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**FOCUSED ACCEPTANCE AND COMMITMENT THERAPY
IN AN INTEGRATED FAMILY HEALTH CENTRE:
SERVICE UTILIZATION, OUTCOMES AND FOLLOW-UP**

A thesis presented in partial fulfilment of the requirements for the degree of
Master of Arts in Psychology at Massey University.

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Abstract

Many New Zealanders experience mental health difficulties each year, however the majority do not receive professional support to cope with their distress. One strategy for reducing this treatment gap is to increase access to mental health supports through the provision of evidence-based ultra-brief psychotherapy in primary care settings. The current study investigated the delivery of Focused Acceptance and Commitment Therapy (FACT; Strosahl, Robinson, & Gustavsson, 2012) in a New Zealand primary care setting. Referral, service use and outcome data from the first twelve months of the service was analysed, along with follow-up data from a small sub-sample of patients. A total of 708 people were referred to the FACT service during the first 12 months. Clients attended an average of 2.00 (SD 1.50) sessions with the majority of sessions lasting 30 minutes or less ($M = 37.45$, $SD 11.94$). Analysis of available first and last scores on the Hospital Anxiety and Depression Scale, Outcome Rating Scale, Acceptance and Action Questionnaire II, and client reported severity of target issue indicated statistically significant improvements in target issue, anxiety, depression, global distress and psychological flexibility with medium to large effect sizes. Thirty participants provided follow-up data approximately two months after their last session of FACT. Statistically significant improvements in target issue severity and global distress were maintained at follow-up. No statistically significant change was found between first, last and follow-up scores on the measures of anxiety, depression or psychological flexibility. Overall, the results of this study provide support for the effectiveness of FACT delivered in primary care. Limitations of this study included lack of control conditions, participant sampling issues, and lower than ideal fidelity to the FACT model.

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Introduction

Mental health prevalence and treatment gap

Coping with mental health difficulties is a challenge familiar to many New Zealanders. It has been estimated that almost half of the New Zealand (NZ) population will meet diagnostic criteria for at least one mental health disorder at some stage during their lifetime, and that approximately a fifth of the population will experience mental illness in any given year (Ministry of Health, 2019; Oakley Browne, Wells, & Scott, 2006). For over 320,000 people their psychological distress is current, occurring at high or very high levels within the previous four weeks (Ministry of Health, 2019).

Unfortunately, many people do not receive the mental health treatment that they need. In 2016/2017 only 3.7% of the population accessed secondary mental health and addition services (Health and Disability Commissioner, 2018), and earlier research has shown that little more than half of New Zealanders with a serious disorder in the previous year report attending a mental health appointment (Oakley Browne et al., 2006). Access rates are even lower for the bulk of people experiencing moderate (36.5%) or mild (18.5%) psychological distress (Oakley Browne et al., 2006). The estimated treatment gap (i.e. the percentage of people who would potentially benefit from treatment but don't receive it) in NZ has been reported to be 55.5% for generalized anxiety disorder and over 60% for major depression (Kohn, Saxena, Levav, & Saraceno, 2004).

There are a number of complex factors that likely contribute towards the treatment gap. These include personal factors such as mental health literacy and stigma concerns, as well as systemic factors such as cost and specific policies (Kazdin, 2017). For example, in New Zealand most people with mental health concerns do not qualify for funded specialist mental health services which are targeted at only the most severely affected 3% of the population (Government Inquiry into Mental Health and Addiction, 2018). A recent New Zealand government inquiry has highlighted the lack of support for the rest of the population with mental health issues, sometimes known as the “missing middle”, as being a significant gap in the country’s existing provision of mental health services (Government Inquiry into Mental Health and Addiction, 2018).

Compounding the above, is the shortage of registered psychologists available to provide evidence-based treatment in New Zealand (New Zealand Psychological Society, 2019), meaning that even if the eligibility for secondary services was broadened, individuals may still find it a challenge to access the right treatment at the right time. Rucklidge, Darling, and Mulder (2018) estimate that based on a full-time psychologist seeing 20 clients a week for ten sessions each, the current psychology workforce may only be able to serve around 200,000 people a year, with the potential for non-psychologists offering evidence-based treatments (e.g. counsellors) to see another 200,000. Based on the estimate that 20% of New Zealanders, around 950,000 people, experience mental health issues in any given year, this leaves over half a million people each year potentially unable to access evidence-based therapy, with the existing workforce needing to be at least tripled in order to address this gap (Rucklidge et al., 2018).

Additionally, when a client does receive mental health treatment, they may not receive the optimal amount. Research shows that many people drop out of psychotherapy before it is considered complete, and potentially before the essential elements (or “active ingredients”) have been delivered (Swift & Greenberg, 2012; Wierzbicki & Pekarik, 1993). Although dropout can be defined in a variety of different ways Wierzbicki & Pekarik (1993) argue that *therapist judgement* may be the most flexible and appropriate definition. In their meta-analysis of 125 studies across a wide range of settings, disorders and treatment modalities the mean therapist judgement dropout rate was 48.23% (SD 23.59%). In a more recent meta-analysis, Swift and Greenberg (2012) reported a dropout rate of 37.6% when determined by “therapist judgement”. Another common approach to calculating dropout is to classify clients as having prematurely terminated treatment if they fail to attend a scheduled appointment without subsequently returning to treatment. Drop-out rates defined in this way tend to be lower, with approximately one quarter to just over a third of clients meeting this definition (Swift & Greenberg, 2012; Wierzbicki & Pekarik, 1993). Not only do many patients terminate treatment before it is considered complete, they often do so rapidly, and in many cases the therapist may only get one session with a client. For example, in one study 39.2% of patients being seen for psychosocial issues in a primary care setting only attended one appointment despite a second one being scheduled (Bryan, Morrow, & Appolonio, 2009).

Integration of mental health treatment into primary care

Although most people with mental health concerns do not receive specialist mental health support, many of them will receive some level of support from their General Practitioner (GP). In New Zealand 23.2% of people have reported visiting their GP in relation to mental health concerns (Oakley Browne et al., 2006), GPs have estimated that approximately half of their patients have experienced psychological problems in the previous year (MaGPIe Research Group, 2003), and over a third of patients attending their GP may meet criteria for a diagnosable mental health condition (MaGPIe Research Group, 2003). These statistics are consistent with, although somewhat lower than, international findings on prevalence rates in primary care settings. For example, in a Belgium study, psychiatric disorder (either threshold or subthreshold) was found in 42.5% of all adult primary care patients, despite only 5.4% stating psychiatric issues as a primary reason for attending (Ansseau et al., 2004). In Spain 53.6% of a systematic sample of primary care patients were found to have at least one psychiatric disorder with almost a third having more than one (Roca et al., 2009). Additionally, many patients will be burdened with multimorbidity; that is they will have two or more long-term conditions such as a mental health or substance abuse disorder, physical health condition, chronic pain, or sensory impairment (National Institute for Health and Care Excellence, 2016) which further complicates their support needs. As Robinson (2015, p. 52) eloquently puts it: “Often, in the waiting rooms of primary care, a person with poor lifestyle habits (not yet diseased) sits between one with multiple chronic conditions and one with crippling anxiety”.

The high number of people presenting with mental health issues in primary care makes these settings an ideal place to target mental health interventions, providing an opportunity

to reach more people and contribute towards reducing the treatment gap. The World Health Organization & World Organization of Family Doctors (2008) has suggested that the integration of mental health services into primary care may be the best approach to cope with the current challenges facing the primary care and mental health sectors, and there is significant evidence to support this approach. For example, the availability of on-site mental health practitioners delivering interventions in primary care settings has been associated with a reduction in primary care consultations, reduction in prescription of psychotropic medication, and reduction in referrals to specialist mental health services (Felker et al., 2004; Harkness & Bower, 2009). Importantly, patients may be more likely to access mental health support when it is provided in a primary care setting (Ogbeide, Landoll, Nielsen, & Kanzler, 2018), giving it potential to improve the low access rates.

In 2018 Massey University School of Psychology and a large primary health care service in the MidCentral region of New Zealand recognized this potential and collaborated to develop a new service in which on-site psychologists provide a small number of half hour sessions of Focused Acceptance and Commitment therapy (FACT; Strosahl et al., 2012) to patients identified experiencing mild to moderate psychological distress. The purpose of the present study was to investigate the utilization and effectiveness of this service and the FACT intervention.

Structure of the current thesis

Chapter one described the significant gap in New Zealand between those who need mental health support and those who receive it, and the opportunity for brief, effective

interventions that can be delivered in primary care settings. Chapter two will review the literature on brief therapy, with a focus on its implementation within primary care settings. Next, the theoretical foundations and empirical support for Acceptance and Commitment Therapy will be reviewed before discussing the evidence for the condensed version, Focused Acceptance and Commitment Therapy. Following this the primary research aims and hypotheses of the current study will be described. Chapter three will then outline the methods of the current study with results detailed in chapter four. Finally, chapter five will discuss the main findings of the study in comparison to existing literature, limitations of the study and areas for future research.

Literature review

Introduction

There is substantial evidence that psychotherapies which are effective in traditional settings, such as inpatient or secondary mental health services, can also be effective in primary care environments (Linde, Sigterman, et al., 2015; Santoft et al., 2019; Seekles et al., 2013; Zhang, Borhneimer, et al., 2019; Zhang, Franklin, et al., 2019; Zhang, Park, Sullivan, & Jing, 2018). In addition, statistically significant differences are typically not found between the most commonly implemented and researched interventions, including Cognitive Behaviour Therapy (CBT), Problem-Solving Therapy (PST), interpersonal psychotherapy and psychodynamic therapies (Cape, Whittington, Buszewicz, Wallace, & Underwood, 2010; Linde, Rücker, et al., 2015), suggesting that a variety of interventions can be effectively adapted for the primary care environment.

However, merely shifting the physical location of where psychotherapy is delivered will do little to reduce the treatment gap if the overall number of therapy hours delivered to each patient doesn't also change. Illustrating this point Strosahl and Robinson (2018) estimate that based on delivering a standard 12-session intervention protocol, a primary care practice would need to hire at least two full-time psychologists for every one GP just to treat depression alone. The cost and resourcing implications of such numbers are unlikely to be feasible or sustainable for the majority of, or possibly any, primary care services.

Fortunately, there is evidence to suggest that therapy doesn't have to be long to be effective and the rest of this chapter will begin by describing some of the research in this area, with particular emphasis on evidence for the effectiveness of brief psychotherapy in primary care settings. The theoretical foundations and evidence base of Acceptance and Commitment Therapy (ACT) will then be considered before exploring both direct and indirect evidence for the condensed version of ACT, known as Focused Acceptance and Commitment Therapy, that is the subject of the current study.

Brief psychotherapy

Definition of brief therapy.

There is currently no standard definition of what constitutes “brief” psychotherapy. It has sometimes been operationalized as less than 10 sessions (e.g. Cape et al., 2010) and some authors have adopted the term “ultra-brief” to describe treatment protocols designed to be six or fewer sessions (Otto et al., 2012; Shapiro et al., 2003). Even less has been written about brevity in relation to duration of sessions. Sperry and Binensztok (2019) use the term “focused” for interventions that take less than 30 minutes. See Table 1 for a summary of the definitions used in this report. As there does not appear to be a clear term in the literature that encompasses brevity of both session length and session number the term “Ultra-brief Focused Therapy” (UBFT) will be adopted in this thesis to describe psychotherapy that involves both a small number of sessions (six or less) and sessions that typically last 30 min or less.

Table 1*Definitions of brief therapy*

Brief therapy	Therapy intended to last less than 10 sessions
Ultra-brief therapy	Therapy intended to last 6 or fewer sessions
Focused therapy	Therapy intended to be delivered in sessions of 30 minutes or less
Ultra-brief focused therapy (UBFT)	Therapy intended to involve six or fewer sessions lasting 30 minutes or less.

Brief psychotherapy approaches.

Several therapeutic approaches have been specifically designed with brevity in mind or can be effectively adapted to be delivered briefly. One of the most commonly implemented and researched (Cape et al., 2010; Funderburk et al., 2018; Nieuwsma et al., 2012) is Cognitive Behavior Therapy (CBT), which is intended to be a short-term psychotherapy focusing on supporting people to identify, evaluate and change dysfunctional thought patterns and core beliefs (Beck, 2011). Brief CBT has demonstrated effectiveness in primary care settings. For example, a meta-analysis of brief therapies versus GP treatment as usual found small effect sizes in favour of CBT for depression and mixed anxiety-depression, with a large effect for anxiety (Cape et al., 2010).

Ultra-brief CBT in primary care has also demonstrated effectiveness in several studies. In one study primary care patients with co-occurring anxiety and depression demonstrated a reduction in symptoms of both following four sessions of CBT, although anxiety results were not maintained at one month follow-up (Lang, 2003). In another primary care study patients who received an initial assessment session followed by five sessions of CBT demonstrated a significant reduction in psychological distress and depressive symptoms following treatment, with 61.8% of those who were in the clinical range pre-treatment showing reliable and clinically significant improvement (McHugh, Byrne, & Gordon, 2014).

However, adapting standard length CBT to be delivered in more condensed formats and in primary care settings is not without its challenges. In particular, (Shepardson, Funderburk, & Weisberg, 2016) note the poor fit between typically diagnosis-specific CBT protocols and the fast-paced nature of primary care settings, in which clinicians will likely encounter clients with a wide range of range of diagnoses (or subthreshold symptoms) with limited opportunity for selection or preparation of diagnosis-specific interventions.

Another common approach is Problem-solving therapy (PST). PST aims to improve the ability of individuals to cope with both minor and major stressful events in life by developing an adaptive perspective towards problems, combined with the development of effective problem-solving skills (Nezu, Nezu, & D'Zurilla, 2013).

Standard PST typically involves 7 to 14 sessions, however a condensed version, named Problem Solving Therapy for Primary Care (PST-PC; Hegal & Arean, cited in Nguyen, Chen, and Denburg (2018)), typically consists of 4-8 half-hour sessions (Nguyen et al., 2018; Oxman, Hegel, Hull, & Dietrich, 2008) and is therefore of particular relevance to the current study due to its' ultra-brief focused format. For example, in a study by Hassink-Franke et al. (2011) primary care patients received an average of 4.3 sessions of PST delivered by GP registrars, with each session (excluding initial assessment) lasting approximately 30 minutes. Patients who received the PST intervention demonstrated higher somatoform disorder and anxiety recovery rates, reduced limitations due to emotional difficulties, plus improved social functioning and general health perception. A recent meta-analysis provides additional support for the effectiveness of ultra-brief focused PST in treating depression and / or anxiety in primary care settings with a medium effect size versus usual GP care (Zhang et al., 2018).

An example of a psychotherapeutic approach specifically developed with brevity in mind, rather than an adaptation of a pre-existing longer approach, is Solution-Focused Brief Therapy (SFBT; de Shazer, Berg & colleagues, cited in Kim (2008)). SFBT attempts to minimize the amount of therapeutic time spent talking directly about the presenting problem, and instead focuses on client strengths, developing a picture of what life without the problem might look like and the identification of ways to move towards that vision (De Shazer, 1985). Interventions are typically delivered in less than ten sessions (Gingerich & Eisengart, 2000; Gingerich & Peterson, 2013). There is significant evidence that SFBT can be effective with almost three quarters of controlled studies on SFBT demonstrating

significant positive outcomes (Gingerich & Peterson, 2013). However, SFBT is not yet as well-researched in primary care settings. In their systematic review of psychotherapies for anxiety and/or depression delivered in primary care settings Zhang, Franklin, et al. (2019) were unable to find any studies of SFBT that met their inclusion criteria.

Perhaps the briefest approach to the delivery of psychotherapy, although not a therapeutic model or set of specific techniques in and of itself, is known as Single-Session Therapy (SST). As the name implies, SST deliberately aims to achieve meaningful change within the first (and generally expected to be the only) treatment session, with the key goal being “to ensure that the client walks away from a single session with a plan about how to solve their problem, the confidence that they have the skills and resources available, and the knowledge that they can come back at any time for further work” (Campbell, 2012, p. 16). This description has several similarities to the FACT approach in which a behavior plan is established in each session, including the first, clients are asked to rate how confident they are that they can achieve that plan, and clients are informed that they may only need to attend one session but are welcome to come back for more, either now or in the future (Strosahl, 2019; Strosahl et al., 2012). However, there has been limited systematic research into the effectiveness of SST, with studies typically comprising service descriptions, case descriptions and qualitative or other types of uncontrolled studies (See Campbell, 2012 for review). In addition, although the focus in SST is on the majority of clients receiving just one session of therapy (per episode of treatment) it is not uncommon for the length of that session to be equal to several traditional length sessions, (e.g. Ollendick et al., 2009; Perkins, 2006).

