

The Vagaries of Psychoanalytic Interpretation: An Investigation into the Causes of the Consensus Problem in Psychoanalysis (Authors version of a paper published in *Philosophia* 42 (3), pp. 779-799.)

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Abstract

Though the psychoanalytic method of interpretation is seen by psychoanalysts as a reliable scientific tool for investigating the unconscious mind, its reputation has long been marred by what's known as the consensus problem: where different analysts fail to reach agreement when they interpret the same phenomena. This has long been thought, by both practitioners and observers of psychoanalysis, to undermine its claims to scientific status. The causes of this problem, however, are dimly understood. In this paper I attempt to illuminate one important cause of the consensus problem by investigating the role which reliance on 'associative evidence' has in generating consensus failures. Various options for overcoming the difficulties with this form of clinical evidence are then examined. It is argued that these problems can be mitigated by the notion of overall associative fit, though they are exacerbated by certain loose standards used for what counts as acceptable associative evidence. The possibility of using more rigorous standards is discussed.

1. Introduction

Freud's psychoanalytic method of interpretation (PMI), in contrast to specific Freudian theories, is often hailed as the most significant and enduring of his legacies (e.g., Lothane 1998, p. 62). With the PMI, Freud supposedly gave us something more valuable than a set of doctrines, namely, the means for deciphering a wide variety of puzzling human phenomena to ascertain the 'hidden meanings' supposedly encoded in them, including neurotic and hysterical symptoms, dreams, and even cultural products like religions and art-works (all of which may be called 'neurotic symptoms' in an extended sense). By 'hidden meanings' we mean that, unbeknownst to the person to whom they belong, these

symptoms contain ‘references’, references to unconscious thoughts in his/her mind which *caused* those symptoms (by ‘thoughts’ I include memories, fantasies, wishes etc.). By allowing us to identify these references, the PMI helps us to ascertain the unconscious determinants of these phenomena.

The PMI, however, is not without problems. It is well-known, for instance, that analysts often have difficulty reaching agreement over the meaning of these symptoms. That is, when different analysts interpret the same material their interpretations frequently differ, not only when they belong to different schools of thought, but even when they are highly experienced and trained in the same school (Boesky 2008; Fosshage & Loew 1987; Rubovits-Seitz 2001, pp. 3-4 & p. 13). One of the best known instances of this was a project undertaken in 1956 by 8 senior analysts from the Chicago Institute for Psychoanalysis, in which they formulated independent interpretations of the same case material and then worked together to try to reach unanimity. After three years the group disbanded after they failed to overcome their differences (Seitz 1966), and little headway has been made in overcoming this problem since (Boesky 2008, pp. 6-11). This is known as *the consensus problem*.¹

The consensus problem is significant since it calls into question the reliability of the PMI if persistent failures to reach agreement result from its employment. Doubts regarding this method could then jeopardise psychoanalytic theories, which are generalisations based on findings reaped from the PMI (Rubovits-Seitz 2001, p. 336). Although many analysts have been strangely unembarrassed by failures of consensus and the existence of deviant schools (Cioffi 1998, p. 17), others have been greatly concerned by these facts. Thus Judd Marmor worried that ‘[o]ur inability to arrive at a consensus about our interpretations practically invalidates psychoanalytic interpretation as a trustworthy scientific procedure’ (1955, p. 450). Moreover, critics of psychoanalysis often mention the consensus problem when arguing that psychoanalysis is un/pseudo-

¹ It is sometimes said that high levels of agreement among analysts can be found, but this agreement concerns propositions at higher levels of generality. Many analysts agree, for instance, that neurotic symptoms represent unconscious psychic conflicts. This agreement fades when we descend to the level of specific clinical inferences, e.g., regarding the meanings of specific dream elements.

scientific, seeing it as the inevitable consequence of a defective method which allows analysts to ‘confirm’ their own prejudices (see Cioffi 1998, pp. 17-19; 2001, p. 166).

What did Freud himself have to say about this? To my knowledge, references to a consensus problem in his corpus are scarce. Initially he denied its possibility altogether, stating in his analysis of Dora that ‘anyone who sets out to investigate the same region of phenomena and employs the same method will find himself compelled to take up the same position’ (S.E.7, p. 113). He was eventually disabused of this optimism, perhaps by the dissensions of Adler and Jung more than anything else (see S.E.17, p. 48). His best attempt to confront the issue was probably in his last work, where he acknowledged that interpretive disagreements occur but downplayed them by blaming insufficient training, and analyst resistance to accepting hard truths (S.E.23, p. 197; also see S.E.20, pp. 219-220). But subsequent psychoanalytic history suggests that the reasons for these disagreements go deeper than Freud assumed.

The consensus problem is of crucial significance for the evaluation of the PMI as an allegedly probative scientific instrument, yet it has received scant attention, as some psychoanalysts have lamented (Boesky 2008; Rubovits-Seitz 1998; Wallerstein and Sampson 1971), and its fundamental causes have remained little understood. This paper is intended to help rectify this situation by investigating the contribution which the reliance on ‘associative evidence’ for justifying interpretations has in generating this problem, which I take to be only *one* cause of this problem, albeit an important one. The next section introduces this sort of evidence. Afterwards the theoretical basis justifying the use of this evidence is outlined. I then explain how reliance on associative evidence generates consensus problems. Various options for overcoming the difficulties with associative evidence are then discussed, and hope is found in the notion of overall associative fit. However, I explain how this hope is undermined by lax notions of what counts as an acceptable associative fit. A tightening up of analytic standards is urged in the final section, and conclusions about the usefulness of associative evidence are made.

2. Evidence for Interpretations

We will be interested here in a particular range of cases, where the things to be interpreted are supposed to have meaning in a *symbolic* sense. That is, the symptoms are judged to *represent*, *stand for*, or *symbolize* certain things, generally pertaining to a repression; such interpretations consist of propositions of the form ‘*x* represents *y*’. For example, Freud interpreted the horses in Little Hans’ horse phobia to represent (among other things) the boy’s father, so fear of the horses stood for (repressed) fear of the father. This differentiates psychoanalytic interpretation from standard folk-psychological interpretation, which typically isn’t of this nature. That is, when we interpret Frank’s sigh as meaning he’s bored, or his gulping as meaning he’s thirsty, we don’t say that the sigh stood for his boredom, or that his gulping symbolized his thirst. This isn’t to imply that psychoanalytic interpretations are always concerned with symbolic meaning. Sometimes they resemble more the folk-psychological sort, as when an analyst interprets a patient’s ‘accidentally’ mislaying his car-keys, and thus being late for therapy, as being done out of the (unconscious) motive to avoid it (here the mislaying didn’t symbolize the motive). But interpreting the *symbolic* meaning of symptoms has a rationale behind it, and a logic of validation, which is peculiar and must be studied separately.

According to the psychoanalyst Rubovits-Seitz (2001, p. 47), at their worst psychoanalytic interpretations are ‘doctrinally driven’, where symptoms are interpreted in line with theory with little heed given to clinical evidence. At their best, they are ‘data-driven’: proposed because of and justified by clinical evidence instead of theory. I believe, however, that the deepest sources of the consensus problem lie with problems with the evidence which analysts rely on. So what do analysts count as *good evidence* for an interpretation?

