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Impact of the Patient-Clinician Relationship on Medical and Psychological Outcomes:

A Meta-Analysis of Randomized Controlled Trials

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Honors Thesis

Department of Psychological Sciences

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Abstract

The role of the patient-provider relationship in delivery and effectiveness of medical treatments is an emerging research area that should benefit from a review and synthesis of its findings. Therefore, I conducted a meta-analysis of peer-reviewed randomized-controlled clinical trials that attempted to improve the patient-provider relationship by targeting the behavior of the provider. The sample of studies that contributed effect sizes to the meta-analyses were published between November 1, 2012 and October 31, 2018 (a 6-year span). Only studies that measured the intervention effectiveness using objective medical outcomes or verified subjective psychological/behavioral outcomes were included in the meta-analysis. Eight randomized controlled trials met eligibility criteria and their effect sizes were analyzed using a random effects model. The studies' combined effect-size was small but statistically significant, a finding that remained unchanged when the effect sizes were grouped by type of outcome, medical vs psychological. The results suggest that manipulation of the patient-provider relationship via the provider has small but statistically significant and positive effects on healthcare outcomes.

Impact of the Patient-Clinician Relationship on Medical and Psychological Outcomes:

A Meta-Analysis of Randomized Controlled Trials

Every time an individual patient comes in contact with the healthcare system a unique relationship develops. This relationship sees patients at their most vulnerable and requires providers be taught and trained to the highest standard to treat and protect these patients. Medicine prioritizes technological advancement and evidence-based medicine to maximize efficiency and thus cut down on cost, but it comes at the expense of empathetic interactions (Jeffrey, 2016). Heavy workload and burnout as a result of high-output health care services can act as barriers for providers to consistently practice empathy (Elayyan, Rankin, & Chaarani, 2018). Elayyan et al. (2018) identify also lack of organizational support, which includes both reduced supervisor support and decreased resources for empathetic care, as an obstacle for physician empathy in clinical settings. Notably, Elayyan et al. (2018) identified role modeling of the professional climate as a factor that may foster physician empathy. Environments in which many colleagues are treating their patients with more empathy encourage others to practice more empathy. Alluding to potential for change in the profession, Elayyan et al. (2018) noted that empathic environments could be cultivated by training workshops.

While it is unclear why or how patient-provider interactions that miss the empathetic touch may impact the effectiveness of medical treatments, there is increasing interest in studying how the patient-practitioner relationship can be improved and whether that improvement translates into better treatment outcomes (Kelley, Kraft-Todd, Schapira, Kossowsky, & Riess, 2014). A noticeable body of research pivots around the ideas of “patient-centered care” and “shared decision making.” Patient-centered care has been defined as care that is “respectful of and responsive to individual patient preferences, needs, and values, and ensuring that patient

values guide all clinical decisions” (Epstein, Fiscella, Lesser, & Stange, 2010, p. 1489). Patient-centered care prioritizes seeing the patient as a person beyond their medical condition and taking additional considerations when addressing specific populations such as children, the elderly, or other vulnerable populations.

“Shared decision making” aligns with patient-centered care, as it has been defined as “the formal process or tool that helps physicians and patients work together to choose the treatment option that best reflects both medical evidence and the individual patient’s priorities and goals for his or her care” (Franklin & Zhang, 2014, p. 619). Shared-decision making can be implemented in a one-on-one meeting with a provider and a patient, or it can be implemented across multiple providers for individual patients, providing coordinated care that reduces pressure on the patient to act as an intermediary between their providers (Kalter-Leibovici et al., 2017). Research shows that shared decision making is most influential in the goal-setting process, with patients reporting many beneficial effects such as a sense of ownership and confidence in their treatment plan (Rose, Rosewilliam, & Soundy, 2017).

Literature investigating the therapeutic impact of empathy and related interactions between medical practitioners has been systematically reviewed before. Many of the reviews have included studies with weak internal validity due to high within subject variability and use of questionable treatment outcome measures (Kelley et al., 2014). Some reviews have included studies of patients who do not have a specified disease, and as such the studies included in these reviews use studies that do not include objective disease state markers (Griffin et al., 2004). Given the scarcity of randomized clinical trials examining the therapeutic impact of enhancing the patient-provider relationship, most systematic reviews of this literature to date have been purely narrative (Blasi, Harkness, Ernst, Georgiou, & Kleijnen, 2001; Ong, de Haes, Hoos, &

Lammes, 1995; Stewart, McWhinney, & Buck, 1979). Another group of reviews included observational studies alongside randomized controlled trials, but these reviews are also limited about the extent to which many of the studies they reviewed could address causality questions (Harrington, Noble, & Newman, 2004).

Although narrative or descriptive reviews provide a good foundation for understanding a body of research, narrative reviews often do not go beyond simple descriptions and counts of how many studies found significant treatment effects and of how many studies did not. Kelley et al. (2014) sought to elevate the field of research synthesis by conducting a meta-analytic review of only randomized controlled trials that manipulated the patient-practitioner relationship. Unlike prior literature reviews, their analysis included only studies conducted with patients with a specific medical condition, and only studies for which the impact of modifying the patient-provider relationship was tested using either objective medical outcomes or validated subjective outcomes. In their meta-analysis, Kelley et al. (2014) also excluded studies that examined patients with mental health conditions and substance abuse disorders, as well as studies that investigated children.

