NOTE TO USERS

The Identity of Psychology: A Qualitative Exploration and a Descriptive Account of the Crisis and Unification Literature

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Abstract

This thesis is divided into two sections. The first section features a qualitative analysis of the contemporary phenomenology of the identity of psychology. For this analysis, focus groups were conducted with faculty members, graduate students, and honours students from a large psychology department. The participants in each group were asked seven key questions, including a question about their own personal perceptions and experiences of the identity of psychology. The results are presented thematically and then discussed. The second section features a detailed descriptive account of the crisis and unification of psychology literature. The account covers three time periods: 1892-1930, 1931-1969, and 1970-2005. Within each time period, the writings of the major figures from that period are presented first, followed by the rest of the literature, which is presented thematically. In the general discussion, the findings from these two sections are discussed, along with suggestions for future research.

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Table of Contents

List of Tables

Preface

I first became interested in the identity of psychology while taking an undergraduate course on 'systems of psychology'. In that course, we had a pair of debates. The first was on whether or not behaviourism had any contemporary relevance and the second was on whether or not psychoanalysis had any contemporary relevance. After participating in those debates, it became clear to me that deeper, philosophical issues were underlying the system-specific issues which were discussed. It also became clear to me that those underlying philosophical issues were being glossed over in favour of, what I considered to be, the superficial system-specific issues. After completing my undergraduate degree, I then came to the History and Theory of Psychology Programme at York University to investigate those deeper, philosophical issues, which I believed were related to psychology's identity.

After I came to York, I first encountered the 'unification of psychology' literature and I later encountered the 'crisis of psychology' literature—which are, I argue, one literature written from the perspectives of the 'problem' (i.e., the crisis of psychology) and the 'solution' (i.e., the unification of psychology). My initial reading of this literature reinforced some a priori assumptions which I had been grappling with since the undergraduate course. First, psychology seemed fragmented: its specializations, in general, failed to communicate with one another and were, in fact, often hostile to one another. Secondly, without any common basis, there was the potential for psychology to dissolve into, or be absorbed by, multiple disciplines. Finally, there were aspects of psychology, as a unified discipline, which were valuable and worth preserving; thus,

cultivating and preserving a 'unified' psychology is desirable. As a result of these assumptions, the crisis and unification literature intrigued me, so I set out to find as many books and articles which explicitly discussed this topic as possible.

Two years later, the result of my Master's research is this thesis—which is not a thesis in the traditional sense. I have no thesis statement to defend, and I have no hypothesis to test. My thesis addresses two pragmatic issues which were not addressed in previous research, and it is therefore divided into two sections. The first is entitled: "The identity of psychology: Contemporary phenomenology" and addresses the pragmatic issue that, despite the fact that a substantial amount of literature has been published on the topic of psychology's identity, a negligible percentage of this literature includes any empirical research. How do contemporary psychology students and faculty perceive and experience the identity of psychology? Prior to this thesis, this was an unexplored empirical research question. I conducted focus groups with honours students, graduate students, and faculty members to explore this research question for the first time; and the first section of this thesis presents my findings.

The second section is entitled: "A descriptive account of the crisis and unification of psychology literature". Two quotes had a strong influence on my decision to develop this section as a descriptive account with a de-emphasis on analysis. The first was by Koch (1993) who argued: "The integration, integratability, coherence, or unity of psychology—whether as a scientific or some kind of sui generis discipline—has been questioned in so many ways that one might raise second-order questions concerning the integratability of the critiques" (p. 903). The second was by Vygotsky (1997) who argued: "there has been

no *theory of the crisis* in anything so far discussed, but only subjective communiqués compiled by the staffs of the quarrelling parties. Here what is important is to beat the enemy; nobody will waste his time studying him" (p. 294, italics in original). And, after reading well over 300 published sources on the crisis of fragmentation and potential of unification in psychology, I also cannot help but echo Yanchar and Slife (1997a) who argued: "the proposed solutions to fragmentation are as fragmented as the discipline they are attempting to unify" (p. 235). It is clear to me that, prior to this thesis, the crisis and unification literature was fragmented in terms of the ideas put forward, but also in terms of where it was published; it was scattered throughout many journals and books—which span a time period of over a century—and no detailed review of this literature had ever been conducted. What was needed for this topic was not another theoretical spin—a plethora of theoretical spins had been published on fragmentation and unification. Instead the pragmatic issue which needed to be addressed was a detailed review of this literature needed to be conducted to bring the literature together for the first time in one document. Too many contemporary authors are unmindful of the wealth of material and ideas which have been produced on this topic. In defense of these authors, it is extremely difficult to be mindful of an entire literature when it is so fragmented; however, the detailed descriptive account presented in the second part of this thesis addresses this concern.

The Identity of Psychology: Contemporary Phenomenology

Aside from a few contributions (Fuchs & Kawash, 1974; Harari & Peters, 1987; Kawash & Fuchs, 1974),[1] there is a significant dearth of empirical research on the topic of psychology's identity. As a result, contemporary psychologists—especially those whose expertise lies outside of historical and theoretical areas—may be left wondering whether the literature on the topic is anything more than the domain of a specialized few within psychology at best or 'mere armchair speculation' at worst. Furthermore, without empirical research it is difficult to determine how contemporary psychologists and students of psychology *actually perceive and experience* the identity of psychology. Do they experience fragmentation and a lack of communication amongst psychology's specialized areas? If so, does it affect their work or mood? Do they believe the specialized areas of psychology have common features? Even a flawless proposal for unifying psychology's diverse specializations could fail simply because members of the discipline experience no need for it on pragmatic or personal grounds. Therefore, I conducted three focus groups with members of the discipline to explore the contemporary phenomenology of the identity of psychology.[2]

Methodology

Data Collection

[1] These empirical studies are discussed in the second section of this thesis.

[2] This study was approved by the York University, Office of Research Administration, Human Participants Review Sub-Committee (July 6, 2004).

Participants. Students and faculty members both experience and construct the identity of psychology.[3] Therefore, the first focus group involved faculty; the second involved graduate students; and the third involved honours students. A variety of specializations were also specifically targeted for each group since one goal for the research was to make comparisons between and within groups with respect to the different findings. Convenience samples of faculty, graduate students, and honours students were obtained through open calls for participants on corresponding listservs in a large psychology department.[4] For the faculty members and graduate students, volunteers were screened according to specialization to ensure a variety of specializations were included. For the honours students—who were more difficult to recruit and whose specializations were somewhat ambiguous—screening was not employed. In terms of remuneration, faculty members and graduate students each received a meal and honours students each received twenty dollars in place of a meal since their focus group was conducted in the evening. A summary of the participants is included in Table 1; pseudonyms are used for each participant to ensure confidentiality.

Using focus groups. I determined that focus groups were the best methodology to use since they provided the opportunity to capture the *dynamic, intersubjective* nature of psychology's identity. In short, psychology's identity is co-constructed by members of the discipline. This dynamic nature would have been lost if individual interviews or

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[3] There are, arguably, others who experience and construct the identity of psychology (e.g., practitioners, the general public, funding bodies, etc.). Therefore, the scope of this research is limited to faculty and students working within academic psychology.

[4] Due to this recruitment method, there is a possibility of a sampling bias. This is another reason why more research is needed to assess the generalizability of the research findings.

questionnaires had been used. Furthermore, focus groups also provided the opportunity for disagreement and tension, which would have been lost if certain other methodologies had been used. Nine questions were posed to the participants (see Appendix A), using a style suggested by Krueger and Casey (2000), and the focus groups were recorded to allow for transcription and analysis.

Data Analysis

The method of data analysis for this project was adapted from an earlier project (Goertzen, Fahlman, Hampton, & Jeffery, 2003). For that project, our method involved a combination of the inductive qualitative methodology grounded theory (Glaser and Strauss, 1967) and a top-down conceptual component.[5] Specifically, we used a logic model framework as a guide for coding our data into logic model categories and then we employed grounded theory to allow the inductive process to take place *within* each logic model category. In this way we were able to combine conceptual and inductive analyses to meet our specific research goals.

For this project I have adapted the original methodology. First, instead of a logic model framework, I utilized a general theoretical framework as a top-down conceptual component. The identity of psychology is a very broad topic, which includes a number of important sub-topics. These sub-topics were reflected in my focus group questions and include: the definition of psychology, what it means to take a psychological perspective, subject matter, methodology, psychology as a science, and unity-disunity; I

[5] This conceptual component is unnecessary when a topic is quite specific or narrow, but when the topic is broad, includes multiple sub-components, and/or has specific research goals, we have found that adding a top-down conceptual component is a successful way to facilitate the qualitative analysis.

also included the personal experience of the identity of psychology since that is one of the major concerns for this thesis. Secondly, since I am interested in differences between and within groups, I also coded the data within each sub-topic category in terms of specialization (e.g., social-personality, clinical) and status (i.e., faculty, grad student, honours student). So, in sum, I coded the data into theoretical sub-topic categories, then coded the data within each sub-topic category in terms of specialization and status, and then utilized the inductive process of grounded theory within the sub-topic categories to allow for emergent themes. Then, since I used the qualitative analysis software program NVivo (see Richards, 1999), I was able to examine the themes which emerged within each sub-topic category, as well as assess which specializations and statuses were linked to which themes. (Note: The coding scheme I used for specialization and status was as follows: F=faculty, Gr=grad students, H=honours students, Cl=clinical, Sp=social-personality, Dv=developmental, Nbc=neuro-bio-cognitive, Ge=general—i.e., methodology, history, philosophy, and/or theory.)

Results

Definition of Psychology: Mind and Behaviour

The central theme related to the definition of psychology was *psychology studies mental life and behaviour* (F: Cl, Sp, Dv; Gr: Ge, Dv, Nbc; H: Ge, Sp, Cl): JOHN—"[Psychology is] the study of mental processes—including thoughts, feelings, and even imagination and intuitions—and also the study of behaviour". The other common themes focused on the definition of psychology including: 1) *an applied aspect* (F: Cl, Sp, Dv; H: Cl, Ge): JENNIFER—"More and more I'm seeing an important applied element to

psychology...To me, the applied aspect is almost inherent now in the definition of psychology"; 2) *the study of the brain* (F: Nbc; Gr: Dv; H: Ge, Cl, Sp): DEBBIE—"[Psychology is] the study of the brain and behaviour"; 3) *an interpersonal or social aspect* (Gr: Ge, Sp, Dv; H: Ge): DIANE—"I also think that [psychology] looks at...how we interact in groups, how we get along with one another"; 4) *the study of human nature* (Gr: Sp; H: Ge, Sp): CARLA—"Psychology is, to me, about human nature most of all"; and 5) *the study of animals as well as humans* (Gr: Ge; H: Sp): ROBERT—"Yes, animals should be included in my previous definition...animals we should study".

Although the common themes covered much of what the faculty members discussed, they also tended to *define psychology as a science* (F: Cl, Sp, Dv, Nbc): JOHN—"...at the university level, we try to study [our subject matter] scientifically, using the scientific method". However, there was some disagreement: MICHAEL—"I think descriptively, it's clearly false to say that psychology is scientific. There's lots of people who describe themselves as psychologists who say, who deny, that they are scientific and don't want there to be a scientific study". There was also some disagreement among some of the faculty members about whether or not *science is simply a game* to be played:

> JOHN: I think we'd be fooling ourselves if we think we're getting at natural or universal truths or anything—except maybe minute ones. I think it's a game, actually, mostly, a game that we decide to play and we love it...I think that's what most of us are engaged in.
>
> ...

MARK: I echo [the] remark [that] usefulness may be the destiny of psychology as a science. And of course the truth there is praxis. I don't like the definition of science as a game, etc., which is very trendy among many great and somewhat cynical scientists.

Generally, the common themes also covered most of what the graduate students had to say about the definition of psychology. However, there was some tension about whether psychology should emphasize its *subjective dimensions versus observable-testable research dimensions* (Gr: Cl, Sp, Ge, Nbc, Dv):

LISA: Why isn't someone's perception of their own experience just as valid as someone else observing?

SUSAN: Because I think it's not. That I have a problem with. If I think I'm brilliant and I'm not...You can't just be self-proclaimed.

LISA: But you can say that person thinks they're brilliant.

ROBERT: That's not what she's trying to measure though.

SUSAN: That's not what I'm trying to measure, right, so if I'm trying to measure how quick they are to anger, you know, there's always a) there's social desirability which is one thing, but b) there's just people who pretend to be things that they're not until they're put in a situation and they have a rude awakening. A friend of mine was just telling me he was at a conference, and he's an investment expert, and he always starts out the conference by saying how many of you people in the room think you're above average drivers—and 497 people lift up their hands. Right, well the truth is

well only 250 people are above-average and that's true of everything, you know? So I think it's a problem when you're studying peoples' personality with self-report.

And they also proposed that *psychology is defined by its methods* (Gr: Ge): JILL—"I guess I feel psychology has been defined by its methods. You know, we're all being very nice and, you know, being very politically correct, and we're good that way, but I think there really are people who think if you don't use this method, that's what defines whether something is psychology or not".

In addition to what they contributed to the common themes, the honours students argued that *psychology is scientific and non-scientific* (H: Ge, Cl, Sp): CHELSEA—"There's aspects [of psychology] that are scientifically grounded—like if you're checking the pulse of someone, you can count exactly—but there's a lot in psychology that doesn't give you a definite answer". And they added that *there are two streams in psychology* (H: Ge, Cl, Sp):

> ELLEN: I got a sense moving through my undergraduate degree that you can go into one stream or the other: you can kind of take more of a working with people, working with ideas, emotions—I guess you could say the kind of clinical-counselling aspect of it—and then there's the more 'sciency' perception, sensation, computer simulation, interaction type thing. So my sense of psychology is very split I think along the lines of how it's taught.

They also proposed that *psychology is dynamic and complex* (H: Ge, Sp, Cl): CARLA—"My experience of psychology is that...it's always moving, it's always changing"; DIANE—"We're complex creatures, so there's no reason to think that the definition of

how we study ourselves would be any less complex". Finally, they proposed that *psychology is young and still struggling with its identity* (H: Ge): DIANE—"Psychology is a fairly young discipline. I kind of think of psychology as an adolescent, sort of struggling to decide what it wants to be when it grows up. And if I had to use an abnormal psyc label, I'd say it has like dissociative identity disorder".

Psychological Perspective: Multiple Perspectives

The central theme related to taking a psychological perspective was that *there is no one psychological perspective* (F: Ge; H: Cl, Ge, Sp):

> MICHAEL: There's all kinds of people calling themselves psychologists and they do wildly different things—often at cross-purposes—and from single-cell readings up to the level of national politics; and so there isn't a single psychological perspective...I think the way a social psychologist would take a psychological perspective is wildly different from the way a brain scientist would take a psychological perspective is wildly different from the way a human clinical psychologist would take a psychological perspective.

DIANE added: "I don't know what it means to take a psychological perspective; it almost assumes that there is just one psychological perspective—but as others have pointed out, there is no agreed-upon psychological perspective". And CARLA even suggested: "to be psychological is to take multiple perspectives".