In fact, there are different criteria which analysts use for accepting interpretations. For this reason it might be better to avoid speaking of *the* PMI, and to speak instead of psychoanalytic *methods* of interpretation. At the most general level it’s useful to distinguish between external and internal forms of evidence. External evidence is when the analyst appeals to something external to the interpretation’s content to validate it. Internal evidence is when one appeals to this content as somehow indicating the

interpretation's accuracy. Analysts may often appeal to external and internal evidence in combination.

An example of external evidence would be the patient's emotional/behavioural reactions to hearing an interpretation, which according to Rubovits-Seitz (1998, p. 191) has remained a widely relied upon criterion for accepting/rejecting interpretations in analysis (though an unreliable one he thinks (2001, p. 346)). Freud, for instance, sometimes regarded 'defensive' responses by the patient upon hearing an interpretation, such as a vehement denial, as evidence supporting it (S.E.7, p. 57), taking the degree of vehemence with which it's denied 'as a gauge of the repression's strength' (Ibid., p. 58). Using as a criterion the fact that the patient's symptoms abate (or worsen) after being told or accepting the interpretation would be another example of external evidence. So would extra-clinical sources such as checking with a patient's relative to confirm whether some event posited in an interpretation occurred.

In this article I will focus on a particular kind of internal evidence however, where analysts work with *associations* between the *content* of the interpretation and the symptoms to justify those interpretations, and the role this has in generating consensus problems. But I must emphasise that there may be other causes than this, and that a complete explanation is not being offered. For instance, reliability problems with the first two forms of external evidence mentioned above, where they may tend to generate false positives, could cause division among analysts.

Take the second criterion for instance, which is based on what Grünbaum (1993, p. 184) has called the 'tally argument'. According to this argument a patient's neurosis would only be overcome if he is brought to accept the *true* interpretation of his illness. Therefore, patient recovery after accepting an interpretation would count as evidence of its truth. However, as is often noted (e.g., Boesky 2008, p. 129; Grünbaum 1993, p. 193-196), different analysts tend to get about equally good results despite their different theoretical persuasions. Assuming they can't all be right, and that these improvements result from the patients embracing the interpretations, this suggests that a patient's *belief* that he has achieved insight, and the *expectation* that this is curative, can bring amelioration of symptoms by itself (placebo effects). But if rival analysts are each taking

their therapeutic successes to signal the truth of their interpretations, this could be one explanation of how factionalism develops.

However, it's not clear that therapeutic results play a dominant role in justifying interpretations in psychoanalysis. At least they weren't of central importance to Freud. Though Freud occasionally invoked therapeutic success to justify his interpretations, this factor may not have had the pivotal role in his thinking that Grünbaum assumed. We know from Freud's remarks in his later works, and also from his private correspondence in earlier years (when he was trumpeting the effectiveness of psychoanalysis in public), that he believed that the curative power of psychoanalysis was nothing to boast about (Esterson 1996). He was also aware of the phenomenon of placebo cure². His conviction in his interpretations, in that case, must largely have been based on something other than therapeutic success.

It will be remarked that it would be bad practice to accept an interpretation based on just one criterion; good analysts rely on different criteria (external and internal evidence) in combination. For instance, the fact that a patient assents to an interpretation as ringing true, *and* that her symptoms subsequently ease or other positive changes occur, *and* that there are internal connections between the interpretation and symptoms, might be taken as particularly strong evidence for the interpretation, while either criterion taken singly would warrant no conclusion. However, this reasoning would only be valid if these criteria are known to be individually reliable, to some degree at least; combining *unreliable* methods (or mixing reliable with unreliable methods) does not give one a reliable method. Using a plurality of criteria does not relieve us of the duty to evaluate each individually.

I briefly mention the tally argument simply to show that there are other directions one might go in to explain how factionalism has developed in psychoanalysis besides the one I wish to explore here, and that I'm not offering a complete explanation. The topic deserves more attention of course. At any rate, this and other external criteria seem of secondary importance. I will now turn to what is often regarded as the most important

² Freud once said, 'I do not think our cures can compete with those of Lourdes. There are so many more people who believe in the miracles of the Blessed Virgin than in the existence of the unconscious' (S.E.22, p. 152).

evidential criterion for accepting interpretations, which is a form of internal evidence. Some of the complications associated with this criterion have received, in my view, insufficient critical attention.

So what is this internal evidence which analysts rely on? As Freud put it on one occasion, his confidence that some postulated repressed memory/fantasy is the pathogen of certain neurotic symptoms tends to rest ‘above all, on the evidence of there being associative and logical ties between those scenes [in the postulated memory/fantasy] and the ... symptoms’ (S.E.3, p. 210). These ‘associative and logical ties’ or ‘associative connections’ are also sometimes called ‘analogies’ (Rubovits-Seitz 2001, p. 108 & p. 218; Cheshire 1975, p. 196), ‘thematic affinities’ (Grünbaum 1993), ‘coincidences’ (Freud, S.E.4, p. 148), or simply ‘associations’. More generally, we can say that Freud looks for associations between the thought-content postulated in the interpretation, and the symptoms. An example would be supporting his conjecture that the horses in Little Hans’ phobia represented Hans’ father (or the unconscious thought of him) by highlighting the fact that the horses wore black muzzles or bridles over their mouths, supposedly reminiscent of the father’s black moustache (S.E.10, p. 49 & pp. 53-54), and the fact that the father used to play ‘horsey’ with the boy, acting like a horse (Ibid., p. 126). Others would include interpreting a nun in one’s dream to represent (the unconscious thought of) one’s sister, since she too is called ‘sister’, or a hospital as one’s mother, since it too plays a caring role (Grinstein 1983, pp. 83-84). Freud also assigned meaning to the ‘dream symbols’—things which have fixed or regular meanings from dream to dream—on the basis of associations (e.g., people with authority signify parents, and activities involving rhythmical motion signify sexual intercourse).

When such relationships can be found between two things, I will speak of them as being ‘associable’. Many have claimed that such associations are Freud and his followers’ principal evidential criterion for accepting interpretations (e.g., Cioffi 1998, p. 108 & p. 259; Edelson 1990, p. 332; Esterson 1993, p. 247; Rubovits-Seitz 2001, p. 169-174; Spence 1982, pp. 145-148); development and justification of interpretations through ‘associative evidence’ is, in my view, the most distinctive feature of the PMI.

This method may seem puzzling or eccentric when seen out of context, but it becomes intelligible when seen in the light of Freud's 'metapsychological theory'³ (basically, his theory of brain/nervous-system functioning). Examining the rudiments of this will help us appreciate the rationale behind this method, and will set us up better to understand how it leads to such disagreement.