Kelley et al. (2014) searched EMBASE and MEDLINE from their earliest entries to November 1, 2012. Their inclusion criteria limited their review to peer-reviewed randomized controlled trials in which patients were treated for a specific disorder and the patient-provider relationship was systematically manipulated. Studies that solely manipulated the patient-clinician relationship by manipulating the patients' behavior were excluded. Studies where the clinical encounter time was unequal between the intervention and the control conditions were also eliminated. They identified 13 articles that met inclusion criteria and analyzed their effect sizes using a random-effects model. They computed Cohen's *d* for each primary outcome identified in

the studies and found a combined, very modest but significant effect size that was small but consistently positive across studies.

Assuming that the body of research that has used random clinical trial to assess the therapeutic impact of the patient-provider interaction has grown considerably in the last 6 years since the publication of the last clinical trial included in the meta-analysis by Kelly et al (2104), an update to their review should result in a meaningful contribution. The present meta-analysis seeks to further characterize the field and test the therapeutic effect of the relationship between medical patients and their healthcare providers. To this effect, I will calculate treatment effects for randomized clinical trials published since November 1st 2012. I will also code the studies by outcome type into medical and psychological/behavioral outcomes to test whether therapeutic effectiveness differs as a function of outcome type.

Methods

Sample Selection

The purpose of the present meta-analysis was to actualize Kelley's et al. (2014) meta-analysis on the impact of the patient-provider relationship on healthcare outcomes. Therefore, I replicated Kelley's et al (2014) literature-search strategy as follows. First, I used their same keywords and keyword terms and applied them to the *Cochrane Central Register of Controlled Trials* database. Although Kelley et al (2014) searched the *Medline* and *Embase* databases, the *Cochrane Central Register of Controlled Trials* ("*CENTRAL*," 2008) contains all randomized and quasi-randomized controlled trials that are included in *Embase* and *PubMed*, which includes *Medline* and other sources. Once I confirmed that all the studies included in Kelley's et al. (2014) meta-analysis were captured by the search strategy described above, I limited the search

to include only studies published after November 1, 2012 (or the date of the most contemporaneous clinical trial included by Kelley et al., 2014). Thus, my search covered 6 years of publications, or from November 1, 2012 to October 31, 2018. This search yielded a total of 1,127 entries.

Given the large number of “hits” generated by the search strategy, I introduced additional limitations to the search strategy to exclude studies that did not meet the inclusion criteria. Given that my population of focus was adult patients seeking a traditional “Western” medical treatment for an identified physiological condition or illness, I used keyword combinations to exclude studies that targeted children (<18-years of age), patients with psychiatric disorders, and/or studies that used mental health professionals (e.g., counselors, psychotherapists, psychologists, psychiatrists, social workers), acupuncturists, and/or telemedicine practitioners. This strategy reduced the number of randomized clinical trials potentially eligible for inclusion to 556. (See Appendix 1 for a full list of inclusion/exclusion terms).

The resulting 556 title and abstract entries from the search were downloaded and numbered. Professor Cepeda-Benito and I read the titles of all 556 entries to independently judge whether any of the entries could be eliminated for not meeting the inclusion criteria (i.e., a randomized controlled clinical trial seeking to measure the differential effectiveness of improving the patient-clinician relationship on medical outcomes in adult patients). That is, at this point we only excluded studies we both agreed we could eliminate because their title clearly identified one of our exclusion criteria (see Figure 1). We then read the abstracts of all the studies we could not eliminate from their titles alone (i.e., studies either only one of us, or none of us, flagged for elimination). After reading the abstracts, we eliminated studies both of us independently flagged

for exclusion but discussed whether to keep or exclude all other entries. After removing repeat entries, studies without accessible abstracts, and studies we both agreed should be eliminated, 133 studies remained.

I used the full text of each article to create an Excel table that summarized the specific interventions and outcome measures of each of the 133 papers. Once the table was created, Dr. Cepeda-Benito and I discussed whether each of the studies met inclusion criteria. This process excluded 94 additional studies that did not meet the inclusion criteria and 8 studies for which a full text could not be found. The 31 remaining studies were then coded by intervention and outcome type. Interventions were either active or passive. Active procedures aimed to train or educate the practitioner ($k = 8$), or the patient ($k = 12$), or both ($k = 3$). Studies with passive procedures ($k = 8$) were those that used methods to make information more readily accessible or available but did not engage clinicians or patients in active training or educational activities.

Outcomes were classified as “medical” or “behavioral/psychological.” Medical outcomes were defined as any measure that assessed the presence or amount of an illness or medical condition, including biological markers of the disease state, adverse events, hospitalization events, death, or symptom-specific burden. Behavioral/psychological outcomes were measures that assessed global function and psychological well-being, including measures of distress, anxiety, decisional conflict, quality of life, and patient empowerment. Patient compliance with treatment was coded as a behavioral/psychological outcome. Given that the Kelly’s et al. (2014) meta-analysis included only studies that manipulated the provider demeanor, the present report focuses on the results of the 8 randomized studies with interventions that aimed to educate and train the healthcare provider.

