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## A philosophical account of interventions and causal representation in nursing research: a discussion paper

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### **Abstract:**

*Background:* Representing is about theories and theory formation. Philosophy of science has a long-standing interest in representing. At least since Ian Hacking's modern classic *Representing and Intervening* (1983) analytical philosophers have struggled to combine that interest with a study of the roles of intervention studies. With few exceptions this focus of philosophy of science has been on physics and other natural sciences. In particular, there have been few attempts to analyse the use of the notion of intervention in other disciplines where intervention studies are important, such as in nursing research. One unintended consequence of this is that the relations between representing and intervening tend to be less understood outside the natural sciences.

*Objectives and design:* This article highlights a number of possible topics on which nursing science and analytic philosophy of science can fruitfully interact. The basic idea is simple. Building on a characterisation of interventions in terms of (i) what is intervened on and (ii) with respect to what, we suggest that interventions in nursing research typically are a blend of varieties belonging to the three dimensions of agency, epistemology, and ontology. The details of the blend determine the relation of the particular intervention study to traditional representational categories such as inductivism and hypothetico-deductive method, and have a bearing on its explanatory power and other more theory independent features of research as well. The framework we suggest should be relevant for nurse researchers who want to adopt a more general and analytically entrenched perspective on representing and intervening than the methodological boundaries in nursing research typically allow.

**Key words:** Intervention, Theory, Philosophy of science, Induction, Hypothetico-deductive method, Explanation

### **What is already known about this topic?**

- There is an emerging philosophical discussion on the relations between *ideal* interventions and causal representations, i.e. causal hypotheses and theories, but this discussion is so far seldom related to in discussions within nursing research. Furthermore, there is a considerable body of methodological insights on how causal inferences depend on design in real intervention studies. Campbell and Stanley (1963) is a representative example.

### **What this paper adds**

- A new conceptual framework and ample illustrations of how real A-, E-, and O-interventions (belonging to the dimensions of agency, epistemology, and ontology) relate to matters of representing, thereby facilitating the exchange of theoretical and methodological insights between nursing research and philosophy of science. A short introduction to A-, E-, and O-interventions is in place here. We often talk about interventions precisely when we intend to achieve something. Since intention reflects agency we refer to interventions with intended effects as A-interventions. E-interventions capture another common feature of interventions—what we assume or examine about them. To be assumed or examined has to do with the dimension of knowledge or belief—or, as philosophers use to say, with epistemology. O-interventions, finally, are interventions with actual causes and effects, regardless of whether they are A- or E-interventions. To be actual is a matter of ontology, and hence we label them O-interventions.

*Representing* has been a focus of philosophy of science for a long time. Topics of representation such as theory formation and truth have been on the philosophical agenda for millennia. Representing is considered the primary scientific aim in many disciplines.

*Intervening* is sometimes presented as a separate aim of science. Scientists can be interested in changing the world rather than finding out truths, and *vice versa*. Intervention can

nevertheless be used to further aims of representing. This was already part of the philosopher Francis Bacon's (1561-1626) view of the way in which scientific knowledge is acquired. It is not enough to observe, he claimed; we must also "twist the lion's tail" to learn its secrets. Hacking (1983) elaborates Bacon's legacy, and many books in experimental design take it for granted that although "passive observation" (Shadish et al., 2002: 2) can teach us some things, active manipulation is often required to find causal truths. Intervention is also becoming increasingly important to philosophers of science in understanding what explanation is. Scientists themselves appear to have known that for a long time. Weinberg (1985), for one, links the explanatory power of molecular biology to its ability to manipulate and control. The example is presented and discussed in Woodward (2003: 9), and his book *Making things happen* develops a perspective on the relation between causal explanation and the *ideal* intervention.

In contrast, this article approaches the relationship between representing and intervening from a different, less ideal and more realistic, angle. The reason for this is that causal words, like "intervention", are used in such different contexts that we sometimes have reason to disambiguate them first. Only then can we examine their relations to phenomena of interest in a systematic way. "Cause" itself is perhaps the best example. It can occur almost anywhere. We use it as frequently in our daily descriptions of life as we do when we are reporting the outcomes of randomised controlled trials in various disciplines. Whether there is one or many concepts of cause associated with these different uses has been a philosophical issue for a long time. The opinion now in vogue is that there are several concepts of cause; see, for example, Hall (2004) and Cartwright (2004). These different concepts will very likely differ with regard to aims of representing. "Intervention" is, as far as we can tell, a causal term of similar character and importance. Since it has decidedly a more technical ring to it than "cause" has, its use does not, perhaps, travel across as many disciplinary boundaries. However, *within* different scientific contexts "intervention" is used in different ways. Hence, in order to say something relevant about how intervention and representing interact in real science, whether or not this is recognized by the scientists themselves, we need to take a closer look at what in fact goes on in a few intervention studies.

A few illuminating examples from intervention studies involving nurses, targeting nurses or carried out by nurses and published in this journal of nursing studies are provided in the next section. Of course, there is always a risk involved in providing illustrations—perhaps not for

the philosopher, who is often searching for interesting possibilities, but for the scientist who wants the facts of the matter. The philosopher's appeal to experience may sometimes appear to degenerate into "reasoning supported by casual examples" (Drake, 1981: xxi). Similarly, the philosopher's habit of sometimes using invented cases is often looked upon with suspicion: "to use obviously invented or constructed cases to build nursing knowledge is [...] quite dubious" (Weaver and Mitcham, 2008: 191).

