteria,

compared to only approximately one-third who meet criteria for grave disability (see Monahan & Shah, 1989). There have been no empirical comparisons of the relative effectiveness or cost of these two approaches. Such an evaluation would be very challenging to conduct, as patients committed under these divergent criteria are quite likely to differ in clinically meaningful ways, precluding randomization to type of commitment criteria and requiring matching procedures or identification of jurisdictions with divergent commitment criteria. As reviewed earlier in our summary of the conditional release evidence, the latter may be more or less successful. That said, there have been evaluations of outcomes for individuals placed on guardianship or conservatorship, following determinations of incompetency. We turn to this evidence in the following paragraphs.

Guardianship and Conservatorship

Guardianship and conservatorship have been suggested as ways to increase the likelihood of outpatient service utilization for those who do not meet the dangerousness criterion, but who are determined to be incompetent or gravely disabled due to mental illness. Guardianship and conservatorship both represent a legal relationship between a protected person and one or more individuals appointed by the court to make decisions on his or her behalf. While guardianship invests the guardian with decision-making authority over all personal affairs (including treatment), the decision-making authority of the conservator is limited to the management of property and financial affairs. These terms, however, are often used interchangeably. In the sections that follow, we focus on guardianship due to its relevance to treatment.

In the United States, petitions for guardianship or conservatorship may be made by professionals in charge of agencies providing assessment or treatment services (e.g., psychiatric hospital director). A guardianship hearing is scheduled at which time the court or a jury may accept or reject the recommendation. If the recommendation is accepted, an appropriate guardian or conservator is identified and appointed by the court. There are four different types of guardianship (and conservatorship): (1) full, (2) limited, (3) temporary, and (4) joint. Full guardianship invests the guardian or

conservator with all decision-making responsibility for a set period of time (e.g., one year). In crisis situations, temporary guardianship may be granted for a limited period (e.g., 30 or 90 days). Some jurisdictions have limited guardianship, in which the guardian or conservator has responsibility for some decisions, while the protected person retains other decision-making authority. Finally, under joint guardianship, the decision-making authority and responsibilities may be shared among more than one party. Regardless of the type, the guardian has three primary responsibilities or roles of particular relevance to the present review: (1) provide treatment consent on behalf of the protected person; (2) require the protected person to live at a specified place, if not in custody; and (3) require the protected person to adhere to treatment. If s/he does not adhere to treatment, the guardian can consent for the protected person to be admitted to hospital.

A handful of studies have examined the effectiveness of guardianship in reducing adverse outcomes among adults with mental illnesses. Lamb and Weinberger (1992) examined the California conservatorship² process by conducting a naturalistic, 4-year follow-up study of a group of 60 psychiatric patients for whom temporary (30-day) conservatorships were obtained. The outcomes of interest were whether 1-year conservatorship was granted, instability (defined as six or more months of hospitalization, or three or more months of psychiatric hospitalization plus at least one of serious physical violence, arrests involving jail time, and homelessness), and family support. In total, 58% of patients ($n = 35$) were granted a 1-year conservatorship during the follow-up period. Statistical tests showed no differences in family support or stability between those placed conservatorship and those not, but the authors nonetheless conclude that guardianship may serve to increase stability and help prevent rehospitalization. (The conclusion may, in part, reflect subgroup comparisons revealing significant associations between stability and family support for the patients placed on conservatorship but not for those not placed guardianship). However, the nonsignificant

² Our reading of this paper suggests that the intervention was more closely aligned with our definition of guardianship than conservatorship; however, we have retained the authors' use of the term conservatorship for the sake of simplicity.

between-group differences, lack of randomization, and small sample size limit the strength of evidence and conclusions regarding guardianship based on this the findings of the study.