Effect-size Calculations and Groupings

Data analysis was completed using *Comprehensive Meta-Analysis Software (CMA)*, a user-friendly software that allows for a variety of data input types and is broadly used and cited in meta-analytic reports (Bax 2017). CMA computes combined effect sizes according to two different models, or assumptions, based on the characteristics of the sample of studies from which the data is collected. If the collection of studies used procedures judged to be relatively homogeneous or consistent across studies, there should be an expectation that the results across studies should be relatively homogeneous. That is, the assumption is that there is a single, true effect size and that effect-size variability across studies will be “null” or statistically not different from zero. This expectancy of homogeneity across study effect-sizes uses a fixed-effects model approach to calculate effect sizes. In addition to a test that examines whether the combined effect-size across studies is significantly different from zero, the fixed-effect size model requires a test for the homogeneity of effect sizes. To accomplish this, CMA provides a “*Heterogeneity*” statistic or Q . The Q statistic and associated degrees of freedom (k studies minus 1) is similar to a Chi-square distribution and absence of statistical significance is indicative of effect size homogeneity. CMA also produces the I^2 statistic, which quantifies the dispersion of effect sizes as the percentage of variation across studies that is due to heterogeneity rather than chance. In the fixed-effect size model, larger studies are also given more weight than smaller studies.

Given that the sample of studies in the present meta-analysis was rather heterogeneous, particularly with respect to the diagnoses and medical conditions of the patients, I did not expect the studies would yield a single, true effect size, but rather that the combined effect size would represent the average of a heterogeneous distribution of effect sizes. Thus, I assumed a random effects model to calculate and interpret effect sizes, where all studies were weighed equally.

Computationally, fixed and random-effect models may yield different effect sizes, and because computationally the random model assumes more error, the random-effects model is in theory more conservative, or not as likely to find a statistically significant overall effect sizes (Borenstein, 2013). However, because the fixed-effect size model allows for an examination of the amount of variability across effect sizes, and as there are not heterogeneity statistics associated with the random-effects model, I will report also heterogeneity statistics associated with the fixed-effects model.

When possible, effect sizes were converted to Cohen's d by imputing into CMA the raw data or the summary statistics reported in the results sections or appendices of the studies. For studies that provided "hazard ratios" (Kalter-Leibovici et al., 2017), and "relative risk ratios" (Tinsel et al., 2013), these estimates were converted to an effect size that was imputed directly into CMA. Hazard ratios can be converted to Cohen's d by using a derived natural log formula: $d = \ln HR * 6/\pi$ where HR is the hazard ratio (Azuerro, 2016). Relative risk ratios were converted into an odds ratio with confidence intervals, which were entered into CMA to generate their corresponding Cohen's d . I specified that combined effect sizes be grouped by study and by type of outcome (medical vs psychological). For each study and each study-outcome grouping, CMA computed mean effect sizes, their associated p values, and forest plots (mean effect size and its 95% confidence interval).

Results

The eight studies that met criteria for inclusion in the meta-analysis varied considerably with regards to their respective populations of focus (i.e., their medical conditions or reasons for seeking medical care), the specific experimental treatment or intervention that aimed to enhance the quality of patient-provider interactions, and the operational definitions of the variables used

to measure the health outcomes of the intervention. Table 1 provides a summary and description of each of the studies. With regard to the types of medical conditions of the 4,656 patients included across the eight investigations, only medical complications associated with elderly status (Schafer et al., 2018; Wehling et al., 2016) and acute coronary syndrome (Fors et al., 2015; Fors, Swedberg, Ulin, Wolf, & Ekman, 2017) were investigated in more than one study. Each of the remaining four studies investigated intervention outcomes in patients with different medical conditions: chronic heart failure (Kalter-Leibovici et al., 2017); uncontrolled hypertension (Tinsel et al., 2013); cancer (Fujimori et al., 2014); and diabetes mellitus (Akturan, Kaya, Ünal, & Akman, 2017).

The studies varied also with regard to nationality and world region. With the exception of three studies carried out in Germany by different research teams, and two studies carried out in Sweden by the same research team, the remaining three studies took place in separate countries by different research teams: Turkey, Israel, and Japan (see Table 1). Patient sample sizes ranged from as few as 112 (Akturan et al., 2017) to 1,360 (Kalter-Leibovici et al., 2017), with an average of 582 patients per study ($SD = 425$). The number of participating providers per study ranged from 8 (Akturan et al., 2017) to 55 (Schafer et al., 2018), with an average of 32 providers per study ($SD = 17$). However, there were four studies that did not report the number of providers involved in delivering the experimental and control interventions (Fors et al., 2015; Fors et al., 2017; Fujimori et al., 2014; Wehling et al., 2016).

