Title	Knowledge translation interventions for facilitating evidence-informed decision-making amongst health policymakers
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Publication date	2022-10-13
Original Citation	Toomey, E., Wolfenden, L., Armstrong, R., Booth, D., Christensen, R., Byrne, M., Dobbins, M., Katikireddi, S. V., Lavis, J. N., Maguire, T., McHugh, S., Schmidt, B.-M., Mulholland, D., Smith, M., Devane, D. (2022) 'Knowledge translation interventions for facilitating evidence#informed decision# making amongst health policymakers', Cochrane Database of Systematic Reviews, 2022(10), CD009181 (19pp). https://doi.org/10.1002/14651858.CD009181.pub2
Type of publication	Article (peer-reviewed)
Link to publisher's version	https://doi.org/10.1002/14651858.CD009181.pub2
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Download date	2025-04-17 18:36:07
Item downloaded from	https://hdl.handle.net/10468/16918

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Knowledge translation interventions for facilitating evidence-informed decision-making amongst health policymakers (Protocol).
Cochrane Database of Systematic Reviews 2022, Issue 10. Art. No.: CD009181.
DOI: 10.1002/14651858.CD009181.pub2.

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Knowledge translation interventions for facilitating evidence-informed decision-making amongst health policymakers

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Editorial group: Cochrane Public Health Group.
Publication status and date: Amended to reflect a change in scope (see 'What's new'), published in Issue 10, 2022.

Citation: Toomey E, Wolfenden L, Armstrong R, Booth D, Christensen R, Byrne M, Dobbins M, Katikireddi SVittal, Lavis JN, Maguire T, McHugh S, Schmidt B-M, Mulholland D, Smith M, Devane D. Knowledge translation interventions for facilitating evidence-informed decision-making amongst health policymakers (Protocol). *Cochrane Database of Systematic Reviews* 2022, Issue 10. Art. No.: CD009181. DOI: 10.1002/14651858.CD009181.pub2.

ABSTRACT

Objectives

This is a protocol for a Cochrane Review (intervention). The objectives are as follows:

The aim of this review is to determine the effectiveness of knowledge translation interventions for facilitating evidence-informed decision-making amongst health policymakers.

BACKGROUND

Health policymaking is an important part of public health. For example, policy and legislative changes played a vital role in the top 10 public health achievements of the 20th and early 21st centuries (CDC 1999a; CDC 2011). However, health policymaking operates in a complex environment. Decision-making in health policy is influenced to varying degrees by research evidence, as well as by a range of other contextual factors existing at the individual (e.g. attitudes, beliefs), organisational (e.g. culture, finance), and external levels (e.g. political climate, interest-group pressures, public opinion) (Bowen 2005; Davies 2000; Dobrow 2004; Haynes 2018; Masood 2018; Oxman 2009; Partridge 2020). Although important considerations on their own, these factors are a suboptimal basis for health-related decisions that affect the lives of many, and research evidence has a central role to play (Sarkies 2017). However, failure to use high-quality research evidence in health decision-making can diminish the intended benefits of such decisions, reducing the efficiency and productivity of health services and the quality of life for patients and the public (Grimshaw 2012; McGlynn 2003; Schuster 2005; Yost 2014). In addition, there are several good examples of research informing public health policymaking with positive outcomes for patients and the public (Carden 2009; Qasba 2020). As such, the systematic use of high-quality, up-to-date research evidence by health policymakers is vital to make the best use of health resources; ensure the provision of high-quality, safe, and effective health services and programmes; and maximise the overall health outcomes and impact of research for patients, communities, and end-users.

Despite its importance, research evidence in health policymaking is suboptimal (Armstrong 2014; Campbell 2009; Lorenc 2014; Oliver 2017). Whilst it is difficult to explicitly define what is meant by the 'use' of research evidence, research use is commonly categorised as being instrumental, conceptual, symbolic, or imposed (Masood 2018; Weiss 1979; Weiss 2005). Instrumental use refers to the tangible and direct use of research evidence to realise change within a policy, programme, and/or clinical practice, whilst conceptual use involves indirect use of research evidence to improve knowledge and understanding of, or improve attitudes towards, a particular problem or topic. Symbolic use is also known as 'tactical', 'strategic', or 'political' use and refers to the use of research evidence to confirm or validate an existing policy or programme (Masood 2018; Petkovic 2018; Redman 2015). Imposed use refers to the use of research evidence to meet organisational, legislative, or funding requirements (Redman 2015; Weiss 2005).

Evidence-informed decision-making in health policy is a process whereby "multiple sources of information, including the best available research evidence, are consulted before deciding to plan, implement, and (where relevant) alter policies, programmes and other services" (Langer 2016, p 11). This definition posits that health policy decisions should be informed by a spectrum of types of evidence, including local contextual information and existing resources, as well as careful consideration of the best-available research evidence, rather than relying on singular sources (Ciliska 2012; Oxman 2009; Parkhurst 2016). In health policymaking, evidence-informed decision-making involves a non-linear, multidisciplinary process (Bowen 2005; Ciliska 2012; Fafard 2020; Yost 2014). For example, the Canadian National Collaborating Centre for Methods and Tools (NCCMT) outlines distinct stages of evidence-informed decision-making involving a clear definition of the question or problem, sourcing, appraising, and synthesising the best-available research evidence before interpreting and adapting the information to the relevant context (NCCMT 2021). The final stages involve considering whether and how to implement and evaluate the adapted evidence into policy.

Description of the intervention

Knowledge translation (KT) is a term often used in health research, policy, and practice settings to describe the activities and processes needed to facilitate evidence-informed decision-making and enhance the use of research evidence. The most commonly cited definition of KT, Azimi 2015, is "the exchange, synthesis and ethically sound application of knowledge - within a complex system of interactions among researchers and users - to accelerate the capture of the benefits of research for [citizens] through improved health, more effective services and products, and a strengthened health care system" (Canadian Institutes of Health Research p 1). Within this process, or series of processes, knowledge goes through an iterative pathway of exchange, synthesis, and application with the involvement of researchers and users of knowledge. With an ever-increasing research evidence base and an increasing focus on the use of research evidence in policymaking (Dyakova 2017; Kuchenmüller 2021; United Nations 2020), there has been a proliferation of KT interventions designed (by both researchers and health decision-makers) to facilitate evidence-informed decision-making amongst health policymakers.