Geller and colleagues (1997, 1998) reported findings of two studies examining guardianship in Massachusetts. Using demographic, clinical and service utilization data gathered in a statewide tracking system, 19 patients placed on guardianship were compared to patients matched on sex, diagnosis, age, and time of first appearance in the tracking system (“demographic matches”) and to patients matched on those variables, as well as on the number of admissions and hospital days during the 6-month pre-intervention period (“inpatient use matches”) in the first study (Geller et al., 1998). For each set of analyses, the authors compared the 19 cases to all matches ($n = 53$ and $n = 38$, respectively) and to the best matches (n 's = 19). Pairwise comparisons showed that patients placed on guardianship showed significantly greater decreases than their demographic matches in the number of inpatient days and number of admissions in the 6-month follow-up; however, they also had significantly more inpatient days and admissions in the 6-months pre-intervention. There were no significant differences pre- and post-intervention between the patients placed on guardianship compared to their inpatient matches.

In the second study, the pre- and post-intervention periods were extended to two years (Geller et al., 1997). Neither total inpatient days nor number of admissions differed between patients on guardianship and their demographic and inpatient matches over the full 2-year follow-up (though comparisons within 6-month quarters showed some differences). The authors conclude that the lack of follow-up differences between groups provides evidence of the effectiveness of guardianship due the poorer clinical status of the guardianship group. However, the strength of the evidence in both these studies is weak because of the lack of random sampling and of random assignment to groups, the failure to control for relevant covariates in analyses, and the very small sample size.

Finally, Hatfield and colleagues (2004) reported findings of a quasi-experimental study conducted in England comparing 74 psychiatric patients subject to guardianship with 153 subject to supervised discharge (Section 25A leave). Data were collected on hospital admissions, psychosocial functioning, and adverse outcomes (self-harm, harm to others, self-neglect, exploitation by others and being the victims of abuse) over 6- and 9-month follow-up periods. Results showed that a higher number of patients in the supervised discharge group were hospitalized during follow-up (32%) compared to those in the guardianship group (22%); the prevalence of harm to others also was higher for supervised discharge (63%) compared to guardianship (42%). Ratings on standardized scales indicated that psychosocial functioning improved significantly over follow-up for the guardianship but not the supervised discharge patients. However, there were significant differences between groups on relevant demographic and clinical characteristics that may have biased results. Also, because of the lack of a non-intervention group, the direct impact of community supervision on rate of hospitalization cannot be examined.

In summary, results of these four studies provide some evidence that guardianship may be effective in reducing inpatient service utilization, though findings are mixed. Additionally, there is some evidence that guardianship is associated with higher levels of psychosocial functioning compared to conditional release. However, many methodological issues limit the strength of these findings, including those associated with the selection process. It bears repeating that, in all studies, the baseline characteristics of the intervention and control groups differed significantly in a majority of studies, precluding meaningful comparisons. Moreover, those who met the grave disability and/or incompetency criteria of guardianship may, and likely do, differ on relevant clinical characteristics from those who meet criteria for involuntary outpatient commitment, as well as involuntary inpatient commitment, thereby limiting the generalizability of findings to the target population. As such, though a competency-based approach to court-mandated community treatment may represent a viable alternative to a dangerousness-based approach, at least conceptually, further research

is needed to demonstrate how this process would work and whether or not is effective in comparison to OPC.

There also are many issues that may arise in the application for and implementation of guardianship that merit consideration. For example, the process of obtaining full guardianship can be quite slow and time-consuming involving different courts than those involved in civil commitments of persons with mental illness, and, thus, may not be appropriate in urgent care situations. As noted earlier, temporary orders (e.g., 30-day or 90-day orders) may be issued in such circumstances; however, the subsequent full guardianship requests may be unsuccessful, despite limited improvements in the individual's psychosocial functioning, as the criteria for full guardianship are much more stringent than for temporary orders.

Additionally, identification and appointment of an appropriate guardian may be challenging. Family members, for example, may be reluctant or unable to become guardians or may be unavailable altogether. Guardians may be provided through public guardianship systems or social service agencies; however, these guardians typically have very large caseloads and are overburdened. Moreover, though the court-appointed guardian is expected to act as an advocate for the protected person and to make decisions in their best interest, there is potential for abuse of guardian role and exploitation of the protected person for personal gain. Finally, the appeal process is onerous and case reviews are relatively infrequent. Consequently, the protected person may remain under guardianship long after competency has been restored.