Whereas each of the randomized clinical trials defined their control groups as “treatment as usual” (TAU), the patient-provider enhancement-interaction interventions varied widely. Two studies collaborated on patient care plans between patient and providers (Fors et al., 2015; 2017) and one study facilitated the sharing of treatment plans between providers and with patients

(Kalter-Leibovici et al., 2017). The fourth study structured three meetings for the physician and patient to collaborate on a treatment plan, drug list, and goals for future treatment (Schafer et al., 2018). The fifth study trained physicians in a medication risk assessment protocol specifically tailored for the elderly (Wehling et al., 2016). The sixth study trained the providers in shared decision making skills (Tinsel et al., 2013), a seventh trained the providers in a therapeutic interview technique intended to improve empathy (Akturan et al., 2017), and the eighth study by Fujimori et al., (2014) trained oncologists on to deliver bad news to patients with maximum efficacy and empathy (see Table 1).

The length of time elapsed since the end of the intervention and the assessment of patients' outcomes ranged from as soon as at discharge to two years post-treatment. In fact, Fors et al. (2017) is the two-year follow up to the trial reported by Fors et al. (2015), who reported outcomes at a 6-month follow up. Except for Tinsel et al. (2013), who assessed outcomes at 6, 12 and 18-month follow-ups, all other studies assessed outcomes at a single follow up.

Assessment outcomes fell into two categories, physiological/medical and psychological/behavioral. Medical outcomes were measured by providers or taken from hospital records and included biological markers of disease state, composite scores of disease state, adverse events, and death. Psychological and behavioral outcomes were grouped together for the purposes of analysis. These outcomes were self-reported through questionnaires and included quality of life, patient empowerment, patient distress, adherence to treatment, and activities of daily life. In total there were 25 medical outcomes and 8 psychological/behavioral outcomes. (See Appendix 2 for full list of outcomes).

Given the diversity of patient populations, types of interventions, length of follow ups, and outcomes measured, we expected that the effect-size variance across the studies would be

highly heterogenous. Therefore, in our analyses we assumed a random effects model. Nonetheless, we also report the heterogeneity statistic associated with a fixed model analysis because an analysis of the variability of effect sizes should enrich the interpretation and discussion of the results. Using Comprehensive Meta-Analysis (Borenstein, 2013), mean outcome differences between the intervention and control groups were divided by their pooled standard deviation to obtain Cohen's d . When outcomes were measured either as correlations, percentages, odds ratios or risk ratios, effect sizes were converted to Cohen's d . Because the studies differed in the number and types of outcomes examined, we grouped the effect sizes by study (see Figure 2) and by outcome type (i.e., medical vs psychological/behavioral; see Figure 3).

When grouped by study, the combined effect size was small but statistically significant $d = 0.165$ ($p = 0.004$), and, surprisingly, the study effect sizes were rather homogeneous across studies as the heterogeneity statistic was not significant ($Q = 11.283$, $p = 0.127$). The I^2 statistic reflects the percentage of effect-size variation across studies that is due to true differences (or heterogeneity) and not chance. The I^2 value associated with the eight effect-sizes grouped by study was 37.96, meaning that only 38% of the observed variance was due to true differences in the effect size, while 62% of the observed variance was due to random error.

When grouped by type of outcome, the combined effect size for medical outcomes was small but statistically significant ($d = 0.176$; $p = 0.016$), while the combined effect size for psychological/behavioral outcomes was small to medium and also statistically significant ($d = 0.235$; $p = 0.001$) (Figure 3). Whereas the heterogeneity between the medical effect sizes was not statistically significant ($Q = 11.223$; $p = 0.082$; $I^2 = 46.54$), heterogeneity between the psych/behavioral effect sizes was statistically significant ($Q = 20.126$; $p = 0.001$; $I^2 = 75.16$).

Discussion

The purpose of this study was to conduct a systematic review of the literature on the effects of interventions aimed to improve the patient-practitioner relationship on medical patients' treatment outcomes. Overall, the results of the present study replicated Kelly et al. (2014) findings and show that interventions that aim to increase the providers' ability to improve the quality of their interactions with their patients do have a small, but reliably positive effect in their patients' medical outcomes.

The present meta-analysis expands beyond the work of Kelley et al. (2014) not only in that it actualizes their findings by covering a more recent period of research, but also in that I grouped, and therefore can contrast, effect sizes by outcome type into two broad categories: Medical and Psychological. Overall, although I found that medical outcomes averaged effect sizes that were smaller than the effect sizes found for psychological outcomes, these differences were not significant because their respective 95% CIs around their averaged effect sizes overlapped considerably (see Figure 3). Thus, I can confidently conclude that regardless of outcome type, interventions that aim to improve the patient-provider relationship within medical settings have a small but noticeable positive impact on both the medical and psychological wellbeing of their patients. This finding appears to be robust as all the studies in the present meta-analysis represent the interactions of more than 4,451 medical patients with their health-care providers.

Narrowing the focus of the meta-analysis proved challenging, as Professor Cepeda-Benito and I identified many studies that aimed to enhance the delivery of medical services and the experience of the patient in many different ways. We considered, but ultimately ruled out, expanding the scope of the meta-analysis to include studies that attempted to improve treatment

outcomes by enhancing patients' agency and/or their ability to advocate for themselves in their interactions with their providers, and studies that targeted both patients and providers with also the aim of enhancing the quality of their interactions. We also chose not to pursue interventions we qualified as "passive," for they simply gave more medical information, or provided easier access to medical information. In the end, although all of the studies included in the meta-analysis had in common an intervention that targeted medical personnel with the intention of enhancing the way with which they approached their patients and medical conditions, the heterogeneity of the interventions represented in our sample was considerable. In the presence of this intervention heterogeneity, as well as the heterogeneity of the sample with regard to the medical conditions treated across studies, finding that the effect sizes across studies were mostly homogeneous, and particularly across medical outcomes, was remarkable.