Early sudden gains.

The previous section highlighted evidence suggesting that psychotherapy does not have to be long to be effective. It has also been suggested that patients with common mental health problems may fall into two subgroups; “rapid responders” who demonstrate improvement within four sessions of traditional length psychotherapy and “gradual responders” who may require as many as 26 sessions to achieve clinically significant improvement (Robinson, Delgadillo, & Kellett, 2019). Some clients may derive benefit from more than 26 sessions however beyond this point the probability of reliable improvement is typically low (Robinson et al., 2019).

Consistent with the concept of “rapid” versus “gradual” response Watzlawick (1974, cited in Strosahl et al., 2012) described two types of change; type one change in which changes in perspective are small, with limited impact on the larger dysfunction, and type two change in which shifts in perception and understanding are rapid and radical. Research into early and sudden gains in psychotherapy provide support that the latter types of change is indeed possible. For example, in one study involving data from over 16,800 individuals, 16% of clients experienced a “rapid response” (improvement rate greater than 90% of the population) after just one session, 17.7% between sessions 2-4, and 10.3% between sessions 4-6 (Erekson, Clayson, Park, & Tass, 2018). Similarly, Keinonen, Kyllönen, Astikainen, and Lappalainen (2018) reported early sudden gains in over a fifth of patients diagnosed with depression after just two sessions of therapy. Approximately 35-50% of

sudden gains occur by session five (Lutz et al., 2013; Stiles et al., 2003; Tang, DeRubeis, Hollon, Amsterdam, & Shelton, 2007).

Some indication as to which clients may be more likely to respond rapidly to psychotherapy treatment can be found in a recent study by Beard, Delgadillo, and Beard (2019) who examined the early change in symptoms of over 400 outpatients at their first, third and fifth sessions of psychotherapy. They identified four classes of change trajectories and then examined whether client characteristics (age, gender, personality traits and level of psychological distress at intake) predicted which class of change trajectory they belonged to. The largest class in this study (41.3%), was made up people who started therapy with a moderate/low level of distress and demonstrated a strong reduction in symptoms. This is of particular relevance given that clients with mild to moderate mental health issues were the intended target group for the FACT service evaluated in this study, and who also represent the “missing middle” who currently receive limited psychological support in New Zealand (Government Inquiry into Mental Health and Addiction, 2018)

Primary Care Behavioral Health

Despite the evidence for the effectiveness of brief psychotherapy and potential for rapid and radical change outlined above, interventions delivered in primary care settings continue to be of traditional length more often than no. In a review of 44 studies of interventions delivered to primary care patients with anxiety, less than a third of interventions were offered in six or fewer sessions (Shepardson, Buchholz, Weisberg, & Funderburk, 2018). Sessions that are brief in duration are even less common, with the average primary care based CBT session lasting approximately 48 minutes (Zhang,

Borhneimer, et al., 2019) to just under an hour (Santoft et al., 2019), and Shepardson et al. (2018) identifying that less than 14% of primary care interventions are offered in sessions of half an hour or less. The duration of sessions is often not even reported in many studies (Shepardson et al., 2018), perhaps due to the “50 minute hour” (Pomerantz, Corson, & Detzer, 2009, p. 44) being so engrained in the delivery of psychotherapy that session length is only seen to be worth commenting on if it differs significantly from this norm.

One particular framework for the integration of psychological and behavioural health services into primary care that has a specific focus on utilizing ultra-brief focused interventions is known as the Primary Care Behavioral Health model (PCBH; Robinson & Reiter, 2007, 2016; Strosahl & Robinson, 2008). PCBH operates on the principle that the health of all patients is better served by providing “some care to the many rather than a lot of care to the few” (Robinson, 2015, p.53). To achieve this specialist staff called Behavioural Health Consultants (BHCs) are integrated into primary care settings, providing support to high-volumes of patients with a wide range of mental, psychosocial and health concerns, in typically no more than four 20-30 minute consultations per patient (Robinson, 2015). Reiter, Dobmeyer, and Hunter (2018) characterize the PCBH model as a highly accessible team-based approach that can ultimately enhance primary care services for the entire clinic population.

Although there are still many research gaps the PCBH model has been associated with high levels of patient satisfaction, improved functioning, reductions in symptoms of anxiety, depression and PTSD, reduction in sleep issues, reduction in tobacco use and

improved weight management (see Hunter et al., 2018 for review). It has also been linked with reductions in referrals to specialist mental health services (Felker et al., 2004) and possible reductions in emergency department visits (Serrano, Prince, Fondow, & Kushner, 2018).

Under the PCBH approach ultra-brief focused CBT for specific populations, such as patients attending United States military and air force primary care facilities, has demonstrated effectiveness for a variety of difficulties (commonly depression, anxiety, marital problems, insomnia, chronic pain and stress) in four or fewer 20-30 minute sessions with medium to large effect sizes (Bryan et al., 2009; Cigrang, Dobbmeyer, Becknell, Roa-Navarrete, & Yerian, 2006). For example, in a sample of almost 500 active and non-active military personnel and their family members, 40.5% showed reliable and clinically significant improvement in global mental health following an average of 2.51 half-hour appointments (Bryan et al., 2012). Interventions included mindfulness, relaxation, behavioral activation and cognitive restructuring. In an earlier military primary care study significant improvements were demonstrated in client's wellbeing, life functioning and global mental health following 30 minute consultations with a Behavioral Health Consultant, with medium effect sizes reported for clients who attended two sessions, and large effect sizes for clients who had three consultations (Bryan et al., 2009).

PCBH has also demonstrated effectiveness in more general primary care populations. In one study significant reductions in anxiety scores as measured using the HADS were found between pre-intervention, post-intervention and follow-up, with significant reduction in

depression also found between pre-intervention and follow-up (Angantyr, Rimner, Norden, & Norlander, 2015). In another study, almost half of primary care patients with mood disorders had an improvement in depressive symptoms of at least 50% following four 30-minute problem-solving focused consultations with a Behavioral Health Consultant (McFeature & Pierce, 2012). Additionally, it has also been demonstrated that clinical improvements in global mental health made during PCBH interventions can be maintained up to two years later (Ray-Sannerud et al., 2012)

A variety of interventions can be utilized under the PCBH approach (Robinson & Reiter, 2016) and techniques tend to be cognitive-behavior based (Bridges et al., 2015; Funderburk et al., 2018). However, there is a strong relationship between the PCBH model and Acceptance and Commitment Therapy (ACT; Hayes, Strosahl, & Wilson, 1999). For example, Strosahl is one of the original founders of ACT (Hayes et al., 1999) as well as the PCBH approach (Strosahl & Robinson, 2008). In addition, Focused Acceptance and Commitment Therapy (FACT; Strosahl et al., 2012) was developed as a result of Strosahl Robinson and colleagues extensive and ongoing work in primary care, and alongside the development of the PCBH model (Mountainview Consulting Group, 2019; Robinson & Strosahl, 2009).

In the next section, an overview of the theoretical foundations and evidence base for Acceptance and Commitment Therapy is presented, followed by a description of the existing evidence for Focused Acceptance and Commitment Therapy, the intervention that is the focus of the current study.

Acceptance and Commitment Therapy

Introduction to Acceptance and Commitment Therapy.

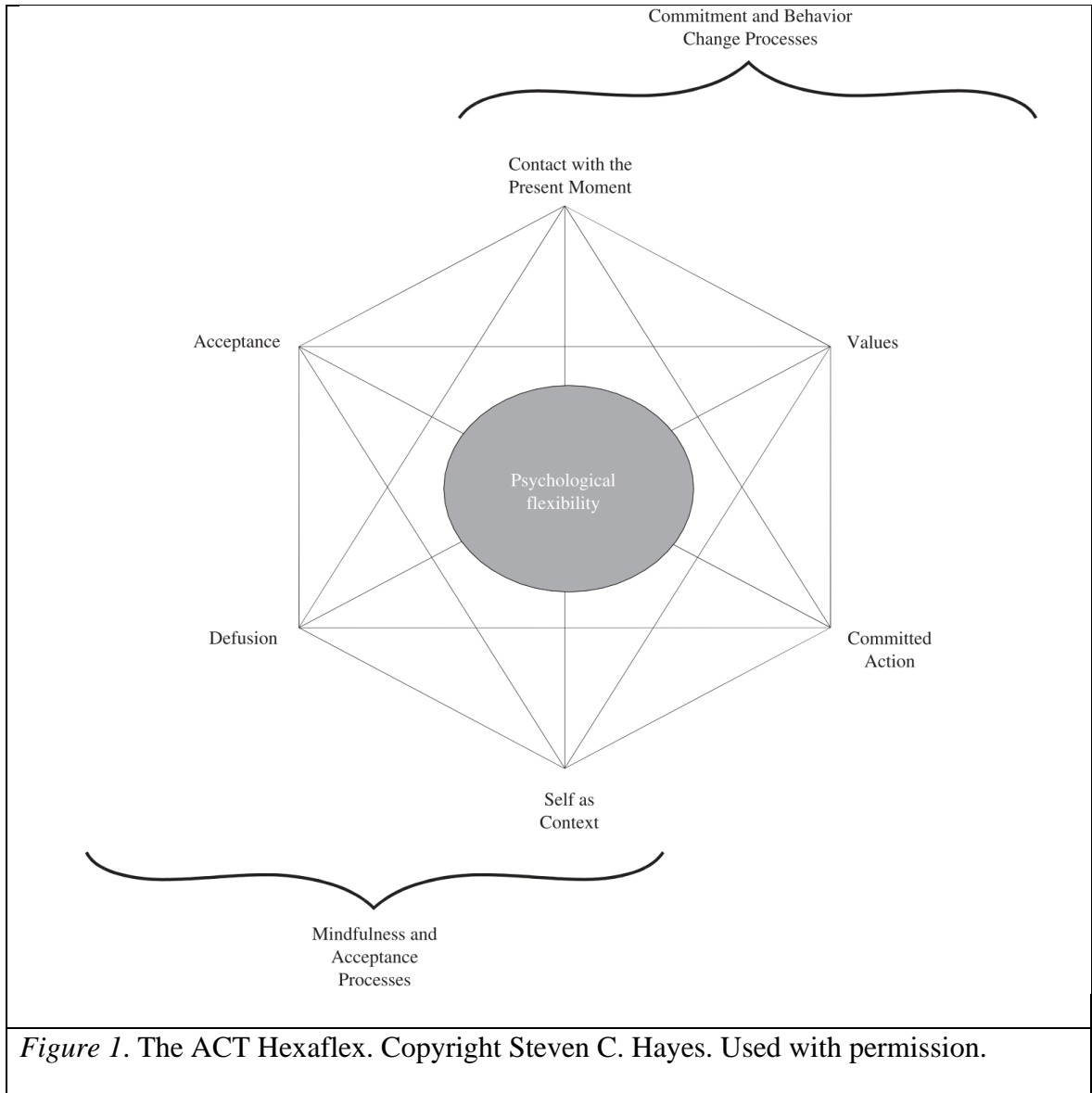
Acceptance and Commitment Therapy (ACT; Hayes et al., 1999) has been described as a transdiagnostic psychotherapy (e.g. Hayes, Pistorello, & Levin, 2012; Sauer-Zavala et al., 2017) that is effective across multiple conditions. “Transdiagnostic models obey the principle of parsimony by explaining a host of seemingly different disorders by specifying a much smaller set of common underlying psychological processes” (Strosahl & Robinson, 2018, p.14). In the ACT model of psychopathology this underlying process is *psychological inflexibility*.

Psychological inflexibility is a broad construct that encompasses six interconnected processes: cognitive fusion, experiential avoidance, loss of flexible contact with the present, attachment to the conceptualized self, values problems and inaction (Hayes, Levin, Plumb-Villardaga, Villatte, & Pistorello, 2013; Hayes, Strosahl, & Wilson, 2012). The psychologically inflexible individual may relate and respond to distressing thoughts, emotions and sensations as if they were literally true (*cognitive fusion*) and these unpleasant experiences can be seen as something essential to avoid, suppress or otherwise control (*experiential avoidance*), despite such attempts often having a paradoxical effect. In addition, the person may overidentify with the narratives they have developed about themselves (*attachment to the conceptualized self*), ruminating on the past or the future rather than remaining in contact with the “here-and-now” (*loss of flexible contact with the present*), losing touch with what is important and meaningful to them (*value problems*) and

failing to take committed action in the direction of their chosen values and goals (*inaction*).

There is evidence that psychological flexibility may be a protective factor against the negative physical and mental effects of stressful life events, daily stress and low social support (Gloster, Meyer, & Lieb, 2017). Whereas psychological inflexibility has been associated with current and lifetime rates of depressive and anxiety disorders (Kashdan & Rottenberg, 2010; Levin et al., 2014; Twohig & Levin, 2017), suicidal ideation in US veterans (DeBeer et al., 2018), internet addiction, depression and suicidality in tertiary students (Chou, Yen, & Liu, 2018), rumination, neuroticism and difficulties with impulse control (Stabbe, Rolffs, & Rogge, 2019).

ACT aims to enhance psychological flexibility through focusing on four acceptance and mindfulness processes (acceptance, defusion, contact with the present moment and self-as-context) and two behaviour change processes (engagement with values and committed action) (Hayes et al., 2013; Robinson & Reiter, 2016). These change processes are typically portrayed as six interconnected “points” on the “ACT Hexaflex” as seen in Figure 1.



These six core processes can be further collapsed into three dimensions known as the “Pillars of Flexibility” (Strosahl et al., 2012). These three “pillars”, and the original “hexaflex” processes they encompass, are:

- 1) **Openness** (*acceptance, defusion*): the willingness and ability to give “space” to negative private events (e.g. thoughts, emotions, memories or sensations), allowing

them in and experiencing them as they are without evaluating them as literal truths, or attempting to control, suppress or avoid them.

- 2) **Awareness** (*present-moment awareness, self-as-context*): the willingness and ability to be present in the current moment, flexibly adapting to the “here and now” rather than ruminating on the past or the future, and being able to “step back” and observe the flow of one’s own experiences.

- 3) **Engagement** (*values, committed action*): the willingness and ability to identify what makes life meaningful, connecting with important values in different life domains and taking committed action to move in the direction of those values.

Evidence for Acceptance and Commitment Therapy.

There have been over 300 randomised controlled trials (RCTs) investigating the effectiveness of ACT since its development, and the number of ACT meta-analyses is expected to reach 50 by the end of 2019 (Hayes, 2019). Across these studies ACT has generally demonstrated superiority over treatment as usual (TAU), waitlist (WL) and placebo (PL) conditions with typically large effects. For example, in an early meta-analysis Hayes, Luoma, Bond, Masuda, and Lillis (2006) reported large effect sizes post-treatment and at follow-up in favour of ACT relative to WL, TAU and PL conditions. These results have consistently been replicated as the research into ACT has grown, and medium to large effect sizes have since been reported in a number of other meta-analyses (A-Tjak et al., 2015; Öst, 2008, 2014; Powers, Zum Vörde Sive Vörding, & Emmelkamp, 2009). The

average person who receives ACT treatment is reported to be more improved than 66% of participants in WL/TAU/PL control conditions (Powers et al., 2009).