This review will focus specifically on KT interventions designed to facilitate the use of research evidence by health policymakers, that is KT interventions that aim to optimise the meaningful consideration of research evidence alongside other forms of evidence in the decision-making process. KT interventions are "activities intended to increase KT at the level of practice, systems and policies" (Colquhoun 2014). The categorisation of KT interventions is complex given the multitude of approaches possible; however, previous research has suggested broadly categorising KT interventions aiming to increase research use as consisting of 'push', 'pull', and/or 'exchange' strategies (Lavis 2006). Push strategies are typically researcher-driven and involve supporting the dissemination of research (end-of-grant KT), for example the development and distribution of publications, reports, evidence summaries, or provision of access to materials and resources. Pull strategies are usually decision-maker driven, for example capacity-building and training for decision-makers to use and appraise research or employment of knowledge brokers (also referred to as facilitators and information specialists) within decision-making contexts. Exchange strategies are typically mutually driven, such as developing networks or partnerships, prioritisation efforts, deliberative dialogues, or integrated KT (i.e. a collaborative process involving the integration of knowledge users throughout the research process) (Canadian Institutes of Health Research). Exchange strategies can also include the use of knowledge brokers, where their role is to facilitate partnership development or knowledge translation and exchange (rather than to simply assist with making sense of research evidence for decision-makers as identified above). KT strategies can be further categorised in more specificity according to the Expert Recommendations for Implementing Change (ERIC) taxonomy, which is a validated taxonomy of KT strategies with associated definitions, for example 'Mandate change' which is defined as 'Have leadership declare the priority of the innovation and their

determination to have it implemented' (Powell 2015; Waltz 2015). A lack of consensus exists regarding the difference between the terms KT 'interventions' and KT 'strategies', therefore for the purposes of this review, we will conceptualise KT strategies as the individual methods or tools (e.g. training, incentives) and KT interventions as the overall 'package' which could be a single KT strategy or combination of multiple strategies. In this review, we will consider all categories of KT strategies, that is push, pull, or exchange.

How the intervention might work

The relationship between research evidence and its use within health policy is not straightforward. The role that research evidence plays in the decision-making process is often difficult to determine (Cairney 2016). Health policymaking is inherently political, and decision-making typically involves a broad range of policy actors and networks from different backgrounds with varying and potentially competing interests or opposing views on the value of research (Fafard 2020). Political science theories commonly highlight the complexity of policymaking because of the number and diversity of actors involved and the levels at which decision-making occurs (Smith 2013). Research evidence can influence which issues capture decision-makers' attention (agenda setting), which policy and programmatic options are considered, how they are characterised, and how a preferred option can best be implemented. Moreover, as previously outlined, research evidence can be 'used' in different ways, for example instrumentally, conceptually, or symbolically. As such, KT interventions to facilitate the use of research within health policymaking occur within a complex research-policy ecosystem. Systems-thinking approaches have recently become increasingly popular within public health literature (Chughtai 2017), and have also been specifically highlighted as a means of strengthening KT (Haynes 2020). This is because systems-thinking approaches acknowledge the dynamic, interconnected, and hard-to-predict nature of systems like the health research-policy ecosystem and offer perspectives, concepts, and tools that can be applied within KT practice and research to better understand and leverage change within this system (Haynes 2020; Kitson 2009).

Several factors at multiple levels influence the use of research evidence in health policymaking. These factors are similar to those influencing the general decision-making process within health policymaking. For example, previous systematic reviews of the barriers and enablers to the use of evidence by practice and policy decision-makers identified the influence of factors relating to the characteristics of the research itself (e.g. relevance, accessibility, credibility) as well as individual decision-maker characteristics (e.g. skills, knowledge, and attitudes), organisational and contextual characteristics (e.g. time, resources, training, cultural and political pressures), and social factors (e.g. collaborative partnerships and relationships between stakeholders, power imbalances in how knowledge is constructed and used), as well as factors specific to the research itself (e.g. accessibility, credibility, format/presentation, content/relevance, etc.) (Lavis 2005; Masood 2018; Oliver 2014; Orton 2011; Zhao 2020). However, whilst there has been a substantial exploration of the barriers and enablers associated with research use, and increased investment in doing KT to increase research use, the KT strategies used do not always target relevant barriers and enablers (Grimshaw 2012; Masood 2018). In 2017, Sarkies and colleagues identified a unidirectional process of four interrelated factors associated with effective KT interventions to promote evidence-informed decision-making in health care (Sarkies 2017). These factors were: (1) establishing an imperative for change, (2) building stakeholder trust, (3) developing a shared vision, and (4) actioning change mechanisms. This process was underpinned by (5) effective communication and (6) adequate resources to support change.

There are many theories, models, and frameworks (TMFs) within the field of KT that attempt to explain how KT interventions might facilitate the uptake of knowledge. A recent scoping review identified 36 existing KT TMFs for incorporating evidence into health practice and policy (Esmail 2020). However, only two TMFs specific to policy or organisational contexts were identified. Moreover, the vast majority of TMFs found were process models, that is a model which describes and/or guides the process of translating research into practice (Nilsen 2015), rather than seeking to explain or predict the causal mechanisms underpinning KT interventions aiming to increase research use in policymaking specifically. Given the complex political nature of health policymaking, it has also been recommended that general models of KT frameworks, such as those included in the scoping review by Esmail and colleagues (Esmail 2020), need to be supplemented with insights from political science and policy theory (Cairney 2016; Fafard 2020). Accordingly, Langer and colleagues conducted a systematic scoping review of reviews regarding the use of research evidence in decision-making more broadly, including decision-makers from multiple levels and settings such as health, education, political science, or social science backgrounds (Langer 2016). They identified six underlying mechanisms through which they hypothesised KT interventions aiming to enhance evidence-informed decision-making work, either individually or in combination, using 'push', 'pull', and/or 'exchange' strategies, as follows.

- **Awareness:** building awareness of and/or positive attitudes towards evidence-informed decision-making. For example, KT interventions leveraging this mechanism could involve 'push' strategies presenting information to policymakers on the cost-effectiveness of evidence use.
- **Agreement:** building mutual understanding and agreement on policy-relevant questions and the kind of evidence needed to answer them. KT interventions working via this mechanism could involve seeking policymaker feedback on the relevance of received evidence.
- **Communication and Access:** facilitating communication of and access to evidence. KT interventions targeting this mechanism could utilise 'push' strategies such as emailing policy briefs to policymakers or providing access to evidence repositories.
- **Interaction:** building interactions and collaborations between policymakers and researchers. KT interventions working via this mechanism could include 'exchange' strategies such as organising joint events, such as workshops or knowledge brokering.
- **Skills:** supporting decision-makers to develop skills in accessing, appraising, and interpreting evidence. KT interventions leveraging this mechanism could involve delivering critical appraisal skills training for policymakers.
- **Structure and Process:** influencing decision-making structures and processes such as environmental and social norms. KT interventions targeting this mechanism could include

restructuring organisational protocols or committees or using 'pull' strategies by providing incentives.