3. The Theoretical Basis of the PMI

A well-known fact about our minds is that thinking of certain things or becoming aware of certain stimuli often causes us to think of other associated things. These relationships of association can be based on contiguity, similarity of form, content or function, contrast, relationships of part to whole, cause to effect, and more. Furthermore, they may be idiosyncratic in nature, as when hearing a particular song reminds you of an old flame (contiguity). Or they may be 'universal' in nature, based on generally discernable objective similarities between things, as when the image of the Italian peninsula makes us think of a boot (similarity of form).

Freud was influenced by a particular way of explaining these facts physiologically developed by T.H. Meynert, the brain anatomist who Freud studied under (see Macmillan 1997, pp. 163-183; Peled 2008). This view, which Macmillan calls *physiological associationism*, employed the theoretical, quasi-electrical notion of 'excitation'. Excitation stands to the nervous system somewhat as electricity stands to a machine: as the postulated energy that makes it function. For instance, Freud held that a thought only becomes capable of entering consciousness when the neural structure identical with it becomes 'cathected', or charged/infused with this excitation.

Meynert conceived of thoughts and memories as physical structures in the brain. He then supposed that physical connections get established between associated structures, which facilitate the transmission of excitation between them, such that the cathection/activation of one structure can lead to the activation of associated ones. These connections may get established through experience. For instance, my frequently

³ My understanding of Freud's metapsychology is gleaned from various remarks scattered throughout his works, but see in particular his paper 'The Unconscious', section IV.

experiencing Frank whistling a tune would mean that the neural structures ‘realising’ (as we’d say nowadays) the thoughts of Frank and of that tune were frequently excited simultaneously. This would cause a neural ‘pathway’ to form or consolidate between them. Subsequently, when I hear the tune in Frank’s absence, the thought of that tune becomes excited, and because of the pathway, excitation flows to the thought of Frank, cathecting it and causing me to be reminded of him (Macmillan 1997, p. 169). Figuratively speaking, we might say that each thought exists as if at the centre of a web, with threads radiating out from it attached to associated thoughts, along which excitation can travel, thus explaining associative thinking.

Now one important thing to note about Freud’s *physiological* notion of excitation, is that he used ‘excitation’ interchangeably with ‘affect’, a *psychological* term, meaning emotion and desire. (E.g., ‘in mental functions something is to be distinguished – a quota of affect or sum of excitation – which possesses all the characteristics of a quantity’ S.E.3, p. 60). The best explanation for this, I suggest, is that he thought of excitation and affect as being identical, excitation being what affect is at a physiological level of description (he was, in this respect, a materialist about the mind)⁴. Moreover, this would explain why affectively significant thoughts emerge in consciousness so persistently (think of how traumatic memories, or pleasurable fantasies, can repeatedly intrude on our minds), since on this view, an affectively significant thought is one whose neural realiser is highly cathected, and this, Freud believed, is what enables thoughts to enter consciousness.

For Freud, the tendency of affect-laden thoughts to enter consciousness and influence action serves an important function. Freud conjectured that build-ups of affect/excitation (such as sorrow or libido) put pressure on the ‘mental apparatus’ (or nervous system), resulting in feelings of tension and discomfort for the subject. Because of this, the apparatus regulates the level of excitation within itself by ‘discharging’ excess

⁴ My point that Freud’s notion of excitation was a physiological one, and that he thought that excitation is what affect is at a physiological level of description, is consistent with his occasionally using ‘excitation’ in talk at the psychological level. Indeed, it explains such uses. For if affect and excitation are identical, one can use ‘excitation’ in contexts where one would use ‘affect’ and vice versa (just as one can do with ‘water’ and ‘H₂O’).

amounts (experienced as pleasure or relief). This discharge is primarily accomplished through our becoming conscious of the affect, giving us the opportunity to express and hence ‘release’ or ‘vent’ our emotion (e.g., through weeping), or to act on and gratify our urges (e.g., have intercourse), which reduces excitation levels.

However, there is one important circumstance where the natural tendency for affect-laden (cathected) thoughts to emerge into consciousness is inhibited: when they are *under repression*. Repression is a psychological defence mechanism whereby certain thoughts get barred from consciousness, either because they are too painful to bear (e.g., a traumatic memory), or because they are deemed (by the ‘ego’/‘superego’) incompatible with our aims for self-preservation, to live in civilised society, or with our moral sense (e.g., incestuous urges). Since cathexis causes thoughts to enter consciousness, repression is achieved by an ‘anti-cathexis’, where the excitation is drained and kept from the thought. But this excitation must go somewhere, and must be discharged. Consequently, it’s forced along the *pathways leading to the associated thoughts*, thoughts which may *not* be under repression. Invested now with the energy previously possessed by the repressed thought, these associated thoughts become vigorously excited, manifesting in consciousness and action in ways which can disturb or bewilder the subject, and which constitute *neurotic symptoms*. By these means, discharge takes place, but this discharge is only partial; full or adequate discharge can only be achieved (in therapy) by returning the excitation to the original repressed thought, which brings it to consciousness, a process called ‘abreaction’.

Analysts typically speak of symptoms as being ‘symbols’ for (or ‘metaphors’ of or ‘allusions’ to) unconscious thoughts. This manner of speech is licensed by the similarity between how the symptom and the repressed thought are related and how symbols are typically related to what they symbolise, in that things are often chosen to act as symbols for other things because of some association they bear to them. The cross, for instance, symbolizes Christianity because of the role a cross had in the story of Jesus (part represents whole), or the skull and crossbones symbolizes danger by depicting a

feared consequence of it (effect represents cause). Neurotic symptoms thus are, or at least have *the appearance of being*, metaphors or symbols for their causes.⁵

4. Associative Evidence and the Consensus Problem

I suggest that the PMI should be understood in light of this theory, sometimes called the metapsychology⁶. For this purpose, we can extract the following key proposition from the theory, proposition *M*:

M: Neurotic symptoms are caused by unconscious, repressed thoughts which are associated, in the subject's mind, with the thoughts/ideas expressed in the symptoms.

M, I contend, suggests a certain *method* for identifying the causes of neurotic symptoms. For, granting *M*, it seems that we may *test* a hypothesis regarding what unconscious thoughts are causing certain symptoms by seeing if they could be associated with the symptoms in the subject's mind. If we fail to find relevant associations, then we can discard the hypothesis, whereas if they can be found this may, if not prove it, at least speak in its favour.

To look at it another way, suppose that *M* is not true. Suppose, for instance, that neurotic symptoms are caused by brain lesions, or by repressed thoughts which are *not* related associatively to the symptoms (unless by chance). Then apparently there would be no point in highlighting associations between the symptoms and some repressed thought (discovered or postulated) of the subject. They would not be indicators of potential etiological significance. The practice thus seems to presuppose a theory of this sort.

Incidentally, the 'in the subject's mind' part of *M* is worth emphasising, as analysts sometimes lose sight of this. For instance, they sometimes back up their

⁵ Moreover, as analysts have noted, the way we interpret neurotic symptoms is not unlike the way we interpret the metaphors of poets: by intuiting similarities and noting associations between things (e.g., Boesky 2008, p. 123).