However, although there was general consensus that there is no one psychological perspective—and that *one's psychological perspective depends on one's definition of psychology* (Gr: Dv; H: Cl, Ge)—there were two other common themes which emerged.

First, *a psychological perspective involves being holistic* (F: Dv; Gr: Ge; H: Sp, Cl): MARK—"When you study the organisms…so that you examine the behaviour of organisms, okay, then you are doing psychology. If you study one aspect of that organism—the more you fragment the organism, the less you are doing psychology". Finally, *being psychological involves an empirical component* (F: Nbc; H: Sp): DEBBIE—"There's two things that I think of when I think of perspective toward psychology. One is because of my training and scientific background—truly an empiricist approach. For everything that you want to learn or want to know about, there's a question that you can ask…so you want to collect the data and talk about it somehow".

However, despite these two common themes, the central theme was clearly illustrated by the variety of proposals given with respect to taking a psychological perspective. For example, JENNIFER suggested that a psychological perspective involves *"[examining] the individual's history and personality, including temperament, in trying to explain the phenomena"*. JOHN suggested that *an interdisciplinary perspective is more necessary than a psychological perspective*: "A psychological perspective, for me, that's not the right question…I think we have to take into account genetics, biology, child-rearing, culture, politics, economics, as well as psychology and behaviour". Finally, SCOTT suggested that a psychological perspective involves *focusing on an individual or small group*: "Essentially, the individual is the minimum unit of analysis—the whole individual is the minimum unit of analysis—but you can go up to groups of eight and that's still psychology".

The graduate students also weighed in with diverse opinions. For example, KATHLEEN proposed that a psychological perspective involves *finding "the proper balance between being subjective and objective"*. LISA suggested that "a psychological perspective...[involves] *looking at the underlying or mediating factors that cause something"*. JILL proposed that a psychological perspective involves *examining "the meaning behind an object or a person or how people interpret external objects"*. SUSAN proposed that a psychological perspective involves *"more in-depth attempts to understand motivation and consequences of behaviour"*. Finally, ROBERT suggested that a psychological perspective involves *"trying to get to things you cannot see" by examining "things that we can measure directly"*.

The honours students also had individual opinions. For example, MAUREEN suggested that "a psychological perspective embodies *a bit of the cognitive perspective, the behavioural, the biological, the emotional as well—it's a mixture of these four major types of perspectives*". In contrast, CHELSEA argued: "Those four different kinds of perspectives you are looking at...they're almost like conflicting groups in psychology". Instead, she suggested that a psychological perspective involves *"[being] critical of people and of things"*. She added that "psychology courses give you the understanding and the discourse to be able to do that". DIANE, who was largely in favour of the central theme of multiple perspectives, suggested that: "*...you could only apply a psychological perspective to human beings* by virtue of the fact that we have a psyche...you would never take a psychological perspective when studying rocks". Finally, CARLA added that a psychological perspective "*depends on your education, your background*".

Psychology as a Science: Yes and No

The central theme related to psychology as a science was that *parts of psychology are scientific and parts are not* (F: Ge; Gr: Nbc, Cl; H: Ge, Cl, Sp): KATHLEEN—"I would say [psychology is] a bit of both. It's scientific in a sense that we try to explain and ultimately try to predict certain things, but not scientific in the sense that, well, there are a lot of things that we can't do in that way". However, another common theme was that *psychology aspires to be a science* (F: Cl; H: Ge), particularly because of *the credibility and prestige which are attached to science* (Gr: Ge, Sp; H: Ge, Sp): MAUREEN—"I think psychologists try to make sure that psychology is a science because more important is the prestige factor and because, if it's not a science, then it's not going to be taken as seriously as the other sciences...therefore, psychologists try to aim for psychology to be a science and it's in their benefit, their best interest [to do so]". But, although there was some consensus that psychology aspires to be a science, another common theme was that *definitions of science and psychology are fuzzy* (F: Ge, Cl) and *it is necessary to define science* (F: Cl; Gr: Ge, Cl; H: Ge, Cl): ELLEN—"I guess it's important to define what it means to be scientific in the first place—and I think, again, there are assumptions that are built into that". The final common themes were that *psychology can't be* (Gr: Ge; H: Ge, Sp, Cl) or *shouldn't be* (F: Nbc; H: Ge, Sp) *a science*:

> DIANE: I don't think psychology is a science if you define science as being objective. We're human beings studying ourselves. We cannot remove ourselves from the subject matter...the same way that we can remove ourselves from the rocks and the water and study that very objectively.

...

DEBBIE: I would say why should we not allow psychology to become fully a science like biology or physiology or something? And the reason for that is, yeah, it's nice to think that we can explain every little thing about the human being but we're never going to be able to do that. This is why robots will never act like us...you can't get a machine that...uses the emotional kind of baggage that they're carrying to make a decision about something.

As I already covered in the definition sub-topic category, the faculty members also tended to present *psychology as a science by definition* (F: Cl, Sp, Nbc, Dv), although there was some disagreement (F: Ge); also, there was some disagreement about whether *science was just a game* (F: Cl, Sp) or not (F: Dv). Specifically with respect to psychology as a science though, they also mentioned that psychology was divided in terms of *natural versus human science* (F: Ge, Cl): MICHAEL—"[Qualitative researchers are] a great mix, they're part of the fuzz, right? They call it human science...where their methodologies are quite different, but they're trying to be mindful of the problems of rigour...and [natural scientists] are going to say that's b.s.". Finally, SCOTT added that *the reference point for hard science had disappeared*: "It started for me with...*zeitgeist* and then we get the philosophy of science, the history of science, the sociology of science, the psychology of science—all that expansion going on around. So, as a result...our reference point for hard science has gone".

The graduate students added a few other thoughts. First, they discussed *clinical psychology's relationship to science* (Gr: Ge, Cl):

JILL: I would totally agree with that and I think in psychology it's particularly complicated because of the clinical aspect. So it's almost like, it seems like the clinical aspect doesn't lend itself as readily to how we've defined science in terms of generating laws and things like that—and it makes up at least a good 50% of our discipline, you know?

LISA: Yeah, at least in the clinical world there is that split between whether—at least clinical psychology especially—the relation that happens in therapy should be measured in a scientific way or if whether it's more of an art-form and more something not so tangible; and, yeah, I think that's a problem and yet I don't think we should be working toward making it more scientific.

They also discussed *psychology pretending to be a science* (Gr: Sp, Cl, Ge):

SUSAN: I hoped psychology would be scientific. And I've been disappointed. I feel like it's not—and I wish it were—and I think that it's not; and I think it's important for it to be and I just think it's a losing battle. I don't think it ever will be...Part of it for me is that it's so trendy. For me science and trendy are mutually contradictory, so that's right off the bat...You know what, to me, that's not science. If you told me to take a drug and it accidentally killed me like, you know...and this is it: I feel that is one of the biggest problems I've had coming into the program is I'm a person that likes science, that likes those things, and I feel it's not there.

LISA: I think it's hard to have a world out there that doesn't have values, we have to have something that drives our theories or certain guesses, you know it's a social world that we live in that drives that. So I don't think that we would ever say it's not

scientific, it tends to be scientific—we try to find out if it predicts something that we're expecting—and yet we can see that there is a whole many other variables that we have to look at. Whether the typical scientific method that we use is the best method for studying psychology I think is an interesting question.

JILL: I agree with what both of you are saying—I totally hear what you're saying—and I think the analogy of like you know someone gave me a drug and it killed me…I think with other, well there's probably a lot that goes on that we don't see, but there it just seems that with other sciences there are more yes-no answers. There's more like what you're saying…but I think you're right, there's always going to be, and not necessarily is it a bad thing.

SUSAN: But I think it tries to mask itself as science. It's fine, I mean perhaps psychology is not a black-and-white thing, but it pretends to be.

Finally, they discussed *whether it was important for psychology to be a science* (Gr: Dv, Ge):

NICOLE: I don't know. I guess my problem with [whether or not psychology is a science] is that I don't consider it important. Maybe that's why I'm having some troubles answering it. I'm kind of practical in that sense, so I don't see the point of asking whether it is scientific or not as long as you know where you're going.

ROBERT: Could I ask why, I mean why would you consider it not important, whether it's scientific or not? I'm just curious.

NICOLE: Because I'm practical [laughs].

ROBERT: Okay.

SUSAN: Why does it interfere with being practical?

JILL: Well, why is it important? Let's look at the other side. Why is it important to you whether it's a science or not?

ROBERT: When I looked at the question...I mean there are different ways of interpreting psychology in that question. I guess my immediate interpretation is as a discipline. And so, to that, I mean, as a discipline, then it somehow deserves a bit of credibility—it should have credibility. And I think for it to develop and exist as a discipline, maybe it should be scientific.

NICOLE: I think when you add that word scientific you are kind of assigning a value, like science is important. So maybe I'm trying to get away from that. Like one other thing is a lot of really good work, things that are happening in psychology, are not based on very scientific findings.

(The main themes from the honours students' focus group for this sub-topic category were all able to be included under the central and common themes.)

Subject Matter: Behaviour and Mental Processes

After considering the definition sub-topic category, it comes as no surprise that the central theme related to subject matter was *behaviour and mental processes* (F: Cl, Sp, Nbc; Gr: Ge, Dv, Nbc; H: Sp, Ge, Cl). Nor is it a surprise, after considering what has already been presented, that the *brain* (F: Nbc; Gr: Dv; H: Ge, Cl, Sp), *human nature* (Gr: Sp; H: Sp, Ge), *animals* (Gr: Ge; H: Sp), the *whole organism* (F: Dv; H: Sp), and *social behaviour* (Gr: Ge, Sp; H: Cl, Ge) were also commonly discussed as important subject

matter for psychology. Furthermore, it was already mentioned that some of the participants discussed the *complexity of psychological subject matter* (F: Cl; H: Ge, Cl).

However, other common themes included: 1) *psychology's subject matter is often unobservable* (Gr: Ge; H: Sp): ROBERT—"...compared to other disciplines, we have to deal with the most measurement error because...we try and measure...things that we cannot see...No matter how good an instrument that you have...you still cannot pin down...[the phenomenon] that you hope to measure"; 2) psychology should *not* study *phenomena which exist independently of human perception* (Gr: Ge; H: Ge): ROBERT—"...any phenomenon that can exist, and does, absent any human perception or interference should not be studied by psychology, such as let's say black holes...I mean to take the very extreme example"; and 3) psychology should study *subjects which increase the quality of life on Earth* (F: Cl, Gr: Nbc): KATHLEEN—"...what should we study, it's anything, anything which has the possibility of making living creatures on this planet happy".

However, there was also some disagreement about psychology's ability to increase the quality of life on Earth:

> JENNIFER: ...what we should study is what is necessary to sustain life on this planet, a good quality of life. That's very utopian...but, you know, if we're going to pick a goal where we have a unique advantage, making a difference, then...
>
> MICHAEL: No one's against world peace, but that's just silly, to think that psychology has anything to contribute to goals that large...Let's just find out a few

things about how people act and think and...that'll get integrated into a much broader, you know, proposal as to how we might actually affect world peace.

...

JENNIFER: But, like, if we understand, I feel a little bit attacked here. I never talked about world peace. I just talked about goals that are meant to improve the sustainability of this planet. So it might not come through peace, but given what we know [about issues, such as terrorism,] the understanding that exists now, I think, puts us in a position to say, alright, now to reduce the incidence of this problem...this is what we need...

The faculty members also emphasized *the impact of funding bodies on the subject matter of psychology* (F: Ge, Cl, Dv):

JENNIFER: Another way to tackle the question is to look at the kinds of stuff that gets funded now and what's encouraged in terms of research in psychology and more and more what you're seeing is the encouragement of interdisciplinary studies, collaborations with other disciplines, so it seems like it's broadening I think the scope of psychology or at least that's the way in which we're getting pushed.

SCOTT: The granting agencies are going for interdisciplinary and program research more?

JENNIFER: Yes.

MICHAEL: I wonder if that's a result of government priorities or intellectual priorities I mean they always market it as though it's an intellectual thing but there's

all kinds of good government policy reasons to do that. It saves money, or at least they think it saves money.

JENNIFER: So in other words it's not up to us to decide.

MARK: It's never been up to us [group laughs]. You decide what you want to do and the agency decides if they want to fund you.

MICHAEL also added that *it is difficult to determine a priori which subject matter psychology should study*:

I don't know if you can answer a question like this up front. We can talk about it in terms of the really bad research that's been done. Like I'm tempted to say something like psi phenomena, the paranormal, psychologists should get the hell out of that. Well they should get out of it because it's been a boondoggle; it makes them look bad. That doesn't mean you can't do good research on putatively paranormal phenomena; mostly it's that it's been awful and just cast ill repute on the discipline. So I don't know that you can say, you know, from the outset what psychology should and shouldn't pursue.

The graduate students also added a few additional thoughts. As previously mentioned, they discussed whether or not psychology *should emphasize its subjective dimensions over its empirically-focused research dimensions* (Gr: Cl, Sp, Ge, Nbc, Dv). And they also discussed *motivation* (Gr: Sp) and *cognition, memory, and learning* (Gr: Dv) as subject matter for psychology.

The honours students also discussed additional subject matter which they felt psychology should and should not study. For example, they discussed the importance of studying *cultural and political topics* (H: Ge, Sp):

> ELLEN: I feel very passionate about this. I really think that there even needs to be a branch of psychology that deals with political issues somehow; and some way, shape, or form you need to put politics on the agenda because this affects human beings. It affects their mental health, it affects the way they interact with each other, and I mean it goes also down to multiculturalism...Employers don't want to give their employees two weeks off to mourn the loss of their family because time is money and you should be at work making money for the company. And they don't want to deal with multicultural issues because God forbid you give another culture, you know, more respect enough to actually sit down with someone from that culture and understand what they're thinking...

ELLEN further added:

> I really think that psychology needs to be more aware of itself, and the role that it has in the world, and in society. I think psychology is being used...without even knowing that it's being used. I mean if you think of all the money that gets thrown into things like organizational psychology and into the more science aspects of psychology—and less toward the things that can actually really truly improve the quality of peoples' lives rather than it befitting the state or it befitting the economy...I think it needs to be more cognizant of the role it plays in such things, simply because we say that we want to help people—and I really think that most people who go into

> psychology with that applied aspect in mind...and I think a lot of people...shouldn't be assisting in parts of the things that create problems in peoples' lives.

They also discussed *whether or not psychology should study topics which are closer to the natural sciences* (H: Ge, Cl, Sp):

> DIANE: I guess my opinion is that psychology shouldn't study the more hard sciences, like cognition, memory, perception, sensation—I just think those can be enveloped in biology. Yeah, I think we just need to separate out, everybody's just sitting here thinking oh I couldn't stand cognition, I have no interest in perception or memory, and those can easily be studied in existing scientific disciplines [and] we don't need to include those studies in the study of psychology. That's not to say there's never any overlap; but there's already overlap between psychology and sociology and psychology and philosophy. So, yeah, I'd like to see more work between disciplines; but, I don't know, I think it would just be much simpler if psychology sort of distanced itself from the more scientific...but that's never going to happen, so...

Finally, they also discussed *industrial-organizational/applied topics* (H: Sp), as well as *spirituality* (Ge, Cl, Sp) as important subject matter for psychology.