Langer and colleagues also emphasised that the end goal of 'enhanced use of research evidence by decision-makers' depends on behaviour change at individual and organisational levels, drawing on the well-known Behaviour Change Wheel model of behaviour change (Michie 2014). The Behaviour Change Wheel was developed based on a systematic search of behaviour change frameworks relevant to individual and policy-level change and posits that changing any behaviour involves satisfying three essential conditions: the capability, opportunity, and motivation to perform that behaviour (Michie 2014). The model also identifies nine intervention 'functions' (e.g. education, persuasion, modelling) most likely to be effective in changing behaviour by targeting the conditions of capability, opportunity, or motivation. In line with systems-thinking approaches for individual and organisational change, this allows for a focus on the core principles or underlying goals of an intervention rather than a rigid focus on the 'form' of individual KT strategies (Hawe 2015), which may be more relevant facilitating change within complex systems through a process of adaptive learning (Haynes 2020). Langer and colleagues outlined how the six underlying mechanisms influence one or more components of behaviour change (i.e. capability, opportunity, or motivation) to facilitate the final outcome of 'research use' (Langer 2016). Specific to the use of research evidence within a health policymaking setting, Redman and colleagues developed the SPIRIT Action Framework following a comprehensive review and interviews with policymakers to guide the development and testing of KT interventions in this area (Redman 2015). The SPIRIT framework is consistent with the Langer model, citing the importance of targeting behaviour change at the individual and organisation levels. Specifically, the SPIRIT framework hypothesises that an initial catalyst prompts the need or requirement for research use, the response to which is determined by organisational and individual capacity (i.e. values, tools/systems, knowledge and skills). These capacity elements align with the capability, opportunity, and motivation components described by Langer and colleagues (Langer 2016). Where there is sufficient capacity, the SPIRIT framework hypothesises that several behaviours in the form of 'research engagement actions' occur (e.g. accessing, appraising, commissioning research or interacting with researchers) that facilitate conceptual, instrumental, tactical, and/or imposed research use.

Why it is important to do this review

A number of existing evidence syntheses evaluate the effects of KT interventions on evidence-informed decision-making in health settings. However, several of these focus only on KT interventions for promoting the use of systematic reviews or systematic review products (e.g. summary of findings tables) (Conway 2017; Murthy 2012; Perrier 2011; Petkovic 2018), or specific KT interventions such as technology-enabled KT strategies, Brown 2020, or knowledge brokers (Bornbaum 2015). Others have focused on KT interventions targeting public health practitioners (LaRocca 2012), as opposed to policymakers specifically. Two relevant systematic reviews explored the effectiveness of implementation strategies for promoting evidence-informed policymaking and management decisions in health care (Sarkies 2017), and the use of research evidence in decision-making more broadly at multiple levels across multiple settings (Langer 2016). These reviews have identified considerable variation in the types of KT interventions used to enhance research use, with potential promise for active KT interventions that facilitate access to research evidence (e.g. tailored targeted messages), skills-based interventions (e.g. workshops), interventions that promote interaction between researchers and policymakers and/or changes to organisational infrastructure. Langer and colleagues also highlighted that the effectiveness of these interventions was often conditional on ensuring that decision-makers' capability, opportunity, and motivation were simultaneously addressed. However, the search strategies for these reviews included several restrictions, such as not searching for non-English language publications, Langer 2016; Sarkies 2017, or unpublished literature (Sarkies 2017), which may be particularly important for policy-focused interventions that may be reported in organisational repositories or elsewhere as opposed to being published in peer-reviewed journals. In addition, the most recent search across all relevant reviews was carried out in February 2016 (Sarkies 2017), with additional studies in the area conducted since this date, such as the SPIRIT trial (Williamson 2019). Moreover, given the heterogeneity of KT interventions typically included in reviews, this review will build on the work of Langer and colleagues in applying a behaviour change perspective to research use in policymaking by categorising intervention components using existing behavioural taxonomies and frameworks (Langer 2016). This will facilitate the future development of KT interventions that can better select intervention components to target known barriers and facilitators. This review will also draw on key political science perspectives to ensure it is fully cognisant of the complex political realities of health policymaking.

Although we recognise the inherent challenges and limitations of synthesising complex KT interventions given the diversity and potential heterogeneity we have described above, we believe there is a vital need for an up-to-date synthesis of the empirical evidence on the overall effectiveness of KT interventions for facilitating evidence adoption in health policy, drawing on both behavioural science and political science perspectives. Our subgroup analysis will also explore heterogeneity according to specific intervention subgroups using the Lavis push/pull/exchange framework (Lavis 2006). This will enable our synthesis to explore the effectiveness of KT interventions in more detail regarding what particular types of KT strategies may be most useful. However, given our focus on determining effectiveness, this review will be limited in terms of what it can tell us about this complex area, and it may be difficult to completely disentangle exactly how and why change may or may not have occurred. Systems-thinking approaches posit the need for varied research methods to explore the full impact of such interventions and influences of context (Haynes 2020). Our review is being conducted concurrently with a Cochrane qualitative evidence synthesis (protocol currently in submission) exploring health policymakers' views and experiences of knowledge translation approaches to facilitate evidence-informed decision-making, led by a co-author on this review (BS). The lead author of this review (ET) is also a co-author on the concurrent qualitative synthesis, which will facilitate the cross-linkage of insights across both reviews. Our review will draw on the qualitative evidence synthesis findings to help contextualise and provide a more holistic interpretation of our findings; taken together, both reviews aim to provide a more nuanced understanding of this topic.

The recent COVID-19 pandemic has shone an increased global spotlight on the importance of evidence-informed decision-making within a health policy context, with the role of KT to facilitate this positioned at the fore (El-Jardali 2020). Given an increasing focus on ensuring that health policymaking is informed by evidence, a fully comprehensive and exhaustive, up-to-date Cochrane Review is required to bring together the growing body of research to inform the selection and implementation of knowledge translation strategies in health policymaking contexts.

OBJECTIVES

The aim of this review is to determine the effectiveness of knowledge translation interventions for facilitating evidence-informed decision-making amongst health policymakers.

METHODS

Criteria for considering studies for this review

Types of studies

We will include randomised trials, cluster-randomised trials (CRCTs), non-randomised trials, controlled before-after studies (CBA) and/or interrupted time series studies (ITS) and repeated measures studies in this review as defined by the Cochrane Effective Practice and Organisation of Care (EPOC) group (Cochrane EPOC 2017a). We will include non-randomised designs, as interventions in policy settings are commonly implemented in single sites and are not randomised. To be eligible for inclusion, non-randomised trials, CRCTs, and CBA studies must have at least two intervention and two control sites. We will require that ITS studies have a clearly defined intervention point and at least three data points before and three after the intervention. Stepped-wedge randomised trials and hybrid trial designs will also be eligible for inclusion.

Studies in any income setting (e.g. high or low), and those involving government or non-government organisations, will be eligible for inclusion. We will not place any restrictions on the language of publication.