⁶ This term is also used to cover other aspects of Freud's thought, but I will use it to refer to these ideas.

conjectures that some x represents some y by referring to some ancient practice or folk-tale where it does. But unless the *analysand* knew about that, this can't provide a reason for thinking that such an association exists in *her* mind (Cioffi 1998, p. 51). Freud sometimes follows Jung in speculating that mental associations can be inherited (e.g., S.E.18, p. 242), but even if this were true, then in mentioning ancestors who used certain symbolism, we would need reasons for thinking that there's a bloodline connection between them and the analysand.

This relationship between the PMI and the metapsychology raises the question of whether evidence for the latter exists. Unfortunately, we won't have space to scrutinise this issue. Elegant as the theory is, however, we can at least note that many elements of it are disputed. Neuroscientists do not seem to work with a notion of excitation corresponding to Freud's for instance, a notion which even many analysts have rejected (see Gill & Holzman 1976). The theory may not have to be true in its entirety for the PMI to receive its rationale however, and I've suggested that M is most essential. But even a key postulate of that, repression, is nowadays repudiated by many (e.g., Pope et al. 1998; Pendergrast 1995, pp. 516-518).

Whatever one's opinion is of the metapsychology or M , to fruitfully discuss the PMI, and examine the logic of the method, it is important to consider it within the context of the assumptions which Freud was making. We can thus recognise the metapsychology as an important assumption underpinning the PMI, and proceed while bearing in mind its disputed status.

It is common to observe analysts accepting interpretations once they discern associations between it and the symptoms as proposition M may encourage. But we are immediately confronted with a difficulty here, which relates to the consensus problem. For surely all we can say in such circumstances is that (assuming Freud's general theory of neurosis etiology) our interpretation *may* have identified the pathogenic thought, since we are confronted with the fact that every thought/idea may be associated with *various* other thoughts/ideas of potential etiological significance. Or returning to our web analogy, every thought exists as if it is located on a web with numerous threads radiating out from it, connecting it to different associated thoughts. According to the theory, the

troublesome excitation was displaced along one or more of such threads, but which one(s)?

A study by Fosshage & Loew (1987), where analysts from different schools of depth-psychotherapy were invited to interpret the dreams of the same patient, Martha, illustrates this problem well. A brief introduction to the patient, her problems and background, a number of her dreams, and a few of her associations were all that were given, so interpretations were only tentative, but it nevertheless shows us how analysts can be led, by different associative pathways, in different interpretive directions.

In one of Martha's dreams, she entered a large building and descended a long, dark staircase to a basement where among other things, there was a group of men laying cement. For both A. Garma, a traditional Freudian analyst, and W. Bonime, representing the 'culturalist' approach, the large building represented Martha herself, who was a large woman, with her descent representing her investigation of herself in therapy. However, interpretations of the men laying the cement differed. Bonime supposed that this referred to 'the sense of her own progress [in therapy], her own constructive work' (1987, p. 119) and the men were probably those who have helped her to 'create her life's solid foundation' (1987, p. 122), while for Garma, it 'could signify increased repression as psychotherapy delves deeper' (1987, p. 50). Associations can be adduced for both hypotheses, since cement can be used for construction and laying foundations, or for sealing things in respectively.⁷

Another dream featured cockroaches, which Martha was phobic towards, and these also received different interpretations. For Garma these creatures, which tickle when crawling over the skin, represented the tickles and caresses she received from some boys when she was a child during flirtatious play (this memory was one of her associations to the dream), caresses which Martha supposedly considered 'dirty', like the cockroaches (1987, p. 44). Bonime supposes that they represent the penis or 'cock' (1987, p. 109). Alternatively, Fosshage suggests that they represent her parents: her mother, because she was intrusive like cockroaches, and her father, since he was disgusting like cockroaches because of his boorish eating habits and poor personal

⁷ Another possibility would be that this signified a lewd fantasy, where Martha identifies with the cement which is 'getting laid' by the men.

hygiene (1987, p. 314). Once again, for all these contenders, and presumably many more, associations can be adduced to support them. But how can we know which is pointing us in the direction of the culpable unconscious thoughts?

The problem, then, is that analysts often discover an association or two, and take this to count as good evidence that some x represents y (Spence 1982, p. 145), when it would have been possible to discover equally good associations between x and other items of potential etiological significance. Cockroaches, for instance, are intrusive, disgusting, ticklish on the skin, are called '*cockroaches*', *and a hundred other things*, and will be associable with any of the subject's thoughts relatable to any of those features. Thus numerous interpretations of them will end up looking just as well-supported associatively. The true meaning of the cockroaches, by hypothesis, should be contained in one of these interpretations, but it seems as though there's no way of knowing which, going on the associations alone.

The problem could be even more serious in light of the often-made point that everything is similar to everything else in some way or another. For instance, if we include general or higher-level features such as being a living creature or a material thing, cockroaches would be trivially associable with almost everything. I think that this is taking the criticism too far. Only certain sorts of features seem to be involved in forming active associative pathways. Smith might remind you of Jones because of a similar accent, or a similar sarcastic wit, but not because they both have hands or are both people (so is everyone else). I would conjecture here that the kinds of features that move associative thinking are ones shared by a select number of things; features shared by many things tend to bring nothing in particular to mind (this is related to the concept of a striking association, which I'll discuss later). Nevertheless, it spells trouble enough for the associative evidence criterion if things tend to share non-trivial similarities (however that may be defined) with a great many other things.

It's hard to imagine that analysts have been oblivious to this problem with associative evidence, but if they were aware of it, many have appeared undaunted. I suspect the reason is that they use their background theories to tightly constrain the range of acceptable interpretations, then using associations to select a specific interpretation within that small range. Thus they might assume that the symptoms' meaning will be

oedipal in nature, which allows them to ignore associations to non-oedipal material/hypotheses. However, if the reliability of associative evidence is at issue, we might not be satisfied with this. For what would we hear if we asked about why we should trust the background theory? We'd typically be told that we can trust it because of the wealth of clinical evidence supporting it. But what was this evidence exactly? Was it largely constituted by the wealth of *associations* found to oedipal material in past cases? But now we may wonder whether in those past cases too we could have found equally good associations between the symptoms and alternative, non-oedipal hypotheses. If the background theory was confirmed by other reliable means, then this appeal could be legitimate, but not if it occurred through the very method under question.

5. Free-Association and the 'Charge of Arbitrariness'

There is a popular conception of free-association (see Grünbaum 1993, p. 26 & p. 118; Erwin 1995, p. 132), probably encouraged by Freud's remarks, which suggests that it was this technique which was to allow Freud to overcome this difficulty. As we know, in free-association the subject tells what he thinks of in relation to his symptoms. And according to this popular conception, on doing this the patient's train of thought follows the *same associative route* along which the pathogenic excitation originally travelled, terminating at the pathogenic thought. (The justifying assumption here might be that because this pathway has recently been in use in the process of symptom formation, it is primed and ready to facilitate further transmissions of excitation, so that the subject's train of thought is more likely to follow it).