Methodology: Multiple Methods

The central theme related to methodology was *psychology should use multiple methods* (F: Nbc, Sp, Cl, Ge, Dv; G: Ge, Dv; H: Sp, Cl, Ge): DEBBIE—"I think we should be able to use any method that we want to use as long as when we read about the study we are an informed reader so that you don't treat [the method] like a magical kind

of thing". One common theme was *using qualitative methodologies as opposed to, or in conjunction with, quantitative methodologies* (F: Ge; Gr: Cl, Dv; H: Ge, Cl, Sp):

> MICHAEL: Like all these guys running around with qualitative methods now. I don't have anything against them in principle, but frankly when I read their studies I go 'that doesn't tell me anything I wanted to know or didn't know' most of the time. But if somebody came along with a qualitative study and what it got out the other end was something that was reasonably reliable and it told you something you weren't going to get from the traditional quantitative methodologies, I'd be in there like...

In contrast, CARLA argued:

> I think there should be more qualitative research, more dialogue-based approach...it's having people express their feelings, their thoughts, in an open way, whatever they feel like, whatever is ok for them to do. It is really hard for me to understand how somebody can get to the ways of other individuals by giving him a piece of paper and telling him ok fill out this and then I'll do my little analysis and...I would be able, I can do that, but it's not what I feel...I think we need to do more dialogue-based research.

Another common theme was *the popularity of certain methods, especially fMRI (functional Magnetic Resonance Imaging)* (F: Dv, Cl, Ge; Gr: Ge): JILL—"...a common joke around funding time is, you know, well when you're applying for your funding, just throw the word MRI in there and you're good to go [laughs]". The final common theme was *limitations associated with using quantitative questionnaires* (Gr: Sp; H: Ge, Sp, Cl):

LAURA: … when I was handing out my surveys, I got a lot of feedback when I was giving out the debriefing forms, but I never used any of that feedback in trying to improve…

DIANE: And I bet that was some of the best information you got, too…

LAURA: It was!

DIANE: And there's no place for it.

LAURA: Exactly, like there were these items that people had problems with like the wording of certain items…so yeah maybe including, I don't know if it's already done, but including room at the end for them to write down…

Faculty members also added some other themes related to methodology. For example, they discussed *the use of specific methods* (F: Cl, Ge, Dv), including simulation, hypothetical-deductive approaches, and n of one studies. MICHAEL also argued that *psychology's methods change over time*: "the boundaries of scientific methods change decade by decade by decade. In the 19th century probability was out, in the 20th century, probability was in. In the early 20th century simulation was out, in the late 20th century simulation was in. Clinical case studies used to be in, now they're out. These boundaries change all the time". Finally, there was some disagreement among faculty members with respect to *the complexity of psychology's methods* (F: Nbc, Cl):

DEBBIE: Well I think it's pretty horrible that everything is so sophisticated now. I mean you can't even answer a simple question, you have to do structural equation modelling. I mean I just say the words, I don't know what [structural equation modelling] is. I mean I remember when I was working on my PhD, my supervisor

didn't know what SPSS [Statistical Package for the Social Sciences] was or how to do ANOVAs [Analyses of Variance], he only knew how to use t-tests. I feel like I've had the same thing, where I have all these students coming in who want to know all these sophisticated stats and I don't even know how to begin to tell them how to do it. I can send them off for courses, but that's about it…I mean why can't we just answer questions in a really basic way?

JENNIFER: Well, I would say that this is real progress. This is really finally acknowledging, in our methods, the complexity of the subject matter. It's true that you need structural equation modelling to explain anything because it's multi-determined, everything now, you've got to account for as many factors as possible, I think, if you want a comprehensive model. So I'm glad that the methods are getting more complex because we're starting to do justice to the phenomena.

The graduate students also added two more themes related to methodology. First, there was disagreement about *using scientific versus non-scientific methods* (Gr: Sp, Cl, Ge):

LISA: I was just going to throw out basic intuition [as a method], you know. Like we were saying you need the .05s to prove that it's valid and yet all of us know, on some level, know that there are certain things that are valid, that we share—as human beings—that we agree on without having to develop a .05 [significant statistical result]. So, you know, in other cultures [they] really rely on intuition and whether that's more of a truth or not a truth is quite relative I think. Intuition is just one example.

SUSAN: And I would argue that intuition is often not accurate—and that's been proven—and I don't want to follow someone's theory that's based on intuition, [when] I think that their intuition is inaccurate.

ROBERT: Well, there are certain peoples' intuitions that I do trust.

SUSAN: Well, yes, I understand that, but there are more that you probably don't.

LISA: What I was getting at was our shared intuitions, things that we do share, we just know, without having to study.

...

SUSAN: Yeah, I'm torn, like I've always felt like that side, that narrative side, like that to me is the realm of literature, philosophy and its place is...and I'm torn because I do believe that insight into human nature that some people just have and they don't need to have some scientific experiment saying, you know, to validate it is certainly there; but then there's this other side of me that does want proof of things, that does want psychology to, you know...I do want them—when they're telling me, you know, what is going to make my child do this or something—I want it to have proof, I just don't want it to just be someone's intuition, you know?

Finally, KATHLEEN added that psychology should use *methods which are ethical and efficient.*

The honours students' contributions to this section were mostly able to be included under the central and common themes. However, they also mentioned two specific methods which psychology should use more often. First, CHELSEA argued in favour of *philosophizing*:

> I really think that for psychology to grow as a field, they could maybe respect the idea of philosophizing a little more; or maybe even when you're doing a psychology study and you're trying to get published in a journal, if you were to see the discussion section more as a philosophizing section so that it's growing—so that the ideas are growing; and when you're doing a study and you're building on the ideas of the past—and you have to because you're not going to get anywhere otherwise...

Finally, MAUREEN argued in favour of *field experiments*: "...psychology should do field experiments ...where...the psychologist actually goes into the other person's...your situation, your surrounding, your environment..."

Unity/Disunity: Science versus Non-science

As alluded to in quotes mentioned in the previous sub-topic categories, the central theme related to unity/disunity in psychology was *science versus non-science* (F: Ge; Gr: Cl, Nbc, Ge; H: Ge, Cl, Sp). However, there were other, related themes which were discussed, including: 1) *science versus practice* (F: Nbc, Dv, Cl: Gr: Ge, Sp; H: Ge, Cl): JENNIFER—"I think this is where we could all agree that in training psychologists are exposed to the scientific approach and will use scientific means as we define it...to obtain their degree and then after that what we do depends. If you are a professional psychologist doing practice, it's different than if you're an academic"; and 2) *basic versus applied research* (F: Dv, Ge; Gr: Ge):

> JILL: I don't feel like I'm in a helping profession at all. I'll be honest, I'm here to understand. [group laughing] To me it wouldn't be any different from being a sociologist or an economist, the goal is to understand the subject matter.

LISA: And you don't see that that kind of supports our evolution or that that helps us evolve and grow.

JILL: It might, but when I go at this, I don't go at it going I want to help people, I really don't. I don't [laughs].

LISA: And that's why I got out of it [group laughing].

SUSAN: But I think that's a very big difference and I think that it makes the clinical people always a bit different because your aim is definitely to help.

LISA: mmhmm.

SUSAN: And I think in the other areas, it's a little more to understand.

LISA: That's really fascinating.

NICOLE: But is it really? What is your reason? Like, my reason...I agree with you and I'm not in a clinical program.

KATHLEEN: Yeah, me too.

NICOLE: Like I would feel bad doing a study that I think had no implications.

SUSAN: I think knowledge is a pursuit of its own.

JILL: Yeah.

SUSAN: It doesn't need a practical implication.

NICOLE: Well, but then knowledge is the implication.

LISA: And what are we doing that for?

JILL: To know.

SUSAN: For knowledge, exactly.

JILL: Like that, I mean, let's be honest here, I mean I study [a historical topic] and yeah maybe there's some tiny part of me that thinks you know maybe if I look at this and sort of show how it came to be what it is that maybe changes will come about, maybe there's some small part of me that hopes that I can have some impact on how the field is shaped, but more than anything I just want to know how it got to where it is, I don't really care if…

LISA: Do you feel like it's an individual pursuit then?

JILL: Kind of, yeah.

LISA: Yeah?

JILL: Mmhmm.

SUSAN: Versus?

LISA: I would say just a more activist pursuit like something that is trying to come together and actively make a change.

Finally, there was also some conflict about whether or not psychology has *a common, unifying goal* (F: Ge, Cl; H: Sp, Ge):

JENNIFER: I see the common, which you might not see, the common goal and that's something we didn't talk about. Well we have in the common features, but it seems like we all share the same, a similar goal, and we're tackling it from different angles, and that's what I identify with.

MICHAEL: I don't see a common identity for psychology at all. It's splintered all over the place. Psychology as psychology departments, as a kind of a historical and institutional identity, we're all together because somebody decided to start up

psychology departments in the 19-teens, 20s, 30s, 40s, depends on what school you're at; and if we had it all to do over again I'm sure we would break up into a bunch of different departments. We don't have a lot in common with each other.

JENNIFER: Yes we do!

MICHAEL: No, I don't think...

JENNIFER We're all interested in the same thing.

MICHAEL: No, and actually I notice it most strongly when the APA tends to make announcements for psychology as a whole which almost invariably make me cringe and be vaguely embarrassed that I'm part of the discipline; when they send off teams of psychologists to Bosnia you know and this kind of crap. It makes me want to be part of another discipline. It's all over the place, and the reason it's got this one name is entirely historical and indeed there are some departments that are beginning to break up. You'll have the department of clinical psychology and you'll have the department of research psychology or they break up in other ways. I don't see an identity at all.

JENNIFER: It depends on how you look at it.

Faculty members also added a number of other themes related to unity/disunity. In terms of unifying aspects, they offered: 1) *a common training in statistics* (F: Ge, Cl): JOHN—"The statistics used are common in most, go across areas, the methods, most of them"; 2) *a growing emphasis on applied research due to the influence of funding bodies* (F: Nbc, Cl): DEBBIE—"Maybe it's this whole theme of applied psychology; and again, we're being driven by what our granting agencies will give us money for"; 3)

psychologists sharing a building and getting paid by the same department (F: Ge, Cl): SCOTT—"...the only thing that holds psychology together in most cases is the building that we meet in and who pays us"; and 4) *psychologists sharing a common, 'wide open' territory with respect to subject matter* (F: Ge, Cl): SCOTT—"...this is how I perceived psychology...a wide open and expanding area in which to be employed".

However, many of these points did not go unchallenged. With respect to statistics, MICHAEL argued: "Less and less though you know...We've got the [neuro-bio-cognitive] guys who say they don't want to take the stats class because they just use graphs and they have very little invariance [and] you've got the social psychologists using structural equation modelling and no one else knows what...they're saying...you know statistics are actually splintering along disciplinary lines". With respect to sharing a building, MICHAEL argued that in some departments, psychologists are actually in different buildings. Finally, with respect to psychologists sharing a 'wide open' territory, MICHAEL argued: "But there's a downside to that...it opened up in the 60s and it's been opening up ever since; but I think the downside of that is now we don't have an identity. In fact, now we are beginning to splinter and you see it in the journals, you see it in the societies, associations, and you're finally beginning to see it in departments which are the hardest to break up of them all".

Finally, with respect to disunity, JOHN argued that *differentiation is a natural phenomenon*: "this splintering, it's a cosmic phenomena believe it or not—I call it differentiation in more specific terms—but everything is differentiating to be more and more specialized or idiosyncratic. Nations, subnations, cultures, technologies, look at the

models of cars we can get, and on and on and on...You said splintering, I think it's a general phenomena".

The honours students also added a number of thoughts related to unity/disunity. In terms of disunity, they mentioned that psychology has *many conflicting groups* (H: Sp) and *many schools of thought* (H: Sp), but also mentioned that *psychology is not unique in terms of degree of dissent* (H: Ge): DIANE—"I don't think that psychology is unique in terms of the degree of dissent. I think in all disciplines, you've got a wide variety of approaches. In political science, there's tons of dissent, in philosophy and sociology...so I don't think we're unique in that". In terms of unity, they suggested that emphases on *behaviour* (H: Sp) and *meaning* (H: Sp, Ge) were unifying.

(The main themes from the graduate students' focus group for this sub-topic category were all able to be included under the central and common themes.)

Personal

The material for the final sub-topic category comes from a question about how participants' personally experienced and/or perceived the identity of psychology. Due to the personal emphasis, there was no central theme; and there was only one common theme, which was *experiencing others' reactions to finding out the participants studied psychology* (Gr: Ge; H: Sp, Cl, Ge): ROBERT—"I think of those situations when I tell people that I'm in psychology, before I tell them that I do [General psychology] really, so I just tell them that I'm in psychology. And my own experiences in the past with that is people immediately imply that I am analytical, they interpret me to be analytical, being able to understand their personality or tell them things about their personality".

Although the other significant themes in this section were quite individual, they were also quite meaningful. For example, among the faculty members, there was a theme of *identification versus alienation* (Nbc, Cl):

JENNIFER: It's hard for me to answer this question because I feel like it's my identity. So, the identity of psychology, I would say I very much identify with. That's sort of redundant, but it's familiar, it's harmonious with who I am and I find that it's me.

...

DEBBIE: I find it interesting that the clinical person is identifying with the discipline and the people who are not are not so far. You know, there's a huge change, right, historically in terms of psychology—we know that from intro psych—it used to be that the halls of academia were filled with psychologists and now they're filled with clinical psychologists or they're out in the real world doing other things. So maybe APA is now being determined by clinical psychologists, it's no longer experimental quotes 'experimental psychologists', so maybe that's why you can identify with it so easily whereas we can't.

JENNIFER: mmhmm, yeah.

...

DEBBIE: I really don't identify with [psychology], in fact I feel like I'm so not part of it, it's way out there somewhere and that's why I love stuff like this [focus group] to tell you the truth. It's because I get to maybe hear what drew me to the discipline to begin with. You know when you're in university you experience this a lot. When

you're talking with colleagues and friends and professors and things like that, but I find that's all lost now and so I have no identity with psychology, I mean I identify with my computer and that's, I'm being honest...I just don't find that there's a united group of people who I am a member of, I don't belong to anything. I'm not a psychologist—clinical psychologist—I...have no one in my area to identify with, so I actually...you know I might identify with the fact that there's this department that I'd like to consider myself a part of and I enjoy my colleagues, but no...I have no identity with the area of psychology per se, I'm actually struggling to find my identity within it...

In the graduate student focus group, SUSAN discussed *disappointment*: "I hoped psychology would be scientific. And I've been disappointed. I feel like it's not and I wish it were and I think that it's not and I think it's important for it to be and I just think it's a losing battle, I don't think it ever will be".

Finally, in the honours student focus group, *psychology cultivating qualities of a person* (Sp, Ge, Cl) was discussed: CHELSEA "[Psychology's been] able to make me look at each person and not necessarily...look at what they're doing and how they're acting and analyze in comparison to how I would look and act because I look at them as an individual...I realize that the circumstances are different, there's reasons behind it...but I think before psychology I was a little more, like less empathetic toward that, yeah, so it's helped me grow as a person and how I understand people".[6]

[6] There were other personal reactions to the identity of psychology, but they have been included under other sub-topic categories.