There should be little concern that we are investigating mere possibilities in this case. That one and the same word is associated with several concepts is the rule rather than the exception, especially if the word in question is important and enjoys scientific or political use. To demonstrate the existence of multiple meanings of "intervention" would almost be too easy. It would also be of limited value. What might be much more fruitful is the idea that the dimensions along which different senses of "intervention" overlap and differ tell us something worth knowing about the nature of intervention, and about the strengths and limitations of the relation between intervention studies and scientific aims of representing. This idea informs the present article.

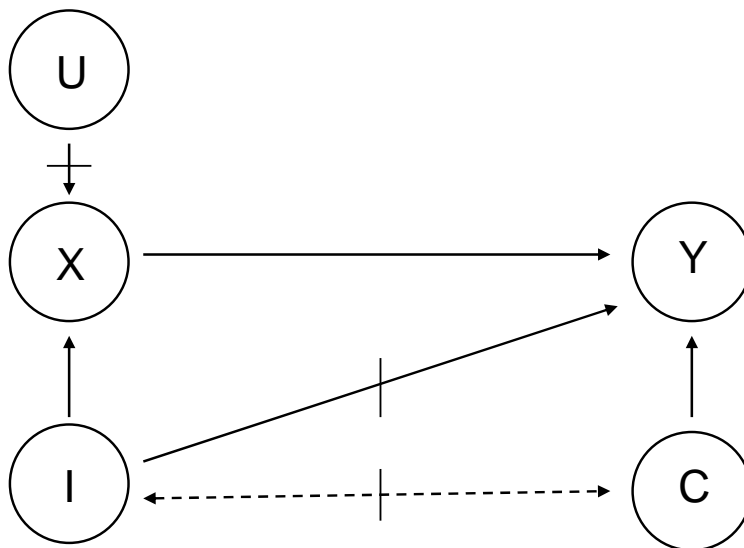
## ***1. The Concept of Intervention and Three Examples***

Interventions often take place in a scientific study of some sort. For example, randomised controlled trials are sometimes defined in terms of the three key features: intervention, control, and randomisation. More generally, Shadish et al. (2002: 507) define the term "experiment" as the attempt "to explore the effects of manipulating a variable." While marking the essential role of interventions in experimentation, such definitions clearly allow intervention to take place outside the scope of experiments as well. Routine interventions in healthcare, for instance, are not ruled out by this or similar definitions.

One of the major works on intervention in philosophy of science, Woodward (2003), also approaches interventions in a way that allows interventions to occur without a control group and/or without randomisation. In his case, however, a number of other constraints on the precision of the intervention limit the applicability of the concept to ideal cases:

The idea we want to capture is roughly this: an intervention on some variable X with respect to some second variable Y is a causal process that changes the value of X in an appropriately exogenous way, so that if a change in the value of Y occurs, it occurs only in virtue of the change in the value of X and not through some other route. (Woodward, 2003: 94)

What is required in order to have an intervention in Woodward's sense can be represented by the following figure adapted from Craver (2007: 97) picturing the intervention (I) on (X) with respect to (Y):



TEXT TO BE INSERTED BELOW FIGURE 1: “U and C represent conditions external to I, X and Y; solid arrows represent causal relations; dotted arrow represents correlation; hashes represent the absence of the cause or correlation.”

Below we introduce three examples that enrich our understanding of intervention in real cases both within and beyond experimentation. Of the three, only the last is related to a randomised controlled trial.

#### *Case 1 (C1)*

Tai-Chi is a traditional Chinese martial art and a low-intensity exercise. In an intervention study by Chen and colleagues (2008) the effects of a simplified Tai-Chi exercise program



(STEP) on the physical health of older adults in long-term care facilities were examined. A single group design with three pre-tests and four post-tests was used. 51 males aged 65 or older were recruited from two veteran homes in Taiwan. Inclusion criteria included that they should have no previous training in Tai-Chi, be able to walk without assistance, and have a score of at least eight on the Short Portable Mental Status Questionnaire. The intervention consisted in the STEP being implemented three times a week, 50 min per session for six months. The outcome measures included cardio-respiratory function, blood pressure, balance, hand-grip strength, lower body flexibility, and physical health actualization. 41 of the subjects completed the program. The perceived results of this intervention included the following observations:

[...] after one month of the STEP practice, subjects' hand-gripping strength significantly improved, and at the end of six months intervention, the blood pressure, both systolic and diastolic, decreased. Yet, balance and physical health actualization of the subjects did not reveal significant changes. (Chen et al., 2008: 505)

#### *Case 2 (C2)*

Low competence levels among nursing staff have been associated with lower quality of elderly care. A study by Hasson and Arnetz (2008) examined whether an educational intervention on nurses in an elderly care organisation had any effect on residents' and/or their family members' ratings of the quality of elderly care. Practical instruments and educational materials for improving staff competence and work practices were collated in a "toolbox" and introduced in the intervention organisation. Care recipients and their relatives' ratings of care were measured pre- and post-intervention by questionnaire and compared to quality ratings in a reference organisation, where no toolbox was introduced. The researchers notice:

Compared to baseline measurements, care recipients' and their relatives' ratings of the quality of care did not change significantly in the intervention organisation after the introduction of the educational toolbox. There was also no significant interaction effect between the intervention and the reference municipality over time for the quality of care ratings, indicating that the intervention did not have an impact, either positive or negative, on care recipients' or relatives' perceptions of the quality of care. (Hasson and Arnetz, 2008: 172)

### *Case 3 (C3)*

To investigate the effectiveness of an educational intervention that used both the cellular phone and the Internet to provide a short-messaging service (SMS) relating to plasma glucose levels, Kim (2007) randomly assigned 25 patients to an intervention group and 26 to a control group. During 12 weeks patients in the intervention group were asked to access a website by using a cellular phone or to wiring the Internet and input their blood glucose levels every day. All participants were sent the optimal recommendations by both cellular phone and the Internet weekly. HbA<sub>1c</sub>, FPG and 2HPMG were measured pre- and post-intervention. Kim concludes:

This study revealed that an Internet-based intervention by a nurse could improve HbA<sub>1c</sub> levels in patients with type 2 diabetes mellitus in a randomized controlled trial. In this trial, HbA<sub>1c</sub> levels decreased 1.15% in the intervention group after 12 weeks. (Kim, 2007: 690)

## **2. The Serial Double-Effect of Interventions**

Clearly, in these briefly described but representative examples of intervention studies, involving nursing either as the target or the agent, the way in which intervention is conceived is to some extent shared. Borrowing the useful notation and characterisation from Woodward (2003), we might say that a striking feature is that in all three examples interventions (I) are understood in relation to something (X) that is intervened upon and the further effects (Y) of this intervention—a characterisation we will refer to as “the serial double-effect of interventions”. In (C1) the intervention (I) is STEP being implemented on a group of older adults (X). The intended further effects of this intervention (Y) concern different aspects of their physical health. In (C2) the intervention (I) is the introduction of an educational “toolbox”. But rather than, as in (C1), to describe the experimental subjects as the (X) being intervened on, there is reason to select the nurses’ competence levels as the variable being intervened on in (C2). The further effects (Y) of this intervention, of interest to the researchers, are changes in the quality of elderly care. In (C3), it is easy enough to see what the examined further effects (Y) are, namely changes in HbA<sub>1c</sub>, FPG and 2HPMG levels. It is slightly more difficult to see what the intervention is in terms of (I) and (X). The reason, we suspect, is that the intervention is under-described in those aspects. The intervention is

presented by Kim as consisting of “a nurse short-message service”. But this dynamic situation is far more complex than the description reveals. It starts with information about inputting data about self-monitored glucose levels and drug information into a website. After combining these data with the patient’s personal history, the nurse sends an SMS with recommendations, such as “Lack of exercise may be the cause of the aggregated glucose level” or “Please check the amount that you eat”, back to the patient (Kim 2007: 689). This entire electronic vehicle of communication is lacking in the control group. It is this complex that is properly referred to as the intervention (I). Furthermore, no explicit mentioning of this being an intervention on informational and motivational levels is presented in the text. As in (C1), it seems fair to interpret this as an intervention on patients directly (X).

In this description we have sometimes used terms like “intended”, “examined” or “assumed” when we have characterised (X) or (Y), and in (C1) and (C3) we haven’t really been able to characterise the interventions (I) in terms that do not also make reference to what these were supposed to be interventions on, i.e. (X). For someone who approaches real interventions from an understanding of ideal interventions, such as Woodward provides, this may seem imprecise and provisional at best. We think that impression is wrong with regard to our illustrations as well as in general. As far as we can tell few features of these examples couldn’t be found in disciplines outside nursing science as well (and this is, of course, a good thing for any reconciliatory attempt such as ours). However, we acknowledge that there are specific kinds of intervention where such a reaction is perfectly valid.

The serial double-effect of interventions (I)—first on (X) and then on (Y)—provides a direct conceptualisation of interventions *in the ideal and actual form, where (I), (X), and (Y) occur and are related in this order in a causal chain* (as in figure 1). To be actual and to occur have to do with existence or being in the world—i.e. with matters of fact independent of our knowledge of them. These are, philosophers say, matters of *ontology*. Accordingly, this is an understanding of intervention as ontological—or, as we will say for short, of O-intervention.

The plain reason why not every intervention is an O-intervention—and why it is partly difficult to judge whether our three intervention studies fit this description—is that most interventions are compatible with the possibility that the intervention may not, in point of fact, have an effect on at least one of (X) and (Y). And there may even be interventions where (X) and (Y) are caused in a parallel fashion rather than serially. Even Woodward’s ideal

interventions may be less than O-interventions since they only require that *if* there is a change in (Y) this is due to the relations depicted in figure 1. That said we note that it is interesting to compare the similarities and dissimilarities in respect of this double-effect in light of the dimensions added by the use of “intended”, “examined” or “assumed” above.

### **3. Agency, Epistemology, and Ontology**

Let us start with the sense in which the three intervention studies are interventions on (X). We remarked above that it was difficult to interpret (C1) and (C3) in another way than that the interventions were direct interventions on the subjects whereas in (C2) it was an intervention more precisely on one of the features of the nurses: their competence levels. In (C1) and (C3) there is simply too little consideration of the mechanisms by which the intervention is supposed to work for us to go further than that. This makes a difference with respect to the sense in which the intervention on (X) is interpreted.