However, support for ACT versus structured and established treatments, predominantly CBT, have been mixed and conclusions more tentative. While evidence appeared to be in favour of ACT over CBT in an early meta-analysis this was based on only four studies comparing ACT with CBT (Hayes et al., 2006). Several other meta-analyses since that time have found no significant difference between the two treatments. For example, Powers et al. (2009) found a small but non-significant effect when ACT was compared to established treatments (50% of which were CBT based). However, this result was challenged by Levin and Hayes (2009) who conducted a reanalysis after making changes to the way treatments were categorised. Contrary to the findings from the initial meta-analysis they concluded that ACT did outperform the established treatments. Another significant meta-analysis reviewed 16 studies comparing ACT to traditional CBT/BT for a diverse range of problems, reporting that in respect to primary outcomes ACT was superior to CBT in 11 studies, equivalent in two and outperformed by CBT in two (Ruiz, 2012). Consistent with the study by Levin and Hayes (2009), ACT was found to significantly outperform CBT on primary outcomes at post-treatment and follow-up. However, when analysis was conducted specifically on depression and anxiety outcomes (regardless of whether they were primary or secondary outcomes) no statistical difference was found between ACT and CBT/BT (Ruiz, 2012). Öst (2014) later analysed 21 RCTs comparing ACT with CBT, including 12 of the 16 studies assessed by Ruiz (2012) and was unable to replicate the latter's results, concluding that ACT did not result in significantly higher

effect sizes than CBT/BT. This conclusion was also reached by A-Tjak et al. (2015) following an analysis of 39 RCTs considered to meet high standards of quality, however it was also acknowledged in this study that the results were in the direction of favouring ACT. Most recently Hacker, Stone, and MacBeth (2016) conducted a cumulative and sequential meta-analysis with a specific focus on depression and anxiety and concluded that the evidence for ACT compared to control conditions has reached statistical sufficiency and “no further randomized clinical trials are required” (p. 52), however were again unable to conclude that ACT is more effective than traditional approaches such as CBT.

In summary, although there are some mixed results, it seems reasonable to conclude that ACT is superior to a variety of control conditions including waitlist, treatment as usual and psychological placebos, and is generally as effective as CBT in many circumstances.

Benefits of Acceptance and Commitment Therapy.

ACT possesses several benefits which, although not necessarily unique to ACT, may prove useful in addressing the gap between those who need treatment and those who receive it. Firstly, instead of the development of specific protocols for specific diagnoses (and which typically require specific training), ACT emphasizes the application of universal principles that can be applied to support people with a broad range of challenges to lead a more vital and meaningful life (Hayes, Pistorello, et al., 2012; Sauer-Zavala et al., 2017). This focus on processes over protocols, and scope over precision, has the potential to reduce the training burden on clinicians, allowing them to learn a “limited set of transdiagnostic

principles with a very wide range of clinical application” (Strosahl & Robinson, 2018, p. 14) instead of requiring development in numerous evidence supported treatment protocols that may still only address a small number of specific disorders. This broad applicability is of particular importance in primary care settings due to the wide variety of patients and presenting problems that a clinician is likely to encounter in a typical day.

ACT also has the potential to be effectively delivered by practitioners without full psychological training (Kohtala, Lappalainen, Savonen, Timo, & Tolvanen, 2015; Richardson, Bell, Bolderston, & Clarke, 2018), which has important implications given the current workforce shortage in New Zealand, as it may allow for the relatively rapid training of a high number of clinicians with sufficient skills to effectively support individuals with mild to moderate mental health concerns.

Another aspect of mitigating the treatment gap is ensuring that interventions are culturally responsive and effective. It is important in all countries to consider cultural aspects to the delivery of psychotherapy, and it is of particular importance in New Zealand with the psychology profession’s strong commitment to biculturalism and the Treaty of Waitangi (1840). It is also a highly relevant consideration in the primary care context with research showing that Māori attending their GP are more likely to present with a mental health disorder than non-Māori (MaGPie Research Group, 2005). Although there have been attempts to ensure CBT is culturally responsive to Māori patients (Bennett, Flett, & Babbage, 2014, 2016) there is yet to be specific research in to the use of ACT with this population. However, there is some preliminary evidence that ACT may be an effective

and appropriate intervention for Māori. For example, Jourdain and Dulin (2009) demonstrated the effectiveness of ACT with an older male of Māori descent with health anxiety, and postulated that ACT's holistic and non-evaluative nature (i.e. not seeing internal experiences as "true" or "false") and focus on values may make it a better cultural fit than more mechanistic therapies such as traditional CBT. In another study Harvey et al. (2018) delivered an ACT based wellbeing course to military staff from the New Zealand Defence Force which was effective in reducing alcohol consumption, anger, aggression and perceived stress. Although results for different ethnicities were not analysed separately almost two fifths of the sample, the single largest ethnic group, were Māori.

Focused Acceptance and Commitment Therapy

Introduction to Focused Acceptance and Commitment Therapy.

Focused Acceptance and Commitment Therapy (FACT) is a highly condensed version of ACT, and similarly focuses on changing client's relationship with unwanted experiences (such as feelings of anxiety or depression, negative self-evaluations or unpleasant bodily sensations) while at the same time behaving in ways more in line with personal values (Strosahl et al., 2012).

In their description of Focused Acceptance and Commitment Therapy Strosahl et al. (2012) place no specific parameters on the number or length of sessions other than to suggest that brief therapy should recognize that many people drop-out of therapy in less than six sessions, and intentionally aim to achieve meaningful behaviour change within

this natural timeframe. However, as a result of the overlap in authorship, development and dissemination of FACT and the PCBH approach described previously, a number of key features of PCBH are generally understood to also be key features of implementing FACT in primary care (e.g. Arroll, 2016; P. Robinson, personal communication, February 18, 2019). The most pertinent of these key features are summarized in Table 2.

Table 2

Key features of Focused Acceptance and Commitment Therapy

Transdiagnostic	Suitable for patients with a wide variety of mental, behavioral, psychosocial and health related concerns.
Accessible	Patient's first session with a FACT clinician will occur soon as soon as possible after their medical consultation. This will preferably occur on the same day as their appointment with their medical practitioner who may personally introduce the patient to the FACT clinician (known as a warm handoff).
Ultra-brief	Majority of patients expected to have no more than 4 sessions
Focused	Majority of sessions are expected to last 30 minutes or less

Evidence for Focused Acceptance and Commitment Therapy.

Indirect evidence for FACT.

Indirect support for the potential of FACT to be effective can be drawn from the research areas highlighted in the previous sections, including evidence for the effectiveness of ACT, the benefits of the PCBH approach, other brief and ultra-brief interventions and evidence that people can make substantial change very early in therapy.

Although number and duration of sessions varies, there is also evidence that ACT can be delivered in a relatively small number of therapy hours. For example Kohtala et al. (2015) reported a 47% mean reduction in depressive symptoms following four hours of ACT (4 x 60min sessions), compared to a 4% reduction in those in the waitlist group. A within-group effect size was reported for depression and a medium effect size for psychological flexibility. Results continued to be maintained at a subsequent five year follow-up with approximately 40% of participants reporting no or minimal depressive symptoms (Kohtala, Muotka, & Lappalainen, 2017).

In another study involving six hours of ACT treatment (6 x 60 minute sessions) administered by novice therapists 70% of participants diagnosed with depression either recovered or improved following treatment, compared to 32% in the waitlist control group (Kyllönen et al., 2018). Statistically significant medium to large effect between group (ACT versus waitlist) and within-group effect sizes were reported on measures of depression, anxiety, stress, and psychological flexibility.

In a particularly interesting study Kroska (2018) compared varying durations of single-session group ACT (90 minutes, 3 hours and 6 hours) and found no difference in the level of reduction in depressive symptoms across the different time conditions, although 67.9% of participants assigned to the 90 minute group attended compared to just under half for the 3 hour and 6 hour groups (46.3% and 48.2% respectively), suggesting that briefer options may be more acceptable to clients.

Some studies report that ACT can result in significant improvements in just one or two sessions, bringing them close to the number of therapy hours that might be delivered in FACT. In one study 23.2% of clients diagnosed with a major depressive disorder improved or recovered following an acceptance and values-based intervention (Keinonen et al., 2018). In another study two 60-minute sessions of an ACT-based protocol targeting delusion and values was administered to patients with repetitive negative thinking and severe scores on depression and/or anxiety measures (Ruiz et al., 2018) with results demonstrating that just the first session accounted for the majority of the resulting improvements.

Direct evidence for FACT.

There is currently very little published research specifically investigating the implementation of FACT in primary care. Despite extensive searching of the literature and personal communication with several experts in the field (P. Robinson, April 18, 2019; B. Arroll, September 13, 2019; S. Malthus, September 21, 2019) only one peer-reviewed

study specifically claiming to implement a FACT approach was able to be located.

However, although the identified study (Glover et al., 2016) took place in a primary care setting and strategies used were based on those outlined by Strosahl et al. (2012) in their description of FACT, the mode of delivery consisted of four 90-minute group sessions. Therefore, it did not incorporate the key elements of FACT outlined above, resulting in at least twice the number of therapy hours that would typically be expected for individual FACT delivered in primary care (i.e. 6 hours versus 3 hours or less).

Looking beyond the peer-reviewed literature a poster presentation delivered at two international conferences describes an unpublished RCT on FACT conducted in New Zealand (Arroll et al., 2019). In this study 57 patients were recruited from a primary care waiting room prior to seeing their G.P. Participants in the FACT intervention group completed a brief contextual assessment and were then prompted to choose up to three areas that they wanted to focus on changing, and were confident that they could change, over the following week. The control group completed the contextual assessment only. In most cases the intervention occurred prior to the patient's health appointment and was very brief, typically lasting about 10 minutes. At one week following intervention there was a statistically significant difference in depressive symptoms but not for psychological flexibility. Although the rapid reduction in depressive symptomology is encouraging, the extremely short nature of the intervention, the restriction to one session and the intervention prior to consultation with the GP make this atypical of the general delivery of FACT conceptualised in this thesis.

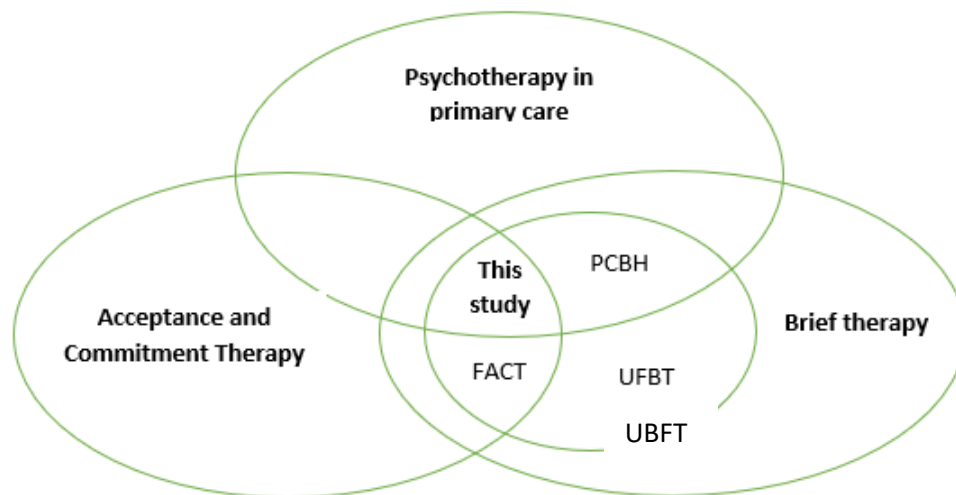
The most relevant support for FACT delivered in an ultra-brief number of focused sessions appears to come from “*Te Tumu Wairoa*”, a pilot project that has been implemented in seven primary care practices in New Zealand over the last two years (Appleton-Dyer, Andrews, Reynolds, Henderson, & Anasari, 2018). In this model, registered mental health professionals known as Health Improvement Practitioners (HIPs), were introduced into primary care teams with the purpose of providing brief, targeted behavioral support to any patient with psychosocial issues impacting on their wellbeing. HIPs received specific training in the FACT approach and key features of the model included a focus on quick access, warm handoffs and ultra-brief focused consultations. A recent evaluation of the service by Appleton-Dyer et al. (2018) identified a number of promising results including:

- High engagement – over 90% of people referred to the HIPs subsequently engaged with the service.
- Quick access – 55% of initial appointments happened on the same day as the referral and 88% occurred within 5 working days.
- Ultra-brief – 96% of patients had less than four appointments with 68% having only one consultation with a HIP.
- Focused – 79% of HIP consultations were 30 minutes or less.
- Improvements in wellbeing – 71% of patients reported an improvement in their total raw score on the DUKE Health Profile (Parkerson, Broadhead, & Tse, 1990) with 51% of people shifting into a lower clinical category (e.g. crossing the clinical threshold from moderate to mild).

The current study

Research gap.

The above introduction chapter and literature review briefly covered three key areas of research: 1) psychotherapy in primary care; 2) brief psychotherapy and 3) Acceptance and Commitment Therapy. A significant amount has been written about each of these three areas separately, and a smaller number of studies reach across two fields (e.g. brief psychotherapy in primary care; brief ACT; ACT in primary care). However, there is currently little written about the convergence of all three areas, especially for the sub-group of brief therapy defined in this thesis as ultra-brief focused therapy, and it is this research gap that the current study aims to contribute to. The crossover of these research areas and position of this study is conceptualized in Figure 2.



PCBH: Primary Care Behavioral Health; FACT: Focused Acceptance and Commitment Therapy; UFBT: Ultra-brief focused therapy.

Figure 2. Position of current study within wider research context

Research aims.

The current study set out to investigate the delivery of Focused Acceptance and Commitment Therapy (Strosahl et al., 2012) in a New Zealand primary care setting in order to address the following three key questions:

1) *How did clients utilize the FACT service?*

As so little has been published regarding the delivery of FACT in primary care the decision was made to place emphasis on investigating the typical patterns of service utilization (including referral reasons, drop-out rates, frequency of attendance and rates of treatment return) and subsequent implications for mitigating the treatment gap.

2) *Is Focused Acceptance and Commitment Therapy effective?*

The primary goal of this study was to determine what, if any, impact the FACT intervention had on the severity of client's presenting problems (target issue), psychological inflexibility, and symptoms of anxiety, depression and global distress. It is hypothesised that participants in this study will show improvements in all five outcome variables.

3) *Were results maintained at follow-up?*

Given the evidence that outcomes of brief and ultra-brief focused interventions can be maintained at follow-up even after several years (Kohtala et al., 2017; Ray-Sannerud et al., 2012), and the relatively short monitoring period used in this study,

it was predicted that improvements in target issue, anxiety, depression, global distress and psychological flexibility would be maintained at follow-up.

Method

Design

This study used a within-subjects repeated measures design to evaluate the impact of the FACT service on five different variables (severity of target issue, anxiety, depression, global distress and psychological flexibility).

The research project consisted of two interconnected studies:

- Study 1 - 12-month evaluation: An evaluation of the first 12 months of the FACT service, using data gathered as part of the standard operation of the service. In addition to outcome data this study also involved investigation into patterns of service utilization.
- Study 2 - Follow-up study: Analysis of follow-up data collected from a sub-sample of patients recruited over a 20-week period.

Due to the highly overlapping nature of these two studies the methods for each study are presented together, with differences in procedures identified where required.

Ethical approval

12-month evaluation.

The review of data from the first 12 months of the FACT service was considered an audit of the FACT service and low risk. Broad approval for such an audit had previously been

given by the Massey University Human Ethics Committee and a low risk ethics review process was also completed.

Follow-up study.

Investigation into follow-up outcomes was considered higher risk as this was not currently part of standard practice for the service. Particular consideration was given to maintaining patient confidentiality and ensuring that clients were aware that the decision to participate or not was voluntary and would not impact on their treatment. The study was reviewed and approved by the Massey University Human Ethics Committee: Southern A (Application 18/81).

Participant characteristics

12-month evaluation.

708 clients were referred to the FACT service during the first 12 months (10th June 2018 – 11th June 2019). 73.7% ($n = 522$) of these clients identified as female and 26% ($n = 184$) as male. Two clients identified as non-binary. A total of 86.6% of clients ($n = 613$) stated their ethnicity as NZ European and 8.5% ($n = 60$) as Māori. Age ranged from 14 to 92 years ($M = 41.79$, $SD 17.45$). Over 70% of clients were between the ages of 20-59. Additional information about ethnicity demographics can be found in Table 3.

Table 3*Demographics of clients referred to FACT service during first 12 months*

	<i>n</i>	%
Gender		
Female	522	73.7
Male	184	26
Non-binary	2	.3
Ethnicity		
NZ European	613	86.6
NZ Māori	60	8.5
Pacific	9	1.2
Asian	3	.4
Indian	2	.3
Other European	2	.3
Other	19	2.5
Age		
< 20 years	54	7.63
20-29 years	151	21.33
30-39 years	134	18.93
40-49 years	110	15.54
50-59 years	105	14.83
60-69 years	63	8.90
70-79 years	31	4.38
80+ years	20	2.82
Unknown	40	5.65

Follow-up participants.