Discussion

This small, exploratory qualitative investigation is a first step toward cultivating more empirical knowledge on the topic of psychology's identity. However, a well-known criticism of qualitative research is that its generalizability is quite limited. In fact, due to the small amount of participants, it was very difficult to even detect or infer patterns amongst the specializations and, therefore, this aspect of the coding was limited to simply labelling which specializations corresponded to each theme. Clearly, subsequent research is needed—particularly research which has the potential to tap a much larger sample; such research would allow differences between specializations to be more adequately discerned. However, the knowledge which was gained from this exploratory study should not be casually minimized. Some interesting themes emerged within each of the sub-topic categories.

With respect to the definition of psychology, the participants generally agreed that psychology studies mental life and behaviour, but also the brain, interpersonal/social behaviour, human nature, and animals; they also included the application of psychological research in their definition. The faculty members tended to also define psychology as a science, although there was some disagreement on this point and also debate about whether science is just a game to be played. The graduate students suggested that psychology is also defined by its methods, and there was some debate about whether the discipline should emphasize subjective or observable data. The honours students claimed that psychology is both scientific and non-scientific, and that there are two corresponding streams of training in the undergraduate curriculum. They

also suggested that psychology is dynamic and complex, and a young discipline struggling with its identity.

Although the participants all made interesting contributions regarding what it means to take a psychological perspective, the central theme of *no one psychological perspective* was evidenced in their responses. Many of them simply stated that there is no one psychological perspective, while this theme was also illustrated by the assortment of other responses which were given. The few common themes mentioned were that a psychological perspective: depends on the definition of psychology used, involves being holistic, and involves an empirical component. However, a variety of other themes were discussed. In the faculty focus group, it was suggested that a psychological perspective includes an emphasis on the history and personality of an individual; it was also suggested that a psychological perspective is limited in application to an individual or small group. However, it was also suggested that an interdisciplinary perspective is superior to a psychological perspective for all social science research in general. The graduate students also offered a variety of suggestions related to taking a psychological perspective. They suggested that a psychological perspective includes: finding a balance between being subjective and being objective; looking at underlying or mediating factors causing a phenomenon; looking at the meaning behind an object or person; making in-depth attempts to understand the motivations for, and consequences of, various behaviours; and studying unobservable phenomena via observable phenomena. Finally, the undergraduate students disagreed as to whether a psychological perspective includes a mixture of cognitive, behavioural, biological, and emotional perspectives, or whether the

four perspectives correspond to conflicting camps within psychology. Other suggestions made by the honours students were that a psychological perspective: involves being critical of people and things, can only apply to humans, and depends on one's education and background.

In terms of psychology as a science, many of the participants agreed that parts of psychology are scientific and parts are not. Some participants, however, also suggested that psychology aspires or pretends to be a science largely due to the credibility and prestige which are attached to the name 'science'. Some participants also stressed that psychology could not or should not be a full science due to the subjective nature of psychological subject matter. However, it was also mentioned that the definitions of 'psychology' and 'science' are fuzzy and, therefore, need to be clarified before a decision can be made as to whether psychology is or should be a science. Despite this need for clarification though, most faculty members defined psychology as a science (although there was some discussion about a divide within psychology between natural and human science). It was also mentioned that the demarcation criterion for hard science had disappeared in the wake of critical perspectives on science generated in the latter half of the 20th century. Finally, the graduate students also discussed the relationship between clinical psychology and science, as well as whether or not it is even important for psychology to be considered a science.

In terms of subject matter, the general consensus from the focus groups was that psychology studies the mind and behaviour; however, the brain, human nature, animals, the 'whole organism', and social behaviour were also commonly mentioned as

psychological subject matter. It was also mentioned that psychology should study subject matter which increase the quality of life on earth—although there was some disagreement as to whether psychology could accomplish such a goal. Other commonly discussed issues were that psychology's subject matter is complex and unobservable, and that psychology should not study subject matter which exists independently of human perception. Faculty members also discussed the impact of funding bodies on psychological subject matter and how it is difficult to determine a priori which subject matter psychology should study. The graduate students added that psychology should study motivation, cognition, memory, and learning, and they debated as to whether psychology should emphasize subjective or observable subject matter. Finally, the honours students added that psychology should study political and cultural subject matter, as well as industrial-organizational topics and spirituality; they also questioned whether psychology should study subject matter which could be assimilated by existing natural sciences.

The consensus opinion amongst the participants was that psychologists should make use of multiple research methodologies. In general, participants agreed that both quantitative and qualitative methods should be used—although there was some opposition to the use of qualitative methods. Participants also mentioned that certain methods—particularly fMRI (functional magnetic resonance imaging)—are fashionable in psychology. They also mentioned that certain limitations are inherent to questionnaire research. Amongst the faculty members, there was some debate regarding the necessity of newer, more complex methodologies. The faculty members also discussed the use of

specific methodologies—including computer simulation, hypothetical-deductive methodologies, and n of one studies—and it was mentioned that the methodologies which are used changes over time. Amongst the graduate students, there was some debate about whether non-scientific research methodologies—including intuition—should be used. It was also mentioned that psychologists should use methods which are ethical and efficient. Finally, amongst the honours students, it was mentioned that psychologists should use field experiments more often and also place more value in philosophizing.

The central centrifugal tension within psychology which the participants discussed was scientific versus non-scientific perspectives. Two related tensions which were also discussed were research versus practice and basic versus applied research. There was also some debate amongst some of the participants as to whether or not psychology has a unifying common goal. Some of the faculty members suggested that training in statistics, sharing physical space, being paid by the same department, sharing a vast terrain with respect to subject matter, and emphases on applied research dictated by funding bodies are all centripetal forces within psychology; however, there was some evidence given to the contrary with respect to the former three suggestions. It was also suggested that differentiation is a natural phenomenon which takes place in many domains, including psychology. Finally, the honours students mentioned that psychology contains many conflicting groups and schools of thought, but that psychology is not unique in terms of its degree of dissent; they also mentioned that emphases on behaviour and meaning are unifying forces.

In terms of personal experiences of the identity of psychology, some of the participants shared a common experience of having members of the general public assume certain personal characteristics about them or areas of study when the members of the general public are told that they study psychology. Amongst the faculty members, there were also some differences with respect to whether participants personally identified with, or felt alienated from, psychology. In the graduate student group, a theme of disappointment was discussed surrounding psychology not being a full science. Finally, some of the honours students agreed that psychology cultivates certain positive personal qualities in a student which are, in general, not cultivated by other disciplines.

These findings, though limited in their generalizability, provide valuable insight into how psychology students and faculty perceive and experience the identity of psychology. Initial data have now been generated with respect to: how contemporary members of the discipline define psychology; what students and faculty think about what it means to take 'a psychological perspective'; what kinds of subject matter and methodologies are endorsed; how members of psychology view the discipline in relation to science; what kinds of centrifugal and centripetal forces are experienced; and how students and faculty personally experience and perceive the identity of psychology. The findings suggest that issues related to psychology's identity affect faculty and students—from a variety of specializations—on professional and personal levels, and are not limited to a few specialized members within the discipline. More research is needed to fully determine the generalizability of this claim.

A Descriptive Account of the Crisis and Unification of Psychology Literature

The themes which were discussed by the focus group participants—while important and interesting—are not representative of the entire literature on the topic of psychology's identity; in fact the literature is quite extensive and contains a wide range of perspectives. Therefore, for the second section of this thesis, I developed a detailed descriptive account of the crisis and unification of psychology literature (psychology has been perennially described as experiencing an ongoing *identity crisis*). In developing this descriptive account of the literature, I have limited my focus to *explicit crisis and unification of psychology writings published in English.* By 'explicit' I mean writings which *specifically* discuss the crisis and/or unification of psychology and which focus *primarily* on these topics.[7]

My rationale for focusing on published, explicit crisis and unification writings is simple. When one scans the reference lists from crisis and unification writings, one finds, interestingly, that—aside from a few well-known books or articles (e.g., Koch, 1981; Staats, 1983)—there is negligible overlap. The authors cite sources which can be identified as crisis and unification writings, but they often reference very *different* sources. At first glance, this is of little concern. However, when one considers that: a)

[7] In general, I will *not* be addressing: 1) explicit crisis and unification of psychology writings *published in languages other than English*; 2) *unpublished*, explicit crisis and unification of psychology writings (all languages) (e.g., archival letters, conference presentations); 3) published and unpublished, *implicit* crisis and unification of psychology writings (e.g., 19th century psychologists' and philosophers' writings on the nature of psychology, such as Dilthey's, 1977 descriptive and explanatory psychologies [note: Dilthey, 1977 includes an English translation of his Ideen über eine beschreibende und zergliedernde Psychologie, which was originally published in his *Gesammelte Schriften* from 1924-1927]); 4) *the academic contexts* which influenced the writings; 5) *the broader socio-cultural and socio-historical contexts* which influenced the writings (e.g., the impact of World War II on psychology); and 6) published and unpublished, explicit and implicit crisis and unification writings which are specific to the *specializations* of psychology (e.g., the crisis of social psychology cf., Pancer, 1997).

there are over 300 published, explicit crisis and unification writings in total; b) most of the sources individually only reference maybe 10-20 of this total; and, *most importantly*, c) a detailed treatment of the crisis and unification writings has *never been conducted*, then it is not implausible to consider that a naïve reader, picking up one or two sources on the crisis and unification of psychology, might get a partial—and perhaps biased or inaccurate—treatment of these complex topics.

A quick retort might be that this is the case for any research topic; however, an equally quick counter would be that for at least some of these topics there exist handbooks, detailed works (e.g., theory or extensive literature reviews), or authoritative works—which the majority of the researchers in the area cite—to which a casual reader (student, scholar, or otherwise) could turn. There is no 'handbook' for the crisis and unification of psychology. No detailed or (agreed-upon) authoritative work exists to date. Furthermore, this state of affairs stands in *ironic contrast* to the fact that some authors (e.g., Staats, 1983) specifically recommend—as part of the solution to the crisis of psychology—that much more integrative work needs to be conducted to bring together fragmented bodies of knowledge within psychology. *The crisis and unification of psychology literature is a fragmented body of knowledge* (Drob, 2003; Koch, 1993; Yanchar & Slife, 1997a). Therefore, my goal for this descriptive account is to apply the recommendation found *within* the literature *to* the literature in an attempt to bring together what is currently a fragmented body of knowledge with the product being a first attempt at a detailed and, hopefully, authoritative source on the crisis and unification of psychology literature.

Since I am presenting a detailed descriptive account, I have limited the amount of analysis I contribute;[8] however, since there is such a wealth of literature, it has been necessary to provide a loose organizational framework in order to render the material coherent. First, I have divided the literature into three time periods: 1892-1930, 1931-1969, and 1970-2005. There were two significant surges of crisis and unification literature which occurred in the past century—the first in the 1920s and the second in the 1970s—and I have used them to help organize the flow of the material. Secondly, at the beginning of each of the three time periods, I have provided a theoretical overview, which is provided to orient the reader with respect to the material presented in that section. Finally, I have divided the material within each time period based on whether or not it was written by a major figure.[9] The material written by major figures is presented first, followed by the rest of the literature, which is presented thematically. Within this organizational framework, the material is then presented descriptively and chronologically, with an emphasis on who wrote the source and what he or she contributed to the crisis and unification literature.

1892-1930: The Emergence of the Crisis and Unification Literature

Overview

[8] Following this thesis, I will be continuing with this program of research and will move on to conducting a theoretical analysis of the various issues brought forward in the literature. However, first this detailed descriptive account needs to be presented so that it is available for critique, expansion (e.g., adding non-English sources or unpublished material), and to inform contemporary researchers interested in the topic. If I included more analysis at this stage, I would be adding yet another slant to the literature which would have to be negotiated while trying to appreciate the original contributions. This detailed descriptive account is desperately needed for this literature. Analyses can be developed in subsequent projects.

[9] My criteria for referring to an author as a major figure is that he or she published a book and/or a series of articles on the crisis and/or unification of psychology.

From James (1892/1983) and Ladd (1892) to McDougall (1930), the crisis of psychology was described during this time period as being *fundamentally dualistic* (Vygotsky, 1997), a tension between a more objective, empirical, natural scientific psychology and a more subjective, metaphysical, descriptive psychology. Although the superficial concern was often the plethora of competing schools which existed during this time period, the deeper issue was this dualistic tension, and it was argued that the plethora of schools could be categorized according to this tension (McDougall, 1930). As Willy (1899) noted, the dualism seems to have stemmed from the substantive problem of mind-body dualism, which psychology inherited from philosophy, but it was compounded by disagreement surrounding psychology's attempts to make a primary commitment to the methods and philosophies of natural science (James, 1892/1983; Ladd, 1892). Eclectic (Bühler, 1927), divisive (Vygotsky, 1997), and reconciliatory (Kantor, 1922/1971) proposals were offered for addressing the fundamental dualism, but the crisis of psychology remained unsolved at the end of this time period.

Introduction

Many historical start dates are arbitrary to some degree. My start date is no different. I have selected a debate between William James (1842-1910) and George Trumbull Ladd (1842-1921) which occurred in 1892, since it featured some of the important themes which continued throughout the rest of the literature. Also, when I examined material published *prior* to 1892, I found it difficult to distinguish *explicit* crisis and unification writings. This is not to say that the issues discussed then did not overlap significantly with the issues which were discussed following 1892—one need look no further than the

mind-body problem to see that the more things change the more they stay the same. However, there appeared to be a qualitative difference between the way these issues were discussed in the 19th century and how they were discussed at the turn of the 20th century and beyond. As I will discuss shortly, however, Rudolf Willy (1855-1920) perceived the crisis of psychology to be chronic in 1899 and his use of the word 'chronic' illustrated to me that, though my descriptive account of the explicit, published writings begins in 1892, the implicit roots of this literature likely extend back beyond this selected starting point. In sum, although I am aware that 1892 is a somewhat arbitrary starting point, I will proceed with the belief that it is a good one and leave it to others to determine if I am correct.

Ladd and James: Psychology as a "Natural Science"

Psychology as so-called "natural science." Ladd (1892) began his article with a brief consideration of James' (1892/1983) recently published *Principles of Psychology* but then launched into his critique. Ladd first argued that, on the one hand, he appreciated James' range of scholarship; on the other, he lamented the personal style with which James wrote which often made interpretation and critique difficult. His primary concern, however, was not with James' writing style; rather, it was with James' conception of psychology as a natural science. Ladd argued that James defined psychology as "the Science of Mental Life, both of its phenomena and of their conditions" (p. 27) and defended psychology as a natural science "by excluding from it all metaphysical assumption whatsoever" (p. 29); and he admitted that James was being consistent in avoiding metaphysics—since his goal was to present psychology as natural science.

However, Ladd pointed out: "psychology as a natural science, without metaphysics, is wholly cerebral psychology" (p. 30), referring here to James' insistence on grounding mental states firmly in neural events.