Types of participants

Participants eligible for inclusion are health policymakers. We will define a health policymaker as "someone employed in a health policy agency who drafts or writes health policy documents or develops health programmes, or who makes or contributes significantly to policy decisions about health services, programmes or resourcing" (Haynes 2014). This includes staff at different levels within health policy agencies who focus on population-level programme development and resourcing, but excludes contractors and staff who do not contribute to policy or programme development, such as administration and operations. We will define a health policy agency as "(a body within) a state or federal government department, or a statutory authority, whose focus is to develop policy which has an impact on state-wide or national services and programmes intended to improve individual, family or community health" (Haynes 2014). To be included, the agency must develop health policy or programmes as its core business. The focus of these policies and programmes must relate to 1) clinical programmes, services, and products, 2) public health programmes and services, and/or 3) health system arrangements (e.g. governance, financial as defined by Partridge and colleagues) (Partridge 2020). We will use the Centers for Disease Control and Prevention (CDC) broad definition of public health programme as any organised public health activity such as direct services, community mobilisation efforts, research and surveillance systems, policy development activities, outbreak investigations, laboratory diagnostics, or communication campaigns (CDC 1999b). Health agencies whose primary role is operational and not the development of policy (e.g. delivery of healthcare services) will be excluded. We will define policy as a "formal statement or action plan developed by a government agency or statutory body in response to an identified problem. This includes state-wide or national legislation, policies, programmes, directives, protocols, guidelines, and service models" (Haynes 2014). We will also expand the previous definitions to include health policymakers and policy agencies at a local government level for this review. In many contexts, local governmental authorities play a key role in public health policymaking (Fell 2020). We will exclude studies including policymakers with other professionals, such as healthcare professionals or clinicians making decisions about individual clients, unless it is possible to extract the data separately for health policymakers.

Types of interventions

KT interventions to be included in the review aim to facilitate the use of research evidence by health policymakers in the development of health policies or programmes, or both. As discussed above, this review will include KT interventions consisting of ‘push’, ‘pull’, and/or ‘exchange’ strategies, for example researcher-driven interventions (typically push), decision-maker driven interventions (typically pull), and interventions that represent meaningful partnerships between researchers and decision-makers (typically exchange). Interventions may comprise single strategies individually, such as educational workshops, knowledge brokers, tailored messaging, or evidence champions, or a combination of these strategies within a multicomponent intervention.

We will exclude interventions that do not explicitly aim to facilitate the use of research evidence by health policymakers, for example generic skill development initiatives or project evaluations that may include steering groups with membership drawn from the policy and research environments, but that lack an explicit focus on promoting the use of research evidence in policymaking specifically. We will also exclude implementation interventions targeting healthcare providers designed to facilitate the implementation of evidence-based interventions into practice (Barwick 2020), for example educational workshops for public health nurses to support the scale-up of evidence-based childhood obesity prevention interventions into routine health services or healthcare practice. This is because we are interested in identifying interventions focused on getting research into policy and programme development processes by targeting policymakers, as opposed to interventions focused on implementing research into practice by health practitioners based in service organisations, as it is likely that contextual characteristics of these settings will differ substantially (Haynes 2018).

We will make the following comparisons in the analysis.

- **Comparison 1:** any KT intervention versus usual practice, no intervention, delayed intervention, waitlist, or attention-only control, e.g. knowledge brokering compared to no intervention. This will be the primary comparison of interest in the review. We

will review the descriptions of usual practice provided within the included studies and subsequently determine whether it can be considered equivalent to 'no intervention' or whether it should be considered in comparison 2 below.

- **Comparison 2:** one KT strategy versus an alternative KT strategy, e.g. knowledge brokering versus targeted messages. In each study, the intervention considered 'most intensive' in the judgement of the review authors (e.g. more components, more exchange focused, longer) will be compared to the 'less intensive' intervention group, and all studies will be grouped for synthesis. For further investigation of intervention types, see Subgroup analysis and investigation of heterogeneity.

We will not exclude studies based on the type of comparator groups.

Types of outcome measures

Primary outcomes

The primary effectiveness outcome domain will be the use of research evidence (i.e. instrumental, conceptual, tactical, or imposed) as defined previously (Weiss 2005). Given the variability in existing definitions, we will take an inclusive approach to define research evidence as either "analyses of quantitative or qualitative data, or theory, found in peer-reviewed papers, technical monographs or books, or in grey literature such as internal studies and evaluations, and reports on authoritative websites" (Haynes 2014, p 153), or as Type 1 research ("describes risk disease relations, and identifies the magnitude, severity, and preventability of public health problems"), Type 2 research ("identifies the relative effectiveness of specific interventions aimed at addressing a problem"), or Type 3 research ("information on the design and implementation of an intervention; the contextual circumstances in which the intervention was implemented; and information on how the intervention was received") (Rychetnik 2004). This could include primary or secondary and descriptive, taxonomic, analytic, interpretive, explanatory, or evaluative study types.

Secondary outcomes

As guided by the SPIRIT Action Framework (Redman 2015), other major outcomes are as follows.

- Domain 1: Individual policymakers' research engagement actions, such as:
  - accessing research (e.g. individual studies or research platforms);
  - appraising research;
  - generating research;
  - interacting with researchers.
- Domain 2: Individual policymaker capacity to use research, such as:
  - individual values/attitudes/perceptions towards research use;
  - intentions to use research evidence;
  - knowledge/understanding/awareness (e.g. increased knowledge of best-available research evidence, or increased knowledge of how and why to use research evidence in decision-making);
  - skills (e.g. increased ability to locate, appraise, and/or interpret research evidence);
  - confidence/self-efficacy to undertake research actions and/or use research.
- Domain 3: Organisational capacity to use research, such as:
  - organisational values/research culture;
  - availability of systems and tools in place to support research use.

We will also include outcomes regarding the broader or indirect impacts of research use on health outcomes, such as outcomes at the public, individual or organisational level, including the following.

- Domain 4: Community/population-level health outcomes, e.g. health status, health service use, public trust in health policy decision-making
- Domain 5: Service, health system, or organisation-level outcomes, e.g. quality of public health services
- Domain 6: Economic outcomes, e.g. cost of the intervention

We will review the outcomes reported by the included studies and align the outcomes reported in each study as per the framework of domains and outcomes detailed above. We will use the definitions of these domains provided by the SPIRIT Action Framework to help provide further conceptual clarity (Redman 2015). We will synthesise and report at the outcome level. We anticipate considerable heterogeneity in outcome measurement. For example, a specific outcome may be measured objectively, such as via audit or document review (e.g. the instrumental use of research evidenced by referencing public health policy document) or subjectively via self-report (e.g. self-reported conceptual use of research to understand an issue). Where more than one relevant outcome measure is reported for the same outcome domain within a study, we will select outcome measures according to the following hierarchy.

1. Reported as the study's primary outcome
2. Used in a sample size calculation
3. Measured using a validated tool
4. Objectively measured rather than self-report

We will document all available outcomes in the 'Characteristics of included studies' table, with the selected outcome measure for each domain indicated.

In our description of included studies, we will describe the time periods of outcomes according to following timeframes.

- Short term: ≤ 6 months
- Medium term: greater than 6 months but less than 12 months
- Long term: ≥ 12 months

However, for analysis purposes we will analyse the data according to the final follow-up from all studies.

Adverse effects

We will extract, synthesise (if possible), and report data on any unintended, adverse or harmful effects as reported in the included primary studies.

Search methods for identification of studies

We will attempt to include all relevant studies, both peer-reviewed and grey literature, with no restrictions on language or date.