The intended participants of the follow-up study were clients who were no longer actively participating in the FACT intervention. As patients are never formally discharged from the service the following definitions of treatment stage were decided in consultation with the FACT team:

- Active: Attended at least one appointment within the previous 8 weeks.
- Inactive: No appointment attended in the previous 8 weeks.
- Treatment return: Attended at least one appointment after a treatment break of at least 8 weeks.

Although 8 weeks was the cut-off indicating a client was no longer actively involved in the service, it was decided to initiate contact with potential follow-up participants when there had been a treatment break of 5 weeks (i.e. still technically “active”). This decision was made due to delays predicted to occur as a result of follow-up measures being distributed by post, and in order to give participants sufficient time to complete and return the forms.

Potential participants were therefore patients of the primary health care practice who attended at least one appointment with the FACT service during a 20-week period (11th February – 1st July 2019), followed by at least 5 weeks without a subsequent appointment. Participants were later excluded if they either disclosed during initial contact or if subsequent checks revealed that they had attended a further appointment without an 8-week treatment

break (i.e. confirmed as still “active”). Participant’s data was also excluded from analysis if there was less than 6 weeks between their “final” session and completion of the follow-up measures.

Prior to follow-up data collection a power analysis was performed using the online calculator Statulator (<http://statulator.com/SampleSize/ss2PM.html>). As effect sizes are varied in the literature it was decided to target an effect size halfway between standard thresholds for small ($d = .2$) and medium ($d = .5$) effect sizes (Cohen, 1988). This analysis indicated that a sample size of 67 would be required to detect an effect size of Cohen’s $d = .35$ with 80 power and 95% confidence. Unfortunately, approximately halfway through the recruitment process the primary care practice involved in this study made changes to their check-in system resulting in a period of time where a considerable number of patients did not complete assessment measures and did not receive information inviting them to take part in research on the FACT service. This had a significant impact on the number of patients eligible to take part in this study, resulting in less participants than anticipated and less than the power analysis indicated.

Thirty participants were ultimately included in the follow-up data analysis. Follow-up participants ranged in age from 22 to 77 years ($M = 51$, $SD = 13.11$). The majority of participants were female (86.7%) and identified as NZ European (80%). See Table 4 for further demographic information.

Table 4*Demographics of follow-up participants (n = 30)*

	<i>n</i>	<i>%</i>
Gender		
Female	26	86.7
Male	4	13.3
Ethnicity		
NZ European	24	80
Māori	4	13.3
Indian	1	3.3
Other	1	3.3
Age (years)		
20 – 29	4	13.3
30 – 39	2	6.7
40 – 49	6	20
50 – 59	11	36.7
60 – 69	5	16.7
70 – 79	2	6.7

For the follow-up group the greatest proportion of primary referral reasons were for stress (23.33%, $n = 7$), anxiety (20%, $n = 6$), sleep issues (16.7%, $n = 5$), depression (10%, $n = 3$) and adjustment difficulties (10%, $n = 3$). The remaining referral reasons were distress ($n =$

2), fear / phobia ($n = 1$), lifestyle ($n = 1$), self-image ($n = 1$), and employment related ($n = 1$). The mean number of sessions attended by follow-up participants was 3.03 (SD 1.92) and the average session length was 36.69 minutes (SD 11.97).

The time between participant's last session date and the date their follow-up measures were completed ranged from 42 to 86 days ($M = 53.83$, $SD = 10.44$) for the follow-up group. The majority of participants (70%, $n = 21$) completed their follow-up measures within 6 – 8 weeks of their final session, 23.33% ($n = 7$) completed within 8 – 10 weeks, 3.33% ($n = 1$) completed in 10 – 12 weeks and 3.33% ($n = 1$) completed in just over 12 weeks.

The FACT service and intervention

The FACT service.

The FACT service in this study involves on-site psychologists providing a small number of half hour sessions of Focused Acceptance and Commitment therapy (FACT; Strosahl et al., 2012) to patients identified by their GP (or other healthcare practitioner) as experiencing mild to moderate psychological distress. Whenever possible patient's initial FACT session is scheduled the same day as their appointment with their GP. There is no formal identification or screening process in place and GPs use their own clinical judgement to determine who to refer. Patients under the age of 18, who have an identified intellectual impairment or who are experiencing serious mental health distress are not eligible for the service.

Wherever possible patient's initial FACT appointment is on the same day as their appointment with their GP. Ideally the patient is introduced to the psychologist by their GP and the referral issues outlined as part of a "warm handoff" process. Alternatively, if a psychologist is not readily available, the GP can directly book the patient the earliest possible appointment using a shared booking system.

The FACT service is operational three full days and two half days each week. Three psychologists do a day each with a fourth doing the remaining two half days. The level of integration between the primary care team and the FACT psychology team can be described as semi-collocated (with psychologists delivering the service onsite but having their regular office offsite) and partially integrated (with some shared systems and irregular attendance at team meetings). Collaboration with and understanding of the FACT approach was also supported by a two-day training attended by members of both the primary care and FACT teams. The service commenced seeing patients in June 2018.

The FACT intervention.

Consistent with the typical FACT approach (Strosahl et al., 2012) practitioners did not follow a standardised protocol but instead focused on several key FACT questions (What are you seeking? What have you tried? How has it worked? What has it cost you?), exploring TEAMS (thoughts, emotions, action tendencies, memories and sensations) and enhancing the three pillars of psychological flexibility (openness, awareness, engagement).

Clients are never formally discharged from the service and are able to request additional sessions during a particular episode of treatment, or to re-refer at a later date for "booster"

sessions or new referral issues. Clinical judgement and discussion with the responsible GP is used to determine if there is a need to refer patients to more traditional or intense support services.

The FACT clinicians.

Clients attending the FACT service were seen by one of six registered psychologists. Two clinicians left the service in early 2019 prior to the start of this thesis and accurate information about their professional experience was unavailable.

Post-registration experience of the four current FACT clinicians ranged from approximately 2-12 years. Two of these clinicians (as well as the two previous team members) also attended a 5-day workshop with Strosahl and Robinson, founders of FACT and the PCBH model, and all four attended a two 2-day workshop on FACT alongside other members of the health care service. Three clinicians reported completing a 6-week online course in ACT (ACT for beginners) and one of these clinicians completed two additional 6-week online courses (ACT for anxiety and depression; ACT for trauma). One member of the team was significantly more experienced, attending multiple advanced ACT training opportunities and conducting research in the area of ACT.

Ongoing professional development was supported through individual clinical supervision and weekly group peer supervision. Clinicians also regularly participated in group supervision sessions facilitated by Russ Harris, world class trainer and author of multiple ACT texts (e.g. Harris, 2008, 2009).

Fidelity

Assessing the extent to which FACT is implemented with fidelity is challenging as there are no topographically defined strategies or protocols that are considered to be either necessary or sufficient requirements of ACT (Levin, Smith, & Smith, 2019; Plumb & Vilardaga, 2010). It has been argued by Plumb and Vilardaga (2010) that in order to effectively assess the fidelity of ACT a study-specific written coding manual should be developed and that this will likely need the input of a team of experienced ACT clinicians. Furthermore, they suggest that the focus should be on coding observable therapist behaviour, with consideration to its function and whether it is ACT-consistent or ACT inconsistent (Plumb & Vilardaga, 2010).

Unfortunately, due to practical and financial limitations development of such a coding manual and in vivo assessment of fidelity was not feasible as part of this study. Therefore, advice was sought from Robinson and Strosahl, authors of FACT, regarding recommended fidelity metrics.

Ten standards with corresponding thresholds for fidelity, expressed as a percentage of sessions in which the standard was met, were suggested by Robinson and Strosahl (P. Robinson, personal communication, February 18, 2019). These included rapid access, short sessions and essential elements of intervention delivery (focus on awareness, openness and engagement skills). In addition, a small number of sessions (i.e. 6 or fewer) was also considered an important characteristic of FACT delivered in primary care and was

included as another marker of fidelity. The resulting fidelity metrics are summarized in Table 5.

Table 5

FACT fidelity metrics

50%	Initial sessions should occur on the same day as the referral
80%	Sessions should be 30 minutes or less
80%	Clients should attend six or fewer sessions
70%	Initial sessions should include a life context interview
80%	Initial sessions should include a problem context or functional analysis
50%	Initial sessions should identify a target issue with a severity rating of 7 or above
80%	Sessions should identify values important to the target issue
80%	Sessions should identify a specific behaviour change plan with a confidence rating (i.e. how confident the patient was that they could achieve the plan) of 7 or above
50%	Sessions should focus on awareness skills
50%	Sessions should focus on openness skills
50%	Sessions should focus on engagement skills

Data about some of the above metrics (i.e. time between referral and initial session, number of sessions, duration of sessions and target issue severity) was already routinely collected by the service.

The remaining items (covering in-session intervention elements) were developed into a self-report form (Appendix A) which clinicians were asked to complete for all sessions during three one-week periods (April, June, July). Unfortunately, due to an oversight the initial round of fidelity self-reports did not ask clinicians to note if there had been a focus on engagement skills. This was subsequently added into later rounds of measurement.

Following each collection of fidelity information, the team was provided with the results for reflection and discussion in peer supervision.

Initial sessions same day as referral.

Only 33.93% ($n = 245$) of clients were seen on the same day as their referral, falling below the 50% suggested as a marker of fidelity. The modal number of days between referral and the first scheduled session was zero. The first session was scheduled within one week of referral for the majority of clients (66.07%, $n = 477$) and over three quarters were scheduled within 14 days. A further 29 had referrals that were completed retrospectively (i.e. after the first scheduled session). It is possible that in many cases this occurred as a result of a “warm handover” process on the same day as the client saw their GP, however it was not possible to confirm this from the data provided. Even if same day and retrospective referral results were combined the fidelity threshold would still be unmet.

Session duration.

Due to administration requirements of the FACT service the first session a client attended was typically coded as lasting one hour regardless of actual duration of the session.

Therefore, the first session attended by clients was excluded from analysis of session

length to avoid artificially skewing the results. Duration of all other sessions was documented by clinicians in 5-minute increments (rounding up).

When the first session attended by patients was excluded the mean duration of all subsequent sessions was 37.45 minutes ($n = 638$, $SD 11.94$) with a range of 15 to 90 minutes. Although the modal session length was 30 minutes, the overall percentage of sessions that were 30 minutes or less (61.1%, $n = 390$) fell short of the suggested 80%. A total 82.6% ($n = 527$) of sessions were 45 minutes or less and 15.6% ($n = 99$) were recorded as lasting an hour or longer. The frequency of session duration for all sessions excluding client's first session can be found in Table 6.

Table 6*Duration of subsequent sessions*

<i>Duration (minutes)</i>	<i>n</i>	<i>%</i>
15.00	3	.5
20.00	4	.6
25.00	4	.6
30.00	379	59.4
35.00	29	4.5
40.00	53	8.3
45.00	55	8.6
50.00	11	1.7
55.00	1	.2
60.00	93	14.6
70.00	1	.2
75.00	1	.2
90.00	4	.6
Total	638	100.0

Number of sessions.

For the purpose of exploring the typical number of sessions attended clients were excluded from analysis if they were still active (i.e. had attended or scheduled an appointment in the previous 8 weeks) at the date that data was extracted (15 August 2019). If clients had

multiple referrals prior to 11 June 2019 only data from the currently active referral was excluded. This was in order to get the most accurate representation of the total number of sessions attended per episode of treatment without data potentially being skewed by clients who had either only recently started, or had not yet finished, treatment.

At the time of data extraction there were 576 “non-active” referrals with at least one session attended. The average number of sessions attended per referral was 2.00 (SD 1.50) with a range of 1 to 15 sessions. A total of 94% attended 1-4 sessions and 98.2% attended 6 or less, exceeding the set fidelity standard of 80%. Almost half of clients attended only one session ($n = 286$, 49.7%) and over a quarter ($n = 148$, 25.7%) attended only two.

Further information can be found in Table 7.

Table 7

<i>Sessions attended per non-active referral (n = 576)</i>		
<i>Sessions attended</i>	<i>n</i>	<i>%</i>
1	286	49.7
2	148	25.7
3	69	12
4	38	6.6
5	16	2.8
6	8	1.4
7-10	9	1.6
>10	2	.4

In-session intervention elements.

Results of the self-report fidelity forms completed by clinicians during the three fidelity assessment weeks can be found in Figure 3. Response rates from clinicians was high with fidelity self-assessments completed for 87.5% ($n = 14$) of sessions in the first week, 90% ($n = 18$) of sessions in the second week and 88.2% ($n = 15$) of sessions in the third week.

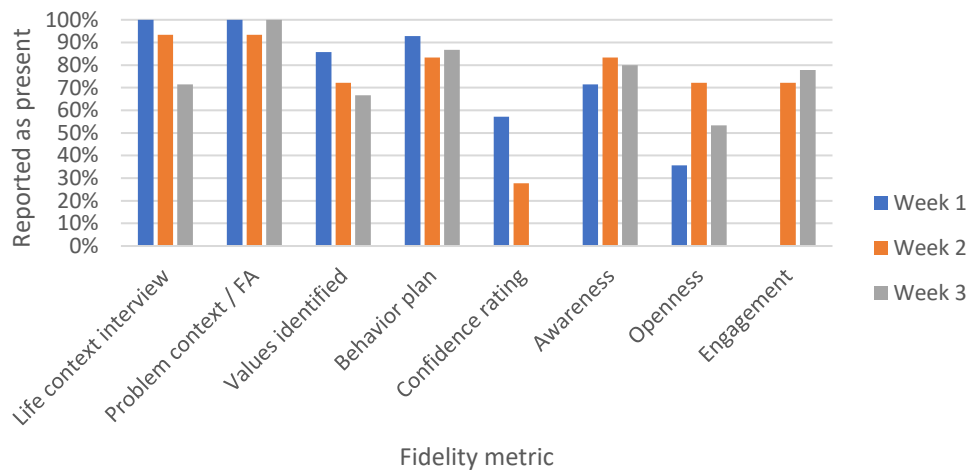


Figure 3. Clinician self-reported adherence to fidelity metrics

At least 70% of initial visits included a life context interview and at least 80% included a problem context or functional analysis in all three weeks, meeting the suggested fidelity standard, although there was a notable decline over time in the proportion of initial sessions that included a life context interview.

The identification of values important to the target issue was over 60% each round of fidelity assessment, however only reached the required standard of 80% in the first round.

Clinicians appeared to routinely identify a behavior plan (over 80% of sessions for all three weeks). The proportion of sessions where a confidence rating of 7 or above was identified was under 60% week one, under 30% week two and was not recorded as being present in any of the fidelity forms completed during week three. Feedback from the FACT team identified that this result reflects clinicians typically not seeking a confidence rating from clients, rather than confidence scores being less than 7 (S. Malthus, personal communication, September 26, 2019).

Awareness and engagement skills were a focus in over 70% of sessions. There was less likely to be a focus on openness skills however this did reach the required 50% in two of the three fidelity assessment weeks and when the results of all three weeks were combined (55%).

Finally, a separate analysis of outcome data showed that 59.93% ($n = 181$) of clients had an initial target issue severity rating of 7 or above, just exceeding the suggested standard of 50%.

Measures

As part of the standard practice of the FACT service a number of assessment measures were provided to patients for completion. The selection of these measures was outside the

control of the researcher. It was chosen to focus on the following measures as part of this thesis:

Target Issue.

During each session participants were asked to identify the key issue that was causing them concern and to rate how much of a problem it was for them in the last week on a scale from 0 = “not a big problem” to 10 = “a very big problem”. This is a recommended part of an initial consult and functional analysis in both the FACT and the PCBH approaches, as well as being a useful in-session strategy for reviewing and discussing progress (Robinson & Reiter, 2016; Strosahl et al., 2012).

As noted above Robinson & Strosahl suggest that, in order to be delivering the FACT model with fidelity, at least 50% of FACT visits should identify a target issue with a severity rating of 7 or above (P. Robinson, personal communication, February 18, 2019). However, for the purpose of evaluating whether there had been meaningful change in patients target issue following intervention the decision was made to include patients with initial target issue scores of 5 or above in analyses. The rationale for this was to maximise the chance that clients experiencing mild to moderate difficulties were included, not just those with the most severe challenges.