Thus free-association was to ensure that we arrive at the *right* associated thoughts, and it was also to shield the analyst from the 'charge of arbitrariness' (S.E.5, p. 353). This charge Freud said was justly levelled against certain ancient dream interpreters who relied on *their own* associations when interpreting other people's dreams, the problem being that different interpreters would have different associations to specific dream-elements. But by 'impos[ing] the task of interpretation upon the dreamer himself' (S.E.4, p. 98), Freud thought that he dodged this problem. The symptom simply represents whatever the analysand free-associates to.

However, this popular conception does not represent Freud's considered view about how free-association works (otherwise analysis would be a simple matter). For Freud quickly noticed that his patients, when free-associating, would not recollect the material that he believed lay at the root of their neurosis (e.g., S.E.2, p. 287). But this wasn't, he momentarily conjectured, because his convictions were incorrect. Rather, he supposed it was because the relevant repressive forces were *still operating, resisting* the analytic effort, closing off the critical associative routes. In other words, free-association would only lead to the pathogenic thoughts *in the absence of 'resistance'*⁸. It might proceed in the above way in cases involving weak repressions, but not in more chronic cases where the pathogenic thought remains stubbornly unconscious.

Freud believed that under resistance, free-association would, at best, lead to thoughts *within the associative orbit* of the pathogenic thought: 'an association often comes to a stop precisely before the genuine dream thought: it has only come near to it and has only had contact with it through allusions...'. On other occasions, under stronger resistance, it would lead to thoughts associated with the pathogenic thought indirectly, through intermediate links (S.E.22, pp. 13-14. We will discuss this phenomenon later). The analyst must then take the initiative: '...[a]t that point we intervene on our own; we fill in the hints, draw undeniable conclusions, and give explicit utterance to what the patient has only touched on in his associations' (S.E.22, p. 12). But this means that with the product of free-association, the analyst is left in the same position he was in with the symptom: he has something which he believes is associated (directly or indirectly) with the pathogen, from which he must infer it (see **Fig1**). However, this returns us to our previous problem: there will be multiple associative pathways radiating out from that, just as with the original symptom. How can we tell which one leads to the pathogen? What's to stop different analysts from following different associative leads, thus generating a consensus problem?

⁸ There is the further well-known worry here that, even with resistance absent, free-association could be determined by certain 'suggestive' influences (Grünbaum 1993).

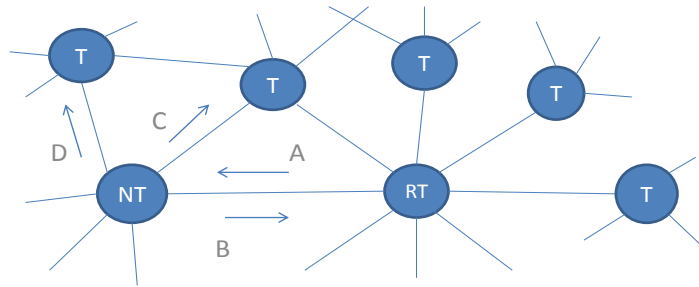


Figure 1: Ts represent thoughts, with the lines between them being the associative pathways. RT is the repressed thought, and NT is the thought where the excitation accumulates after the repression, which manifests in the neurotic symptom. Arrow A represents the direction of excitation flow during repression. Arrow B is the direction of excitation flow during free-association without analysand resistance. Arrows C and D represent possible directions of excitation flow during free-association under resistance. With C, the analysand free-associates to a thought directly associated with the pathogen. With D, resistance is stronger, and the analysand free-associates to a thought indirectly associated with the pathogen.

6. Goodness of Fit

All is not lost for this interpretive method, however. For psychoanalysts (remaining with the case of dreams) typically interpret *whole dreams* rather than dream-elements (e.g., Martha's cockroaches) within these in isolation. And moreover, Freud claimed that it's 'a strict law of dream-interpretation that an explanation must be found for every detail' (S.E.17, p. 42); that is, with a correct interpretation, it should be possible to show that *all aspects* of the dream cohere associatively with it, because every dream-element is, by hypothesis, a symbolic expression of the unconscious thoughts postulated in that interpretation. (Note, however, that Freud often failed to live up to this standard with his interpretations, as will be discussed later). And this 'strict law' can be generalised further: the interpretation should also cohere with *other* dreams and symptoms which are manifestations of the same neurosis/condition; that is, it must show 'convergent validity' (Jahoda 1977, p. 115). Freud sometimes expresses this idea in saying that we can know by the *context* which interpretation is correct (e.g., S.E.5, p. 471), by which he means the context of the entire symptomatology and life-history of the analysand, which the interpretation must account for and be consistent with.

This offers a possible solution to the problem. To see this, suppose that on average, there is a 50% chance that any two randomly selected thoughts/ideas will be associable. Now imagine a neurosis which consisted of just one simple symptom, a horse phobia say. In that case we would know of only one idea (horses) associatively connected to the pathogenic thought, and thus we could never infer this thought, since many false interpretations will be associable with the symptom by chance besides the correct one. But now imagine a neurosis consisting of ten (simple) symptoms. Suppose this gives us knowledge of 10 ideas associatively connected to the pathogenic thought. In that case, the correct interpretation should be associable with all 10 things, and here, there will be far fewer false interpretations associable with the neurosis by chance. Indeed, for any random false interpretation (positing a single pathogenic thought), there will be a 0.00097% likelihood of it being associable with all the symptoms by chance.⁹

So it is the fact that a neurosis typically manifests in a *range* of symptoms, some of which are complex with many aspects, which may create the opportunity to successfully infer the pathogenic thoughts through associative evidence. Incorrect interpretations will be revealed as such when they are found to be associable with only a subset of the symptoms, while correct interpretations will cohere with them all. Only when such ‘copious and intertwined associative links’ (S.E.4, p. 191) are found does ‘one [know] that one has solved the puzzle and that there is no alternative solution’ (S.E.19, p. 116).

On this view, we adjudicate between rival interpretations by seeing which associatively connects most comprehensively with the neurosis’ manifestations. Or as Rubovits-Seitz puts it, ‘...the choice of one theory or interpretation over others is based upon relative *overall goodness of fit*’ (1998, p. 99, my emphasis; Also see 2001, p. 180). To illustrate, in Martha’s first dream mentioned above, the large building she explored was taken to represent, by Garma and Bonime, Martha’s exploration of her (physically large) self in therapy. Martha said that this building was supposed to be an amusement building, and it was not clear how this fits with the idea that it represents herself. Though it might have become clear how this detail fits later in therapy, it might also have remained as a loose end. Garma makes another hypothesis, however, that the building

⁹ For similar reasoning, see (Edelson 1990, p. 343).

represents Martha's vagina, and that her entering the building represents 'genital exploration' (1987, p. 49). This conjecture fits better with the idea that it was an amusement building, but would it make sense of the other details of the dream and manifestations of her neurosis? Whichever accounts for all these details would be considered best supported.