In both (C1) and (C3) it is clear that the researchers *assume* that the intervention on (X) is *actual*. It is, for instance, never examined whether the implementation of STEP really is an intervention on the senior citizens taking part in the programme. Similarly in the SMS study. But being assumed and being actual are different. These two features of interventions belong to different dimensions. To be assumed has to do with the dimension of knowledge or belief—or, as philosophers use to say, with *epistemology*. To be actual, we have already acknowledged, is a matter of ontology. That these two dimensions do not have to be correlated as in (C1) and (C3) is clearly seen in (C2). In (C2), it is by no means taken for granted that the “toolbox” intervention is an *actual* intervention on (X), i.e. the competence levels of the nurses. It is an *intended* intervention on (X), but it is *examined* whether it is an actual intervention on (X). To be examined concerns the epistemological dimension, just as to be assumed does, but to be intended has to do with still another dimension—that of *agency*.

Let us continue with the sense in which the intervention studies are interventions on (Y). In this respect all three studies resemble one another in that this aspect is *examined*. (C1) examines whether there are changes in cardio-respiratory function, blood pressure, balance, hand-grip strength, lower body flexibility, and physical health actualization. (C2) (indirectly) examines whether the quality of care has improved by measuring care recipients’ and their

relatives' ratings of the quality of care. (C3) examines whether HbA<sub>1c</sub>, FPG and 2HPMG levels change.

The most striking difference between Woodward's ideal interventions and our three cases may indeed be with respect to their relation to (Y). In the ideal case, in order for something to be an intervention it has to be *necessary* in the circumstances for a change in (Y) to occur. That is, (Y), the studied effect variable, is not allowed, in the circumstances and following the intervention, to vary for any other reason than the fact that there has been a change in (X). In one important respect this is a rather strict requirement. It seems clear that whether or not there is a change in (Y) in our three cases, and whether or not such a change is caused by the change in the corresponding (X), these three manipulations would still be referred to as interventions. Hence, the connection between interventions and their serial double-effects is more pronounced in Woodward's framework than it is in any of C1-C3.

To sum up: we can understand the connection between intervention (I) and its double effects (X) and (Y) in at least three ways:

- \* *Intended* double-effects on (X) and (Y). Since intention reflects agency we will call this sort of intervention A-intervention
- \* *Assumed* or *examined* double-effects on (X) and (Y). These concepts belong to the epistemological dimension and we will refer to both as varieties of E-intervention
- \* *Actual* double-effects on (X) and (Y). Actual effects are part of what there is or what exists, i.e. they belong to the ontological dimension and will be referred to as O-interventions.

As we have already hinted at and as will become clearer in a moment, most things that we refer to as interventions are so in more than one of these three senses. But let us first briefly discuss the senses separately. A-interventions may be the most frequent type. We often talk about interventions precisely when we intend to achieve something (Y) by doing something else (X). Such interventions require us to have some ideas about the desirable state (Y) the interventions will result in. Health care decisions, recommendations and advice are often *primarily* A-interventions.

E-interventions come in two kinds. They are either interventions we believe, or assume, to have certain effects (all A-interventions are E-interventions of this kind); or they are

interventions whose effectiveness we have not yet determined and whose precise upshot is presently unclear. Interventions in nursing studies are typically of this kind.

O-interventions are the causally effective interventions. *Knowing* which E-interventions are O-interventions, or which parts of an established A-intervention is an O-intervention, is what laboratory science often aims at. However, there is reason to believe that many A- and E-interventions are not O-interventions. We are, for instance, often mistaken about and can seldom prove what the effects of our interventions are. It may also be difficult to isolate O-interventions from the multi-faceted real life situations on which we intervene. The limiting case of O-intervention, where both (X) and (Y) are caused by the intervention, generates a view of intervention on (X) with respect to (Y) as (X) causes (Y). In fact, the expanded view of cause as O-intervention occurs in several places:

The paradigmatic assertion in causal relationships is that manipulation of a cause [(X)] will result in the manipulation of an effect [(Y)]. (Cook and Campbell, 1979: 36)

(X) causes (Y) if control of (X) renders (Y) controllable. A causal relation, then, is one that is invariant to interventions in (X) in the sense that if someone or something can alter the value of (X) the change in (Y) follows in a predictable fashion. (Hoover, 1988: 173, *variables relabelled*)

Even more importantly, it is easy to blend these three pure conceptions of intervention. In fact, among our three cases, (C1) and (C3) are presented as combinations of A-, O- and E-interventions. In (C1) the effect on the exercise of the senior citizens of STEP (X), is actual, while the further effect on their health, i.e. (Y), is examined and intended. In (C3) the electronic intervention by the nurse on the patients (X) is assumed to be actual while the effect on glucose levels (Y) is intended and examined. (C2) can be presented either as a combination of E- and A-intervention, with the effect on competence levels (X) being examined while the effect on the quality of care (Y) is intended and examined; or, in a case of less value-laden but more hypothesis-driven research, as a pure instance of epistemology, with the manipulation on (X) and (Y) both being examined and assumed. Possible variations in the mood of the presentation are plentiful. Everything that is called an intervention will have effects on both (X) and (Y) in at least one of the three senses corresponding to dimensions of agency, epistemology, and ontology. Quite which respects are filtered out in a

study depends exclusively on the perspective taken. The possibilities are, at any rate, represented in the following simple table. Since it may be difficult to determine whether an intervention study—such as (C1), (C2) or (C3)—assumes the value “Yes” or “No” for any given kind of effect on (X) or (Y), we have allowed cells in the table to contain the indeterminate value “?” as well.