Psychiatric Advance Directives

Another option to OPC is the use of psychiatric advance directives (PADs). These are legal documents that permit competent adults to make choices in the present about their future psychiatric treatment if they lose their decision-making capacity and to authorize a legally appointed proxy to make decisions on their behalf during incapacitating crises (Srebnik & LaFond, 1999; Swanson). In the U.K., advance

directives are known as “joint crisis plans” (Thornicroft et al., 2011). PADs are viewed as an alternative to the coercive interventions that sometimes accompany mental health crises for persons with mental illness. However, PADs are also viewed as having the potential to positively affect a variety of other clinical outcomes, including but not limited to treatment engagement, treatment satisfaction, and working alliance (Van Dorn et al., 2010). By their very nature, PADs are only applicable as an OPC option in situations when an individual is competent to make future treatment decisions and thus are not relevant in all crisis situations.

Swanson and colleagues report findings from a survey of 1,011 psychiatric outpatients in five U.S. cities about their interest in, and completion of, PADs (Swanson et al., 2006). Across the sites, only 4 to 13 percent of participants had completed a PAD; however, between 66 and 77 percent reported wanting to complete one if given assistance. Significantly higher demand for PADs was found among participants who were female; were nonwhite; had a history of self-harm, arrest, and decreased personal autonomy; and those who felt pressured to take medication. Actual completion of PADs was more likely among participants with higher insight, those reporting leverage by a representative payee, and those who felt external pressure to keep outpatient appointments for mental health treatment.

In a related study, Van Dorn and colleagues (2006) report findings from a survey of 591 mental health professionals (psychiatrists, psychologists, and social workers) regarding perceptions of barriers to the implementation of PADs. Across the three professional groups barriers related to operational features of the work environment (e.g., lack of communication between staff, lack of access to the document) were reported at a higher rate than clinical barriers (e.g., inappropriate treatment requests, consumers' desire to change their mind about treatment during crises). However, psychiatrists were more likely to report clinical barriers to implementation than both psychologists and social workers. In multivariable analyses, legal defensiveness, employment in public sector mental health services, and a belief that treatment refusals will outweigh the benefits of PADs were associated with more perceived barriers,

whereas age and endorsing positive perceptions of PADs were associated with fewer perceived barriers.

One of the most comprehensive PADs outcomes studies has recently been reported in the U.K. (Thornicroft et al., 2013). The CRIMSON (CRisis plan IMPact: Subjective and Objective coercion and eNgagement) study is an individual level, randomized controlled trial that compared the effectiveness of Joint Crisis Plans (JCPs, n= 285) with treatment as usual (n= 284) for people with severe mental illness. The study was done with 64 generic and specialist community mental health teams in four English mental health care agencies. No significant treatment effect was seen for the number of compulsory admissions in 12-months, the primary outcome, with 18% in the JCP group readmitted involuntarily vs. 20% in the usual care control group. Also, there were no significant differences on the secondary outcomes of any psychiatric admissions, length of stay, perceived coercion, and improved engagement. However, patient with joint treatment plans expressed less perceived improvement in their therapeutic relationships. The study revealed wide variability among participating agencies and teams in how the JCPs were implemented, the buy-in of clinicians, and the extent to which plans were followed. Development and monitoring of JCPs were often combined with routine clinical review meetings which did not actively incorporate patients' preferences.

Here again, the applicability of U.K. finding to the U.S. is an issue. Further research is needed in the U.S. to clarify the extent to which advance directives for individuals who are competent to complete them can substitute for OPC orders. PADs are being actively studied in the U.S. so further evidence is likely to emerge in the near future.

Summary of Alternatives

The empirical literature on these evidence-based alternatives has several strengths. In particular, studies have been conducted in multiple jurisdictions, including studies conducted in the U.S. (with the exception of conditional release), most studies have examined outcomes over 12-month periods or longer, and some studies have directly outcomes for patients on alternative forms of community treatment orders to those on

outpatient commitment. However, a number of limitations have been highlighted in our review. First, with the exception of the OCTET and CRIMSON studies, most research in these areas do not use RCTs to compare patients who have received one of the alternatives reviewed herein to patients in the community without community treatment orders. Second, many studies have had small samples with limited generalizability or power. Third, the alternatives operate under very different legal mechanisms from community-initiated or preventive outpatient commitment orders. As such, the target populations may differ in meaningful ways. Finally, to date, there are no published studies examining cost (overall, cost savings, cost effectiveness, or otherwise) of these alternatives. [A CRIMSON cost analysis is expected shortly.]