It should be noted that although the heterogeneity statistic associated with the psychological outcomes effect sizes was statistically significant, the variability in this subsample was largely influenced by the very large effect-size produced by a single study, i.e., Akturan et al. (2016). This effect size was produced from a measure of patient empowerment referred to as the "Diabetes Empowerment Scale" and assessed the patient's perceived ability to navigate psychological and social demands and challenges they faced with diabetes. The higher the score, the more empowered patients felt. The intervention that produced such a result was the BATHE technique (Background, Affect, Troubling, Handling, and Empathy) (Akturan et al., 2017). This was the only interview technique intervention in this analysis, and is reported to be very effective in other settings such as primary care, psychiatric, and other psychosocial problems (Kim, Park, Park, Cheong, & Choi, 2012; Leiblum, Schnall, Seehuus, & DeMaria, 2008). Thus, it is possible that this technique has novel efficacy as compared to other treatments in the meta-analysis.

The interventions of other studies included in this analysis differed with regard to the aspect of the clinician's interaction with the patient that was targeted. While some treatments targeted how the clinician interviews the patient (Akturan et al., 2017), some treatments targeted how the clinician delivers information to the patient to increase empathic listening (Fujimori et al., 2014; Wehling et al., 2016). Some treatments aimed to impact the goals of treatment either by identifying shared goals between individual physician and patient pairs (Schafer et al., 2018; Tinsel et al., 2013) or by a given patient and their team of providers (Fors et al., 2015; Fors et al., 2017; Kalter-Leibovici et al., 2017). Sharing goals empowers the patient to express their goals of treatment and encourages the clinicians to build rapport with their patients, ultimately leading to more integrative care.

Treatments such as the one utilized in Fujimori et al. (2014) are distinct from the other treatments included in the analysis. Fujimori et al. (2014) trained providers on how to break 'bad' news to patients by maximizing empathy, providing only relevant information, and giving emotional support. Fujimori et al. (2014) identified the component of reassurance and addressing patient emotions with empathy as the most important to emphasize when delivering a poor prognosis. This can work to reduce any harmful effects that may come from a bad prognosis, so that patients are not impacted by excessive negative information on their condition, an effect referred to as nocebo (Schedlowski, Enck, Rief, & Bingel, 2015). This treatment modulates not only how the therapeutic component is administered but also how the patient responds to it.

Medical outcomes had a small positive improvement as a result of treatment, and psychological/behavioral outcomes had a small to medium positive improvement as a result of treatment. Of note here is the difference in heterogeneity between the two groups. Medical outcomes culminated to non-significant heterogeneity, but psychological/behavioral outcomes

did. This may signify underlying differences in the outcomes grouped into this category and the underlying mechanisms that control these outcomes (Schedlowski et al., 2015).

As we have found in this meta-analysis, there is a small, yet significant benefit to training providers in practices that enhance the relationship a patient has with the healthcare system. Patients with different medical conditions may benefit in different magnitudes, but with the exception of one study (Tinsel et al., 2013), all others reported positive effect sizes. What we have learned from the present analysis is that the impact of intervention for the provider does improve patient outcomes in ways that can be grouped as either medical or psychological/behavioral. These outcomes see small but significant magnitude, and it is clear that such efforts to integrate enhanced relationship training into practice are impactful. They appear across different medical conditions and fields of medicine. They also appear across the world, indicating a universal component of treatment. What we have not answered is what mechanisms drive such impact, nor do we know if one type of provider-driven intervention impacts one kind of outcome more than another would.

There is still much to learn about this field of research. Ideally, all these types of provider-targeted interventions would be coded and compared to each other and provide a clear ideal provider-directed intervention type. A larger sample size may be able to determine if there is a meaningful difference in outcome response, or even if certain types of interventions could target certain groups of outcomes but was not feasible in the scope of this study. Categorizing disease types in a meaningful grouping would indicate if there was a specific type of condition that would benefit more or less from such interventions.

Limitations

There were clear discrepancies in the “dosage” of treatment across studies and the studies lacked manipulation checks. Communications skills training for a clinician allows the clinician to have more communication tools at their disposal, but it is not clear to what extent the clinicians used the tool. Heterogeneity also presented a problem for comparing treatment effects. Whereas there are studies that coordinate patient care via a team that has enhanced monitoring, telemedicine, and patient education, other studies simply provided brief training to individual clinicians.

There is a valid argument to be made that this relationship is a two-way street. Interventions that target only the healthcare side of the relationship are only changing half the equation. While in an ideal world both the patient and physician are changed from treatment, for practicality and systemwide change, it is much more feasible and efficient to change things from the healthcare side. Any intervention aimed at patients would have to be voluntary and, considering the huge volume of patients that each provider sees, very difficult to implement consistently. The best place to begin reform on the system is most likely the provider, and that reform can begin at the beginning of a clinician’s education so such lessons can be integrated into their practices.