Electronic searches

We will search the following electronic databases: the Cochrane Central Register of Controlled Trials (CENTRAL), MEDLINE (Ovid), Embase (Ovid), CINAHL (EBSCO) (Cumulative Index to Nursing and Allied Health Literature), ProQuest Dissertation & Theses, Sociological Abstracts (ProQuest), Scopus, Applied Social Sciences Index and Abstracts (ProQuest), and Public Health Collection (NHS Evidence Portal) from inception to present. We will also search the Cochrane Effective Practice and Organisation of Care Group (EPOC) Specialised Register.

We will use filters that are currently in use, or that have been used, by the Cochrane EPOC group to find randomised controlled trial (RCT), controlled clinical trial (CCT), ITS, and CBA designs. We will use these filters to search for studies in combination with subject headings and free-text terms more specific to the topic area. Terminology and definitions in the field of KT are varied and inconsistent, and many terms have been used to describe KT or KT-related strategies (Colquhoun 2014). For example, McKibbon and colleagues identified 100 individual terms as being equivalent or closely related to KT (McKibbon 2010). Such terms include research utilisation, innovation diffusion, knowledge transfer, research dissemination, research implementation, research uptake, knowledge exchange, and mobilisation. Whilst there may be differences in the ways in which these terms are used (Barwick 2020), for the purposes of this review we will generally refer to these activities as KT, as defined by the Canadian Institutes of Health Research. It is therefore important that the search remains broad given that KT terminology is reasonably diverse. Search strategies from previously conducted relevant reviews and relevant EPOC intervention terms were also used to build the search strategy (Innvaer 2002; Lavis 2005; Mitton 2007; Murthy 2012).

The following MEDLINE search for knowledge translation and health policy terms will be used in conjunction with design filters above and modified for each database as necessary.

MEDLINE knowledge translation terms

1	(knowledge adj2 (action or adopt* or application or broke* or creation or diffus* or disseminat* or exchang* or integrat* or implement* or management or mobili* or shar* or translat* or transfer* or uptak* or utili*)).tw.
2	(evidence* adj2 (exchang* or translat* or transfer* or diffus* or disseminat* or implement* or management or mobil* or uptak* or utili*)).tw.
3	(KT adj2 (application or broke* or diffus* or disseminat* or decision* or exchang* or implement* or intervent* or mobili* or plan$ or policy or policies or strateg* or translat* or transfer* or uptak* or utili*)).tw.
4	(research* adj2 (diffus* or disseminat* or exchang* or transfer* or translation* or application or implement* or mobil* or transfer* or uptak* or utili*)).tw.
5	("research findings into action" or "research to action" or "research into action" or "evidence to action" or "evidence to practice" or "evidence into practice").tw.
6	("research utili*" and ("decision mak*" or decisionmak* or "policy mak*" or "policy decision*" or "health* polic*" or practice or action*)).tw.
7	((knowledge or evidence or research or practice) adj2 (gap* or barrier*)).tw.
8	Diffusion of Innovation/
9	(diffusion adj2 innovation).tw.
10	((evidence base* or evidence inform*) adj5 (decision* or plan* or policy or policies or practice or action*)).tw.
11	Knowledge management/
12	((research or knowledge or innovation* or evidence or information or policy) adj5 (brief* or summar* or structured summar* synops* or overview* or bulletin* or synthes* or map or mapping or

	maps or framing* or product* or package* or alert* or comment* or strateg* or algorithm* or decision-aid* or decisionaid*)).tw.
13	(("systematic review*" or "knowledge synthes*") adj5 ("decision mak*" or "policy mak*" or "policy decision*" or "health polic*")).tw.
14	(("systematic review*" or "knowledge synthes*") adj2 (application or implement* or utili?ation or utilize* or utilise* or utili?ing)).tw.
15	1 or 2 or 3 or 4 or 5 or 6 or 7 or 8 or 9 or 10 or 11 or 12 or 13 or 14

MEDLINE health policy terms

16	Health Planning/
17	Health Priorities/
18	Health Services Research/
19	Health Management/
20	(health* adj3 (plan* or priorit* or manage* or service*)).tw.
21	16 or 17 or 18 or 19 or 20
22	public policy/
23	Policy Making/
24	Decision Making, Organizational/ or Decision Making/
25	(decision* or policy or policies).tw.
26	(quality adj2 (assurance or improvement* initiativ* or "plan*" or "program*" or "review" or "audit")).tw.
27	qi.tw.
28	22 or 23 or 24 or 25 or 26 or 27
29	Health Policy/
30	(health* adj3 (policy or policies or planning or priorit* or "modus operandi" or statute or understanding* or law* or legislat* or directive* or ruling* or regulat* or rule* or plan* or protocol* or strateg* or "guiding principle*" or "course of action" or guideline* or procedure* or "decision mak*" or "budget hold*" or "service provi*" or procur* or purchas* or commission*)).tw.
31	29 or 30
32	(21 and 28) or 31

Searching other resources

Grey literature

We will undertake the following activities to identify grey literature using a combination of the keywords 'health policy*' and 'knowledge translation'.

- Run keyword searches in the preprint server Europe PMC (europepmc.org/Preprints).
- Run keyword searches in the grey literature databases BASE and MedNar.
- Search websites of key organisations (e.g. Sax Institute, Transforming-Evidence.org, National Collaborating Centre for Methods and Tools Knowledge Repositories, Centre for Reviews and Dissemination, Evidence for Policy and Practice Information and Coordinating Centre (EPPI-Centre), European Commission Evidence For Policy, SPOR Evidence Alliance, www.pdq-evidence.org/, EVIPNet).
- Web-based clinical trial registries (e.g. ClinicalTrials.gov (clinicaltrials.gov/), the World Health Organization International Clinical Trials Registry Platform (trialsearch.who.int/), and TRoPHI (Trials Register of Promoting Health Interventions)).

We will treat executive summaries or overviews as similar to an abstract. Where neither abstract nor summary/overview exists, we will screen by title only at the title/abstract screening stage, and send potentially relevant texts forward to full-text screening.

Reference lists

We will review the reference lists and citations of included studies and relevant systematic reviews, Langer 2016; Sarkies 2017, for any potentially relevant studies.

Correspondence

We will make contact with experts in the field of KT and evidence-informed decision-making in health policy (as identified by author team) and authors of included studies to supplement our documented search strategy and identify any additional ongoing or completed work.

Data collection and analysis

Selection of studies

We will determine study eligibility based on the inclusion and exclusion criteria listed above. At least two members of the research team will independently screen each citation by title/abstract and assess as included, excluded, or unclear. We will obtain full-text copies of all included or unclear references, and two members of the research team will independently screen these for inclusion in the review. We will use Covidence software to manage screening (Covidence). Any disagreements or discrepancies will be resolved via discussion, with a third member of the research team consulted if necessary. All studies excluded at the full-text stage will be listed in the references section, with reasons for their exclusion documented. Studies which were found to satisfy many but not all of the inclusion criteria at the full-text screening stage will also be listed in the 'Characteristics of excluded studies' table with the reasons for their exclusion.

Data extraction and management

Two review authors will independently extract data. Data extraction forms will be modelled on the Cochrane Public Health and Cochrane EPOC data extraction forms, and the following data extracted.