Thus, our review of conditional release, incompetency determinations, guardianship and psychiatric advance directives suggest they have promise as partial alternatives to preventive outpatient commitment orders. In fact, they may serve complementary functions as they target different populations at different stages of illness and through different legal mechanisms. That said, further research is needed regarding for whom they are appropriate and under what circumstances.

4. Conclusions

Several conclusions can be drawn from the research considered in this report, as follows:

1. To date, there is a moderate amount of evidence that OPC reduces hospital admissions, but not days. There is emerging evidence on greater engagement in treatment, both through greater medication possession and overall treatment engagement, but these studies have considerable limitations, and the only RCT in this area found no effect on medication adherence. There is little solid evidence on reductions in criminal justice interactions or on costs.

2. Whether court orders without intensive treatment have any effect is an unanswered question. Some would argue that this is a meaningless question since OPC, at its core, is a treatment mandate. However, the New York State AOT study does provide moderately strong evidence that OPC reduces inpatient psychiatric admissions and increases service engagement over and above what can be accomplished by intensive services alone.
3. There is a research literature on patient-centered outcomes and quality of life, but these studies were outside the scope of the current review which focused primarily on state-centered outcomes. A comprehensive assessment of OPC should address the evidence and impacts of OPC on individuals and their families or other social supports.
4. Current research does not point to really strong alternatives to OPC with regard to conditional release, competency vs. dangerousness standards for civil commitment, guardianship or conservatorship, or advance directives. Each of these options might apply to some but not all individuals who are placed on OPC. Further research is required to establish whether these alternatives work, for whom, and under what circumstances.
5. These evidence shortfalls do not necessarily point to a need for further randomized trials. RCTs are expensive undertakings that take years to complete. Results are often difficult to extrapolate to other jurisdictions with different demographics, oversight arrangements, and service arrays. Rather, careful analysis of observational data with rich sets of baseline characteristics on study participants will yield more timely and fruitful results at a lower cost.

Appendix A. Biographical Sketches

Joseph P. Morrissey, Ph.D. is a sociologist who has worked on evaluating the organization, financing, and effectiveness of mental health programs at the national, state, and local levels. With research funding from government agencies and private foundations he has studied state psychiatric hospital utilization, community services for homeless people with severe mental illness, jail mental health services, behavioral health carve-outs, and the impact of managed care on persons with severe mental illness. His recent work has focused on the cost-effectiveness of recovery-oriented ACT teams, the impact of expedited Medicaid restoration on community re-entry of state prisoners with severe mental illness, the effectiveness of forensic ACT teams, and the use of medical homes by persons with severe mental illness. His publications on these and related topics have appeared in leading mental health and psychiatric journals. Dr. Morrissey has held positions at the National Institute of Mental Health, Brandeis University, the New York State Office of Mental Health, and currently, at the University of North Carolina at Chapel Hill where he is Professor of Health Policy and Management in the Gillings School of Global Public Health. He is a recipient of the Carl A. Taube Award given by the American Public Health Association for career contributions to the public mental health field.

Marisa E. Domino, Ph.D. is health economist who has evaluated a number of mental health policies and insurance coverage issues. Dr. Domino's research interests include the economics of mental health, agency relationships among physicians, patients and insurers, the diffusion of new technologies, and the public provision of health care and health insurance to low income populations. Dr. Domino completed a K01 Career Development Award from the National Institute of Mental Health to analyze factors that affect the diffusion of psychotropic medications in a variety of populations. She has worked on a variety of projects on insurance design, behavioral health, and chronic illness including papers on the use medical homes in specialty care populations, behavioral health carve-outs, and assertive community treatment. She has also received funding from Changes in Health Care Financing & Organization Initiative at RWJF and AcademyHealth to examine the effect of days' supply policy changes on prescription medications and health services use and from NARSAD to examine the effect that Medicare Part D has had on individuals with severe mental illness. Dr. Domino received her Ph.D. in Health Economics from the Johns Hopkins University and completed a Postdoctoral Fellowship in the economics of mental health at Harvard Medical School's Department of Health Care Policy. She is currently a Professor in the Department of Health Policy and Management in the Gillings School for Global Public Health at the University of North Carolina at Chapel Hill.