Ladd was critical of James' focus on 'cerebral psychology' and argued that James excluded from his conception not only introspective psychology, but also much of physiological psychology—including the work of Gustav Fechner (1801-1887)—which did not always deal explicitly with the brain. In fact, Ladd argued that James' conception of psychology as a natural science, free from metaphysics and focusing primarily on the brain, was so limited that it could not achieve its goal and that it could become inconsistent or *unscientific* in its attempt to *be* scientific.

However, despite the challenges he put forward to James, Ladd was not against psychology as a natural science in principle. He argued that psychology was at least a science because it had "a sufficiently well-defined field of phenomena, which it undertakes to describe and to explain; and because it has ample data, not only for description but also for explanation of these phenomena" (p. 50). Ladd added that, like other sciences, psychology attempted to reduce the complexity of its subject matter to simple elements and explain how these simple elements combine to create complex phenomena. Ladd concluded that he was open to the possibility of a natural scientific psychology, without reference to metaphysics, but added that the task would be incredibly difficult and that few, if any, could achieve it. He emphasized that James had not achieved it in *The Principles of Psychology* and that, if it was possible, psychology as a natural science rested in the distant future.

A plea for psychology as a 'natural science.' In responding to Ladd's critique, James (1892/1983) argued that Ladd had misinterpreted his work and that he had never claimed that psychology, in its then contemporary form, *was* a natural science. He argued that psychology was "hardly more than what physics was before Galileo" and was just "a mass of phenomenal description, gossip, and myth" (p. 270). Furthermore, James argued that he had hoped that in presenting psychology as a natural science, he would be aiding it in *becoming* one. However, James was quick to move from his book to the issues, since he argued the dispute was not over the *Principles per se* but instead over the issues presented *in* the book—particularly the issue of psychology as a natural science.

In stating his case for psychology as a natural science, James argued that all natural sciences made certain assumptions and left the truth or falsity of those assumptions to philosophy. He also appealed to the general public's interest in psychology's ability to present them with practical rules—something a science could and should provide. James did not dispute the fact that "two utterly distinct types of mind" (p. 273) (i.e., physical and metaphysical) existed; but he concluded that, in the pragmatic interest of conservation of labour, those two approaches should be separated, with the physical remaining in scientific psychology and the metaphysical being confined to philosophy. James argued that those two approaches to psychology (philosophical and scientific) could live in harmony, so long as they remained within their own domains and did not dispute each others' underlying assumptions.

James concluded with two either-or propositions. First, he argued that a critic of cerebralism should reject it completely—including well evidenced facts such as

aphasia—or else "accept it in principle" (p. 276), and then also accept that psychology was but at the beginning of the development of such a science, with a long way to go. James added that those who were currently working within this emergent science could at least "clear metaphysical entanglements from their path" (p. 277). The second either-or proposition was hypothetical and referred to the two kinds of psychology James outlined (i.e., physical and metaphysical). He argued that if he were forced to choose between the two approaches, he would immediately select the physical since the "kind of psychology which could cure a case of melancholy, or charm a chronic insane delusion away, ought certainly to be preferred to the most seraphic insight into the nature of the soul" (p. 277).[10]

The implications of James' plea for psychology as a natural science. Amedeo Giorgi (born 1931) reviewed the James-Ladd debate and provided his own critique (Giorgi, 1990). He began by pointing out that James was "an unsystematic, and even paradoxical, writer" and therefore "his explicit statements have to be taken with a grain of salt and have to be understood contextually" (p. 63). He added that James called for psychology as a natural science "even as he researched and commented on multiple personalities, witchcraft, and religious experiences" (p. 63).

Giorgi briefly reviewed James' and Ladd's papers and then turned to his own analysis. He agreed with Ladd that James did not stay within his own conception of psychology as

[10] It is worth noting that James (1892/1983) later (in his presidential address to the American Psychological Association) was apparently willing to give up on his demand that psychology be conceived as a natural science—as long as the metaphysically-inclined psychologists were willing to drop their demand that metaphysical or spiritualistic explanations be included. Where the middle ground would be was unclear (Sarason, 1981).

a natural science, devoid of metaphysics. He added that he did not think anyone could adhere to this conception, since metaphysics is always implicated in psychology by definition. However, he also clarified what James meant with respect to his conception of psychology as a natural science:

> [James] did not mean that the natural sciences had to be slavishly imitated. He meant, first and foremost, that metaphysics had to be overcome as a factor constraining the descriptions of psychologists. That is, connotations of words like soul, transcendental, and so forth, should not influence psychological descriptions. Thus, James wanted to be concrete and specific in his descriptions in order to uncover the facts of psychological experience, but without committing himself to a specific metaphysics. But how was this to be done? (p. 71)

Giorgi then reiterated that James had failed to remain within his own conception, and that James himself had admitted in places that he had to admit some "philosophical presuppositions" (p. 72) in order to explain psychic life.[11]

Major Figures

Willy: Die Krisis in der Psychologie[12]

Rudolf Willy's (1855-1920) *Die Krisis in der Psychologie* (Willy, 1899) provided insight into the crisis of psychology that was, in general, not available in later writings.

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[11] Giorgi (1990) concluded his chapter with a phenomenological reconstruction of James' (1892) proposal, which I have not included since it is of questionable relevance for this descriptive account and it is also questionable as to whether or not James himself would have endorsed the reconstruction.

[12] Although as a general rule non-English sources are avoided in this descriptive account, I am very grateful to Dr. Thomas Teo for explaining the contents of this extremely important German source to me. It should be noted, however, that I accept full responsibility for any misinterpretations or errors presented in this thesis.

For example, though some authors (e.g., Koch, 1951) argued that the crisis of psychology was tied in large part to the establishment of psychology as an independent science in the latter half of the 19th century, Willy argued that, as of 1899, the crisis in psychology was *already a chronic one* ("chronische Krisis der Psychologie", p. 1)—which suggests that the crisis of psychology (at least for Willy) was tied more to substantive issues (e.g., the mind-body problem), which had a long history in psychology *qua* philosophy.

Willy was a strong supporter of Empirio-criticism ("Empiriokritizismus", p. 2), which was championed by Ernst Mach (1838-1916) and Richard Avenarius (1843-1896) (both of whom strongly influenced Willy's views on psychology). He argued that Empirio-criticism represented *the* scientific point of view and that psychology needed to eliminate metaphysical and spiritualistic influences. He further argued that psychology remained in an unconscious, metaphysical bondage of speculation, which needed to be rectified. However, he was aware of the difficulties inherent in psychology. For example, he pointed out that there was a divide within psychology between introspectionists and experimentalists and that this divide was tied in part to the mind-body problem. A related problem was the question of psychic causality and, if such causality was possible, whether natural scientific methods represented the proper methods for its study.

Specifically, with respect to the crisis of psychology, Willy argued that there were two general aspects: metaphysical and methodological. In addressing the metaphysical aspect—which included non-physical substances and concepts, speculation, and other issues—he provided lengthy critiques of Wilhelm Wundt (1832-1920), Johannes Rehmke (1848-1930), and Franz Brentano (1838-1917)—all of whom he described as being too

metaphysical. In terms of the methodological aspect, Willy pointed out a number of issues. For example, psychology's lack of success in producing laws, its over-emphasis on concepts and theories versus observation and data, and its emphasis on hypotheses as opposed to facts (Willy argued psychology's hypotheses were multiple and took on a reality independent of facts, and he argued this development was problematic). Furthermore, Willy argued psychology bordered on *scientism* by directly applying the methods of natural science, and he singled out Hermann Ebbinghaus (1850-1909) as having done so. He contended that psychology was not a science like the natural sciences since natural sciences used measured numbers (i.e., quantification), which had only metaphorical meaning in psychology. Finally, with respect to unification, Willy argued that specialization and diversity were natural outcomes of scientific study and, therefore—although it was desirable—one should not be surprised when psychology made only infinitely slow progress toward this goal.

Kantor: Reconciling Introspectionists and Objectivists?

Jacob Kantor (1888-1984), a former psychology faculty member at Indiana University, provided a proposal for achieving unity between the introspectionists (e.g., Edwin Boring [1886-1968]) and objectivists (e.g., Kantor himself) in psychology (Kantor, 1922[13]). His primary argument was that a shared use of the psychophysical experiment—with an elimination (or at least severe containment) of metaphysical,

[13] Even though Kantor only published one article in the 1892-1930 time period, I am including him as a major figure since he published subsequent articles in the 1970-2005 period. Thus, he is a major figure because he published a series of articles on the unification of psychology, but his work is discussed in two time periods since he published in two time periods.

mental, and philosophical assumptions—could lead to unity between the two opposing schools.

Kantor argued that psychology was a science, but that introspectionists (who assumed a metaphysical mind) and objectivists (who focused on observable phenomena only) disagreed about the fundamental nature of the 'data' (i.e., subject matter) of the science. In other words, they did not simply disagree about interpretations of the data; they fought about the very nature of psychological subject matter. Kantor argued this dispute was counterproductive to the development of psychology as a science. He did not, however, regard disputes over *interpretations* of data as problematic; and he cited examples from physics, where this level of disagreement was natural, in support of his position. Kantor concluded the two schools of thought were simply fighting about two aspects of *the same data*, since psychologists were interested in "the same series of natural events" (p. 174). To demonstrate his point, he turned to an examination of the psychophysical experiment and how he believed it could unify the two schools at the level of the nature of the data.

Kantor argued that the psychophysical experiment was simple, well documented, and able to help psychologists get to the level of "crude data with a complete freedom from philosophic bias" (p. 175);[14] he also argued there were already points of agreement between the two schools regarding the psychophysical experiment. First, and most

[14] In a footnote, Kantor admitted that a 'complete freedom' was probably not possible, but he argued some "philosophic or cultural attitudes are more conducive to the obtainment of correct results in psychology than others" (p. 176) and that psychology would profit from giving up the process of transforming observed facts into "mentalistic" facts. He argued that if psychologists could all agree to make this concession, unity would be inevitable, since it would arise from the study of the crude, observable facts. As a corollary to this point, Kantor added that unity would be more easily achieved in psychology if there would be no pure mentalists (i.e., arguing all psychological facts are mental events) or objectivists (i.e., arguing all psychological facts are muscular or glandular movements).

importantly, both schools used the psychophysical experiment in an attempt to understand, without subjective bias, stimulus-response patterns among human subjects. Also, there was agreement that the participant in the experiment "performs a series of definite acts", which could "be divided into different functional parts" (p. 179).[15] Furthermore, the 'middle processes' studied in the experiment were neural events, not distinguishable from "the reaction system as a whole" (p. 180).[16] Another point of agreement involved attention and that it could be viewed as a *response* of the participant. Also, with respect to perception, Kantor argued: "If the structuralist [i.e., introspectionist] can agree with the objective psychologist upon this proposition, namely that the perceptual factor is a reaction system, an adaptational act of the observer in the experiment, then the two points of view can be brought into substantial harmony" (p. 181). Finally, both schools could agree that the verbal report of the participant represented the "final item in the reaction pattern" (p. 182). In sum, Kantor argued that if the two schools could agree to use the psychophysical experiment, within a stimulus-response framework—which was devoid of mental events, and which viewed the subject as a 'reaction system'—then the disagreement about the nature of the data could be overcome and unity would be inevitable.

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[15] Kantor admitted, however, that this could be a point of contention between the two schools if the introspectionists would not concede that the process occurring between stimulus and response was an action of the person, akin to a verbal response, and not a mental (i.e., metaphysical) process.

[16] Kantor admitted that if the introspectionists could not concede this fact, the two schools might never be unified. He argued he was optimistic that such a reconciliation could be reached, however, since there was already overlap between the two schools with regard to the importance of the brain.

Kantor stressed, however, that metaphysics needed to be eliminated from psychology in order for unity to be possible between the two schools of thought: "the data of psychology cannot be thought of as being anything else but actual responses of a person to specific stimuli" (p. 186). He argued that metaphysics represented a pre-scientific and theological position, which should be eliminated in favour of scientific psychology. He stated that psychology was a science similar to physical and biological sciences in that it studied objects and how they react, and only differed from those sciences in that its objects behaved differently. Kantor argued that metaphysics, if at all, should only enter into psychology when one was interpreting data—and, even then, such metaphysical conjectures should be strictly tied to data and facts. If psychology could achieve this goal, Kantor argued psychologists' would avoid arguments surrounding pre-interpretations of their data and achieve unity.

Driesch (1867-1941): The Crisis in Psychology

Hans Driesch (1867-1941), a biologist-philosopher and former Director of Philosophical Seminars at Leipzig University, began his book *The Crisis in Psychology* by arguing: "psychology, in my opinion, is the most important and the most promising of all sciences at the present time" (Driesch, 1925, p. viii). However, he admitted: "no other science today is so 'problematic' as psychology" (p. ix). In general, he argued psychology's problems stemmed from its subject matter—psychic life—which did not exist in space. This unextended quality made it impossible for psychologists to study psychic life in the same manner as natural scientists studied their subject matter. More specifically, Driesch proposed four main 'problems of the first order' related to

psychology's subject matter: 1) the problem of the fundamental laws and principles of normal psychology; 2) the mind-body problem; 3) the problem of the unconscious; and 4) the problem of psychical research (e.g., parapsychology).

First, in response to what he perceived as a lack of fundamental laws and principles for normal psychology, Driesch proposed a four-component theory.[17] The first component was psychology should start from the 'primordial fact' (or assumption): "*I have something consciously*, or, in brief: I 'know' something, knowing at the same time *that* I know" (p. 1, italics in original). Components two and three were that consciousness was static and that the dynamics of mental life were a product of unconscious forces. Finally, meaning was inherent to the elements of mental life. Driesch concluded that this four-component theory provided a strong foundation for the development of normal psychology.

Secondly, Driesch argued that psychomechanical parallelism was not a viable solution to the mind-body problem in psychology, and instead he presented his own solution, which he argued had to be divided into logical and metaphysical versions. Logically, he argued in favour of three parallels. The first was a parallel between the physical matter of the body and the body's vital essence: entelechy (a non-physical substance that governed the body's actions). The second was a parallel between the entelechy and the soul. The third was between the soul and one's 'conscious havings' (i.e., consciousness).

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[17] Driesch noted that points one and four were his own unique contributions, but that points two and three were in part a product of the work of a number of authors. He cited Oswald Külpe (1862-1915), Karl Marbe (1869-1953), August Messer (1867-1937), Bühler (1927), Narziss Ach (1871-1946), Kurt Koffka (1886-1941), and Otto Selz (1881-1943) as contributors to these two points. However, Driesch maintained that the synthesis of the four points which he developed was also his unique contribution. He also argued that the synthesis resulted in a unique, universal, scientific psychology, which could and should be applied.

Driesch then provided an example in which he argued that visual sensation began at the retina, proceeded to the brain, and then affected the entelechy (parallel one); the entelechy then affected the soul (parallel two), which resulted in 'conscious havings' (parallel three). After experiencing the conscious havings, the will—via the soul (parallel three)—would affect the entelechy (parallel two) to produce a motor response in the body (parallel one). However, metaphysically, Driesch argued: "*my soul* and my 'entelechy' are One in the sphere of the Absolute...Subjects and objects are parts of *One*, namely of *Reality*. It is not a case of Reality '*and* something else,' the subject. This would be nonsense. There is the One Reality, and it is such as to contain, as its most fundamental relation, *knowing*" (p. 147-161, italics in original). Driesch admitted that it was difficult to explain his metaphysical conception, which was why he provided the logical version. However, he called logic "a very artificial instrument" (p. 147) and referred readers to his other writings on metaphysics (e.g., Driesch, 1922, as cited in Driesch, 1925) so they could more adequately comprehend what he was suggesting with respect to his metaphysical conception.