To summarise, what it means for an interpretation to 'fit the data' is for all the details of the symptoms to be explained by it, where this means that we can point to appropriate *associative connections* between those details and the interpretation (and the reason that counts as an explanation is contained in the metapsychology). Other basic standards would have to be met also, for instance, the interpretation should be internally coherent, and shouldn't posit events which never occurred. Note, however, that even granting proposition *M*, the fact that an interpretation fits the relevant facts doesn't entail that it's valid, since false interpretations might fit them perfectly *by chance* (a problem generated by the commonness of associations between things). But the idea is that for a complex range of symptoms, this becomes increasingly unlikely, so an interpretation which fits such a complex data-set will most likely be valid, specifying the cause of those symptoms.

If analysts scrupulously obeyed Freud's (generalised) 'strict law', and meticulously compared clashing interpretations for degree of fit, then the consensus problem could be greatly reduced. Some interpretations would be revealed as being more congruent with the data than others. Or that's what one might expect to happen, except that there are further factors working against this welcome result. For as I'll explain, positions on what counts as a valid association or associative fit may differ, and Freud's lax standards in this regard have the effect of significantly increasing the ease with which any interpretation can be portrayed as fitting even a complex range of symptoms.

7. The Versatility of Freud's Interpretive Techniques

As we have seen, to validate an interpretation by Freud's standards, one must show that it explains all the details of the analysand's symptoms. When faced with a multifaceted and complex range of symptoms, this may not seem easy. As I'll argue however, certain

interpretive techniques, based on certain assumptions about how symptoms and pathogen could be associated, make it easier than one might think to do this.

For one, Freud allowed that symptom and repressed thought could be connected by associative *chains* ('It is much more frequent for an idea to emerge which is an intermediate link in the chain of associations between the idea from which we start and the pathogenic idea we are in search of' (S.E.2, p. 271; Also see S.E.3, p. 195; S.E.22, p. 13-14)). In such cases, symptom s is not associated directly with repressed content r , but rather may be associated with thought x , which is associated with thought y , which is in turn associated with r . As noted, Freud took such chains to indicate a strong repressive force at work.

Note, however, that the likelihood that associations will be found between two things increases exponentially with each link in the chain that's allowed. Let me illustrate this point with a simplified example. Suppose that A has neurotic symptom s , a horse phobia. We then conjecture that repressed thought r caused this symptom. Unless we can relate r to the idea of a horse (or A's idea of a horse, to be precise), we may have to reject that conjecture. Suppose we can't, but we conjecture that s could be connected to r with an associative chain that has one intermediate link: $s-x-r$. Now it's likely that there are numerous thoughts which A associates with horses, if not r (some discoverable by free-association). Suppose there are 10. Then, if we can relate any of these 10 thoughts to r , we will have linked s to r . Failing that, we could suppose that there is another link in the chain: $s-x-y-r$. Then, if the previous 10 thoughts each have 10 associated with them in turn, this would yield 100 further thoughts to work with. Analogously, though the chance of you knowing some person randomly selected from the telephone directory is very low, the chance that you know someone who knows someone who knows that person is far higher (legend has it that there are 'six degrees of separation' between us all).

Alternatively, rather than looking for different candidates for x or y and selecting one which we can associate with r , we could simply keep going in one associative direction, $s-x-y\dots n$, until we arrive at a thought associable with r .¹⁰ Either way, there are

¹⁰ For the ease with which any two things can be connected through associative chains, see (Timpanaro 1976, chap.4).

no two thoughts/ideas that couldn't be found to be associated if chains of sufficient length are condoned.

Another factor to consider is that there are *qualitative* differences between associations; they can vary in relation to *how striking* they are (Michael 2012, p. 68), and Freud was sometimes content to adduce associations which are rather *unstriking*, forced, or tenuous. For instance, the conjecture that in Little Hans' mind horses were linked to and represent his father was supported with the point that the black muzzles/bridles which the horses wear over their mouths look like the father's black moustache (an suggestion which *didn't* come from Hans). But are these really reminiscent of moustaches? Vaguely at best (the straps don't even go between the mouth and nostrils), though Freud adduces a more impressive association when he points out that the father used to pretend to be a horse with the boy (perhaps letting Hans ride on his back around the garden). Though in particular cases it may be intuitively clear that some associations are more striking than others, discerning the underlying principles which determine strikingness is a more difficult matter. I will offer one idea on this in the concluding section.

Another technique which Freud used to great effect is interpreting some symptom or symptom-aspect to represent its opposite. Thus departing can represent arriving, failing can represent succeeding, love can represent hate, etc., a principle of representation called 'inversion' or 'reversal'. Freud correctly notes that opposites are associated notions, and supposes that pathways between opposite thoughts can be exploited in the process of symptom formation: 'This turning of a thing into its opposite is made possible by the intimate associative chain which links the idea of a thing with its opposite in our thoughts' (S.E.5, p. 471).

Note, however, that any symptomatology will consist of multiple elements, many of which have opposites associated with them. Supposing Freud is right that such things can represent their opposites, how can we know *in which instances* they are doing so? On this Freud says, 'every element in a dream can ... stand for its opposite just as easily as for itself. We can never tell beforehand whether it stands for the one or for the other; only the context can decide' (S.E.5, p. 471). Effectively, this means that the analyst can experiment with reversing different elements, until he gets an interpretation which fits the

symptoms, giving him a very powerful tool for dealing with challenging explicanda. Soon I will illustrate how different elements of a dream can be selected for reversal to support different interpretations of it.

It is also important to note that Freud did not always scrupulously obey his rule of having an interpretation account for every detail of a neurosis' manifestations (we will see an example of this below). Rubovitz-Seitz remarks (1998, p. 103) that analysts too often rest content after their interpretations appear to explain a few suggestive elements of the clinical material, 'selectively emphasizing material that fits one's hypothesis' (2001, p. 52) and ignoring a large part of the data. This is problematic because the fact that an interpretation can explain *some* elements from a symptomatology is of no special significance; due to the abundance of associations that exist between things, *any* interpretation could be expected to do so by chance.

With such options at his disposal, Freud gave himself generous room for manoeuvre for getting his interpretations to fit, or appear to fit, the relevant facts. The trouble is that in doing so, he then gives other interpreters the same room for manoeuvre, interpreters who may speculate that the symptoms reflect different meanings. A consensus problem is then likely to emerge.

Let's examine a specimen dream interpretation from one of Freud's showcase analyses to see some of these interpretive strategies in action, and how they can be used to support alternative interpretations: the famous wolf dream interpretation from Freud's analysis of the 'Wolfman', arguably one of his more coherent narrative constructions in his major published case-histories. At the time of this analysis, Freud had ventured forth his theories of the Oedipus complex and castration anxiety, and was keen to find evidence of a castration threat in the patient's past. Since the patient's memory wasn't forthcoming in this regard, the primary evidence he settled for was a nightmare the Wolfman recalled having at the age of 4, and the associative connections it had to a postulated scene of Oedipal-castratory significance.

In this dream, the Wolfman (Russian aristocrat Sergei Pankejeff) was lying in bed at night, when the window doors suddenly opened by themselves and he saw 6 or 7 white wolves (later said to be sheepdogs) with big bushy tails, sitting on the branches of a walnut tree outside, motionlessly staring in at him. He was struck with fear and awoke.