Hospital Anxiety and Depression Scale.

The Hospital Anxiety and Depression Scale (HADS) was developed by Zigmond and Snaith (1983). Two subscales, Anxiety and Depression, each have 7 items, making a total

of 14 items. Each item is rated on a scale from 0 to 3, resulting in a score range of 0-21 for each of the subscales and a total possible score of 42.

In a review of 747 studies using the HADS, Cronbach's alphas for the anxiety subscale ranged from .68 to .93 (mean $\alpha = .83$) and for the depression subscale from .67 to .97 (mean $\alpha = .82$) (Bjelland, Dahl, Haug, & Neckelmann, 2002). Bjelland et al. (2002) also reported medium to strong correlations with other measures including the Beck Depression Inventory (BDI; Beck, Ward, Mendelson, Mock, & Erbaugh, 1961). State-Trait Anxiety Inventory (STAI; Spielberger, Gorsuch, Lushene, Vagg, & Jacobs, 1983), Clinical Anxiety Scale (CAS; Snaith, Baugh, Layden, Husain, & Sipple, 1982) and Symptom Checklist-90 (SCL-90; Derogatis, 1977).

A particular advantage of the HADS over some other measures are the cut-offs proposed by the authors as indicating *possible* (subscale scores >7) or *probable* (subscale scores >10) cases of anxiety or depression. Specific thresholds for mild (8-10), moderate (11-13) and severe (≥ 14) have also been suggested (Zigmond & Snaith, 1994). As this study was particularly interested in support for patients with mild to moderate mental health concerns, not just those more severely impaired, the threshold of >7 was used when determining which participants to include in analyses (i.e. only patients with an initial HADS Anxiety or HADS Depression score of 8 or above were included in the corresponding analyses).

Outcome Rating Scale.

The Outcome Rating Scale (ORS; Miller & Duncan, 2000) is a 4-item visual analog measure of global distress that is intended to be used as an in-session assessment and feedback tool, as well as a general outcome measure. The ORS consists of four 10-centimeter lines, each representing one of four life domains (individual, interpersonal, social and overall well-being). Clients are prompted to reflect on the previous week and to indicate how they have been feeling in relation to each of the four areas by placing a mark on the corresponding line. Marks to the left end of the line represent low levels of perceived well-being and marks to the right indicate high levels. The visual marks on the ORS are converted into numerical scores by measuring the distance of the marks along each of the lines (in centimeters to one decimal place) resulting in a score of 0 – 10 for each subscale, and a total possible score of 40. The clinical threshold for the ORS is a total score of below 25, reliable change is considered a change in score of at least 5 points and reliable change combined with crossing the clinical threshold is considered clinically significant change (Miller & Duncan, 2004, as cited in Anker, Duncan, & Sparks, 2009).

The ORS was initially reported to have high internal consistency with an overall alpha of .93 (Miller, Duncan, Brown, Sparks, & Claud, 2003) and several replication studies have found similar results (Bringhurst, Watson, Miller, & Duncan, 2006; Campbell & Hemsley, 2009). Gillaspay & Murphy (2011; as cited in Duncan, 2014) reviewed several studies and reported average Cronbach's alphas of .85 (clinical samples) and .95 (nonclinical samples). The ORS has also been reported in several studies to have at least moderate concurrent validity with other established measures (Bringhurst et al., 2006; Campbell & Hemsley,

2009; DeSantis, Jackson, Duncan, & Reese, 2017) including the Outcome Questionnaire 45.2 (OQ-45.2; Lambert et al., 1996), Depression Anxiety Stress Scales (DASS; Lovibond & Lovibond, 1995), Quality of Life Scale (QOLS; Burckhardt & Anderson, 2003), and Rosenberg's Self-Esteem Scale (1989; as cited in Campbell & Hemsley, 2009).

Acceptance and Action Questionnaire-II.

The AAQ-II (Bond et al., 2011) and the original AAQ (Hayes, 2004) are the most widely used measures of psychological inflexibility. The AAQ-II has 7 items which participants answer on a 7-point scale from 1 = *never true* to 7 = *always true* and while it has attracted some criticism, particularly in regards discriminant validity (e.g. Rochefort, Baldwin, & Chmielewski, 2018; Wolgast, 2014) it is widely used, has been shown to be internally consistent with a mean alpha of .84 ($\alpha=.78-.88$) and 3-month test-retest reliability of .81 (Bond et al., 2011). It's brevity compared to other measures such as the 62 item Multidimensional Experiential Avoidance Questionnaire (MEAQ; Gamez, Chmielewski, Ruggero, Kotov, & Watson, 2011) or even the 15-item MEAQ (Gamez et al., 2014) recommended by some as an alternative (e.g. Rochefort et al., 2018; Wolgast, 2014) may make completion more likely, especially in situations such as the FACT service where clients were asked to complete several measures in a short space of time. Scores of 24 or above on the AAQ-II have been suggested as the threshold for clinical significance (Bond et al., 2011).

Procedure

12-month evaluation.

As part of the standard practice of the FACT service it was planned that each patient was provided with a pack of the above assessment measures upon arrival at every appointment they attended. It was intended that these be completed by the patient while seated in the waiting room. A few minutes was sometimes allocated at the beginning of the session to finish this process if required. Assessment measures were collected by the FACT clinician and returned to the Massey Psychology Clinic where they were entered into the clinic database by administration staff and later extracted for analysis.

Follow-up study.

In order to recruit participants into the follow-up study a form seeking client's consent to be contacted for research purposes (Appendix B) was developed and included in the information provided to clients as part of the standard practice described above.

The names and contact information of patients who had a) not been seen by the FACT service for 35 days and b) had consented to be contacted were identified each week during the recruitment period (18 March – 2 August 2019). Where patients had more than one appointment their most recent “*consent to contact*” form was used (allowing patients with multiple appointments to change their mind at subsequent appointments). This list of eligible patients was extracted by a Massey Psychology Clinic staff member and provided to the researcher in order each week.

Up to three attempts were made by the researcher to contact eligible patients over the two weeks following them becoming eligible. The nature and frequency of contact attempts was dependent on the contact information available, as outlined below:

- Phone only: Two calls during work hours and one call after hours, voice message left if possible (Appendix C).
- Email only: Initial email followed by second email 5-7 days later (Appendix D).
- Phone and email: Initial phone attempt followed by follow-up email (Appendix D) and one subsequent phone attempt.

A contact phone number that participants could call back was provided in all communication attempts.

Once contact was established participants were provided with a brief overview of the research project and asked if they were interested in participating. If they agreed they were posted a participant pack including additional information about the study (Appendix E), a consent form (Appendix F), a set of assessment measures and a return envelope. As part of the consent form permission was sought to access the client's outcome data from previous sessions. Individuals who disclosed during this initial contact that they had attended an appointment within the last 5 weeks (which occurred occasionally due to administration processing delays) or had a subsequent appointment booked were considered as still

“active” and ineligible to take part at that time. These patients were asked for verbal consent for further contact should they become eligible again later (i.e. go for another 35 days between appointments). During 10 weeks in the second half of the recruitment phase clients were also offered the opportunity to be involved in a co-occurring qualitative study. Clients could choose to be involved in one, both or neither of the studies.

If measures had not been returned at the end of two weeks one follow-up contact was attempted to check patients had received the information and to answer any follow-up questions. No compensation for participation was provided.

A total of 111 clients who had consented to be contacted for research purposes became eligible (i.e. no session in the previous 35 days) for this study during the recruitment period. Of these, 38 (34.23%) of clients were unable to be contacted, 9 (8.11%) were ineligible due to still being considered “active”, and one person declined to be involved. A total of 63 people (56.76% of eligible clients) consented to receiving further information about the study and were sent a participant pack. Follow-up measures were received for 33 participants (52.38% of people sent a participant pack). Three participants were excluded from all analyses due to completing their follow-up measures less than 6 weeks after their most recent appointment (i.e. had attended an appointment since being contacted by the researcher) with one of those participants also not having a total treatment break of at least 8 weeks. See Figure 4 for recruitment flow and attrition.

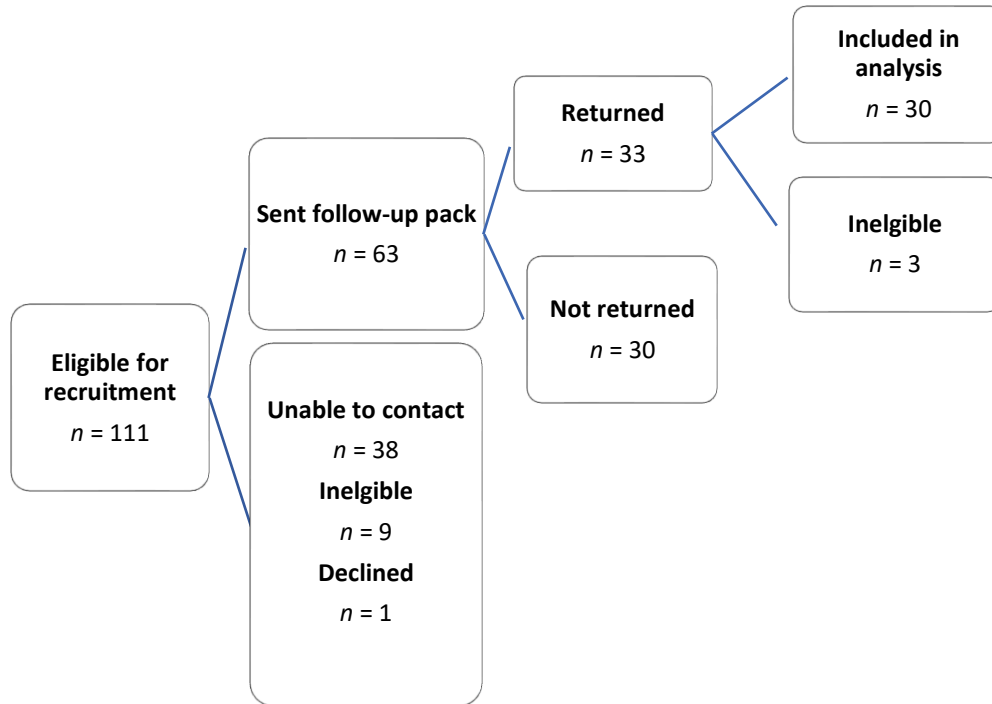


Figure 4. *Follow-up participant recruitment flow*

Data analysis

Data scoring and extraction.

12-month evaluation.

Assessment measures completed by all patients attending the FACT service were entered into the Massey Psychology Clinic database by clinic staff as part of standard practice. The database was set up to automatically score results with the exception of the ORS which required administration staff to manually measure the four individual item scores before

entering. Data was extracted by Massey Psychology Clinic staff and provided with no identifying information to the researcher in a series of spreadsheets.

Follow-up study.

Follow-up measures were hand-scored by the researcher and stored separately to ensure clinicians remained unaware which clients had participated. The identities of consenting participants who had returned follow-up measures were provided by the researcher to Massey Psychology Clinic administration staff who extracted previous session dates, session scores and participant demographics which was provided to the researcher in a series of spreadsheets.

Data from both the 12-month evaluation and follow-up study was subsequently imported into and analysed using the IBM Statistical Package for Social Sciences version 26.0 (SPSS; IBM Corp., 2019).

Data screening and corrections.

Prior to analyses data was screened for missing data and errors with several adjustments were made as a result.

Incorrect client information.

12-month evaluation.

When clients are referred to the FACT service they are assigned a unique identifier number. However, during screening of data for clients seen during the first 12 months of

the service there were three occasions where two clients had been assigned the same identifier. This would have resulted in their outcome data being combined during analysis. A further two clients were found to have been assigned two different identifiers each which would have resulted in their outcome data being separated during analysis. These anomalies were corrected prior to analyses however it is possible that there may be additional errors that were not identified. As the number of errors that were found represented less than 1% of the total referrals during the first 12 months, it was expected the number of unidentified errors was both likely to be small and unlikely to have had a significant impact on the final results.

Follow-up study.

No errors in client information were identified.

Missing data.

The procedure described below applies to both the 12-month evaluation and follow-up study unless otherwise specified.

Where a measure was either missing completely or was partially complete (invalidating the total score for that outcome) this was assigned a missing data value of either “99” (“partial”) or “100” (“missing” measure”) in SPSS. The “exclude cases pairwise” option in SPSS was selected during analyses, as recommended by Pallant (2016), in order to exclude participants who did not have all of the required data for each specific analysis.

In the 12-month analysis of HADS outcomes only total scores, and not individual item scores, were provided for 45.11% of the measures ($n = 406$) and it is unknown whether any of these would have been considered partially complete and excluded from analysis.

Identification of first and last scores.

The procedure described below applies to both the 12-month evaluation and follow-up study unless otherwise specified.

Analysis of outcomes for each variable was based on differences between the first and last measure fully completed by each client. Due to the way data was recorded in the Massey Psychology Clinic database and the format in which it was provided to the researcher it was not possible to tell whether this always corresponded to the first and last session actually attended by a client. For example, a client may have attended six sessions but only have valid measures for session two and session four, and these scores would be the data used in analysis.

However, as the standard protocol for the FACT service was for measures to be completed at every session it was expected that first and last scores would typically be completed at a client's actual first and last session. Therefore, it was anticipated that the percentage of cases where this was not the case would be relatively small and would have a minimal impact on the final result.

It was also theorized that the difference in scores between a client’s actual first and last attended sessions (e.g. session one and session six) could typically be expected to be greater in magnitude than the difference between two middle sessions (e.g. session two and session three). Therefore, the use of data from middle sessions instead of genuine first and last sessions would likely result in more conservative conclusions, and not in an overestimation of the effectiveness of the FACT service.

Clinical thresholds.

Data was only included in each analysis if the client’s first valid score met the clinical threshold for that specific measure. These thresholds are summarized in Table 8.

Table 8

Clinical threshold inclusion criteria

Measure	Included
Target issue	Initial score 5 or above
HADS-Anxiety	Initial score 8 or above
HADS-Depression	Initial score 8 or above
ORS	Initial score 24 or below
AAQ-II	Initial score 24 or above

Assumptions.

Prior to analysis all data sets were checked against assumptions associated with parametric testing, in particular the assumption of normal distribution.

12-month evaluation.

The Kolmogorov-Smirnov statistic was significant, indicating non-normal distribution, for all data sets collected during the first 12 months except for last scores on the HADS-D and AAQ-II measures which were non-significant. However, as visual inspection of the histograms and Normal Q-Q plots suggested most data sets appeared to be approaching normal distribution, and as violation of the assumption of normality is unlikely to cause significant issues with sample sizes larger than 30 (Pallant, 2016), the decision was made to use the parametric t-test for comparison of means rather than a non-parametric alternative.

Follow-up study.

Assessment of Kolmogorov-Smirnov statistics, histograms and Normal Q-Q Plots indicated that follow-up data was not normally distributed. Due to the small size of the follow-up sample making parametric testing less robust in this situation the decision was made to use the Friedman Test, the non-parametric alternative to the one-way repeated measure ANOVA, in order to test for differences in scores across the three time conditions (first, last and follow-up).

Multiplicity adjustment.

Type I errors occur when we incorrectly reject the null hypothesis, and the likelihood of this type of error increases as we increase the number of statistical comparisons we perform (Gueorguieva, 2018). Multiple comparison procedures attempt to control for this issue, with the Bonferroni correction (credited to Dunn, 1961) the most commonly used approach (Gueorguieva, 2018).

The Bonferroni correction aims to control the probability of finding at least one false positive result, known as familywise error rate (FWER), in a defined family of tests (Gueorguieva, 2018). In order to do this the chosen significance level (typically .05) is divided by the number of statistical comparisons performed to arrive at a corrected alpha level that is applied to determine the significance of results (Gueorguieva, 2018).

Alternatively, the p values of each test can be multiplied by the total number of tests and compared to the original alpha (Gueorguieva, 2018).

However, the Bonferroni correction is a highly conservative approach that results in significant loss of power and increased probability of incorrectly accepting the null hypothesis, known as a type II error (Gueorguieva, 2018), and questions are often raised about which tests should be included in the “family” and adjusted for (e.g. Perneger, 1998). As a result, Bonferroni corrections are not universally accepted as being necessary (e.g. Perneger, 1998), and there are differing perspectives on when Bonferroni corrections should be used, including 1) never, 2) multiple analyses on the same data and 3) multiple analyses on the same hypothesis (Cabin & Mitchell, 2000). It has been noted that decision

making about how and when to apply Bonferroni adjustments may be considered subjective in many situations (Cabin & Mitchell, 2000).