- Study characteristics: first author, publication year, country, study design, sample size, funding source.
- Type of policymaker targeted (level of government, jurisdiction or catchment area, type of organisation/political context).
- Area of health specialty.
- Category of use of research evidence considered (i.e. instrumental, conceptual, tactical, or imposed) (Weiss 2005).
- Theoretical underpinning of the intervention.
- Intervention characteristics (number of components, mode of delivery, format, duration, etc.) extracted using the Template for Intervention Description and Replication checklist, adapted for the reporting of population health and policy interventions (TIDieR-PHP) (Campbell 2018). Intervention components will be coded broadly as push, pull, and/or exchange, and also coded more specifically according to the Behaviour Change Wheel definitions of intervention functions, Michie 2014, and the ERIC taxonomy of implementation strategies (Powell 2015).
- Cost of the intervention.
- Unintended adverse effects of the intervention.
- Conflict of interest statements.

Two members of the review team will independently extract data onto a custom-made a priori template. Any disagreements will be resolved via discussion or by consultation with a third member of the review if necessary. Once finalised, data will be entered into Review Manager Web by one review author and checked by a second review author (RevMan Web 2022).

Where authors of this review are also authors of potentially eligible studies, such authors will not be involved in decisions about eligibility, data collection, risk of bias assessment, or GRADE assessment of those studies.

Assessment of risk of bias in included studies

We will assess risk of bias for the effect of assignment to the intervention (i.e. on an intention-to-treat basis) using the criteria outlined in the *Cochrane Handbook for Systematic Reviews of Interventions,* Higgins 2019, and the guidance from Cochrane EPOC (Cochrane EPOC 2017b; Cochrane EPOC 2017c). Two review authors will independently perform the risk of bias assessment, with resolution of any discrepancies in risk of bias ratings occurring firstly through discussion between review authors, or through consultation with an additional review team member if necessary. We will complete a risk of bias table, with a justification for the judgement and source of information for each judgement (e.g. quotation from each study), and present it in the published review. For randomised trials, we will use the Cochrane Risk of Bias 2 (RoB 2) tool to assess trials for the primary outcome of research use as well as the other major outcomes of research engagement actions, individual policymaker capacity to use research evidence, and organisational capacity to use research evidence according to the following domains.

- Bias arising from the randomisation process

- Bias due to deviations from intended interventions
- Bias due to missing outcome data
- Bias in measurement of the outcome
- Bias in selection of the reported result

For CRCTs, we will use the ROB 2 variant for CRCTs and the special considerations discussed in Chapter 23 of the *Cochrane Handbook* (Higgins 2019). We will classify judgements about the risk of bias arising from each domain of the tool as 'low', 'high', or 'some concerns' (Sterne 2019). For assessing risk of bias within stepped-wedge designs, we will use the RoB 2 tool (as planned) plus the additional domain for CRCTs as recommended by the cluster trial supplementary RoB 2 guidance to account for potential identification/recruitment bias (Eldridge 2021). An additional source of bias particular to the analysis of stepped-wedge designs, that is analysis without adjustment for secular or temporal trends, is also addressed in this additional domain within signalling question 2.6. Within the analysis, the treatment effect will need to be time adjusted, and standard error to come from a model that has allowed for clustering (e.g. by applying multilevel with random effects for individual clusters). If studies have not accounted for clustering, we will apply adjustments for clustering using the methods described in the *Cochrane Handbook* to overcome this problem (Higgins 2019). In such cases, we will extract the outcome data (as if naively ignoring the cluster design) for the total number of individuals (e.g. the proportion of individuals with events, and means and standard deviations), whilst carefully collecting the number of clusters randomised to each intervention group and the average size of clusters and estimate an intracluster correlation coefficient (ICC).

For hybrid trial designs (as hybrid trials can use any type of randomised trial design) (Wolfenden 2021), we will use RoB 2 with additional supplementary domains as required. For example, for a hybrid trial with randomisation at the cluster level, we will use the additional RoB 2 cluster trial domain as outlined previously.

For both RCTs and CRCTs, we will use the signalling questions within RoB 2 to determine the overall risk of bias for the specific result being assessed. For example, the overall judgement is high risk of bias if the study is assessed to be at high risk of bias in at least one domain or some concerns for multiple domains; low risk of bias if it is assessed as low risk in all domains for this result; and some concerns if judged to have some concerns for at least one domain, but not at high risk for any domain.

For non-randomised, CBA studies, we will assess the risk of bias for the primary outcome of research use as well as the other major outcomes of research engagement actions, individual policymaker capacity to use research evidence, and organisational capacity to use research evidence according to the following domains as outlined by Cochrane EPOC (Cochrane EPOC 2017b).

- Random sequence generation
- Allocation concealment
- Similarity of baseline outcome measurements and baseline characteristics
- Incomplete outcome data
- Knowledge of the allocated interventions
- Protection against contamination
- Selective outcome reporting
- Other risks of bias

For ITS studies, we will assess the risk of bias for the primary and other major outcomes as described previously according to the following domains as outlined by Cochrane EPOC (Cochrane EPOC 2017a).

- Intervention independent of other changes
- Shape of the intervention effect prespecified
- Intervention unlikely to affect data collection
- Knowledge of the allocated interventions
- Incomplete outcome data
- Selective outcome reporting
- Other risks of bias

For assessments conducted using the Cochrane EPOC tool, we will classify judgements about the risk of bias arising from each of these domains as 'low', 'high', or 'unclear'. We will determine an overall risk of bias assessment for an outcome within each study (i.e. across domains) using the approach detailed in the Cochrane EPOC summary assessment of risk of bias guidance (Cochrane EPOC 2017c). For example, the overall judgement is high risk of bias if there is high risk of bias for one or more of the evaluated domains, and it is determined that this bias has seriously weakened confidence in the results; unclear risk if there is unclear risk of bias for one or more domains and the bias raises some doubts about the results; and low risk of bias if there is low risk of bias for all domains and it is determined that bias is unlikely to seriously alter the results.

Measures of treatment effect

The outcome measures will either be dichotomous extracted as number of events (e.g. number of users of research evidence) out of the total observed (N), or as continuous extracted as the observed mean (or median) and standard deviation (or estimated from any reported dispersion measure). For ITS studies, we will record changes in the level and slope. If these studies do not provide an appropriate analysis or reporting of results but provide the data points in a scannable graph or table, we will reanalyse the data using a time series regression model as outlined in Cochrane EPOC guidance (Cochrane EPOC 2017d).

Dichotomous data

For dichotomous data, we will initially present results using risk ratios (RR) with 95% confidence intervals (CI). If we include any cluster trials, we will adjust the precision (e.g. standard error) for the outcome data to take account of possible design effects (see Unit of analysis issues).

Continuous data

For continuous data, we will initially present the standardised mean differences (SMD) with 95% CI, enabling us to handle multiple studies evaluating the same domain but measuring it with different methods (e.g. different measurement scales). We will use differences at follow-up, controlling for baseline, as our main effect estimate as recommended by Cochrane. Where studies provide follow-up data that have not controlled for baseline, we will contact the study authors for this information. If this is not available, we will use the available follow-up scores and report this. If authors report

data for change from baseline only, we will calculate the difference at follow-up using the data provided in the included studies.