Recommendations

This study has identified notable gaps in the field. There exists a need for better operationalization and standardization with regards to study design and conceptualization of treatment types. While, for practical purposes, it is reasonable to combine types of treatments to maximize effects, it is difficult to determine what aspects of the intervention are changing the

outcome from a mechanistic standpoint. One important factor to add to study design is some measure of how much clinicians choose to use the intervention training in their interactions with patients. Some studies, like Fujimori, et al. (2014) do already have such an assessment step in their study design. The next step is to standardize this practice across all interventions.

Better operationalization of this body of research works to enhance the quality and cohesion of the literature. This need for high quality research and cohesion is essential to understanding the mechanisms that underly these interventions. The studies included in this analysis do not answer the question of how the interventions impact the outcomes. Why does improvement in this relationship between patient and provider improve outcomes? The literature has identified this relationship as a key contributor to the placebo effects that patients are impacted by, yet the current research does not use this lens to focus on the relationship (Brown, 2013). Understanding the cause for this impact would work to change the perception of the relationship from both sides, leading to unilateral and life-changing reform in the way that the patient and healthcare system interact.

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Table 1.

Characteristics of the Randomized Clinical Trials Included in the Meta-Analysis.

Study	N (providers /patients)	Country	Diagnosis	Age range	Intervention	Follow up length	Outcomes (#Med/#PsyB) [number of effect sizes]
1. Akturan et al. (2016)	8/112	Turkey	Diabetes	50-60	Physicians were trained to interview patients using the BATHE method (Background, Affect, Troubling, Handling, Empathy).	6 months	BMI score, HbA1c score, patient empowerment score (2/1) [3]
2. Fors et al (2015) 3. Fors et al. (2017)	-/199	Sweden	Acute coronary syndrome	51-70	Person-centered care training for physicians and RNs. Patient narrative was discussed between physician, RN, and patient, to include patient's values, expectations, and goals with medical expertise.	6, 24 months	Composite score of disease state (physical activity levels, general self-efficacy) death, rehospitalization 2. (3/0) [3] 3. (3/0) [3]
4. Fujimori et al. (2014)	30/601	Japan	Cancer diagnosis	54-74	Communication skills training for oncologists on how "bad" news are delivered: what information is given and how emotional support is given.	1 week	Patient distress (0/1) [1]
5. Kalter-Leibovici et al. (2017)	-/1360	Israel	Chronic heart failure	60-80	Disease management delivered by multi-disciplinary teams, included coordination of care, patient education, monitoring disease symptoms and patient adherence to medication regimen, titration of drug therapy, and home tele-monitoring.	6 months	Time to first hospital admission for heart failure, number of admissions for HF, days of hospitalization for HF, 6 minute walk test, all cause mortality, both mental and physical quality of life. (5/2) [7]
6. Schäfer et al. (2018)	55/650	Germany	Old age	65-84	Physician leads 3 30-min talks with patient: 1. Treatment targets and priorities 2. Discussion of medication regime and compliance 3. goal attainment and future targets.	12 months	Days spent in hospital, quality of life score (1/1) [2]
7. Tinsel (2013)	36/1120	Germany	Uncontrolled hypertension	51-77	Shared decision-making training for the physician.	6, 12, 18 months	Systolic blood pressure, diastolic blood pressure, MARS-D adherence score, cardiovascular risk score, controlled vs uncontrolled blood pressure status all at either 12 months or 18 months with 6 months used as the baseline (4/1) [10]
8. Wehling (2016)	- /409	Germany	Old age	59-97	FORTA training to assess medications for older patients in terms of A (indispensable), B (Beneficial), C (Questionable), or D (avoid) based on complication risks.	At discharge	Adverse drug reaction, activities of daily life, systolic blood pressure, diastolic blood pressure (3/1) [4]

Figure 1. Flow chart of study selection process.