In this thesis the decision was made to treat the five dependent variables of interest (target issue, anxiety, depression, psychological flexibility and global distress) as a single family of tests due to their interconnected constructs. For each of these five variables two separate analyses were planned resulting in a total of statistical comparisons (i.e. analysis of the difference between first and last session scores using data from the first 12 months, followed by a separate analysis of first session, last session and follow-up scores using data from the follow-up subsample). The family-wise alpha level was set at .05 and p values were multiplied by ten prior to comparison with the alpha. Both adjusted and non-adjusted p values are provided in the results.

In the analysis of follow-up data post-hoc testing was intended when initial testing detected a significant effect. However, the sample size available for the follow-up data analysis was small, reducing the power to detect significant results and increasing the chance of type II errors. The chance of type II errors was further increased due to the use of non-parametric tests which tend to be less sensitive and may be less likely to detect differences that exist than their parametric equivalents (Pallant, 2016). Given these factors, and that a Bonferroni adjustment was already being applied at the earlier stage of testing, it was decided not to further decrease power by applying Bonferroni adjustments to post-hoc tests.

Effect sizes.

Cohen's *d* effect sizes were calculated using http://www.psychometrica.de/effect_size.html (Lenhard & Lenhard, 2016) and interpreted as small (.20), medium (.50) or large (.80) based on established benchmarks (Cohen, 1988).

Results

Service utilization

Service utilization results are based on data analysed as part of the 12-month evaluation.

Referral reason.

The primary referral reason for each client is presented in Table 9. There were 710 primary referral reasons identified which differs in number by two compared the total number of referrals ($n = 708$). It was not possible to determine the reason for this discrepancy.

A total of 585 clients completed at least one assessment measure prior to their first scheduled session. Of these clients 91.96% met the clinical threshold on at least one of the formal assessment measures (HADS-A, HADS-D, AAQ-II or ORS). Based on suggested severity thresholds (Zigmond & Snaith, 1994) of the 420 people who scored 8 or above on the HADS-A, 24.05% ($n=101$) were classified as mild, 29.76% ($n = 125$) as moderate and 46.19% ($n = 194$) as severe. Of the 278 people who scored 8 or above on the HADS-D, 52.16% ($n = 145$) were classified as mild, 30.58% ($n = 85$) as moderate and 17.27% ($n = 48$) as severe.

Table 9*Primary referral reason for clients referred during the first 12 months*

	<i>n</i>	<i>%</i>
Referral reason		
Anxiety	276	39
Stress	101	14.2
Depression	97	13.7
Coping and adjustment	86	12.2
Sleep issues	30	4.2
Grief	23	3.2
Motivation	19	2.7
Relationship issues	17	2.4
Anger	10	1.4
Fear / phobia	9	1
Eating disorder	8	1
Distress	6	1
Lifestyle	6	1
Other	21	3
Not stated	1	.1
Total	710	

Dropout.

Treatment drop-out was defined as a referred client either:

- a) Not attending any sessions, or
- b) Not attending their last session (i.e. scheduled a session, did not attend the session and had not returned to the service at the time of data extraction).

Of the 708 clients referred during the first 12 months 23.73% ($n = 168$) were considered to have dropped out from the service. A total of 9.18% ($n = 65$) did not attend any sessions at all, and a further 14.55% ($n = 103$) attended at least one session but subsequently did not attend their last scheduled session.

Treatment return

Treatment return was defined as:

- a) Being referred to the service on more than one occasion.
- b) Attending a session following a treatment gap of 8 or more weeks during which no appointments were scheduled.

A total of 74 clients (10.45%) were considered “treatment returners” during the first 12 months.

Multiple referrals.

There were 14 clients who were referred to the service twice during the first 12 months.

The time between the last attended session of the first referral (or the referral date if no

sessions attended) and the date of the second referral ranged from 66 days (9.4 weeks) to 263 days (37.6 weeks) ($M = 152.15$, $SD 62.51$). When data from both referrals was combined the number of sessions attended per client ranged from 0 – 7 ($M = 3.08$, $SD 2.21$). The primary reason for client's first and second referrals appeared to be the same or similar for 42.86% ($n = 6$) of clients, compared to 21.43% ($n = 3$) that appeared to be distinctly different. There was insufficient information to determine whether the referral was substantially different for 35.71% ($n = 5$) of clients.

Treatment gap.

In addition to people with multiple referrals the number of clients who had a treatment gap of 8 or more weeks between scheduled sessions was investigated. This was only examined across client's first six sessions due to practical limitations in analyzing the full data set. In total, 60 clients (8.47%) had at least one treatment gap of eight or more weeks. Four of these people had two breaks and one person had three breaks. The primary issue of concern remained the same pre and post treatment break for 65 of the 66 people and could not be determined for the remaining one client as referral reason was not recorded. The number of sessions attended by the 60 people who had treatment gaps ranged from 0 – 10 with a mean of 3.42 ($SD 2.04$).

Frequency of sessions.

The frequency at which people scheduled (whether attended or not attended) their first six sessions was examined. The modal number of days between scheduled sessions was 14 and the average length of time between scheduled sessions was 28.56 days ($SD 36.53$).

The majority of sessions (57.5%) were scheduled within 3 weeks of the previous appointment. See Table 10 for further details.

Table 10

Frequency of scheduled sessions

	<i>n</i>	<i>%</i>
0 – 7 days	101	12.1
8 – 14 days	176	21
15 – 21 days	204	24.4
22 – 28 days	140	16.7
29 – 35 days	83	9.9
36 – 42 days	38	4.5
43+ days	95	11.4
Total	837	100

12-month evaluation outcomes

Target issue.

In the data set for the first 12 months the first score for target issue was 5 or above for a total of 567 people. A later score was available for 112 (19.75%) of these patients. There

were 20 clients with missing scores and 435 clients who did not have a second session recorded.

A paired-sample t-test ($n = 112$) demonstrated a statistically significant decrease between mean first target issue score ($M = 8.10$, $SD 1.40$) and mean last target issue score ($M = 5.43$, $SD 2.63$), $t(111) = 9.78$, $p = .000$ (two-tailed). The mean difference in target issue scores was 2.67 (95% CI 2.13, 3.21) with a large effect size of $d = 1.27$.

HADS-A: Anxiety.

In the data set for the first 12 months the first score on the HADS-A was 8 or above for a total of 424 people. A later score was available for 178 (41.98%) of these patients. There were 9 clients with missing scores and 237 clients who did not have a second session recorded.

A paired-sample t-test ($n = 178$) demonstrated a statistically significant decrease between mean first HADS-A score ($M = 13.54$, $SD 3.38$) and mean last HADS-A score ($M = 11.62$, $SD 4.14$), $t(177) = 6.69$, $p = .000$ (two-tailed). The mean difference in HADS-A score was 1.92 (95% CI 1.35, 2.49) with a medium effect size of $d = .51$.

HADS-D: Depression.

In the data set for the first 12 months the first score on the HADS-D was 8 or above for a total 287 people. A later score was available for 118 (41.11%) of these patients. There were 6 clients with missing scores and 163 clients who did not have a second session scheduled.

A paired-sample t-test ($n = 118$) demonstrated a statistically significant decrease between mean first HADS-D score ($M = 11.01$, $SD 2.56$) and mean last HADS-D score ($M = 8.55$, $SD 3.80$), $t(117) = 7.79$, $p = .000$ (two-tailed). The mean difference in HADS-D score was 2.46 (95% CI 1.83, 3.08) with an almost large effect size of $d = .79$.

ORS: Global distress.

In the data set for the first 12 months the first score on the ORS was under 25 for a total of 453 people. A later score was available for 191 (42.16%) of these patients. There were 13 clients with missing scores and 249 clients who did not have a second session scheduled.

A paired-sample t-test ($n = 191$) demonstrated a statistically significant increase between mean first ORS ($M = 13.00$, $SD 6.68$) and mean last ORS ($M = 19.71$, $SD 8.11$), $t(190) = -8.71$, $p = .000$ (two-tailed). The mean difference in ORS score was -6.72 (95% CI -8.23, -5.29) with a large effect size of $d = .82$.

Reliable and clinically significant change.

As thresholds for determining reliable and clinically significant change have been established for the ORS (Miller & Duncan, 2004, as cited in Anker et al., 2009) this was calculated for the 238 clients with first and last ORS scores (prior to applying the exclusion criteria of first score 25 or above used for the previous analyses).

A total of 55 people (23.11%) demonstrated both a reliable and clinically significant increase in scores (i.e. an increase in score of at least five points and crossed the clinical

threshold, representing a decrease in global distress). A further 46 (19.33%) demonstrated reliable change only (i.e. an increase in score of at least five points without crossing the clinical threshold). A total of 11 people (4.62%) demonstrated both a reliable and clinically significant decrease in scores (representing an increase in global distress). A further 17 (7.14%) demonstrated reliable decrease only.

AAQ-II: Psychological Flexibility.

387 people during the first 12 months of data had first scores on the AAQ-II of 24 or above. A later score was available for 164 (42.37%) of these patients. There were 16 clients with missing scores and 207 clients who did not have a second session scheduled.

A paired-sample t-test ($n = 164$) demonstrated a statistically significant decrease between mean first AAQ-II ($M = 34.32$, $SD = 6.07$) and mean last AAQ-II ($M = 30.56$, $SD = 8.11$), $t(163) = 6.03$, $p = .000$ (two-tailed). The mean decrease in AAQ-II score was 3.76 (95% CI 2.53, 4.99) with a medium effect size of $d = .53$.

Results from the 12-month evaluation are summarized in Table 11.

Table 11*Summary of t-test results – 12 month evaluation*

	Target issue	HADS-A	HADS-D	ORS	AAQ
<i>n</i>	112	178	118	191	164
<i>First score: M (SD)</i>	8.10(1.40)	13.54 (3.38)	11.01 (2.56)	13 (6.68)	34.32 (6.07)
<i>Last score: M (SD)</i>	5.43(2.63)	11.62 (4.14)	8.55 (3.80)	19.71 (8.11)	30.56 (8.11)
<i>Mean difference (95% CI)</i>	2.67 (2.13, 3.21)	1.92 (1.35, 2.49)	2.46 (1.83, 3.08)	-6.72 (-8.23, -5.29)	3.76 (2.53, 4.99)
<i>t</i>	9.78	6.69	7.79	-8.72	6.03
<i>df</i>	111	177	117	190	163
<i>Cohens's d</i>	1.27	.51	.79	.82	.53
<i>Non-adjusted significance</i>	.000	.000	.000	.000	.000
<i>Bonferroni adjusted significance</i>	.000	.000*	.000*	.000*	.000*

* *Significant result***Follow-up study outcomes****Target issue.**

First, last and follow-up data was available for 11 participants with first target issue scores of 5 or above. Results of the Friedman Test indicated that there was a statistically significant difference in target issue score across the three time conditions, $\chi^2(2, n = 11) = 18.54, p < 0.001$. Inspection of the medians showed reduction in median scores between the

first target issue score ($Mdn = 8$) and last target issue score ($Mdn = 6$) with a further decrease at follow-up ($Mdn = 3$).

Post-hoc Wilcoxon Signed Rank tests indicated a statistically significant reduction in target issue scores between first and last target issue score ($n = 13$, $z = -2.68$, $p < .01$, $d = 1.25$), first score and follow-up score ($n = 14$, $z = -3.19$, $p = .001$, $d = 1.50$) and between the last score and follow up ($n = 11$, $z = -2.68$, $p < .01$, $d = 1.39$).

HADS-A: Anxiety.

First, last and follow-up data was available for 15 participants with first HADS-A scores of 8 or above. Results of the Friedman Test did not indicate a significant difference in HADS-A across the three time conditions $\chi^2(2, n = 15) = 8.607$, $p = .14$, although results would have reached significance if a Bonferroni correction was not applied.

HADS-D: Depression.

First, last and follow-up data was available for 15 participants with first HADS-D scores of 8 or above. Results of the Friedman Test did not indicate a significant difference in HADS-D across the three time conditions $\chi^2(2, n = 11) = 9.71$, $p = .08$. However, results would have reached significance if a Bonferroni correction was not applied.

ORS: Global distress.

First, last and follow-up data was available for 11 participants with first ORS scores of under 25. Results of the Friedman Test indicated that there was a statistically significant difference in ORS scores across the three time conditions, $\chi^2(2, n = 18) = 14.79$, $p < 0.05$.

Inspection of the medians showed an increase in median scores between the first ORS ($Mdn = 17.5$) and last ORS ($Mdn = 22.45$), with a further increase at follow-up ($Mdn = 26.5$).

Post-hoc Wilcoxon Signed Rank tests indicated a statistically significant improvement in ORS scores between first ORS and last ORS ($n = 18, z = -2.53, p < .05, d = .92$) and between first and follow-up scores ($n = 22, z = -3.75, p < .001, d = 1.39$). No significant difference was found between last and follow-up scores ($n = 18, z = -1.59, p = 0.112$).

AAQ-II: Psychological flexibility.

First, last and follow-up data was available for 13 participants with first AAQ-II scores of 24 or above. Results of the Friedman Test did not indicate a significant difference in AAQ-II across the three time conditions $\chi^2(2, n = 13) = 5.265, p = .072$. Results of the Friedman Test would not have reached significance even with a non-adjusted p value.

Results from the Friedman's Test for each variable are summarized in Table 12.

Table 12*Summary of Friedman's Test results – Follow-up study*

	Target issue	HADS-A	HADS-D	ORS	AAQ-II
<i>n</i>	11	15	11	18	13
<i>df</i>	2	2	2	2	2
χ^2	18.50	8.60	9.71	14.9	5.27
<i>Non-adjusted</i>	.000*	.014*	.008*	.001*	.072
<i>p value</i>					
<i>Bonferroni adjusted</i>	.000*	.14	.08	.01*	.72
<i>p value</i>					

* *Significant result*

Results from the post-hoc Wilcoxon Signed Rank Tests are summarized in Table 13.

Table 13

Summary of post-hoc Wilcoxon Signed Rank Test results - Follow-up study

	<i>n</i>	<i>z</i>	<i>p</i>	<i>Cohen's d</i>
Target issue				
<i>First to last session</i>	13	-2.68	.007*	1.25
<i>First session to follow-up</i>	14	-3.19	.001*	1.50
<i>Last session to follow-up</i>	11	-2.68	.007*	1.39
ORS				
<i>First to last session</i>	18	-2.53	.011*	.92
<i>First session to follow-up</i>	22	-3.75	.000*	1.39
<i>Last session to follow-up</i>	18	-1.59	.112	.56

** Significant result*

Discussion

The current study investigated the delivery of Focused Acceptance and Commitment Therapy (Strosahl et al., 2012) in a New Zealand primary care setting. Referral reason, service use and outcome data from the first twelve months of the service was analysed, along with follow-up data from a small sub-sample of patients, in an attempt to answer the following three key questions: 1) How was the service utilized? 2) Was the service effective in reducing symptoms of psychological distress and improving psychological flexibility? 3) Were results maintained at follow-up?

The study found that most clients referred to the service were female, NZ European, aged between 20-60 years old and met established clinical thresholds on at least one assessment measure (anxiety, depression, global distress or psychological inflexibility). Almost all clients attended six or fewer sessions with almost half of clients only attending one session. Most sessions lasted thirty minutes or less although the overall average was slightly higher. The majority of people seen by the service were considered to be experiencing mild to moderate mental health distress, representing the “missing middle” that are typically unable to access specialist mental health services in New Zealand (Government Inquiry into Mental Health and Addiction, 2018). However, many clients indicated that they were experiencing more significant levels of distress, meeting clinical thresholds for “severe” levels of anxiety and / or depression. This was particularly apparent in regard to anxiety, with almost half of all clients who met the clinical threshold for anxiety falling into the “severe” category.