	Agency – <i>intended</i> effect on		Ontology – <i>actual</i> effect on		Epistemology (A) Belief – <i>assumed</i> effect on		Epistemology (B) Study – <i>examined</i> effect on	
	(X)	(Y)	(X)	(Y)	(X)	(Y)	(X)	(Y)
(C1)	Yes	Yes	Yes	?	Yes	Partly	No	Yes
(C2)	Yes	Yes	?	?	Yes	Yes	Yes	Yes
(C3)	Yes	Yes	Yes	?	Yes	?	No	Yes

#### **4. Intervention, Hypothesis, and Theory**

Depending on the cell-contents of the pair of epistemological columns in the table, intervention studies can clearly be categorised as more or less hypothesis-driven, and also as more or less dependent on auxiliary assumptions with respect to conclusions about the causal impact of the interventions. Let us briefly say something about the latter relation first. The dependence on auxiliary assumptions increases with the number of assumed effects on (X) or (Y) that are not examined in the intervention study. For instance, in (C1) it is assumed without the matter being examined that the intervention (STEP) is an intervention on (X), i.e. the effect on the exercise of senior citizens. This assumption is an auxiliary assumption for the researcher who uses this study to test the hypothesis that Tai-Chi has an effect on the health of older people. Less humble critics of such research are prone to react that blatant dependence on auxiliary assumptions is a sign of bad science, and that the simple remedy is to perform another intervention study to discover whether the auxiliary assumption is in fact correct. Such reactions are both healthy and naïve. They are healthy in that it is always a good idea to base scientific conclusions on empirical evidence. They are naïve in that, unless we are working on a research problem that is limited to the very thing we are experimenting on in the

study, it is not possible to eliminate all auxiliary assumptions in this way. And it is questionable if even then it can be done.

Let us now turn to the relationship between interventions and hypotheses. Hacking (1983) not only combines issues of intervention with traditional philosophy of science, but is also a fruitful attempt to shed new light on the perennial problem of whether a scientific study must be preceded by a hypothesis. In order to further that discussion, Hacking claims, we should first recognise that research can be strongly or weakly hypothesis-driven. There are several advocates of strongly hypothesis-driven science. Hacking quotes one of them, the pioneer of organic chemistry Justus von Liebig (1863: 49):

Experiment is only an aid to thought, like a calculation: the thought must always and necessarily precede it if it is to have any meaning. An empirical mode of research, in the usual sense of the term, does not exist. An experiment not preceded by theory, i.e. by an idea, bears the same relation to scientific research as a child's rattle does to music.

It seems to us that the table we present above can be used to model this kind of research. In fact it is possible not only to mimic this position of von Liebig's, but perhaps even to make it stronger than he intended. If we insert a "Yes" in all but the ontological cells in the table, a situation is depicted in which not only do we intend to do something by manipulating something else, but we consider views about the causal relation *and* we examine whether these thoughts of ours are in accordance with the results of the intervention. An intervention of this kind would rightly be described, in von Liebig's words, as "an aid to the thought." Note that we have not made any assumptions concerning whether an intervention exemplifying this variety is of a quantitative or qualitative kind. Neither the concept of hypothesis nor that of intervention depends, according to this view, on this infamous divide.

Karl Popper (1963: 39-59) distinguished between the *context of discovery* and the *context of justification*. The position described here is one in which an intervention is a tool used solely for purposes of justification. Nothing new is supposed to be discovered. Intervention studies may sometimes belong entirely to the context of justification, but one should not neglect the fact that they offer a powerful tool, also, in the context of discovery.



Often the dependence of experiments on hypotheses seems to be much weaker, and the element of supposed discovery considerably greater, than von Liebig assumes. Sometimes it is required “only that you must have some ideas about nature and your apparatus before you conduct an experiment” (Hacking 1983: 153). Then we are dealing with weakly hypothesis-driven research. According to the conception of interventions we have set out in the table it may sometimes be enough to assume an effect on (X) and look for any future effects of this without having any real idea of the mechanisms in play or the nature of the outcome. So, for instance, the following example could easily be accommodated by a “Yes” in the epistemological cell for assumptions concerning auditory interventions on tulips, i.e. (X), and another “Yes” in the epistemological-examined-effect-on-(Y)-cell:

The physicist George Darwin used to say that every once in a while one should do a completely crazy experiment, like blowing the trumpet to the tulips every morning for a month. Probably nothing will happen, but if something did happen, that would be a stupendous discovery. (Hacking, 1983: 154)

What is sometimes at stake in such cases is the mere creation of phenomena that may or may not have effects of some kind. The interventions may be carried out simply for intellectual satisfaction. Real science is seldom like that. The properties we are interested in are often nodes in complex and relatively complete networks of properties, laws and assumptions. An example here would be the intervention studies of contemporary Nano research. It is of course true that our interventions can also be driven by considerably less far-reaching hypotheses, and even simply by a vague hunch. Such weakly hypothesis-driven interventions are easy to find in the field of healthcare research. To intervene without the approval afforded by a hunch, a hypothesis or a theory seems almost impossible in this area. We must have an expectation of change. Why else would we act? Perhaps this is a trivial observation; nevertheless the precision with which an intervention can be formulated and carried out depends on how well formulated and ontologically resilient our hypotheses are.