Freud conjectured that this dream symbolically expressed the repressed memory of an event from infancy of traumatic import. In this 'primal scene', the patient, at 18 months, woke from his cot, which was in his parents' room, and 'witnessed a coitus *a tergo* [from behind], three times repeated; he was able to see his mother's genitals as well as his father's organ' (S.E.17, p. 37). This led to castration anxiety and the repression of this memory for the following reasons. After having initially assumed that women have penises like men, the boy came to conclude that the father, from whom he desired sexual satisfaction, had castrated the mother for the sake of having intercourse with the remaining wound (the vagina). Thus the child believed that his desire for gratification from his father would also come at the cost of his penis. The shock of this prospect caused his homosexual incestuous wishes and his memory of the primal scene to be swiftly repressed (Ibid., p. 46 & p. 78).

Freud might be accused of having been 'doctrinally-driven' in advancing this interpretation, but he did labour to show that this postulated scene associatively explains all the dream's details, which was suppose to convince us that it was the dream's true meaning. I will only mention the most important associations here. First, the opening of the windows referred to the opening of the child's eyes, upon being awoken by the commotion. Freud then supposed that the sheepdogs stood for the parents. In support, he pointed out that the whiteness of the sheepdogs would be reminiscent of their supposedly white bed-linen, and the white underclothes that Freud speculated they must have been wearing (Ibid., p. 43). Moreover, the father in the primal scene had a similar upright posture to a frightening wolf he saw pictured in a book (Ibid., p. 39), an indirect connection (sitting sheepdogs-wolves-picture of upright wolf-upright father).

However, the wolves were not engaged in rhythmical motion which Freud usually takes to signify intercourse. To deal with this incongruity, he invoked the reversal principle: their stillness represented its opposite, vigorous motion, in particular the vigorous motion of intercourse. Reversal was invoked again to deal with the otherwise inexplicable bushy fox-tails: they represented a contrary state of affairs: lack of tails, that is, the lack of a penis on the castrated mother (the Wolfman had also told of a fable he had heard where a wolf had its tail pulled off). Another incongruity is that in the dream, the wolves were observing the child, which would imply that the parents were observing

him. Freud appeals to inversion here again: the watching symbolizes being watched (Ibid., p. 34-35).

How about the mismatch between there being 6 or 7 wolves which are supposed to stand for a human couple? Freud failed to find any noteworthy link between this number and the primal scene, and instead explained it away with an *ad hoc* hypothesis '[t]he fact that the number two in the primal scene is replaced by a larger number, which would be absurd in the primal scene, is welcomed by the resistance as a means of distortion' (Ibid., p. 43). In other words, this incongruity was purposely introduced (by the 'censorship') to better conceal the dream's true meaning.¹¹

We see here that with techniques such as reversal, a dream could be portrayed as representing something structurally quite different from it. Nevertheless, Freud's interpretation was hardly a perfect fit. As we saw, there were some elements not adequately explained (the numbers 6 or 7, and also the walnut aspect, which Freud passes over). Indeed, it's not uncommon to find such loose ends with Freud's interpretations. However, the demand to postulate the primal scene is based on the assumption that nothing else would have explained that dream (and the free-associations derived from it) as well, that is, nothing else would have had as good an associative fit with it.

But is this the case? We may worry that with these techniques to hand, it may not be so difficult to find alternative interpretations which could be demonstrated to fit the dream's details as well, even obviously false interpretations. Suppose I believed that many neurotics are people who were abducted by aliens and subjected to a terrifying ordeal strapped to an operating-table onboard a spacecraft, and either repressed the whole experience or had amnesia induced by their captors, where these unconscious memories then express themselves symbolically in dreams and other symptoms (this kind of belief is not unheard of, see McNally 2003, p. 229 & p. 270-272). One might explain the Wolfman's dream with this hypothesis as follows: The scene at the bed is a typical abduction scenario, which usually happens in the dead of night when one is taken from one's bed. The rest of the scene underwent distortion. The sudden opening of the windows may have symbolized the opening of his eyes, and may hint at the telekinetic

¹¹ This exemplifies an *ad hoc* hypothesis because it was adduced to explain away incongruent data without any independent evidence being given for it.

powers of the abductors. The sheepdogs stand for the aliens: their silent observation a reconstruction of their chillingly detached behaviour around the operating-table. Their white colour represents the white light glaring in from outside the window, so typical of abduction stories. The tree alludes to the craft, which the beings are 'onboard', 'hovering above the ground'. But why were they depicted as sheepdogs? Firstly, the sheepdogs initially made the Wolfman think of wolves, to which he subsequently associated *fairytale*s he heard beforehand, featuring wolves (S.E.17, pp. 30-31). Fairytales, furthermore, often contain *surreal* and *fictional creatures*. Thus the sheepdogs allude (via a three-step associative chain) to the fantastic, surreal nature of the experience. Secondly, the Wolfman also free-associated from this dream-element to the memory of being taken to see herds of sheep shortly before the dream, where Freud conjectured he probably encountered sheepdogs (Ibid., p. 58). This identification of the aliens with the sheepdogs alludes to the position of power that they had over the helpless, scared sheep, with which he identified, who were rounded up and confined, just as he was on the spacecraft. The connection with the story of the wolf whose tail was pulled off may allude to an invasive operation, where he had something removed, or feared that he would. And the walnut aspect? It wouldn't be surprising if the spacecraft was roughly walnut-shaped, or perhaps the aliens' heads were.

Interpretations positing more plausible scenarios could perhaps be made to fit too. Suppose we try to interpret the dream as representing a repressed, traumatic memory of being bullied by other kids in a playground. The sheepdogs could be viewed as representing the bullies, with the Wolfman identifying with the petrified sheep which they harass and torment. Their being on the tree could represent their greater height, or it could represent the playground (trees also are centres of play for children). His being in bed, a place where he normally feels safe and secure, represents though inversion feeling exposed and vulnerable. And so on.

Of course, we can't say definitively that these interpretations explain the Wolfman's childhood neurosis, or even just that dream, as well as Freud's. Freud's published case-history was only a summary based on a long analysis, and proper judgment on this point would require detailed familiarity with any other relevant facts about the patient (e.g., other free-associations). These examples are only meant to be

suggestive, and to raise this possibility. They are meant to raise the worry that Freud's finding 'copious and intertwined associative links' between Oedipal-themed interpretations and his patients' symptoms does not prove that any special significance attaches to those interpretations, for it is not clear that such links couldn't also have been found with alternative interpretations.

But suppose we find that multiple interpretations are associable with a neurosis about equally well. What exactly would that show? That the associative evidence criterion, as Freud presented it, is defective? That we were gratuitously attributing causal significance to associations that exist between things as a matter of course? Some analysts would say no, it just shows that *more than one* interpretation can be correct, that is, a neurosis can be *overdetermined* by different unconscious complexes. Whether one finds this reply plausible or attractive, however, may depend on the amount and nature of the interpretations found associable. If, for instance, one is a strict Freudian analyst who attributes all neuroses to repressed sexual wishes, but if the other associable interpretations posit non-sexual pathogenic thoughts, say the repressed traumatic memory of one's birth (Rank), or repressed childhood memories of separation and neglect (Janov), then claiming overdetermination may require him to relinquish his cherished etiological commitments.