Consistent with predictions, analysis of data gathered during the first twelve months demonstrated statistically significant reductions in client-reported target issue severity, symptoms of anxiety, depression and global distress, as well as improvement in psychological flexibility. Effect sizes were in the medium range for anxiety and psychological flexibility, and large or approaching large for depression, global distress and target issue. Analysis of data from participants in the follow-up study indicated that there was a significant reduction in the severity of both the client's target issue and levels of global distress between their first and last session measures. A further significant reduction was found between last scores and follow-up scores for target issue severity, and no significant change for global distress. These results were consistent with predictions that reductions in target issue severity and global distress would be maintained at follow-up. However, contrary to predictions and inconsistent with results from the 12-month evaluation, no significant difference was found in anxiety, depression or psychological flexibility across the three time conditions for the follow-up sample, indicating that clients did not experience significant improvement (or deterioration) in these variables. A change in symptoms of anxiety and depression would have reached significance if a Bonferroni adjustment had not been applied to the p value, however the difference in psychological flexibility would have still failed to reach significance.

The remainder of this chapter will explore the relationship of the current study to existing literature in more detail, before acknowledging the limitations of the study and suggesting recommendations for future research. As discussed previously, a comprehensive search of

the literature and consultation with experts in the field, including one of the founders of FACT, failed to identify any peer-reviewed research on FACT in primary care directly comparable to the current study. The most directly comparable information comes from the introduction of Health Improvement Practitioners (HIPs) trained in the use of FACT into seven primary care practices as part of the *Te Tumu Wairoa* (TTW) pilot project (Appleton-Dyer et al., 2018). Although both services involved the use of FACT, for the purpose of clarity and consistency “FACT service” and “FACT clinicians” in the following discussion will refer to the service investigated in the current study, and the terms “TTW” or “Health Improvement Practitioners” will refer to findings from the *Te Tumu Wairoa* evaluation.

Service utilization

In general, the patterns of service utilization in the current study are consistent with patterns often found in other brief psychological interventions based in primary care. For example, females typically make up approximately 60-70% of participants (Appleton-Dyer et al., 2018; Bryan et al., 2012; McFeature & Pierce, 2012) and the mean age is often around 40 years (Angantyr et al., 2015; Bryan et al., 2012; McFeature & Pierce, 2012). In addition, the number of sessions attended by clients in the current study also followed a similar pattern to existing literature on primary care ultra-brief focused interventions with the greatest percentage of people only attending one session (Appleton-Dyer et al., 2018; Bryan et al., 2012; Bryan et al., 2009; Ray-Sannerud et al., 2012). However, two key differences in patterns of service utilization between the current study and existing literature did emerge.

Firstly, approximately twice as many Māori were referred to the TTW services (Appleton-Dyer et al., 2018) compared to those that were referred to the FACT service. The reason for this is unknown. It is possible this finding is associated with regional and primary care centre differences in ethnicity statistics, rather than any particular difference in the effectiveness of the respective services in reaching and supporting Māori. However, the percentage of Māori referred to TTW was equivalent to the proportion of Māori enrolled across the primary care practices involved in the evaluation. In contrast, although the demographics of clients enrolled at the primary health care practice participating in this study was unavailable at the time of writing, the percentage of Māori seen by the FACT service was approximately half the percentage of Māori estimated to be living in the Palmerston North and MidCentral region during the 2013 census (Stats NZ Tatauranga Aotearoa, 2017). Combined with previous research indicating that the prevalence of mental health in primary care is higher for Māori than other ethnicities (Oakley Browne et al., 2006) this data suggests that Māori were under-represented in referrals to the FACT service. Future research into how to address this issue is warranted in order to have the greatest and most equitable impact on reducing the NZ treatment gap highlighted in the introduction to this thesis.

The second difference in service utilization between the current study and existing literature was in regards to the categorisation of issues leading to referral. Anxiety, depression and stress were the three primary reason clients were referred to the FACT clinicians and to the TTW Health Improvement Practitioners (Appleton-Dyer et al., 2018). However, in the TTW evaluation the number of people with a primary referral concern of anxiety was very similar to the number of people with depression as their primary concern;

In contrast, in the FACT study twice as many people were experiencing symptoms of anxiety as their primary issue, compared to those with depressive symptoms. In addition, in the FACT study almost half of individuals who met the clinical threshold for possible anxiety on the HADS-A had scores that fell in the range indicative of “severe” levels of anxiety. In contrast, the greatest percentage of clinical HADS-D scores fell in the “mild” range. Previous research suggests that symptoms of depression are the mental health concern most commonly detected in primary care (Anseau et al., 2004; Roca et al., 2009), and consistent with the TTW findings, other research on ultra-brief focused therapy in primary care has also demonstrated roughly equal percentages of clients with anxiety and depression symptoms (Angantyr et al., 2015; Bryan et al., 2009). The reason for the unusually disproportionate ratio of anxiety to depression in the current study is unknown.

A particular area of interest in regard to service utilization, due to the impact it has on the gap between those who need mental health support and those who receive it, is that of drop-out. Just under a quarter of clients referred during the first 12 months either did not attend any sessions with, or were classified as having prematurely dropped out of, the FACT service. The proportion of people who were referred to the service, but ultimately did not attend any sessions, was consistent with results from TTW (Appleton-Dyer et al., 2018). However, the combined proportion of people who never attended and those who didn't return after failing to attend a scheduled appointment, was lower in the current study than findings reported in studies of psychotherapy delivered in traditional formats and settings (Hampton-Robb, Qualls, & Compton, 2003; Swift & Greenberg, 2012; Wierzbicki & Pekarik, 1993).

One possible explanation for this could be due to the typically rapid access to the FACT service. This possibility is supported by previous research demonstrating that the likelihood of attending a first session of psychotherapy decreases as waiting times increase (Swift, Whipple, & Sandberg, 2012). In addition, lower rates of non-return following failure to attend a scheduled session may be due to a reduced focus on scheduling subsequent appointments in the first place. Under the FACT approach clients are often given the message that they may get everything they need out of a small number of sessions, possibly even one (Arroll, 2016; Strosahl, 2019). As a result, they may be less likely to book “unnecessary” follow-up appointments that they later cancel. Whereas, in a treatment protocol expected to last 12 sessions clients may be encouraged to schedule more appointments regardless of whether they feel they need or want to return, therefore resulting in higher rates of cancellation.

However, although the low drop-out rate in this study is encouraging, it is acknowledged that the true rate may be higher. It is possible that some people would have been considered to have ended treatment prematurely by other definitions (Wierzbicki & Pekarik, 1993) and may not have been captured by the definition used in this study. For example, some of the people who only attended one session may have chosen not to book a follow-up visit, possibly despite clinician recommendations, and perhaps not because they didn't feel they needed it, but potentially because they didn't feel the service was the right fit for them.

Effectiveness

As highlighted at the beginning of this chapter, the current study found statistically significant reductions in symptoms of anxiety, depression, and global distress, reduction in severity of client's target issue, plus improvements in psychological flexibility, following at least two sessions of FACT. These results are generally consistent with other findings on FACT in primary care (Appleton-Dyer et al., 2018), previous research demonstrating that ultra-brief focused therapy can be effective (Bryan et al., 2012; Bryan et al., 2009; Cigrang et al., 2006; Hassink-Franke et al., 2011; McFeature & Pierce, 2012), and that individuals have the potential for rapid and radical change (Erekson, Clayson, et al., 2018; Erekson, Horner, & Lambert, 2018; Keinonen et al., 2018; Lutz et al., 2013; Stiles et al., 2003; Tang et al., 2007).

Effect sizes.

In the current study medium effect sizes were found for anxiety and psychological flexibility, approaching large for depression, and were large for global distress and target issue severity. The largest effect size was found for target issue severity, with the second largest effect size being for levels of global distress. This is not an unexpected result given that the target issue is the reason clients have sought support, and is the direct focus of intervention and behavioral change strategies. It is also likely that the severity of the target issue, and patient's level of global distress are correlated to at least some degree and therefore a reduction in the severity of the target issue is likely to more consistently result in an easing of client's distress levels associated with that issue, although this assumption was not tested in the current study.

Directly comparing the magnitude of change between the current study and the TTW evaluation is not possible as effect sizes were not reported in the latter (Appleton-Dyer et al., 2018). Similarly, comparing effects of the current study to the literature on ultra-brief focused PCBH interventions is challenging due to limited reporting on within-group effect sizes. However, the medium to large effect sizes demonstrated in this study are consistent with the findings from Bryan et al. (2009) who found significant improvements in global mental health, subjective wellbeing and life functioning. In addition, Bryan et al. (2009) found a dose-response effect, with a medium effect size for the group of patients who attended two sessions and a large effect size for the group that attended three. It is not possible to say if there was a dose-response effect in the current study as results were not analysed separately by number of sessions attended.

Small effect sizes were reported by Bryan et al. (2009) in relation to reduction in common emotional symptoms, including anxiety and depression, as assessed by the corresponding subscale on the BHM-20 (Kopta & Lowry, 2002) for both the two-session and three-session groups. This is lower than the medium effect size for anxiety and almost large effect size for depression found in the current study. The most likely explanation for this discrepancy is due to differences in the level and nature of client's distress at baseline. Depression and anxiety were the most frequent referral reasons indicated by Bryan et al. (2009) and the mean global mental health score indicated that in general clients were experiencing poor overall mental health functioning prior to treatment. Despite this, the mean emotional symptoms subscale on the BHM-20 was in the healthy range at baseline, and as such the magnitude of any improvement will inherently be small. In contrast, in the

current study client data was excluded from analysis if their initial scores fell below clinical thresholds, and scores on the HADS suggested that a high percentage were experiencing moderate to severe levels of anxiety and depression. As a result, the potential for large improvements in scores in the current study was greater than in the study by Bryan et al. (2009). While there was a difference in effect sizes between the current study and the one by Bryan et al. (2009), both demonstrated lower effect sizes for anxiety and depression than for other variables measured, although the difference in effect size between depression and global distress was minimal in the current study. This pattern perhaps reflects the assertion by Strosahl et al. (2012) that the goal of FACT is not to reduce symptoms of distress but instead to support people to live meaningful lives despite those symptoms.

The effect sizes for depression, anxiety and psychological flexibility in this study also appear to be similar to ACT delivered in at least 2-4 times more therapy hours than in the current study (Kohtala et al., 2015; Kyllönen et al., 2018). This includes comparable results to the medium effect sizes for depression and perceived mental health functioning following six hours of group-based FACT in primary care (Glover et al., 2016).

However, despite using FACT specific strategies, no significant difference in psychological flexibility was found by Glover et al. (2016), unlike the significant medium effect found in the considerably briefer FACT intervention investigated in the current study. Glover et al. (2016) suggested that the reason for the non-significant results in their study may have been due to the brevity of the group intervention and assessment period,

and that meaningful changes in psychological flexibility may take longer to develop, yet findings from the current FACT study would appear to contradict that. One important difference between the two studies that might go some way towards explaining this discrepancy is the use of different data analysis inclusion and exclusion criteria. In the current study participant data was excluded from analysis if their initial score on the AAQ-II fell below the identified clinical threshold of 24, suggesting that they were not experiencing clinically significant psychological inflexibility. In contrast it would appear that data from all clients was included in analysis by Glover et al. (2016), resulting in a lower pre-intervention mean, and the potential for results to be skewed by clients with non-clinical psychological flexibility scores at baseline.

Clinical improvement.

As well as the statistically significant improvements in all outcomes discussed above, just over a fifth of clients experienced a reliable and clinically significant improvement in global distress as measured using the ORS (i.e. a change in raw score of at least five points that moved them from the clinical to non-clinical category), with a further fifth demonstrating improvement that was reliable only.

It is difficult to directly compare clinically significant change with other studies due to different outcome measures used and different definitions of what is considered clinically significant change. For example, in their evaluation of the TTW services Appleton-Dyer et al. (2018) reported that half of participants experienced a shift to a lower category of clinical severity on the DUKE Health Profile measure (e.g. from severe to moderate). At

first glance this would appear to be just over double the percentage of people in the current study who moved from the clinical to non-clinical range on the ORS. However, in addition to crossing the clinical threshold the magnitude of change for the people attending the FACT service was also considered to be of a reliably sufficient magnitude to be meaningful (i.e. a change of five or more points on the ORS). This level of analysis was unavailable for the TTW study (Appleton-Dyer et al., 2018) and it is possible that for some participants very small changes in raw scores resulted in shifts in their category of clinical severity. Similarly, the 40% of participants who were reported by (Bryan et al., 2012) to have clinically significant improvement in global mental health following PCBH ultra-brief focused therapy was also based on a change in severity category without specifying the magnitude of change.

However, regardless of the method used to determine clinical significance existing studies on FACT, PCBH and brief ACT typically report rates of clinical improvement ranging from approximately 20 – 50% (Appleton-Dyer et al., 2018; Bryan et al., 2012; Glover et al., 2016; Kohtala et al., 2015; McFeature & Pierce, 2012) which is consistent with findings from the current study. Similarly, the proportion of clients in the current study who demonstrated a clinically significant increase in global distress was also consistent with previous research that the proportion of individuals who deteriorate during or after ACT treatment is typically under 10% (Bryan et al., 2012; Glover et al., 2016).

Robinson et al. (2019) suggested that some clients have the potential to be “rapid responders”, making clinically significant improvements in four or fewer sessions. The

findings of the current study support previous research indicating that up to as much as 50% of clients may be expected to fit into this “rapid response” category of clients (Beard et al., 2019; Erekson, Horner, et al., 2018; Keinonen et al., 2018; Tang et al., 2007).

However, this study expands on the existing literature by suggesting that sessions of psychotherapy don’t necessarily need to be the traditional one hour in length in order to achieve rapid and clinically significant improvement.

Follow-up

Previous research has demonstrated that positive outcomes associated with condensed versions of ACT, as well ultra-brief focused interventions delivered under the PCBH approach, can be maintained three months to several years later (Angantyr et al., 2015; Cigrang et al., 2017; Kohtala et al., 2015; Kohtala et al., 2017; Kyllönen et al., 2018; Ray-Sannerud et al., 2012).

Consistent with the above, participants in the current study who returned follow-up measures demonstrated significant and large reductions in target issue severity and global distress between their first and last sessions, with results maintained 6-10 weeks later. There was also a further significant reduction in client’s target issue severity between their last and follow-up scores, suggesting that not only were results maintained but that client’s may have continued to improve during the follow-up period. However, it is important to acknowledge that the last score available for each client was captured at the beginning of their last attended session, before seeing their clinician, and therefore can’t be considered a true “post-intervention” score. As such, it is possible that the apparent reduction in target issue severity during the follow-up period may reflect progress made within that final session. If participants had rated the severity of their target issue immediately following that final session subsequent analysis may not have yielded the same statistically significant results.

In contrast, tests conducted on the anxiety, depression and psychological flexibility variables showed no significant change in scores across the first, last and follow-up time conditions. These results are most likely due to inadequate sample size as discussed in the limitations section below. In addition, anxiety, depression and psychological flexibility are the three variables that showed the smallest effect size in the analysis of the 12-month data and it may be that the magnitude of change for the follow-up participants was not large enough to be detected. Possible support for this theory may be found in the mean number of sessions attended by clients who participated in the follow-up study, which was three, one session more than the overall average for all clients referred. This may suggest that the follow-up group tended towards slower progress, and perhaps smaller gains.

Limitations

The findings of this study should be considered in light of several limitations. Firstly, there were no control conditions included in the design of this study which makes it difficult to determine the extent to which the FACT intervention may have been directly responsible for the observed improvements. For example, it is possible that clients may have improved to a similar (or larger) extent if they had continued to receive treatment as usual from their GP or received alternative brief psychotherapy such as CBT. Another difficulty with the design of this study is that information was not available about concurrent treatment clients may have been receiving. In a recent study it was reported that over a third of patients commenced or increased their dosages of prescribed medication at the same time as they started receiving support from a PCBH Behavioral Health Consultant (Bridges et al., 2019). Although Bridges et al. (2019) found no significant difference in outcomes between patients who initiated/increased medication, patients who had no change in existing

medication and patients who were prescribed no medication, it remains possible that concurrent pharmacotherapy contributed to outcomes in the current study.