Sarah L. Desmarais, Ph.D. is a forensic psychologist who works on issues at the intersection of the mental health and criminal justice systems. Her current research is focused on the development, implementation, and evaluation of evidence-based practices for the prevention of violence, recidivism, and related risks among adults with serious mental illnesses. Dr. Desmarais trains and consults with mental health and criminal justice agencies worldwide on the implementation of structured risk assessment approaches and other evidence-based practices; recent clients include the Council of State Government Justice Center and the California Department of State Hospitals. She is also a member of the Corrections Management and Reentry Task Team, under the Global Justice Information Sharing Initiative which serves as a Federal Advisory Committee and advises the U.S. Attorney General on justice information sharing and integration initiatives. She completed her graduate training in forensic psychology at Simon Fraser University, and postdoctoral research and teaching fellowships in public health and psychology, respectively, at the University of British Columbia in Vancouver, Canada. She was an Assistant Professor at the University of South Florida, jointly appointed in the departments of Mental Health Law & Policy and Community & Family Health from 2010-2012. Currently, Dr. Desmarais is an Assistant Professor of Psychology at North Carolina State University.

B. Characteristics of Studies Reviewed

CHARACTERISTICS OF STUDIES: Outpatient Commitment Review

Author (date)	Design/analysis	Participants	Comparisons	Outcomes	Key Findings	Biases/Comments	Strength of Evidence
Swartz et al. (1999)	RCT, both at person level and longitudinal	Persons with SMI involuntarily hospitalized and awaiting a period of court-ordered outpatient commitment (see other inclusion criteria in manuscript), recruited from one state and three other hospitals. Persons with recent serious acts of violence involving injury or use of a weapon were excluded from randomization	Intervention: involuntary OPC of variable length Control: release from OPC	Primary: Hospitalization (Any, days, number of hospital stays) Other:	No difference in hospitalization in person-level analysis, lower hospitalization in longitudinal analysis	Generalizability concerns both due to RCT and enhanced treatment available to both arms; Step-wise selection of covariates can lead to biases from multicollinearity, not reported	Strong
Swartz et al. (1999)	Treatment stratified by length of orders	Same as above	Those receiving long orders vs. those receiving short orders	Same as above	Long orders associated with lower risk of rehospitalization	Endogeneity – it is likely that length of orders is correlated with other treatment outcomes such as clinical/legal status, which would bias this comparison	Weak
Steadman et al. (2001)	RCT	Persons with SMI involuntarily hospitalized and awaiting a period of court-ordered outpatient	Intervention: involuntary OPC of variable length Control: release from OPC	Rehospitalization, arrest, quality of life, symptomatology, treatment noncompliance,	No difference in any study outcomes	Small sample size, baseline group differences, confounding of intervention, lack of OPC order enforcement	Weak

Author (date)	Design/analysis	Participants	Comparisons	Outcomes	Key Findings	Biases/Comments	Strength of Evidence
		commitment (see other inclusion criteria in manuscript), recruited from one public general hospital. Persons with recent serious acts of violence involving injury or use of a weapon were excluded from randomization.		perceived level of coercion			
Swartz and Swanson, 2013	RCT	Same as Swartz et al., 1999	Intervention: Control:	Primary: total service related costs Other: Components of costs	Difference in total costs between study arms is positive but small, amounting to less than 7% of control group costs.	No additional concerns related to costs, otherwise same concerns as Swartz et al., 1999	Strong
Swartz and Swanson, 2013	Treatment stratified by length of orders	“		“	Costs were much larger for the non-renewed OPC subjects than for the renewed subjects (\$54,857.87 vs. \$25,921.72).	“	Weak
Swartz et al., 2010	Observational, pre-post with propensity score weighting	Adult Medicaid recipients with SMI receiving AOT (n=3609)	Intervention: Receiving court-ordered AOT Control: Same participants, but	Primary: Hospitalizations and days hospitalized Other: Any case management	Odds of psychiatrist hospitalization were 25% lower during initial 6 months of AOT and by 33%	Pre-post analysis has multiple threats to validity, including regression to the mean and other events associated with outcomes occurring	Weak