Unit of analysis issues

In some cases where studies randomise or allocate clusters but do not account for clustering in their analysis, a potential unit of analysis error may occur. For CRCTs, we will adjust the sample sizes using the methods described in the *Cochrane Handbook for Systematic Reviews of Interventions* (Higgins 2019), based on the ICC or potentially derived from the trial (if possible). ICCs may appear small compared with other types of correlations: values lower than 0.05 are typical (Higgins 2019). In general, the larger the cluster sizes the smaller the ICCs. By using these estimates, we will adjust (i.e. reduce) the size of each trial to its 'effective sample size'. The effective sample size of a single intervention group (N) in a CRCT is its original sample size divided by the design effect 'DE':

$$DE \approx [1 + (M - 1) \times ICC]$$

where M is the average cluster size. A common DE and ICC will be assumed across groups. The adjusted sample sizes (n) will thus be estimated as:

$$n_I = N_I / [1 + (M - 1) \times ICC]$$

$$n_C = N_C / [1 + (M - 1) \times ICC].$$

If we are unable to access an ICC value for the individual CRCT, and it is not possible to use estimates from similar studies, we will assume a conservative ICC value to be 0.05 (Higgins 2019), and also a sensitivity analysis based on high (0.2) and low ICC values (0.01). We do not anticipate any special analysis requirements with hybrid designs. Treatment effect and standard error data will be calculated on the data extracted for the relevant outcomes as with all other included studies.

Dealing with missing data

We will carry out analyses, to the greatest degree possible, on an intention-to-treat basis, that is we will include all participants as randomised to each group in the analyses, and all participants will be analysed in the groups to which they were allocated, regardless of whether or not they received the allocated intervention.

We will attempt to contact the lead authors of primary studies through email to locate missing data. All missing outcome data for included studies will be captured on the data extraction form and reported in the risk of bias table.

Assessment of heterogeneity

We will assess heterogeneity through a visual assessment using forest plots and logic-based assessment of study differences (i.e. based on the PICO framework). We will also narratively present study characteristics in the results, and describe heterogeneity on these characteristics. When combined analyses are undertaken, we will use the standard Cochran Q-test (measuring heterogeneity) and evaluate the apparent heterogeneity via the I^2 inconsistency index, which can be interpreted as the "percentage of variability in effect estimates that is due to heterogeneity rather than chance" (Higgins 2019, p 259). We will interpret the I^2 thresholds as outlined by the *Cochrane Handbook* (Higgins 2019, p 259), as follows:

- 0% to 40% might not be important;
- 30% to 60% may represent moderate heterogeneity;
- 50% to 90% may represent substantial heterogeneity; and
- 75% to 100% indicates considerable heterogeneity.

However, these thresholds can be misleading, as the importance of the I^2 value depends on the magnitude and direction of effects and strength of evidence for heterogeneity (e.g. I^2 confidence intervals), and when the number of studies is small, certainty in the I^2 value is less assured. As such, we will interpret this finding with caution.

Assessment of reporting biases

Our comprehensive search strategy will help ensure all eligible studies are identified. Nonetheless, we will explore potential publication bias by comparing published reports with trial protocols or registers. Formal statistical methods for assessing publication bias may not be appropriate given heterogeneity in the included study designs. However, if more than 10 studies are identified and included in quantitative synthesis, we will explore publication bias using funnel plots and visual assessment of funnel plot asymmetry. These plots will help to assess the relationship between effect size and study precision. If asymmetry is suggested by a visual assessment, we will perform exploratory analyses as recommended by Sterne and colleagues to investigate whether the association between estimated intervention effects and study size is greater than that expected by chance (Sterne 2011).

Data synthesis

Even for eligible studies, following the prespecified PICO questions, heterogeneity should be anticipated since there will be variations in interventions, comparators, and populations. We will therefore use a random-effects meta-analysis for combining outcome data where sufficient data are available for a meta-analysis. We will apply the guidance outlined in Chapter 12 of the *Cochrane Handbook* regarding scenarios that may preclude meta-analysis to determine what is deemed 'sufficient', for example where there are major concerns about missing outcomes within the studies or bias in the evidence, or serious clinical heterogeneity violating the protocolised PICO framework (Higgins 2019, p 323). When intervention effects are incompletely reported (e.g. effect estimate with no measure of precision), we will calculate the effect estimate and measure of precision from the available statistics where possible. We will explore the available data thoroughly for any P value metric and a sample size that can be converted into an effect size (e.g. SMD or OR). We will synthesise outcome data by comparison and by study design, that is we will combine estimates for each design stratum and not combine data across different types of design. Where conflicting results from analysis of the same outcomes occurs, we will prioritise the synthesised findings from randomised trial designs. We will analyse data for the final follow-up period from all studies. For ITS studies, if possible, changes in level and changes in slopes will be combined using the generic inverse variance method. If re-analysis is not possible, and the ITS study has ignored trend changes (e.g. performing a simple t-test of the pre- versus postintervention periods without further justification), we will exclude the study from the analysis. For multi-arm studies, only intervention groups that meet the criteria for including studies in the review will be included in the analysis. We will describe all intervention and comparator groups in the 'Characteristics of included studies' table. If multiple intervention groups from one study are eligible, we will follow the approach to including these data in a meta-analysis recommended by the

Cochrane Handbook, that is to combine all relevant intervention groups into a single group, and similarly for multiple relevant comparator groups, where this is reasonable (Higgins 2019). Where this is not reasonable, we will include each pair-wise comparison separately, but divide shared intervention groups evenly amongst the comparisons.

Where meta-analysis is not possible, and we cannot calculate effect estimates or measures of precision, we will use a narrative synthesis ('synthesis without meta-analysis') approach as informed by guidance from the Cochrane Consumers and Communications Group (Ryan 2019). Specifically, we will group the data based on the comparison and outcome domain. Within each category, we will visually present the data in tabular format, and narratively describe the results, as grouped by outcome. Where possible, we will use descriptive statistics (median and interquartile ranges), followed by vote-counting based on direction of effect (Hilton Boon 2021). We will summarise and synthesise randomised and non-randomised study designs separately, and subsequently compare results of these study designs, highlighting any similarities or differences in the review findings. We will explore any potential heterogeneity that might be due to differences in study design. We will also explore potential associations between study outcomes and intervention functions by narratively describing effects. The narrative synthesis approach used will be reported according to the Synthesis Without Meta-analysis (SWiM) guidance (Campbell 2020).

Where we have major concerns regarding bias, missing outcomes, or serious clinical heterogeneity, and where vote-counting or other quantitative synthesis method is inappropriate, we will conduct a structured summary where individual study results are summarised and described narratively, grouped by outcome and by separate study design as outlined previously. In such a situation, we will apply the GRADE approach as outlined in guidance by Murad and colleagues (Murad 2017). This approach "leverages the meaning of the constructs that represent GRADE domains to produce judgements on how these constructs affect our certainty" (Murad 2017, p 85).