The extent to which the results of the current study should be interpreted as evidence in support of FACT must also be considered in relation to how well the service delivered can be considered to meet the definition of FACT; i.e. was the service delivered with sufficient fidelity? Eleven fidelity indicators were considered in this study. The majority of these were met by the service. However, there were four fidelity indicators that were not met by the service (time between referral and initial session, session length, identification of values, and identification of confidence rating). It is also important to acknowledge that the method used to assess these fidelity requirements was not particularly robust. Firstly, the number of individual sessions in which fidelity was reported was small, representing only around 3% of the total number of sessions scheduled during the first 12 months. Second, the fidelity indicator “focus on engagement skills” was missed from the first round of assessment meaning this was assessed for an even smaller percentage of sessions. Finally, while some indicators were measured using objective data (e.g. session length, number of sessions), the presence or absence of in-session elements (e.g. contextual interview, focus on awareness, openness and engagement) was self-reported by the clinician. (Borrelli, 2011) notes that although clinician self-report can enhance fidelity by reminding clinicians of the essential elements that need to be delivered, there is potential for clinicians to rate themselves as more adherent than they are. As a result, while it is clear that the majority of clients referred to the service received an ultra-brief focused intervention (six or fewer sessions typically lasting no more than 30 minutes), it is difficult to say with confidence

that the intervention that they received was sufficiently adherent to fidelity requirements to encompass all the key elements of FACT. However, the statistically significant improvement in AAQ-II scores suggests that the intervention was delivered with sufficient fidelity to improve psychological flexibility, the core psychological process targeted by FACT.

Another limitation to this study is that, as a result of drop-out, missing scores and almost 40% of clients only attending one session, first and last scores for each variable were only available and analysed for 15 – 27% of the 708 clients who were referred. In particular, the lack of outcome data available for the large proportion of clients who only attended one session means conclusions can not be drawn about the effectiveness of a single session of FACT. A key principle of FACT is the importance of delivering active elements of treatment from the very first session (rather than being dedicated to assessment only), including clients leaving the first session with a behavior change plan, and there is optimism that if people don't return it is because they don't need to (Strosahl et al., 2012). However, this assumption can not be empirically supported without obtaining follow-up outcome data for these clients and seeking their perspective on why they did not return. For example, it is likely that a portion of clients did not return because their mental health condition deteriorated, were not comfortable with elements of the intervention (e.g. focus on accepting negative emotions rather than “fixing” them) or did not feel the brief nature of the service would meet their needs and sought support elsewhere.

Finally, the ability to draw conclusions on the maintenance of outcomes was hampered by the small sample size recruited to the follow-up study. Unfortunately, less than half of the number required (in order to detect a small to medium effect size with 80 power and 95% confidence) returned follow-up measures and even less had data available for all three measurement times (first, last and follow-up). Valid data from less than 20 participants was used in all but one analyses (in which there was valid data for 22 participants). As a result, the statistical power to detect significant results was severely limited, increasing the likelihood that the non-significant results for depression, anxiety and global distress represent type two errors (i.e. incorrectly concluding that there was no significant result when one actually exists). It is possible that significant results for these three variables would have been detected if a larger sample had been available. This seems a reasonable prediction given that significant differences with large effect sizes were found for all variables in the 12-month evaluation data, and that results for both anxiety and depression would have been significant in the follow-up study prior to the Bonferroni adjustment being applied.

The small sample size also increases the risk that the follow-up sample was not representative of the larger group in meaningful ways. For example, although the distribution of gender and ethnicity was similar between the 12-month evaluation and follow-up participants there was a 10-year difference in the mean age, and they had an average of one additional session. Another way in which the sample may be biased is if the characteristics of those who completed and returned follow-up measures was different to

those who were eligible to participate but that did not respond (such as bias towards clients who derived the greatest benefit from the service).

Future research

As discussed above, this study had several limitations which ideally will be addressed in future research. Firstly, the inclusion of control conditions with random assignment would enable comparisons to be made between the relative effectiveness of FACT versus other treatment. Regardless of research design, the ability to draw conclusions about the effectiveness of FACT would also be enhanced by the implementation of more stringent measurement of fidelity, such as videoing or audiotaping a significant proportion of sessions, considered to be the ‘gold standard’ approach (Bellg et al., 2004; Borrelli, 2011), and evaluating these based on a specifically developed coding manual as suggested by (Plumb & Vilardaga, 2010).

Monitoring outcomes for clients who only attend one session of treatment, the modal number reported in this study, is also essential in order to understand both the effectiveness of a single session of FACT as well as to gain a greater understanding of the overall effectiveness and acceptability of the FACT approach.

Future research on the maintenance of outcomes with larger sample sizes should also be conducted, including analysis at varying and longer follow-up periods. Recruitment strategies to increase the response rate should be investigated. This might involve establishing alternative options for responding (e.g. online, electronically or by phone). As

most participants are likely to visit their G.P. again at some stage following involvement with the FACT service, it may be feasible to develop an “ongoing” recruitment system whereby clients are provided the opportunity to complete follow-up measures each time they visit the primary care practice.

Although the findings of this study demonstrate that FACT can be effective in reducing symptoms of psychological distress this is not considered to be the primary goal of FACT (Strosahl et al., 2012). Instead the primary goal of FACT is “living vitally, not being symptom-free” (Strosahl et al., 2012, p. 53), supporting clients to engage in meaningful and value-based activities. However, this study did not include any measures that allow conclusions about whether this primary goal of FACT was achieved for clients. Although clients may be experiencing less symptoms of anxiety, depression or distress it is unknown what impact this had on their day-to-day lives such as spending more time with family and friends, engaging in valued activities, and behaving in ways more congruent with their personal values. For example, it is possible that a client may have experienced an overall reduction in symptoms of anxiety through the increasing present-moment awareness, while still avoiding meaningful and value-based, but anxiety-provoking, situations such as supporting their children at sporting events. Future research should include assessment of engagement in value consistent living using psychometrically sound measures such as the Valued Living Questionnaire (Wilson, Sandoz, Kitchens, & Roberts, 2010), Engaged Living Scale (Trompetter et al., 2013) or Valuing Questionnaire (Smout, Davies, Burns, & Christie, 2014).

More broadly, it has been suggested that brief therapies, regardless of theoretical orientation, may share some common features which may aid in accelerating the progress made in therapy (Eckert, 1993; McGuinty et al., 2016). Given that an entire episode of treatment may last as little as 30 minutes, determining the nature of these elements, and which are the most essential or powerful, is a particularly important line of research in regard to FACT, as well as other ultra-brief focused interventions. For example, investigation into the relative importance of the acceptance, engagement and openness skills emphasized in FACT may aid clinicians in making treatment decisions about which areas to focus on first.

Another area of research that may aid in clinical decision making is further exploration of client factors (e.g. age, gender, previous experience with therapy) and presenting issues (e.g. initial severity of symptoms, co-morbidity) that may predict positive response to ultra-brief focused interventions such as FACT. This may help referrers decide the most appropriate referral pathway for specific clients (e.g. to ultra-brief or traditional length services) by identify patients who are more or less likely to be “rapid responders”.

Conclusions

Recently, the New Zealand government acknowledged the need for a different approach to supporting people with mild to moderate mental health issues as part of efforts to reduce the gap between those who require psychological support and those who receive it (Government Inquiry into Mental Health and Addiction, 2018; Government of New Zealand, 2019), and describe an ideal scenario where “when a GP identifies a mental

health or addiction issue they can physically walk with their patient to a trained mental health worker to talk” (Government of New Zealand, 2019, pg 32). The Fact service investigated in this study closely aligns with that vision. Although there were significant limitations which make it difficult to draw strong conclusions about the effectiveness of FACT as a primary care intervention, overall the results of this study suggest that ultra-brief focused interventions can be effective at rapidly reducing the symptoms of clinically significant psychological distress for a large number of people. However, no conclusion was reached about the potential for benefits to be maintained at follow-up due to the small, and likely unrepresentative, sample size.

Reasons for the existing treatment gap are complex and no single strategy is likely to completely eliminate it. However, the delivery of ultra-brief focused interventions in primary care would seem to have great potential to make a significant contribution to reducing the gap, and the findings of this study provide some small support for the use of FACT as a therapeutic approach in this context.

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Appendices

Appendix A – Fidelity Self-Report

FACT fidelity measure (Version 2)

Date	
-------------	--

Initial visit		
Life context interview done	Yes	No
Problem context or functional analysis done	Yes	No
All visits		
Values important to the target problem (≥ 7) identified	Yes	No
Specific behaviour change plan identified	Yes	No
Confidence rating of behaviour change plan ≥ 7	Yes	No
Focus on awareness skills	Yes	No
Focus on openness skills	Yes	No
Focus on engagement skills	Yes	No

Nb: Version 1 did not include "Focus on engagement skills" question

Appendix B – Consent to contact



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Thank you for completing these forms.

We will ask you to fill in these forms every time you use this service. Your responses will enable us to focus on and clarify your concerns, come up with a plan and chart your progress.

If you have any questions, feel free to ask your clinician when you see them.

Thank you again

Focused Acceptance & Commitment Therapy (FACT) team

Would you like to be involved in the evaluation of this service?

We are asking everyone who has used this service if they would like to take part in a study evaluating it.

Please let us know by circling the response below if you are happy to be contacted about this in the future.

If you indicate you are happy to be contacted, we will pass your contact details on to our researcher who will be in touch with you about 6 weeks after your last visit to the service:

I would like to hear more about the study YES NO

If yes, please provide your contact details:

Name:

Phone Number:

Email:

Appendix C – Recruitment Phone Script

*** Confirm identify of participant*

Hi, my name is Alison Burfield. I am a Masters student with the School of Psychology department at Massey University.

Our records show that it has been about 5 weeks since you last met with one of our psychologists based at Xxxx Healthcare, as part of a new service being trialed. We are doing a study on the effectiveness of this new service and are inviting all people who have used it to consider taking part. You indicated that you were happy for us to contact you to tell you a little bit more about the research we are doing. Is now a good time for me to tell you about the study?

*** Yes or confirm alternative time to ring back*

*** Not interested – thanks for your*

time

The goal of this study is for us to find out whether the way the service was provided, i.e. having a few focused sessions with a psychologist, is helpful. The way that we will find out this information is by asking you to complete the same questionnaires that you did at the beginning of your appointments with the service, and then comparing the results.

Before you make any decisions about being involved in the research it is important for you to know that participation is entirely voluntary, and your choice will not affect any of the care you receive. All your information will be kept confidential, we won't access any of

ALISON BURFIELD [REDACTED]

your medical records and your psychologist and medical team won't know whether or not you have taken part in the research.

Does this sound like something you would be interested in taking part in?

*** Yes/Maybe*

*** No – thanks for your time*

Great. I will send you out some more information, a consent form, a copy of the questionnaires and a freepost envelope to return them to me. The information will also include the contact details if you have any questions.

Can I confirm your address?

Thanks for your time.

Appendix D – Recruitment emails

Initial email

Kia ora

My name is Alison Burfield. I am a Master's student with the School of Psychology department at Massey University.

Our records show that it has been about 5 weeks since you last met with one of our psychologists based at Xxxx Healthcare, as part of a new service being trialed. We are doing a study on the effectiveness of this new service and are inviting all people who have used it to consider taking part. You indicated at one of your appointments that you were happy for us to contact you to tell you a little bit more about the research we are doing.

The goal of this study is for us to find out whether the way the service was provided (i.e. having a few focused sessions with a psychologist) is helpful. The way that we will find out this information is by asking you to **complete the same questionnaires** that you did at the beginning of all of your appointments with the service.

Before you make any decisions about being involved in this research it is important for you to know that participation is entirely voluntary, and your choice will not affect any of the care you receive. All your information will be kept confidential, we won't access any of

ALISON BURFIELD [REDACTED]

your medical records and your psychologist and medical team won't know whether or not you have taken part in the research.

If this sounds like something you would be interested in taking part in please let me know your postal address and I will send you some more information, a consent form, a copy of the questionnaires and a freepost envelope to return them to me.

If you would like to discuss the research further please let me know your phone number and best time to contact you.

Kind regards

Alison Burfield

06 951 8074

This project has been reviewed and approved by the Massey University Human Ethics Committee: Southern A, Application 18/81. If you have any concerns about the conduct of this research, please contact Dr Lesley Batten, Chair, Massey University Human Ethics Committee: Southern A, telephone + 646 356 9099 x 85094, email humanethicsoutha@massey.ac.nz.

Follow-up email

Kia ora

My name is Alison Burfield. I am a Master's student with the School of Psychology department at Massey University.

I am just following up on my recent email/phone call inviting you to take part in our research on whether receiving a few focused sessions with a psychologist is helpful and effective.

The way that we will find out this information is by asking you to **complete the same questionnaires** that you did at the beginning of all of your appointments with the service.

Before you make any decisions about being involved in this research it is important for you to know that participation is entirely voluntary, and your choice will not affect any of the care you receive. All your information will be kept confidential, we won't access any of your medical records and your psychologist and medical team won't know whether or not you have taken part in the research.

If this sounds like something you would be interested in taking part in please let me know your postal address and I will send you some more information, a consent form, a copy of the questionnaires and a freepost envelope to return them to me.

ALISON BURFIELD [REDACTED]

If you would like to discuss the research further please let me know your phone number and best time to contact you.

Kind regards

Alison Burfield

06 951 8074

This project has been reviewed and approved by the Massey University Human Ethics Committee: Southern A, Application 18/81. If you have any concerns about the conduct of this research, please contact Dr Lesley Batten, Chair, Massey University Human Ethics Committee: Southern A, telephone + 646 356 9099 x 85094, email humanethicsoutha@massey.ac.nz

Appendix E – Study information sheet

Study Information Sheet

Evaluating the Focused Acceptance & Commitment Therapy (FACT) intervention; a new approach to psychological support in primary care

Who is doing the study?

Massey University Psychology Clinic and Xxxx Health Care are working on this project together. The study is being led by Dr Don Baken from Massey University who is a clinical psychologist and staff member of the School of Psychology. The primary researcher for this study is Alison Burfield who is a Master's student.

What is the study about?

Many people attend their health centre because they are worried about their health or they have stresses in their life that may be affecting their health. Seeing a psychologist for a short-focused session/s can help people to come up with their own plan to manage their worries in the longer term. To see if focused sessions are effective, we need to ask those who have used the service what they think about it. This will help others decide if this approach should be continued and extended further.

We are inviting all people who have used this new service to consider taking part in the study but involvement is entirely your choice and will not affect the care you receive.

What is involved?

If you decide to be involved:

- We will ask
 - your permission to access the scores from the questionnaires you completed when using the service.
 - you to fill in the same questionnaires that you did when you were seen by the service.
 - you to record your views about the service (if you want to)
 - you to consider filling in the same questionnaires in about 6 months time.

Please note: We will not have access to your patient notes.

How will my information be used?

- Your information will:
 - not be linked to information that can identify you.
 - be kept in a secure database and destroyed in 5 years time.
 - be combined with others who have completed the questionnaires.
- The researchers will compare the scores from when people are first seen by the service, when they are last seen and 6 months later to see if there are any changes.
- A summary of findings, which does not identify anyone, will be shared with people who make decisions about health services.

What are my rights?

You do not have to accept this invitation. If you decide to participate, you have the right to:

- decline to answer any question
- withdraw from the study up until the time that you return the survey
- ask any questions about the study at any time during participation
- provide information on the understanding that your name will not be used
- have a summary of the project findings

Remember that you can refer yourself back to the service if you feel that would be helpful to you. If you would like to be involved in the study, you can complete the questionnaires and send them to us in the freepost envelope provided.

If you have any questions feel free to contact

Don Baken

Email: d.m.baken@massey.ac.nz

Phone: 06 951 7975

This project has been reviewed and approved by the Massey University Human Ethics Committee: Southern A, Application 18/81. If you have any concerns about the conduct of this research, please contact Dr Lesley Batten, Chair, Massey University Human Ethics Committee: Southern A, telephone + 646 356 9099 x 85094, email humanethicsoutha@massey.ac.nz.

Appendix F – Study Consent Form

Investigating a new approach to psychological support

Consent Form

I am happy to participate in this study.

Yes

No

I am happy for the study to be given access to my scores from the measures I filled out while being seen by the service.

Yes

No

I would like to receive a summary of the findings when the study is completed.

Yes

No

We are interested in seeing what change there has been 6 months after people are seen by the service. Are you willing to be contacted again in 5 months?

Yes

No

Name: _____ Date: _____

Signature: _____

If you indicated that you would like a copy of the study summary please give your address for us to send the summary to. This will be separated from your answers as soon as we receive it.

Email address: _____

Postal Address: _____