Moreover, it should not be forgotten that sometimes intervention studies serve purposes other than theory formation. The key to von Liebig’s success, according to the *Encyclopædia Britannica*, was an improvement in the method of organic analysis:

Liebig burned an organic compound with copper oxide and identified the oxidation products (water vapour and carbon dioxide) by weighing them, directly after absorption, in a tube of calcium chloride and in a specially designed five-bulb apparatus containing caustic potash. This procedure, perfected in 1831, allowed the carbon content of organic compounds to be determined to a greater precision than previously known. Moreover, his technique was simple and quick, allowing chemists to run six or seven analyses per day as opposed to that number per week with older methods.

Analogous to the perfection in organic analysis is perhaps some kinds of implementation research: the perfection of strategies for the uptake of research findings into routine healthcare—currently “a haphazard and unpredictable process” according to Eccles et al. (2005). In general, it seems to us that this use of intervention studies fits well with the scientific aim of intervening, i.e. of changing the world, but has limited value with regard to representing. Certainly not every intervention study in nursing research is directed primarily at the aim of representing. A number of such studies, we conjecture, should therefore be analogous to von Liebig’s use of experimentation to perfect or secure a certain effect of a specific kind of intervention.

## ***5. Knowing Why, Knowing How, and Knowing That***

Primarily for traditional positivistic reasons—a tradition not acknowledging inferences to the best explanation, explanatory aspects of intervention studies are often downplayed. Another powerful opinion that serves to drive a wedge between intervention and explanation is the view that science emerged when Thales and his colleagues, shortly after 600 BC, started to ask explanation-seeking why-questions instead of trying to predict and control phenomena. This view is represented and advocated by philosophers of science, such as Georg Henrik von Wright (1986) and Stephen Toulmin (Toulmin and Goodfield, 1961), who have been influential in areas well outside the circle of academic philosophers. Both these opposing views of the relationship between explanation and science in general—and intervention studies in particular—are probably mistaken. More plausible things to say about the relations between intervention and explanation can be found, if only we care to look in detail at what goes on in the course of experimentation:

What experiments do best is to improve causal descriptions; they do less well at explaining causal relationships. But most experiments can be designed to provide better explanations than is typically the case today. Further, in focusing on causal descriptions, experiments often investigate molar events that may be less strongly related to outcomes than are more molecular mediating processes, especially those processes that are closer to the outcome in the explanatory chain. However, many causal descriptions are still dependable and strong enough to be useful, to be worth making the building blocks around which important policies and theories are created. (Shadish et al., 2002: 12)

Note that improving a causal *description* is much like coming to *know that*, and that this is far from enough to *explain why* (Hempel, 1965). But an improved *causal* description will deliver increased explanatory power on almost any understanding of what it is to *explain why*. Many theories of explanation build on the idea that the explanandum (that which is to be explained) has a causal relation to the explanans (that which explains). Obviously, the experiment may give a poor explanation of why the causal relation it discovers occurs, because that is another why-question. On the assumption, however, that another experiment can be conducted with the aim of discovering the cause of this causal relation, there is no reason to assume that experimentation is unfit to disclose explanations why (Persson, 2005).

The reason intervention studies nevertheless often face explanatory problems is because the causal characteristics of many interventions have a negative influence on the explanatory power of the results of the study.

We want now to highlight two problems: The problem of complexes and the problem of polygenic effects.

#### *The problem of complexes*

First, note that what we always succeed in intervening on are *complex entities* rather than any specific property or variable. Similarly, the perceived outcome of an intervention—or indeed any causal process—is such a complex entity. What we manage to do is multifaceted, and this creates an inability to distinguish the causal from the causally irrelevant features of the intervention. The more coarse-grained and indirect the manipulation is, the more difficult it will be to disentangle the relevant O-intervention components from the causally irrelevant factors that are components of the manipulation as well.