Third, Driesch argued that the unconscious was inadequately understood and, therefore, he proposed his own theory:

> The "unconscious" belongs to that general realm of empirical being which we call the "psychical" realm of empirical existence. "Unconscious," and yet not physical, we may also say is a *concept* of theory that is presupposed to "explain." But to explain what? The answer is: The sequence of conscious contents as it *immediately* is. Thus we see that the very first step in causal psychology leads us right out of the realm of

> our immediate "possessions" into the realm of a community of *somethings* all of which are merely *meant as if* they existed. (p. 47, italics in original)

Driesch concluded that the unconscious was a component of normal psychology and was responsible for the dynamic nature of consciousness.

Finally, with respect to psychical research, Driesch posed the question: "*Are* there really 'facts' in this field?" (p. 229, italics in original). He argued that, based on his knowledge of the literature, as well as his own personal experience, there *were* facts in psychical research, and he provided a brief summary of telepathy, mind reading, clairvoyance, prophecy, telekinesis, levitation, and materialization. He also addressed immortality and the prospects for life-after-death—including living on in spirit-form, which he called "the strangest phenomenon of all parapsychology" (p. 261). In terms of *how* parapsychological acts were committed, Driesch argued: "the unconscious or subconscious sides of the mind are stronger in performing parapsychological phenomena than the Ego-side of the mind" (p. 234). He also argued that mediums—people "endowed with the faculty of performing *psychical* phenomena" (p. 233, italics in original)—possessed "abnormal faculties of acquiring knowledge and of performing actions" (p. 234).

In his conclusion, Driesch pointed out that *krisis* literally translated to 'decision' and he rhetorically asked: "[What is] on the point of being decided in modern psychology?" (p. 262). He argued that it was the direction that psychology would take as it moved toward the future, and that this direction would be determined by certain discoveries. He argued the discoveries were related to the four substantive problems he had addressed.

With respect to normal psychology, he argued that psychology was discovering meaning inherent among the elements of consciousness and, therefore, associationism was no longer adequate as an explanatory theory. With respect to the mind-body problem, Driesch argued that his proposed solution ensured that "psychomechanical parallelism will *not* raise its head again" (p. 266, italics in original). With respect to the unconscious, he argued that psychology was discovering that it was responsible for the dynamic nature of consciousness. Finally, Driesch argued that psychology was on the edge of discovering whether or not there was potential for facts and analytic formulations in psychical research. He also added that, like biology, psychology was beginning to appreciate that "the concept of the *whole* plays a *fundamental* part" (p. 267, italics in original); he argued that in place of past concepts, such as association and mechanics, psychology could place "the 'totality-concepts,' *soul and entelechy*" (p. 267, italics in original).

Driesch admitted, however, that there were some unsolved questions that psychology would have to address as it moved forward. Somewhat paradoxically, Driesch argued that there remained the need for psychology to develop an alternative to psychomechanical parallelism.[18] Related to this point, he argued psychology needed to learn much more about the brain, since very little was known at that point: "The only way, strange to say, along which definite answers might be possible, would be an experiment carried out by the physiologist or psychologist on *his own* brain" (p. 269,

[18] Since Driesch went to great lengths to describe his body-entelechy-soul-conscious havings model (logical) and the role of the Absolute in facilitating true body-mind interaction (metaphysical), it is somewhat confusing as to why Driesch did not trumpet his own approach at this crucial point of the book but instead suggested psychology should search for a new alternative.

italics in original). He also argued that psychical research remained in a pre-critical stage and that more research was needed in that field. Finally, Driesch concluded that "the greatest, though not the most 'impressive,' of all psychological enigmas stands before every human being...And the psychologist has only formulated, so far, that enigma, but has not solved it. It is the enigma of specific sensation... 'why' [do] I *hear* in one case and *see* in another[?]" (p. 271, italics in original).[19]

Bühler (1879-1963): Die Krise der Psychologie[20]

Karl Bühler (1879-1963), a former psychology faculty member at the University of Vienna, described psychology as a 'Tower of Babel' in his book *Die Krise der Psychologie* (Bühler, 1927; see also Brock, 1994). He argued that the discipline had quickly acquired, and not properly handled, a wealth of ideas, approaches, and research opportunities, which resulted in a crisis. Related to this point, he argued the crisis of psychology was a crisis of construction (i.e., of trying to found a new discipline) and not a crisis of decay (i.e., the dissolution of a discipline after a period of health).

As an indirect resolution to the crisis of psychology, Bühler offered his theory of language. He argued there were three irreducible domains of language: experiential (i.e., first person perspective, subjective), cultural (i.e., second person perspective, intersubjective), and behavioural (i.e., third person perspective, objective), and he argued

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[19] It is curious that Driesch chose to conclude with the 'enigma' of specific sensation as being fundamentally important for psychology after devoting more attention to his four 'problems of the first order' throughout the book. It certainly did not make for an 'impressive' conclusion.

[20] I am once again very grateful to Dr. Thomas Teo for explaining to me the contents of this second extremely important German source. I have also made good use of Adrian Brock's unpublished doctoral dissertation on Bühler. It should be noted, however, that I accept full responsibility for any misinterpretations or errors presented in this thesis.

the various systems in psychology corresponded to the three domains. He also argued that, by accepting the three language domains eclectically, the possibility of achieving unity in psychology increased. However, although he admitted his theory of language was eclectic, he argued that it corresponded to individuals and that individuals *experienced* the three perspectives in a unified way; and he argued psychology should also experience the three perspectives in a unified way.

Bühler concluded that, in defining the person as the ultimate subject matter of psychology, and in utilizing the three perspectives, psychology could find unity. He admitted, however, that in order for unity to be achieved, intertranslation of the three language domains would be needed. He argued such intertranslation was akin to map-making, wherein different perspectives were brought together to form one map, and not multiple maps (Brock, 1994).[21]

Vygotsky (1896-1934): The Historical Meaning of the Crisis in Psychology

Lev S. Vygotsky (1896-1934), a famous Russian psychologist and student of Alexander Luria (1902-1977) and Alexei Leontiev (1903-1979) at the University of Moscow, noted in his manuscript *The Historical Meaning of the Crisis in Psychology*: "Lately more and more voices are heard proclaiming that the problem of general psychology is a problem of the first order" (Vygotsky, 1997, p. 233, originally an

[21] In his final chapter, Bühler presented a critique of psychoanalysis. Brock (1994) argued that, since both Freud and Bühler were in Vienna, and since Bühler was "the only tenured Professor of Psychology at the University of Vienna", he "could hardly afford to ignore Freud" (p. 93). However, since the most relevant material for this thesis was presented in chapters one through three, I believe *I* can afford to ignore Freud in this instance. Anyone interested in this chapter is thus referred to either the book itself or Brock's (1994) dissertation on Bühler.

unpublished manuscript written in 1927,[22] first published in the German translation of Vygotsky's collected works in 1985). The problem, Vygotsky argued, was that psychology had reached a point where 'more of the same' in terms of "the gathering of factual material" (p. 233) would not be fruitful for psychology—it needed to choose a path. The choice—between two competing psychologies—constituted, for Vygotsky, a "methodological crisis" (p. 233).

Vygotsky admitted that some of his contemporaries[23] denied that a crisis existed in psychology. However, he argued: "[this] conception is so blind that it is of no further interest to us" (p. 292). He further argued that those psychologists were only "eclectics and popularizers of other peoples' ideas" (p. 292).

Vygotsky also argued that psychology's strong commitment to "sham empiricism" (p. 278) prevented many psychologists from seeing the true nature of the crisis:

> There is one fact that prevents all investigators from seeing the genuine state of affairs in psychology. This is the empirical character of its constructions. It must be torn off from psychology's constructions like a pellicle [i.e., thin skin], like the skin of a fruit, in order to see them as they really are. Usually empiricism is taken on trust, without further analysis. Psychology with all its diversity is treated as some fundamental scientific unity with a common basis. All disagreements are viewed as

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[22] The eminent Russian psychologist Lev S. Vygotsky (1896-1934) never published his manuscript entitled *The historical meaning of the crisis in psychology: A methodological investigation*, though it is notable that he wrote it in 1927. Contemporary writers suspect he did not publish it because of 'untimely political reasons' (Brossard, 2000). Fortunately, the manuscript was found in his private archives and published (in translated form) posthumously in his collected works (German translation 1985, Spanish translation 1991, English translation 1997). I used the English translation (Vygotsky, 1997).

[23] E.g., G. I. Chelpanov (1863-1936) and N. N. Lange (1914, as cited in Vygotsky, 1997).

> secondary phenomena which take place within this unity. But this is a false idea, an illusion. (p. 298)

He stressed that psychologists needed to cease their blind emulation of the natural sciences and recognize the true nature of the crisis.

Vygotsky argued that the true nature of the crisis was how to reconcile the objective (e.g., behaviourism) and subjective (e.g., personalism) psychologies through the development of a general psychology. He stressed that psychology needed a general psychology. He argued that, throughout its history, psychology's specialized areas had attempted to fill the role of general psychology; however, he argued the fact that specializations attempted to fill this role indicated both the need and the lack of a true general psychology.

For Vygotsky, a true general psychology "can only be defined relative to the [specializations]" (p. 254). He explained: "The general science continues the work of the special sciences...Its single difference from the special sciences is that it carries out its work with respect to a number of sciences...The general science develops out of the need to continue the work of the special sciences where these end" (p. 254). However, Vygotsky did not believe the general science stood 'above' the specialized sciences; instead, he argued that the general science emerged from—and was thus contingent upon—the specializations: "it integrates their sovereignties" (p. 256).

Specifically, with respect to the crisis of psychology, Vygotsky argued: "Only he who elevates his analysis from the level of the critical discussion of some system of views to the level of a fundamental investigation by means of the general science will understand

the objective meaning of the crisis that is taking place in psychology" (p. 257). He added:

> He will see the lawfulness of the clash of ideas and opinions that is taking place, which is determined by the development of the science itself and by the nature of the reality it studies at a given level of knowledge. Instead of a chaos of heterogeneous opinions, a motley discordance of subjective utterances, he will see an orderly blueprint of the fundamental opinions concerning the development of the science, a system of the objective tendencies which are inherent in the historical tasks brought forward by the development of the science and which act behind the backs of the various investigators and theorists with the force of a steel spring. (p. 257)

Vygotsky argued that only this kind of investigator had the potential to realize "the real and correct meaning of the catastrophe that is taking place [in psychology]" (p. 257).

After reviewing the writings of some of his contemporaries regarding the crisis, Vygotsky concluded: "there has been no *theory of the crisis* in anything so far discussed, but only subjective communiqués compiled by the staffs of the quarrelling parties" (p. 294, italics in original). He argued that a theory of the crisis was needed—one which could overcome psychology's 'sham empiricism' and illustrate 'the lawfulness of the clash of ideas and opinions that was taking place'—and he worked at developing such a theory.

Vygotsky began his theory of the crisis by arguing that psychology was in an early stage of its development as a science, which was characterized by a number of competing schools and a great deal of heterogeneity. He also argued that the need for unification

arose when sufficient material had been accumulated by the specializations of psychology. However, he stressed that unification was not achieved "merely by adding one kind of material to another, nor via the conjunction 'and'" (p. 239). Instead, he argued that "unity is reached by subordination, dominion, through the fact that different disciplines renounce their sovereignty in favour of one single general science" (p. 239). He concluded that this unity provided the function and meaning for each of the specializations.

However, although Vygotsky admitted there was a great deal of heterogeneity within the discipline, he argued that psychology's fragmentation was fundamentally dualistic: "*Two psychologies exist*—a natural scientific, materialistic one and a spiritualistic one. This thesis expresses the meaning of the crisis more correctly than the thesis about the existence of *many* psychologies" (p. 300-301, italics in original). He argued that the two psychologies were "irreconcilable types of science" (p. 301) and stressed that, although there were many competing schools of psychology, "the real struggle only takes place between two tendencies which lie and operate behind all the struggling currents" (p. 301). He further argued that psychology had always had a deeply dualistic nature and that the development of a general science—and a resolution to the crisis—would involve a rupture, not reconciliation, of the two psychologies.

In terms of instigating the rupture, Vygotsky argued:

> We view the cause of the crisis as its driving force, which is therefore not only of historical interest, but also of primary—methodological—importance, as it not only led to the development of the crisis, but continues determining its further course and

> fate. This cause lies in the development of applied psychology,[24] which has led toward the reform of the whole methodology of the science on the basis of the principle of practice, i.e., towards its transformation into a natural science. This principle is pressing psychology heavily and pushing it to split into two sciences. (p. 309)

Vygotsky concluded that, because of its ties to applied psychology, the materialistic (i.e., objective) psychology would carry the name of psychology into the future while the 'other' (i.e., subjective) psychology would be confined to the domain of Art since it could not be a science.[25]

Other Crisis and Unification Literature

Two Psychologies

Fernberger (1922), like Kantor, also discussed introspective and behavioural psychology. Fernberger argued that introspective psychology primarily used qualitative methods due to its focus on mental subject matter while behavioural psychology primarily used quantitative methods due to its focus on objective subject matter (e.g.,

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[24] Vygotsky argued that applied psychology was fundamental for deciding between the two psychologies. He argued that when psychology ventured into the practical world, it was forced to "accommodate and introduce into our science the supply of practical psychological experiences and skills which has been gathered over thousands of years" (p. 305). In doing so, psychology was forced to rethink and realign its subject matter, methods, and approaches. Specifically, Vygotsky argued that psychologists would find objective psychology far superior to subjective psychology for negotiating the practical world; he argued: "hardly any [applied psychologists] would entrust the control of a ship to the captain's inspiration or the management of a factory to the engineer's enthusiasm" (p. 306). He concluded that applied psychology was so important for overcoming psychology's crisis that psychologists "might dedicate a hymn to it" (p. 305).

[25] It is worth noting that Vygotsky died before World War II, which was when clinical psychology blossomed within the discipline. Thus he did not live to see the link which developed between applied psychology and subjective psychology via clinical psychology.

gland secretion, reaction times), which lent themselves more readily to statistical analysis. Unlike Kantor, however, Fernberger did not attempt to reconcile the two psychologies; in fact, he argued they should be split into two independent disciplines, which would be overseen by 'the old psychology' (i.e., philosophical psychology).

Weld (1928) also concluded that two psychologies existed. His two psychologies were slightly different from (though they shared some conceptual overlap with) the behavioural and introspective psychologies. The two psychologies he identified were existential and empirical. He argued that existential psychology was based on descriptive science (and therefore was limited to describing sensory experience) while empirical psychology was based on explanatory science (and therefore had as its responsibility the development of "a theory of human and animal conduct", p. 283). Weld argued that psychology dealt with three domains of subject matter: sensory experience, meaning, and behaviour; and that existential psychology focused on sensory experience only—since its goal was description—but that empirical psychology did not. Weld was sympathetic to empirical psychology—and, therefore, to studying all three subject matter domains—but rejected behaviourism as a *sufficient* empirical psychology.