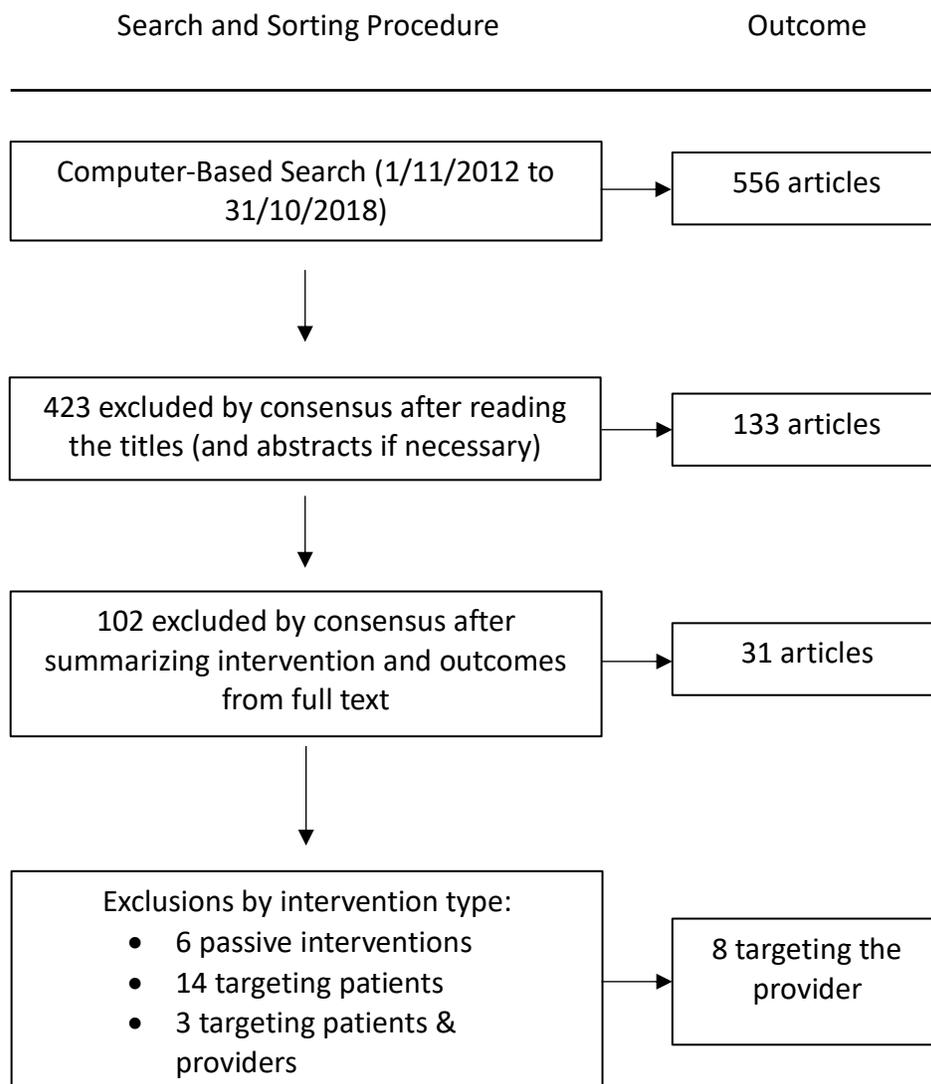


Figure 2. Summary table and Forest Plot with Cohen’s *ds* grouped by study (random effects model).