Author (date)	Design/analysis	Participants	Comparisons	Outcomes	Key Findings	Biases/Comments	Strength of Evidence
			prior to AOT receipt	services, Medication Possession Ratio (MPR) for primary psychiatric disorder dichotomized at 80%.	during subsequent months, as compared to pre-AOT. Similar reductions were observed in hospital days. Improvements were observed in Medicaid possession and case management service receipt.	disproportionately in the post- period (e.g., Medicaid policy changes). Generalizability of the results may be a concern, since additional funding was provided for outpatient services as a part of Kendra's law.	
"	Observational, case-control with multivariate controls	Adult Medicaid recipients, with 12 months of care manager data, and receiving ACT alone or in combination with AOT, or ICM in combination with AOT.	Receiving Court ordered AOT Control: ACT/ICM recipients	Psychiatric hospitalization, motivating to engage in services as determined by case managers	Substantially lowered odds of hospitalization and greater odds of engagement in services from AOT+ICM or ACT groups as compared with ACT alone.	Endogeneity – assumes no selection bias into the three study arms. Generalizability – the same size was reduced substantially due to reliance on complete case manager data	Moderate
Swanson et al., 2013	Observational	634 individuals who started an assisted outpatient treatment order within 30 days of discharge from an index hospitalization between January 2004 and December 2005	Those receiving ICM or ACT within 30 days of a hospital discharge but not on AOT (n=255)	Mental health and medical costs paid for all or in part by state sources, and criminal justice costs	AOT reduces state costs	Similar to above: pre-post analyses likely has substantial regression to the mean; no formal comparison is done between AOT and ACT/ICM controls	Weak
Swanson et al., 2001	RCT	Same as Swartz et al., 1999; reduced	Same as Swartz et al., 1999	Arrests	No difference in arrests between	Same concerns as Swartz et al., 1999 with additional concerns	Strong

Author (date)	Design/analysis	Participants	Comparisons	Outcomes	Key Findings	Biases/Comments	Strength of Evidence
		somewhat due to additional missing data			OPC and controls	about generalizability from additional missing data	
Swanson et al., 2001	Treatment stratified by length of orders	“	Those receiving long orders vs. those receiving short orders	“	Persons receiving longer orders have lower odds of arrests	Same endogeneity concerns as Swartz et al., 1999	Weak
Gilbert et al., 2010	Observational	181 adults with either an affective or schizophrenia spectrum disorder who qualified for AOT in NY, and either received AOT or persons signed voluntary service agreements	Comparisons across five types of observations: those prior to any AOT/voluntary agreements, during AOT only, during voluntary agreements only, post-AOT, and post-voluntary agreement	Arrests	Substantially lower odds of arrest for months with active AOT as compared with pre-AOT/pre-voluntary arrest months (OR=0.39; p<0.01); no other statistically significant differences were noted	Degree of selection bias into the study sample is unknown; Same endogeneity concerns as Swartz et al., 1999	Weak
Busch et al., 2010	Observational			Medication Possession Ratio from Medicaid claims data, dichotomized at >>80%.	Improvements in MPR were noted in all 3 treatment groups	Same endogeneity concerns as Swartz et al., 1999; MPR overstates adherence	
Swartz et al., 2001	RCT	Same as Swartz et al., 1999	OPC vs. controls	Multidimensional subjective adherence with medications and appointments (usually/always)	No difference between groups	No additional concerns in the RCT analysis over Swartz et al., 1999	Strong
Swartz et al., 2001	RCT for non-violent group; OPC only for 45 participants with a	OPC arm from Swartz et al., 1999 plus violent participants not randomized	Long vs. short orders in OPC recipients	“	Participants with sustained OPC orders had substantially greater	Length of orders is endogenous; limited covariates because of stepwise procedure do	Weak