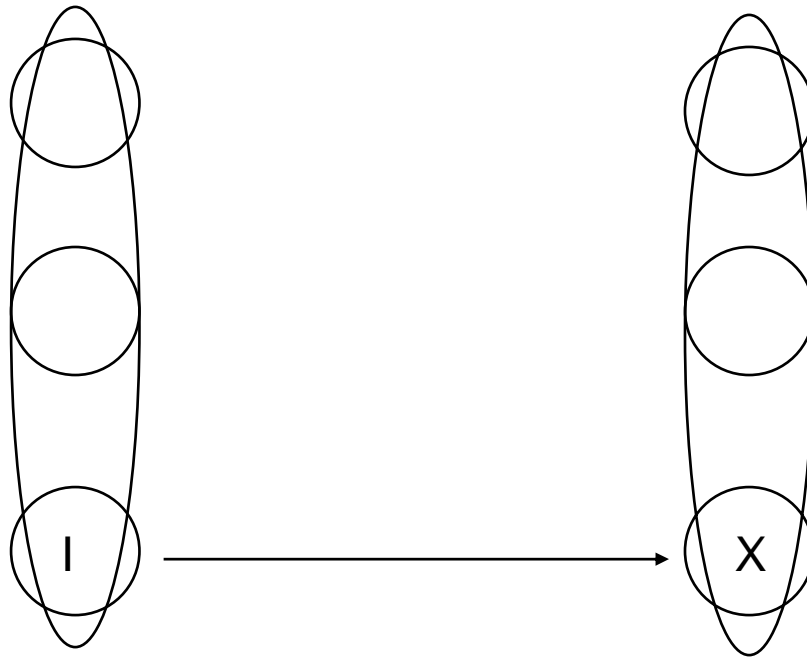


Figure 2: Complexes

But disentanglement is needed in order to be able to explain why something happened. As has been shown in the literature on scientific explanation, irrelevant information efficiently destroys explanatory power: that water dissolves salt may be a satisfactory, although shallow, explanation of the fact that a piece of salt “disappeared” when put in water. However, add the information that the water was holy and the explanation vanishes into thin air—similarly, of course, with other interventions containing a blend of relevant and irrelevant O-intervention properties. The intervention study (C2) may be particularly interesting in this respect. It is of some importance that the researchers successfully make precise the (X) that they are intervening on and which in turn affects their (Y). To only intervene on nurses would efficiently destroy the explanatory power of the findings since nurses have a number of features besides competence levels that would, in the circumstances, be thought to be irrelevant for changes in the quality of elderly care. Correspondingly, the finding in (C3) is explanatorily limited due to its inability to specify exactly the (X) that the intervention is supposed to be an intervention on. A patient receiving an SMS is much too complex a cause to be helpful in any explanation of decreased HbA<sub>1c</sub> levels. Why? Any such patient has a multitude of causally irrelevant features destroying the explanatory power of the attempt. The problem is that the intervention in (C3) is under-developed with respect to the E-intervention

sense. It focuses too heavily on the A-intervention aspects. It is much closer to fulfilling the aim of intervening than of representing.

This marks one interesting divide between interventions that are direct and fine-tuned, experimentally as well as conceptually, on the one hand, and the majority of interventions carried out outside controlled laboratory environments, on the other. Simply blowing the trumpet to the tulips every morning without any idea about the supposed effect of this is a coarse-grained intervention. It is only to be expected that, were there an effect of this intervention, the explanatory power would, accordingly, be low. Recommending patients to do physical exercises such as Tai-Chi, or providing them with internet resources and SMS messages, are other examples in which (as it were) noise obscures the causally relevant and explanatory properties of the interventions.

In order to increase explanatory power, either the intervention has to be made less complex or the hypothesis from which the A-intervention follows as a consequence has to be carefully worked out. There is probably no other route to explanation from an empirical study of the effects of complex interventions. This is not to say that intervention studies belonging to this category cannot fulfil other research goals, such as for instance establishing an effect of a certain kind of intervention or perfecting an already implemented intervention strategy in nursing.

#### *The problem of polygenic effects*

Second, a limitation even more inevitable. The effects we are interested in explaining are often polygenic: that is, they always have *more* than one cause in the circumstances, so the relationship between cause and effect is many-to-one. Improved hand-gripping strength does not only depend on physical exercise. For instance, the researchers in our example assume that previous training and mental state are of importance as well.

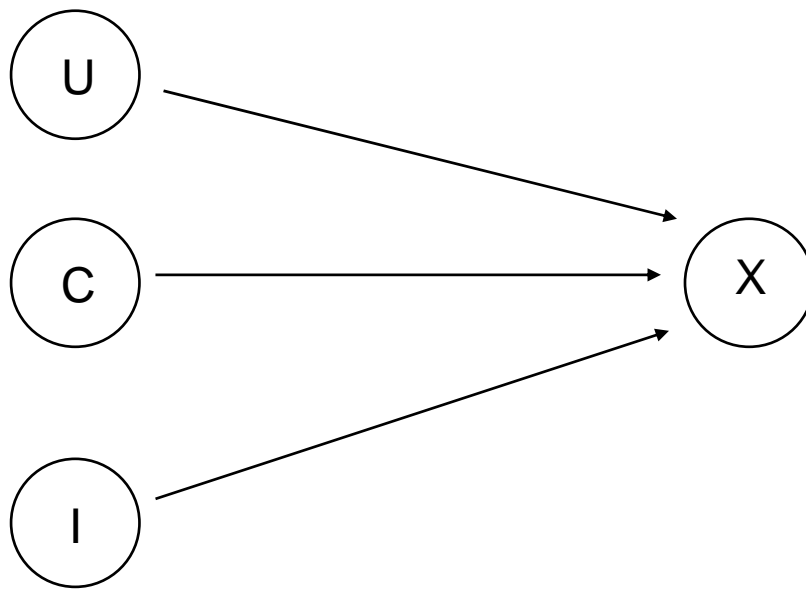


Figure 3 Polygenic

For illustrative purposes we can assume that the causes we examine in intervention studies are something like Mackie's (1974) INUS-conditions, i.e. the causal tokens are *Insufficient* but *Non-redundant* causal contributors to a total cause that is *Unnecessary* but *Sufficient*. For instance, when a house fire is caused by a short circuit, the short circuit is an INUS-condition. It is *Insufficient* because it cannot cause fire on its own (among other causal factors, oxygen is needed as well). It is *Non-redundant* because the rest of the causal factors would not have caused the fire without the short circuit. However, it contributes to a total cause that is both *Unnecessary* (because alternative clusters of causal factors, including cigarette smoking in bed, perhaps, could have brought the fire about) and *Sufficient* for the fire to occur.