McDougall (1930) used the philosophical Apollinian and Dionysian worldviews to classify the two psychologies. He described the Apollinian view as seeing man as happily in control of the world. This view also placed emphasis on mechanism, reason, and quantification, and he argued it culminated in radical behaviourism in psychology.[26]

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[26] McDougall described radical behaviourism as the "*reductio ad absurdum*" (p. 358, italics in original) of the Apollinian view.

McDougall described the Dionysian view as being less optimistic, with man being neither mechanistic nor rational; man was part of nature, not in control of it, and was subject to "blind strivings, insatiable cravings, restless urges towards goals unpredictable, ill-defined, and indefinable, forces at once destructive and creative, forces with which man's Reason was destined to struggle" (p. 355).

McDougall argued that one could and should classify the many competing schools in psychology with respect to the two views he described, and that this classification would be "more significant than any other" (p. 362). Furthermore, he argued that it was premature to believe that a purely Apollinian perspective could succeed (or not) in explaining psychological life. Therefore, he urged psychology to maintain a diversity that would allow both perspectives to prosper in the quest for explanations. McDougall concluded that psychology's "true place [was at] the crown of the biological sciences and the base of all the social sciences" (p. 362).

Problems for Developing a 'Systematic Psychology'

Wheeler (1925a, 1925b, 1925c, 1928a, 1928b) discussed five 'persistent problems' which prevented the development of a 'systematic [i.e., unified] psychology'. In the first article, Wheeler (1925a) argued that psychology's first persistent problem was that it remained tied to philosophy. He argued that psychology could not be unified "until these [philosophical] prejudices and the mischief they have wrought are forgotten and psychology redefined accordingly" (p. 191). He concluded that psychology needed to be "defined as the science of behaviour" (p. 191), and that it needed to employ "a combination of the 'objective' and introspective procedures" (p. 191).

The second persistent problem, which Wheeler (1925b) addressed in his second article, was epistemological and involved the subject-object dichotomy as it related to trying to identify psychology's 'proper datum'. Wheeler concluded:

> The datum is a relative matter, undefinable [sic] in terms of an absolute, the given. Conscious units are complexes, patterns whose limits of complexity are determined by the instruments with which they are observed. Last terms are patterns. The datum is a derived pattern. As long as the processes of derivation are uniform there is a possibility of a science. (p. 262)

The third persistent problem was the stimulus-error, which Wheeler (1925c) addressed in his third article. After a detailed consideration of the problem (i.e., observers biasing their responses due to prior knowledge of characteristics of the stimulus under observation), Wheeler argued: "the stimulus-error resolves itself into a general scientific problem of control, on the one hand, and to an inevitable process of observing, on the other" (p. 456). He added that it was only a problem "when conditions are not properly controlled or when the observer is suffering from an illusion" (p. 456).

Wheeler concluded this third article with a summary statement on the first three articles. He argued that the problems were "raised by the process-psychologist [i.e., functionalist]" and that the problems were "fictitious" and needlessly keeping "subjective and objective psychology" apart (p. 456). He concluded: "there will be no genuine psychology until the subjective and objective viewpoints are shorn of their differences" (p. 456).

In the fourth article, Wheeler (1928a) presented 'structural versus functional analysis' as the fourth persistent problem. He argued that functionalists' negative stance toward structuralists was problematic since both approaches produced mutually exclusive—but equally important—results. He further argued that structuralists provided "the 'whats' of experience" while functionalists provided "the 'whens' and the 'whys'" (p. 91). He concluded that the two approaches needed to work together, lest they each only represent "half of science" (p. 107).

In the final article, Wheeler (1928b) discussed attention and association. He explained his rationale for focusing on the two terms:

> With subjectivism abandoned, together with empirical and rationalistic attempts to account for ideas, psychological theory has become more neutral with respect to metaphysical and epistemological problems. Under these conditions do we still need the concepts of association and attention—two outgrowths of metaphysical and epistemological psychology?" (p. 16)

He concluded that, in fact, both terms were needed. Association was needed because—although it was no longer seen as a cause of behaviour—there was still the need for "the concept to mean integration or organization on the level of conscious behavior" (p. 16). Attention was also still needed in order to address how 'patterns' (i.e., unified or integrated phenomena) were perceived.

1931-1969: The Calm before the Storm

Overview

Although a couple of authors discussed a crisis in psychology (Koch, 1951; Pratt, 1942), and a couple more discussed the fragmentation of psychology (Klüver, 1949; Scriven, 1964, 1969), the 1931-1969 era was relatively quiet with respect to explicit crisis of psychology literature. However, Koch (1961, 1964) and others (e.g., Fearon, 1937; Hitt, 1969) continued to discuss psychology's fundamental dualism between subjective and objective psychology; and this dualism was beginning to be expressed in the form of 'behaviourism versus phenomenology' (Wann, 1964). The problems and prospects of unification were also discussed (e.g., Gladin, 1961; Page, 1956), sometimes under the labels 'systematic psychology' (e.g., Griffith, 1942) and 'integrational psychology' (Leuba, 1955). Although 'the crisis of psychology' was rarely discussed during this time period, and despite the fact that this time period only featured one major figure, its significance should not be overlooked—however easy that might be to do given the glut of material presented in the final time period.

Introduction

The period of 1931-1969 was nestled between two significant surges of crisis and unification literature written by major figures. The first occurred in the 1920s and featured Bühler (1927), Driesch (1925), Vygotsky (1997), and Kantor (1922). The second occurred in the 1970s and featured Staats (1970), Giorgi (1970), Royce (1970, 1976), and Kendler (1970). During the 1931-1969 time period, however, there was only one major figure: Sigmund Koch (1917-1996). Also, the phrase 'crisis of psychology' was rarely used. However, after examining the overall trajectory of the crisis and unification literature—i.e., a growing literature base from 1892-1930, followed by a

relative plateau from 1931-1969, followed by a massive increase from 1970-2005—it was deemed reasonable to present the 1931-1969 period as 'the calm before the storm'.

Major Figures

Koch (1917-1996): Psychology cannot be a Coherent Science

Post-World War II psychology in crisis. Sigmund Koch (1917-1996), a former psychology faculty member at Duke University, argued: "Since the end of World War II, psychology has been in a long and intensifying crisis" (Koch, 1951, p. 295). He argued that the crisis stemmed from a "disaffection from the theory of the recent past" and that it had never "seemed so evident that the development of a science is not an automatic forward movement, and that the *direction* of movement is a function of the plans of men" (p. 295, italics in original). Koch further argued that the crisis had resulted from "two or three decades" (p. 296) of psychology having operated in a positivistic mode: believing that it had accumulated sufficient cold, hard facts—and that theory could fill in the holes where facts were lacking—and that the theory-fact scaffold could soon lead to grand theories, capable of bringing together all of psychology's data under one nice explanatory framework. According to Koch, this mode of operation was accompanied by a firm belief in experimentation, laws, and quantification—all in search of *the* truth.

Koch also argued that the crisis in psychology was a result of an interaction between two forces. First, the pre-World War II systems of psychology had 'stagnated'. Secondly, World War II had brought psychology into the applied domain, which resulted in the need for more adequate and different theory. The combination produced a state of crisis.

In response to the crisis, Koch argued that theoretical psychology could and should be developed, and that psychology had to begin by accepting that it was "*in a pre-theoretical stage*" (p. 298, italics in original). He then argued that theoretical psychology, if developed, could serve five main functions for the discipline: 1) education in the methodology and logic of science; 2) analysis of methodological or 'foundation' problems that are more or less unique to psychology; 3) internal systematization of suggestive, but formally defective, theoretical formulations; 4) intertranslation and differential analysis of conflicting theoretical formulations; and 5) the construction of new theory (see p. 298). Since the fourth point was somewhat contrary to Koch's later writings, I would like to highlight his description of it here:

> By "intertranslation" is meant the logical and semantic exploration of different theoretical language systems with the object of locating those areas of agreement which may be hidden behind different terminological facades. By "differential analysis," I mean the location of specific areas in which "theories" imply divergent consequence, and the execution of experiments designed to test them. It has not been sufficiently appreciated that internal systematization is often a necessary prelude to the proper exercise of this dual function...Intertranslation and differential analysis will become progressively more important as theories generating consequences of comparable specificity with respect to the same empirical domain become available. (p. 299)

However, despite this initial degree of optimism, Koch went on to argue (in a manner more consistent with his later writings):

> Any assumption that diverse limited theories, taking either the same or quite different domains as subject matter, will automatically get integrated is sanguine, to say the least...Whatever integration we achieve will be purchased by repeated applications of the procedure of "intertranslation and differential analysis," by successive extension from a "core" theory...and, finally, by second-order attempts to arrive at more general postulates which would subsume the theorems of two or more limited theories. (p. 301)

Koch concluded: "...I might add that there is no reason to believe that we shall *ever* have unitary, comprehensive theory with an exhaustive range of application to all psychological phenomena" (p. 301).

Critique of behaviourism. Koch (1959a, 1959b) argued that behaviourism—and its accompanying philosophy of positivism—was losing its strength: the intervening variable paradigm was diminishing (with an accompanying increase of interest in perception), the generalizability (and number) of laws was being questioned, and the hypothetico-deductive framework was seen as problematic or incomplete. Furthermore, Koch argued the observation base of behaviourism and the stimulus-response (S-R) paradigm were having their 'objective' foundation crumble beneath them. He also argued that behavioural psychology continued to rely heavily on an outdated philosophy of science (i.e., positivism, including operational definition) and had not 'kept pace' with more recent developments in the philosophy of science literature. He concluded that psychology needed to return to its indigenous problems—ceasing its blind emulation of the natural sciences.

The 'two cultures' in psychology. Despite being an ardent critic of behaviourism, Koch was quite sensitive to the split between objective and subjective psychology. For example, he may have been the first to use C. P. Snow's (1905-1980) *The two cultures and the scientific revolution*—wherein Snow argued academia had two distinct cultures: the sciences and the humanities—to interpret the split. In doing so, Koch (1961, 1964) came to argue that psychology possessed both scientific elements (objective psychology) and humanistic elements (subjective psychology). Furthermore, Koch (1961) argued that psychology needed to have both elements integrated in a meaningful way: "No fertile integration or even interplay between science and the humanities can come about—either in individual minds or in the scholarly community as a whole—merely by juxtaposing scientific and humanistic subject matters in the same curriculum" (p. 636-637). He further argued what was needed was "a *new and more significant mode* of education" (p. 637, italics in original), which would present the two cultures in an integrated manner.

However, although Koch (1961) hoped for this development, he was not overly optimistic. He argued that, in order for an integration of the two cultures to be taught in universities, academics had to possess *both* humanistic and scientific "aptitudes and sensitivities" (p. 639). But he argued: "The absolute number of such individuals turned out by the culture at large is in itself pathetically small" and that "such individuals in general are not attracted to psychology, in that the very sensitivities at issue are what preclude their interest" (p. 639).

In fact, Koch was not overly optimistic about the possibility of unity between the two cultures in general. In addressing the split between behaviourism and phenomenology,

for example, Koch (1964) wrote an article entitled "Psychology and emerging conceptions of knowledge as unitary". However, his opening sentence read: "I can only believe that my title, submitted months ago, was the product of a burst of sabbatical-induced euphoria" (p. 1). He went on to reiterate his critique of behaviourism (Koch, 1959a/1959b) and ended with a critique of phenomenological perspectives as well. He argued that in revolting against behaviourism, many humanists were simply latching onto ready-made philosophical perspectives (e.g., existentialist philosophy). He concluded by reiterating his argument for psychology to return to addressing its indigenous problems.

Ameaningful thinking. One problem Koch (1965) saw as preventing psychology from confronting its indigenous problems was the 'pathology of ameaning':

> I will suggest that there is such a pathology in psychology, and phrase it as a tendency towards *ameaningful modes of thinking*. I mean something *very specific* by "ameaningful thinking," but it is hard to delineate in words, to dis-embed from practice (either one's own or the collective practice of the science). It will be apparent that everything I say about ameaning has already been said in some form by others. But not in a form which is truly functional—which enables us to trace the immensity of its ramifications, which gives us the kind of topographic map that we can *feel*. Otherwise there would be less of the phenomenon! (p. 55)

Despite his initial hesitation, however, Koch went on to provide a rough definition of 'ameaningful thinking': "ameaningful thought or inquiry regards knowledge as the result of 'processing' rather than of discovery; it presumes that knowledge is an almost automatic result of a gimmickry, an assembly line, a 'methodology'...it sometimes seems

to suppose that the object of inquiry is an ungainly and annoying irrelevance, that knowledge can be created by fiat" (p. 56).

Koch argued that meaningful thinking was exceedingly rare in psychology and, furthermore, that the conditions for engendering it were also rare. In fact, Koch argued psychology's mode of operation was quite ameaningful—particularly its blind emulation of the natural sciences. According to Koch, psychology was in the grips of the two main aspects of ameaningful thinking: rule-dependence and a-ontologism. He argued that rule-dependence was a belief that knowledge could be generated by a method and a-ontologism was a denial of any subject matter that could not be understood through the selected method:

> A-ontologism, then, ultimately comes down to a strange kind of subjectivism, such that in the names of *objectivity*, Science, rigor, method, the most extreme autisms are brought into being. Subject matter is conquered by evasion, distortion, or denial; the trivial becomes the profound and vice-versa—all this under the impeccable aegis of method. (p. 69)

Koch concluded that psychology must eventually address its problem of ameaning in relation to facing its indigenous problems.[27]

Psychology cannot be a coherent science. Koch's writings during this time period culminated in an article in *Psychology Today* entitled "Psychology cannot be a coherent

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[27] For Koch (1969b), part of meaningful thinking was investigating the "value properties" (p. 119) of a phenomenon—which are those elements which provide positive or negative meaning for an individual and/or attract or repel an individual from the phenomenon (at the level of meaning or interest). He argued, bitingly, that behaviourism had totally neglected this dimension of phenomena and that psychology had to move to this level of investigation.

science". In this article, Koch (1969a) argued that, starting with John Stuart Mill (1806-1873), psychology had attempted to apply the methods of the natural sciences to its subject matter. However, Koch concluded: "the Millian hypothesis...has been fulsomely disconfirmed" (p. 66)—and his rationale was multi-faceted. In terms of subject matter, he argued: "Anything so awesome as the total domain comprised by the functioning of all organisms can hardly be thought the subject matter of a coherent discipline" (p. 66). In terms of the nature of science, Koch argued 'science' was the label given to an "analytic pattern emerging first in classical modern astronomy" (p. 66). He admitted this analytical pattern had produced numerous achievements *in the natural sciences*; however, he questioned the degree of effectiveness it could have in psychology. He argued that psychologists should not *abandon* empiricism, classification, quantification, "shrewd, though-minded, and differentiated analyses", and other features of natural science (p. 67); however, he also argued "that in many fields close to the heart of the psychological studies, such concepts as 'law,' 'experiment,' 'measurement,' 'variable,' 'control,' and 'theory' do not behave as their homonyms do in the established sciences" (p. 67). Koch concluded that 'psychology' should be renamed 'the psychological studies' so that, among other things, students would "no longer be tricked by a terminological rhetoric into the belief that they are studying a single discipline or any set of specialties that can be rendered coherent, even in principle" (p. 67).