Author (date)	Design/analysis	Participants	Comparisons	Outcomes	Key Findings	Biases/Comments	Strength of Evidence
	history of violence.				adherence than those with shorter orders; this effect was larger among persons who received frequent outpatient services	not mitigate endogeneity concerns	

CHARACTERISTICS OF STUDIES: Conditional Release Review

Author (date)	Design/analysis	Participants	Comparisons	Outcomes	Key Findings	Biases/Comments	Strength of Evidence
Burns et al (2013)	RCT with 12-month follow-up. Data collected from hospital records and standardized assessment tools.	333 patients involuntarily admitted to hospital deemed suitable for supervised outpatient care	Intervention: outpatient commitment Control: conditional release (Section 17 leave)	Primary: readmission during follow-up Other: total duration of psychiatric hospitalizations, number of readmissions, number of multiple readmissions, days until first readmission, clinical and social functioning	No difference in rate of readmission. Patients on outpatient commitment were under legal compulsion during follow-up for longer and had more days in the community under legal compulsion.	There may be differences in the patients deemed suitable for supervised outpatient care in the UK than in the US.	Strong
Segal & Burgess (2006a)	Quasi-experimental between- and within-groups design. Data collected over a 10-year period from the Victorian Psychiatric Register and	24,973 hospitalized in Victoria, Australia	Intervention: conditional release Control: matched patients who were hospitalized but not conditionally released	Primary: inpatient days, inpatient episodes, community treatment days, and treatment days per 100 days in care	Conditional release was associated with fewer inpatient treatment days, shorter inpatient episodes, fewer inpatient days per 100 days in care, and fewer days per month in the hospital after conditional release compared to before.	No randomization. Follow-up period for each patient unknown. Segal & Burgess (2008) suggests not all patients in the intervention group were conditionally released.	Moderate

<p>Segal & Burgess (2006b)</p>	<p>Quasi-experimental between-groups design with propensity scoring.</p> <p>Data collected over 10-year period from the Victorian Psychiatric Register and over a 13.5-year period from the Australia National Death Index.</p>	<p>24,973 psychiatric patients hospitalized in Victoria, Australia</p>	<p>Intervention: conditional release</p> <p>Control: matched patients who were hospitalized but not conditionally released</p>	<p>Primary: mortality risk</p> <p>Other: mortality risk by type (injury-related vs. noninjury-related)</p>	<p>Relative risk of death in the conditional release group was significantly higher.</p> <p>For each day on conditional release per 30 days at risk, 24% decrease in risk of injury-related and 4% decrease in risk of noninjury-related death.</p>	<p>No randomization.</p> <p>Follow-up period for each patient unknown.</p> <p>Segal & Burgess (2008) suggests not all patients in the intervention group were conditionally released.</p>	<p>Moderate</p>
<p>Segal & Burgess (2006c)</p>	<p>Quasi-experimental between-groups design.</p> <p>Data collected over 10-year period from the Victorian Psychiatric Register Index.</p>	<p>8,879 psychiatric patients conditionally released from hospital in Victoria, Australia</p>	<p>Intervention: conditional release within 30 days of hospitalization</p> <p>Control: conditional release after 30 days of hospitalization</p>	<p>Primary: number of inpatient episodes, total number of inpatient days, inpatient days per episode, and inpatient days per 30 days in the mental health system</p>	<p>Patients conditionally released after 30 days had more inpatient episodes, total number of inpatient days, number of inpatient days per episode and number of inpatient days per 30 days in the system compared to patients conditionally released within 30 days.</p>	<p>No randomization or matching.</p> <p>Follow-up period for each patient unknown.</p> <p>Segal & Burgess (2008) suggests not all patients in the intervention group were conditionally released.</p>	<p>Moderate</p>