Now, as long as the various INUS-conditions are all satisfied in the manipulation itself, there is no immediate problem. Since it is complex and coarse-grained, the manipulation may easily contain a lot of causally relevant (as well as irrelevant) features. However, there is no reason to believe that *all* the causes are present in the manipulation. Interventions are typically causally efficacious in certain environments and not in others. Hence, in every causally efficacious intervention some of the INUS-conditions will belong to the context in which the intervention is being carried out. A sensible concept of causal explanation does not require every causal factor to be included in the explanation, but a poor understanding of the kinds of

other cause that are necessary for the intervention to be successful will lead to a lack of knowledge vis-à-vis the explanatory mechanism. And without an appreciation of the explanatory mechanism, one cannot *explain how* something happened or even how, in the circumstances, the effects of the intervention were made possible. As a result the scientific aim of representing will not be met.

In principle, this is a problem that typical laboratory research should have to at least the same extent as intervention studies of the more realistic type we are examining here. The reason is the following. Assume that a certain polygenic effect has three causes or INUS-conditions. Making the intervention more ideal has the consequence that fewer of the causes producing the polygenic effect are manipulated at once. The coarse-grained intervention perhaps includes all three conditions whereas in the laboratory only one of them is manipulated. Keeping the number of causes fixed, environmental causes—and hence the problem of polygenic effects—should be expected to increase the more fine-grained the intervention gets. That this is not the finding typically reported in intervention studies can arguably be explained by the previous observation that coarse-grained interventions bring about a number of features that interfere with causal structures indirectly related to the effect one is interested in. However, looking for a problem of representing that may be of specific relevance to nursing research, the problem of complexes rather than the more familiar problem of polygenic effects should probably be selected.

## **6. Intervening on the Boundaries of Knowledge**

*Pace* Popper, and many other bold advocates of unitary science, scientific method is not always the same. We must accept that there are several ways to acquire scientific knowledge. According to our three cases, there is definitely not even a single kind of intervention study in nursing research.

Among philosophers of science there has been an impulse to regulate scientific practice and method. There has been a strong desire to demarcate real science from speculative or evidentially ungrounded activities. For example, it has been claimed that hypothesis-driven research is better than straightforward inductive research; that explanations have little value beyond helping us to organise phenomena; that we have to verify our theories, or that

falsification is a far better strategy than verification; that there is no causality in nature; that theories lack truth value; and so on.

However, some philosophers of science reject the very idea of a regulated scientific practice; instead they claim that “anything goes”. Perform the interventions you like. Paul Feyerabend is probably the best known of these “anarchists”. Sören Halldén (1989) reminds us of how similar Feyerabend’s way of thinking is to Gorgias’s (late fifth century BC). Gorgias is arguing that nothing exists; that if anything exists, it cannot be known; and, if anything can be known it cannot be communicated. This is, as Halldén puts it, nothing but “charming nonsense” – and, one might add, an equilibristic exercise in making weak arguments appear strong.

There is, however, one thing that the debate between, on the one hand, Positivists and Popperians, and, on the other hand, Feyerabend, has taught us. Excessive regulation leads to methodological rigidity and, in the worst case, a malign lack of scientific creativity. It is important that we do not forget to play the trumpet.

The aim of science is often representing. We want to describe, predict and explain different kinds of phenomenon. We are in pursuit of knowledge. Knowledge is produced by different types of reliable mechanism or process, and if we have knowledge we know that our beliefs will lead to success.

Interventions can be aimed at changing the world but they can also be used to gain knowledge and to test the reliability of knowledge-producing mechanisms. They can be put to work in contexts of discovery as well as contexts of justification. Of course, we can follow Feyerabend and do anything we like. But that will not help us much. As scientists we have a moral obligation to evaluate our scientific results, and that means evaluate our scientific strategies— in this case our ways of intervening. We have to ask ourselves how well-supported our beliefs are, what we know, but also what we do not know.

The point we want to make is, we think, as simple as it is important. Interventions are at the core of scientific research. If we want to acquire empirical knowledge we have to get involved with the world. In this respect there is but one scientific strategy, one scientific method. What differs is the quality and quantity of the information we obtain, depending on the type of



intervention carried out. Controlled laboratory experiments, or other O-interventions, deliver rather curt and rigid evidence. Equally, however, the study of complicated, real life situations demands A- and E-interventions. They yield evidence that is full of nuances but also harbours serious epistemic weaknesses.

The endless debate, in the philosophy of science, between those advocating an inductive scientific method and those advocating a hypothesis driven method is in this respect a rather uninteresting dispute. Both camps often advocate or rely on interventions, but they deal in different types of intervention. Which type of intervention (A, E, or O) we are able to perform depends on the circumstances and on what type of knowledge we are in pursuit of. The important thing is therefore not that we undertake this or that particular kind of intervention, but that we perform the interventions we need to do to obtain the knowledge we hope to acquire; and that we reflect on the stability of our methods—on the knowledge they give us and, as importantly, the knowledge they withhold.

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