Subgroup analysis and investigation of heterogeneity

If possible, we will conduct subgroup analyses to explore heterogeneity according to the following a priori subgroups: researcher-driven 'push' strategies versus decision-maker driven 'pull' strategies versus 'exchange' strategies which represent meaningful partnerships between the researchers and decision-makers (i.e. focus of KT strategy). We will use the previously described definitions and the examples provided by Lavis and colleagues to help categorise strategies (Lavis 2006). If studies include strategies that have been assigned to more than one category, we will allocate all studies into a single category based on where the majority of KT strategies lie. We will conduct subgroup analyses for the primary outcome only. We will investigate interaction effects using the test for subgroup differences in Review Manager Web (RevMan Web 2022), which also reports an I^2 statistic. A minimum of two studies will be required to conduct the pre-planned stratified analysis.

Sensitivity analysis

We will conduct sensitivity analyses to investigate how the intervention effect is affected by risk of bias of the included studies. We will do this by repeating the analysis of primary outcomes retaining studies at overall low risk of bias as judged by the Cochrane RoB 2 and EPOC tools. We will also test the robustness of our findings from CRCTs using different assumed ICC values of a high (0.1) and low ICC value (0.00 as an extreme assumption of no correlation) instead of the proposed ICC = 0.05 used as the default if missing from the trials.

Summary of findings and assessment of the certainty of the evidence

We will include a summary of the results of the data synthesis and assessment of the certainty of the evidence in a summary of findings table for the main comparison, generated using GRADEpro GDT (GRADEpro GDT). We will report the outcomes in the summary of findings table at the final follow-up period as specified in the included studies. Using the GRADE approach, two review authors will independently assess the certainty of the body of evidence for the primary outcome (research use), other major outcomes (research engagement actions, individual policymaker capacity to use research, organisational capacity to use research), and adverse events. An additional review team member will be consulted to resolve discrepancies if necessary. We will assess the certainty of the evidence for each of these outcomes based on the five GRADE considerations: the risk of bias in the included studies, directness of the evidence, consistency of effect, precision of the effect estimates, and the risk of publication bias. We will use the overall risk of bias judgement in the GRADE assessments. For non-randomised studies, we will also consider dose-response relationships, the absence of all plausible confounders, and the magnitude of the effect. We will assess the certainty of the body of evidence as high, moderate, low, or very low. Randomised trials will start at high certainty and may be downgraded by one level for each of the five GRADE considerations where 'serious' concerns are identified, or by two levels where 'very serious' concerns are identified, up to a maximum of three levels for all domains. Guidance from Chapter 14 of the *Cochrane Handbook*, Higgins 2019, and the Cochrane Consumers and Communication Group, Ryan 2016, will help determine what will be considered serious or very serious concerns. For example, in grading for risk of bias, we will downgrade the certainty of evidence for an outcome if the proportion of information from studies at high risk of bias is sufficient to affect the interpretation of results. The decision to downgrade by one or two levels will depend on the extent to which the bias is likely to seriously (downgrade by at least one level) or very seriously (downgrade by two levels) alter the results. For inconsistency, we will visually inspect forest plots for overlap of confidence intervals of effect estimates and use an I^2 threshold of 50% or more to indicate potentially substantial heterogeneity and consider downgrading by one level, and values of 75% to 100% to indicate potentially substantial heterogeneity and consider downgrading by two levels.

Non-randomised trials will start at low certainty and may be downgraded similarly, but may also be upgraded if there is evidence of large estimated effects (e.g. RR > 2 or RR < 0.5) in the absence of plausible confounders. The presence of a dose-response relationship may also facilitate upgrading by one level. All decisions to downgrade or upgrade will be justified and documented using footnotes.

Ensuring relevance to end-users

In order to ensure that the review findings are as meaningful as possible to all relevant end-users and that the overall impact of enhancing research use in health policymaking is considered throughout, we will involve different stakeholders in the review. At least one patient/public contributor and one policymaking stakeholder will be involved throughout the review process as members of the review author team.

ACKNOWLEDGEMENTS

The authors would like to acknowledge the contributions of Elizabeth Waters and Mark Petticrew on a previous version of this protocol (Armstrong 2011).

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WHAT'S NEW

Date	Event	Description
13 October 2022	New citation required and major changes	Protocol updated to reflect changes in Cochrane methods

HISTORY

Protocol first published: Issue 6, 2011

Date	Event	Description
2 September 2022	Amended	Protocol amended to reflect changes in Cochrane methods.

CONTRIBUTIONS OF AUTHORS

ET led the design, development, and writing of the protocol.

LW provided key content and methods expertise and was involved in the protocol design and writing.

RA conceived the original protocol and was involved in the protocol design.

MB provided key content expertise and was involved in the protocol design and writing.

DB was involved in developing and running the search strategies.

RC provided statistical guidance and assisted in writing the protocol.

SVK supported the development of the protocol design and was involved in writing the protocol.

MD supported the development of the protocol design and was involved in writing the protocol.

JL was involved in the design of the protocol and provided an overarching policy perspective. JL assisted in writing the protocol.

TM was involved in the design of the protocol and provided an overarching policy perspective. DM assisted in writing the protocol.

DM was involved in the design of the protocol and provided an overarching policy perspective. DM assisted in writing the protocol.

SMH supported the development of the protocol design and was involved in writing the protocol.

BS provided key content expertise and was involved in the protocol design and writing.

MS provided an overarching patient and public perspective and was involved in writing the protocol.

DD provided key content and methods expertise and was involved in the protocol design and writing.

DECLARATIONS OF INTEREST

ET declares no conflict of interest.

LW declares that his institution has received research grants to undertake trials likely to be included in the review. He is also Coordinating Editor of Cochrane Public Health; however, he was not involved in any stage of the editorial management or assessment of this protocol. No other interests declared.

RA has been involved in research that may be eligible for inclusion in this review. No other interests declared.

MB declares no conflict of interest.

DB declares no conflict of interest.

RC declares no conflict of interest.

SVK declares no conflict of interest.

MD has been involved in research that may be eligible for inclusion in this review. No other interests declared.

JL has been involved in research that may be eligible for inclusion in this review. No other interests declared.

TM declares that previous and current work with the Ministry of Health in Ireland and a public research funder means that she has experience and views on evidence-informed decision-making, knowledge brokering, question formulation and commissioning of evidence reviews, and knowledge translation. She declares no other conflict of interest.

DM declares no conflict of interest.

SMH declares she is a member of the Health Service Executive National Working Group on Knowledge Translation. No other conflicts of interest.

BS declares no conflict of interest.

MS declares no conflict of interest.

DD declares no conflict of interest.

SOURCES OF SUPPORT

Internal sources

- No sources of support provided

External sources

- Health Research Board Applying Research into Policy and Practice Fellowship Awards 2020, Ireland

  ET is supported by a Health Research Board Applying Research into Policy and Practice Fellowship Award.