Segal & Burgess (2008)	<p>Quasi-experimental between-groups design.</p> <p>Data collected over 10-year period from the Victorian Psychiatric Register Index.</p>	8,879 psychiatric patients conditionally released from hospital in Victoria, Australia	<p>Intervention: conditional release</p> <p>Control: community-initiated orders and combination orders</p>	<p>Primary: inpatient episodes, inpatient days</p> <p>Other:</p>	<p>Community-initiated group experienced fewer inpatient days and the mixed orders group experienced more inpatient days compared to the conditional release group.</p>	<p>No randomization or matching.</p> <p>Follow-up period for each patient unknown.</p>	Moderate
Vaughan et al (2000)	<p>Quasi-experimental between-groups design.</p> <p>Data collected from hospital records.</p> <p>Mean follow-up period of 27.7 months (range = 12-60 months).</p>	246 psychiatric patients released from hospital in New South Wales, Australia	<p>Intervention: conditional release</p> <p>Control: matched patients discharged without conditions</p>	<p>Primary: readmission</p> <p>Other: type of readmission</p>	<p>Overall rate of readmission did not differ between groups.</p> <p>Significantly more involuntary in readmissions in the conditional release group.</p>	<p>No randomization; groups were well-matched on demographic variables, but length of index admission was significantly higher in the conditional release group and not controlled for in analyses.</p>	Weak

CHARACTERISTICS OF STUDIES: Guardianship

Author (date)	Design/analysis	Participants	Comparisons	Outcomes	Key Findings	Biases/Comments	Strength of Evidence
Geller et al (1997)	<p>Quasi-experimental between-groups design.</p> <p>Data collected from the Massachusetts Department of Mental Health Client Tracking System for 2-years before and after intervention.</p>	60 psychiatric patients in Massachusetts between May 1991 and November 1993.	<p>Intervention: guardianship</p> <p>Control: patients without guardianship</p>	Primary: number of inpatient days and number of admissions	Neither total inpatient days nor number of admissions differed between patients on guardianship and their demographic and inpatient matches.	<p>No randomization.</p> <p>Significant differences on clinically-relevant variables between groups.</p> <p>Very small sample.</p>	weak
Geller et al (1998)	<p>Quasi-experimental between-groups design.</p> <p>Data collected from the Massachusetts Department of Mental Health Client Tracking System for 6-months before and after intervention.</p>	57 psychiatric patients in Massachusetts between May 1991 and November 1993.	<p>Intervention: guardianship</p> <p>Control: patients without guardianship</p>	Primary: number of inpatient days and number of admissions	Greater decreases in the number of inpatient days and number of admissions in the 6-month follow-up period between guardianship patients and their	<p>No randomization.</p> <p>Significant differences on clinically-relevant variables between groups.</p> <p>Very small sample.</p>	weak

					demographic matches.		
Hatfield et al (2004)	<p>Quasi-experimental between-groups design.</p> <p>Data collected from hospital records and standardized assessment tools over 6- and 9-month follow-up periods.</p>	<p>306 psychiatric patients subject to guardianship or supervised discharge in England between September 1998 and October 1999.</p>	<p>Intervention: guardianship</p> <p>Control: supervised discharge (section 25A leave)</p>	<p>Primary: hospital admissions</p> <p>Other: psychosocial functioning, and adverse outcomes (self-harm, harm to others, self-neglect, exploitation by others, abuse)</p>	<p>Patients on supervised discharge were more likely to be hospitalized and to engage in harm to others.</p> <p>Psychosocial functioning improved significantly over follow-up for guardianship but supervised discharge.</p>	<p>No randomization.</p> <p>Significant differences on clinically-relevant variables between groups.</p>	weak
Lamb & Weinberger (1994)	<p>Quasi-experimental between- and within-groups design.</p> <p>Data collected from court, hospital and Los Angeles County Department of Mental Health Computerized Management</p>	<p>60 psychiatric patients for whom temporary conservatorships were obtained in the Los Angeles County/University of Southern California Psychiatric Hospital between January 1986 and March 1986</p>	<p>Intervention: full guardianship</p> <p>Control: patients for whom full guardianship applications were not successful</p>	<p>Primary: instability (6 or more months of hospitalization, or 3 or more months of psychiatric hospitalization plus at least one of serious physical violence, arrests involving jail time, and homelessness),</p>	<p>There were no differences in family support or stability between those placed guardianship and those not.</p>	<p>No randomization.</p> <p>Significant differences on clinically-relevant variables between groups.</p> <p>Very small sample.</p>	weak

	Information System over a 4-year follow-up period.			and family support.			
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