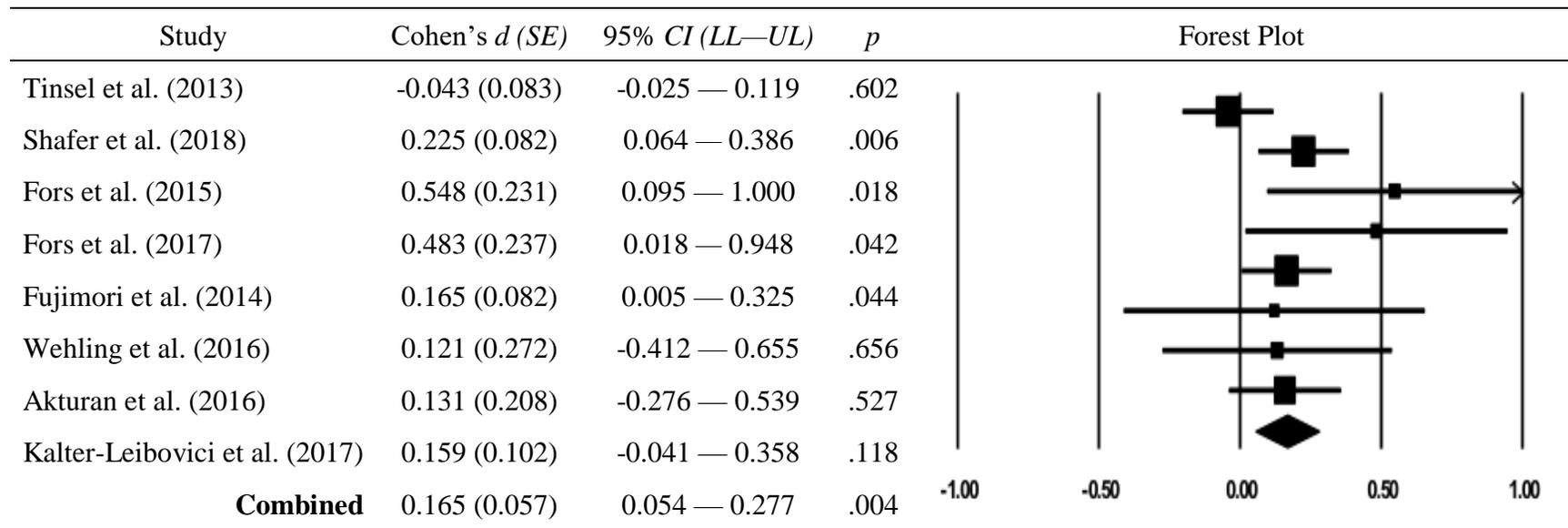
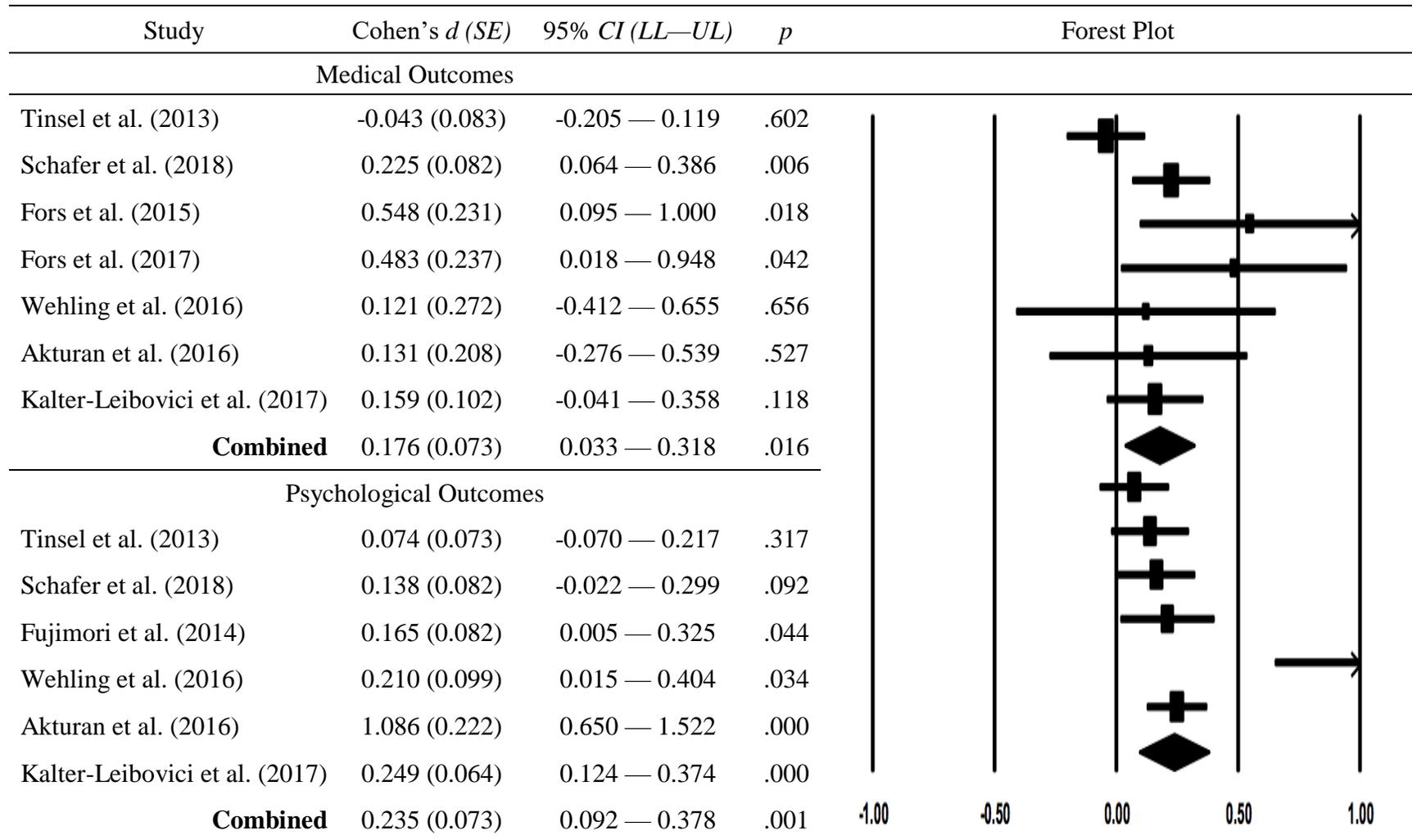


Figure 3. Summary table and Forest Plot with Cohen’s *ds* grouped by outcome type and study (random effects model).



Appendix 1

Inclusion Search Terms

- Relationship*
- GP
- Physician
- Doctor
- Practitioner
- Provider
- Professional

Exclusion Search Terms

- Parent
- Peer
- Dentist
- Midwife
- Midwives
- Acupuncturist
- Psychotherapy
- Smartphone
- Cost
- Self-management
- Antipsychotic
- Mobile and app
- Pediatric
- Adolescent
- Children
- Caretaker
- Healer
- Telemed*
- (psychological and intervention)
- (mental and illness)
- pharmacist
- (depress* and disorder)
- (schiz* and disorder)
- (PTSD)
- Psychotic
- (anxiety and disorder)
- Palliative
- Dementia
- Pilot
- (focus and group)
- (Qualitative and study)
- (text and messages)
- Web*
- Computer*

Appendix 2

Medical Outcomes

- BMI score
- HbA1c
- Composite score of disease state
- Death
- Rehospitalization
- Drug Reactions
- Time to first hospital admission
- Number of admissions
- Days of hospitalization
- 6 min walk test
- All-cause mortality
- Systolic
- Diastolic
- Cardiovascular risk
- Controlled/uncontrolled blood pressure status
- Adverse

Psychological/behavioral Outcomes

- Patient empowerment score
- Patient distress
- Mental quality of life
- Physical quality of life
- Treatment adherence
- Activities of daily life