The less dogmatic might welcome this. But what if we are finding 'copious and intertwined associative links' between symptoms and interpretations positing scenarios which are highly implausible (like the alien abduction drama), known not to have occurred, or which posit thought-content the analysand couldn't possibly have? Then it may begin to look like our criterion is defective, since it is verifiably generating false positives. Granted, a method/criterion generating false positives may not be its undoing, unless it generates them with unacceptably high frequency. But I would suggest that if our notion of acceptable associative fit is relaxed enough, this criterion will generate them in abundance.

8. Conclusion

I have tried to throw light on a significant cause of the consensus problem with the PMI, pointing to problems with the associative evidence criterion used to justify interpretations. The problems are, in the first place, the abundance of associations which ordinarily exist between things, which can lead interpreters in different directions. This problem is mitigated if we are dealing with a complex set of symptoms and looking at overall associative fit, though it is exacerbated again by the use of certain loose standards for what counts as an acceptable associative fit, and other interpretive techniques.

Let me emphasise, however, that there is nothing grossly fallacious in using associations to justify interpretations. *If* Freud is right about what causes neurotic symptoms, then associations should be evidentially relevant. Better interpretive consensus could then be reached by replacing these lax standards with more rigorous ones for accepting when an interpretation counts as having a good overall associative fit with the symptoms. M. Michael (2008, p. 58) mentions three criteria, specifically in relation to dream interpretation but adaptable to psychoanalytic interpretation generally, which would be a good start in this direction and which directly address some of the problems raised earlier.

First, Michael suggests that it should depend on *how striking* the associative connections are between them. What makes a connection striking? There may be many factors involved here, but I will venture a suggestion on what one might be. When the connection is a shared feature, it may have something to do with how *rare* the feature is. Regarding rarer features, it may come as a strong coincidence, and something requiring explanation perhaps, when two things share them or a few of them, whereas more common features being shared will elicit little surprise as there's a high prior expectation that two things will share them by chance. Michael perhaps expresses a similar idea when he says that when two things share 'distinctive' features we find the connection/similarity between them to be striking (2012, p. 69).

Secondly, it should depend on *how many details* of the neurosis' manifestations the interpretation accounts for (this relates to Freud's 'strict law' of dream interpretation). I understand this as including two closely connected ideas: the interpretation, ideally, should account for all the individual *symptoms* of a neurosis, which includes accounting

for all the *details* of each symptom (e.g., all the elements of a relevant dream). As mentioned earlier, falling short of this is problematic because one will always be able to find *some* elements from a symptomatology which cohere with any interpretation by chance, given the abundance of associations that exist between things. It connecting with *all* of the details, however, might be something that could be expected only of the one which identifies the symptoms' cause.

Thirdly, it should depend on *the number of links* in the associative chains: the less the better. I have explained why we should be suspicious of associative chains. Direct connections are of much greater value than indirect ones, since the likelihood that there are chance indirect connections between any two ideas is so high.

Meeting these standards would surely help *cut down* on disagreement. But would they eliminate it? It is difficult to say. The theoretical assumptions may be such that there may still be a bewildering array of possibilities for analysts to consider which could lead them to diverge. Should we reverse these elements or those? Should we follow this associative thread or that? Should we assume that this element is linked to the pathogen directly, or via an intermediate thought? Given that multiple independent pathogenic 'nuclei' can reside in a subject's unconscious as Freud admits (S.E.2, p. 290), are we dealing with one neurosis, or more than one? Different decisions on these questions might still lead to equally plausible interpretations. Ideally perhaps, we would want to use the associative evidence criterion in conjunction with some other reliable criterion¹².

There is also a deeper question that can be asked here. Though it may be admitted that these standards would ensure better agreement among analysts, even granting proposition *M* why believe that they would allow analysts to home in on the *true* interpretation? Why assume that symptoms and pathogens are connected in the 'tight' ways which these more rigorous standards reflect, rather than in 'looser' ways (with long associative chains, to some rather than all symptom-elements etc.)?

It seems that this is an empirical question, and since there is, as far as I am aware, no good evidence showing that *M* is true, there is *a fortiori* no reason to think that symptoms are connected to their unconscious causes in tight ways (this idea presupposes *M*). Again, in this article *M* has simply been assumed for the sake of argument. All I want

¹² The trustworthiness of the other justificatory criteria used in analysis is disputed however.

to point out here is that *if M* were true, the usefulness of the PMI would *depend on* symptoms and pathogens being tightly connected. For *incorrect* interpretations will always be associable with the symptoms in loose ways (everything is connected to everything ‘loosely’: with long associative chains, etc.). In that case we could not use such connections *as a criterion* for distinguishing true from false interpretations. But if symptoms and pathogens tend to be connected by multiple, striking, direct connections, we might reasonably hope that few if any incorrect interpretations would display similar connections to the symptoms by chance. This is what’s supposed to allow us to use such connections as a criterion for an interpretation’s truth.

I have suggested that the PMI *could* prove useful in discovering the determinants of neurotic symptoms, if Freud is right about how ‘neurotic symptoms’ are caused (a ‘big if’), if symptoms and pathogens are connected in tight rather than loose ways, and if the corresponding rigorous standards were adopted. But it is hard to know exactly *how* useful or reliable it would be, without submitting it to more systematic testing. I have some notions about the general form that such testing would take, but it seems there would be certain obstacles to undertaking it.

It seems that the way to put a method’s reliability to a decisive test is to see whether its application regularly gives the right results. This presupposes that we can know what the right results are, independently of using the method whose reliability is in question. For instance, we can test the reliability of using litmus paper to tell whether a substance is acidic or alkaline, because we can know, independently of using litmus paper, whether a substance is acidic or alkaline. Or we can test the reliability of coin-tossing as a predictor of football game outcomes because we can know who wins football games independently of observing coin-tosses.

Similarly, we might suppose that to definitively test the PMI, we should discover what unconscious thoughts are causing certain phenomena (assuming they are) *independently* of using the PMI, and then check whether analysts, ignorant of these discoveries, reliably get these results using their method. However, it’s not clear how we can establish such things independently of using the PMI, which is usually presented as the *only* way, currently available at least, of exploring the dynamic unconscious. It would help if the forces of repression eventually loosened their grip on these thoughts, allowing

independent corroboration of their existence ‘through introspection’ (though showing that they *determined* the symptoms would be another matter). But going by Freud’s case studies, these thoughts, if they exist at all, seem to *remain* repressed (e.g., the Wolfman never recalled any primal scene, Dora never recalled feeling attracted to Herr K., and the Ratman never recalled hating his father).¹³ Until we have a way of knowing what results we should be getting with it, final judgment on the PMI’s reliability would have to be suspended.

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¹³ Freud’s admission that ‘often we do not succeed in bringing the patient to recollect what has been repressed’ (S.E.23, p. 265) may have been an understatement.

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