Other Crisis and Unification Literature

Crisis, Fragmentation, and Two Psychologies

Crisis. Outside of Koch's writings, 'the crisis of psychology' was a rarely used phrase during this time period. One exception, however, was Pratt (1942), who preferred to talk about 'cris*es*' in psychology: "The science of psychology has undergone a succession of...crises. It has been uncertain of its subject matter and methods, and some have reported no restrictive boundaries to prevent it from being engulfed by biology or by sociology" (p. 365). He admitted though that if psychology did have a single crisis, it was a crisis of character:

> The major crisis in psychology, however, is one of character. It is produced by the substitution of sales appeal for scientific adequacy and impartiality in the selection of content for textbooks. If past crises or pseudo-crises in psychology may be limned by the pun, "Psychology first lost its soul and then its mind," the present crisis may be expressed by the extension, "it now has no character and its reputation, according to point of view, is fabulous or scandalous". (p. 366)

Pratt went on to argue that eclecticism, and other approaches in psychology, were akin to marketing tactics, which threatened the discipline's scientific quality—particularly since psychology was then marketing itself to "the more inferior students matriculating in our colleges and universities" (p. 366).

Fragmentation. Although there was little mention of a 'crisis of psychology', there were some writings about the discipline's fragmentation. Klüver (1949), for example, discussed "the impending dismemberment of psychology" (p. 383). He argued—based on psychology's then-current state of being better understood as 'psycholog*ies*', and the discipline's history of experiencing crises (e.g., he mentioned the books by Driesch, 1925

and Bühler, 1927)—that there was little unity in psychology. However, Klüver did not lament psychology's fragmentation. On the contrary, he argued:

> ...we see no cause for regretting the "impending dismemberment" of psychology...In fact, the heterogeneity of certain fields of inquiry in "psychology" is so great that the unity often appears to be established by simply employing the *word* psychology, that is, by some form of "word magic." But neither the unity of consciousness nor word magic nor any other tenuous tie should prevent the future development of relatively independent areas of inquiry in a field once upon a time called—psychology. (p. 397-398)

Klüver went on to argue that attempting to hold onto a unity of psychology would result in new trainees having to "combine in his person the abilities and the training of a first-rate histologist, physiologist, theologian, biochemist, anthropologist, physicist, electrical engineer, etc." (p. 398). He concluded that progress in the natural sciences occurred through the "splitting off" (p. 398) of new disciplines and that, to progress, psychology should follow suit.

Although he did not advocate for the breaking off of new sub-disciplines, Scriven (1964, 1969) offered a series of arguments similar to Klüver's. His primary argument was that psychology could not achieve a paradigm analogous to Newtonian physics. Scriven (1969) argued that psychology's subject matter were vastly more complex and were of a very different nature than physics' subject matter; he added that much of psychology's subject matter—as it existed in the natural world—was uncontrollable in an

experimental sense (e.g., complex social variables). In short, the nature of psychology's subject matter precluded the development of a paradigm.

Scriven also rejected the argument that psychology was simply young or preparadigmatic and that psychology could hope to eventually achieve a paradigm. He argued that psychology had had a long history *qua* philosophy that extended back past the establishment of scientific psychology in the late 19th century; and he argued that, throughout this long history, psychology had struggled with many of the same problems that continued to plague its 20th century, scientific form. Despite its impossibility, however, Scriven admitted that psychology remained committed to searching for a paradigm, and he argued there were three main reasons for this sustained commitment: 1) the illusory benefits promised by a paradigm were too great to resist; 2) rigorous alternatives to positivism were lacking; and 3) the degree of conformism within psychology's sub-disciplines was high.

Scriven concluded that psychology was not a 'continent', but rather "an archipelago, a cluster of islands—some small, some large—which together constitute the territory of useful psychology" (p. 1). He argued that psychology should let its research be guided by practical problems—not limit its theories to single paradigms—and tolerate 'low-level approximations' while research and theory development were in progress.

Two psychologies. The theme of two psychologies (objective and subjective) remained strong in the 1931-1969 writings. I have already mentioned Koch's writings on this theme, but he was not alone in discussing this topic during this time period. The most extensive treatment of this theme was a symposium held at Rice University (Wann,

1964), which focussed on the competing schools of behaviourism (objective psychology) and phenomenology (subjective psychology). Contributors to the symposium included B. F. Skinner (1904-1990) (advocating for behaviourism), Carl Rogers (1902-1987) (advocating for phenomenology), and (as already mentioned) Koch (1964) (who was critical of both schools, but especially of behaviourism).

Although no major reconciliation was reached (see Day, 1976), the symposium at least clarified that, within psychology, there existed "two models of man", which Hitt (1969, p. 651) summarized. In terms of 'the behaviouristic model', he explained it proposed that "man can be described meaningfully in terms of his behavior; he is predictable...he lives in an objective world; he is rational...and he is knowable in scientific terms" (p. 657). In terms of 'the phenomenological model', he explained it proposed that "man can be described meaningfully in terms of his consciousness; he is unpredictable...he lives in a subjective world; he is arational...he must be studied in a holistic manner...and he is more than we can ever know about him" (p. 657). Hitt argued that both models had merit and usefulness, and he concluded that "the behaviorist and the phenomenologist should listen to each other" (p. 657).

Fearon (1937) also described two psychologies and he labelled them physical and metaphysical psychology. He argued that they both had "their own spheres" but that "neither can adequately get along without the other" (p. 25). He argued that psychologists who operated in the physical sphere employed empirical and rational methods to "study the manifestations of material substances...[the] immediate phenomena and causes of life" (p. 25). In contrast, psychologists who operated in the

metaphysical sphere "[study] human life from the philosopher's point of view" and focus on "interpreting the deeper reasons for human behaviour" (p. 26). Fearon argued that physical/experimental (he used the two words interchangeably) psychology's goal was to empirically understand the mind and its 'organs' (i.e., the brain, nervous system, etc.), while metaphysical psychology's goal was to explain *why* humans exist and do the things they do.

Cronbach (1957) discussed "the two disciplines of scientific psychology" (p. 617), which he labelled 'experimental' and 'correlational'. He admitted that Dashiell (1939) had "optimistically forecast a confluence of these two streams", but argued that the confluence was "still in the making" (p. 671). He argued that psychologists continued to limit their research to only one of the two streams, rather than to psychology as a science more generally; and he went on to argue that this single-stream dedication needed to be overcome. In support of his argument, he cited the recent integration of physics and chemistry which, he argued, was beneficial for science.

Cronbach concluded by calling for a unification of the two streams. He argued that it was "not enough for each discipline to borrow from the other" (p. 681). He argued that the correlational stream studied within-group variance while the experimental stream studied between-group variance, but that what was important was "the otherwise neglected interactions between organismic and treatment variables" (p. 681). He concluded that the unification of the two streams would ensure "that no psychologist can dismiss one or the other as error variance" (p. 683).

Chein (1966) argued that psychology continued to suffer from "socio-political schism in its ranks" (p. 333). In terms of the social element, she argued that psychologists succumbed to ingroup-outgroup distinctions, which created a great deal of interpersonal and inter-group conflict. She argued, for example, that this ingroup-outgroup process was largely responsible for the science-practice tension. In terms of the political element, she argued that groups of psychologists vied for power, and she cited the increase in power and funding given to clinical psychologists following World War II as a source of political tension between scientists and practitioners. She concluded that this socio-political conflict led to "a double standard of scientific morality" (p. 335)—where work done by one's ingroup was 'good' and by one's outgroup was 'bad'—and a resulting decline in scholarship since the work of one's outgroup was largely ignored.

Specifically, with respect to the two cultures, Chein argued that the "two clashing subcultures in psychological science" were 'scientism' and 'clinicalism'. However, she argued: "*Scientism* should not be confused with *science* because it is built around only one approach to the development of scientific knowledge...The most pervasive and fundamental aspect of what I am here calling scientism is its profound commitment to what most psychologists have been taught to identify as scientific method" (p. 337). And she argued: "Note, *clinicalism*, not clinical psychology. Many clinicians are not clinicalists and many nonclinicians are...The key to clinicalism...is its urge to be able to comprehend every instance in a domain of inquiry in all of its particularity and unique individuality" (p. 338). Chein concluded:

> The two [cultures] should welcome each other's existence...Insofar as a scientist remains a scientist, he should welcome having around one to whom the uniqueness of the individual event is precious; for, in the latter's passionate regard for the unique, there is a protection for the former against smug complacency in *over*simplification...And the clinicalist, insofar as he is genuinely concerned with comprehension, should welcome the fact that there are others around testing other approaches to comprehension. Science does not aim at impoverishing reality, but at comprehending it. (p. 341-342)

Although Watson (1967) did not argue that *two psychologies* existed, he did argue that psychology was preparadigmatic and that psychologists, instead of operating within a paradigm, were oriented by eighteen *dichotomous* 'prescriptions':

Conscious mentalism-Unconscious mentalism (emphasis on awareness of mental structure or activity—unawareness)

Contentual objectivism-Contentual subjectivism (psychological data viewed as behavior of individual—as mental structure or activity of individual)

Determinism-Indeterminism (human events completely explicable in terms of antecedents—not completely so explicable)

Empiricism-Rationalism (major, if not exclusive source of knowledge is experience—is reason)

Functionalism-Structuralism (psychological categories are activities—are contents)

Inductivism-Deductivism (investigations begun with facts or observations—with assumed established truths)

Mechanism-Vitalism (activities of living beings completely explicable by physicochemical constituents—not so explicable)

Methodological objectivism-Methodological subjectivism (use of methods open to verification by another competent observer—not so open)

Molecularism-Molarism (psychological data more aptly described in terms of relatively small units—relatively large units)

Monism-Dualism (fundamental principle or entity in universe is of one kind—is of two kinds, mind and matter)

Naturalism-Supernaturalism (nature requires for its operation and explanation only principles found within it—requires transcendent guidance as well)

Nomotheticism-Idiographicism (emphasis upon discovering general laws—upon explaining particular events or individuals)

Peripheralism-Centralism (stress upon psychological events taking place at periphery of body—within the body)

Purism-Utilitarianism (seeking of knowledge for its own sake—for its usefulness in other activities)

Quantitativism-Qualitativism (stress upon knowledge which is countable or measurable—upon that which is different in kind or essence)

Rationalism-Irrationalism (emphasis upon data supposed to follow dictates of good sense and intellect—intrusion or domination of emotive and conative factors upon intellectual processes)

Staticism-Developmentalism (emphasis upon cross-sectional view—upon changes with time)

Staticism-Dynamicism (emphasis upon enduring aspects—upon change and factors making for change). (p. 436-437)

Watson argued that the prescriptions were dynamic, that "psychologists accept, reject and combine prescriptions" (p. 438). He also argued that the prescriptions need not remain in polar opposition indefinitely; he held out the possibility that some or all of them could become synthesized. Furthermore, he argued that prescriptions were not always consciously selected by psychologists; often, psychologists simply assumed various combinations. Finally, he argued that schools in psychology were combinations of prescriptions institutionalized and adhered to by groups of psychologists; similarly, *Zeitgeists*, according to Watson, were combinations of prescriptions adhered to by individuals within a particular socio-historical era.

Systematic and Integrational Psychology

Systematic psychology. Use of the label 'systematic psychology' was on the decline during this time period; however, it was still used occasionally and the most extensive discussion of the topic was Griffith's (1943) lengthy volume entitled *Principles of Systematic Psychology*.[28] In his preface, Griffith noted: "It is almost literally true that no two men of distinction have been agreed concerning the problems, the methods, the subject matter, or the personal and social worth of what might otherwise be *the* science of psychology" (p. vii). He admitted that he was distressed by this state of affairs since the

[28] Much of Griffith's book is beyond the scope of this descriptive account; however, the preface and first chapter—entitled "Problems of Systematic Psychology"—are highly relevant.

study of psychological subject matter was "well over three generations old" if one limited it to experimental study, and "several thousand years old" if one did not limit it to experimental study (p. vii). Griffith argued that psychologists were divided with respect to psychology's direction, methods, subject matter, desirable interdisciplinary alliances (especially biology and philosophy), and more. He also argued there was even disagreement about whether psychology was, or should be, a science.

Griffith argued that it was impossible to understand the abovementioned disciplinary dynamics without having a critical appreciation of the historical trajectory which the discipline had traced. He explained that this critical appreciation would be the focus of his attempt to conduct 'a systematic appraisal of psychology'. He added that his perspective would be influenced by "a psychological or methodological behaviorism" (p. viii), though he rejected radical forms of behaviorism and positivism. He also admitted that the use of the term behaviour was potentially problematic since it could mean decidedly different things in either the laboratory or naturalistic settings. He admitted he found this 'discrepancy' frustrating.

In chapter one, Griffith discussed some 'problems of systematic psychology'. He first argued there had simply been a great variety of "conflicting points of view" (p. 1) throughout the history of psychology, and that bringing them together was no easy task. He then discussed the problem of explicating the scope of systematic psychology:

> By definition, it should be the task of a systematic approach to the problems of psychology to find a way through such mazes of fact, opinion, judgment, and practice, and to reconstruct the principles of system making as fast as the facts may

> require...But this does not mean that the principles of systematic psychology constitute a new kind of psychology, or even a special area of the whole field of possible enquiry. On the contrary, the contention is that, with respect to a wide range of data, postulates, and conclusions, an orderly pattern of basic points of reference can and must be found which will adequately define the domain of psychological science, identify its indigenous types of data, lay bare the express or implied assumptions about the domain, designate the methods to be used, and illuminate the various ways in which resulting facts and generalizations are dynamically related to one another in their role as parts of an organic unity. (p. 8-9)

Griffith then outlined two possible approaches to achieving unification from amongst psychology's diverse schools. The first was finding reference points between the various phenomena, theories, etc. and building unity via a series of bridges and interrelationships. The second was reconstructing consciousness [i.e., subjective psychology] in terms of behaviour [i.e., objective psychology] or vice versa—in other words, finding a way to reduce one set of schools to the other. Griffith also considered a pragmatic eclecticism as a possible means for loosely 'unifying' psychology but concluded: "The eclectic will roughen the edges of otherwise fine, and quite necessary, distinctions, so that heterogeneous parts really give nothing more than the appearance of fitting together" (p. 12). He also admitted that another form of eclecticism was one based on using the scientific method as a common denominator for all of psychology's research activities; however, he ultimately argued that the reference-point-integration approach was preferable: "A final, although not a finished, science can be expected to emerge from

existing classes of particulars whenever agreement is reached on critical points of reference" (p. 18). He concluded: "Somewhere among the cardinal points of reference which underlie psychological theory, there is a pattern which it is the aim of systematic psychology to appraise" (p. 18).

Griffith concluded by discussing "the position of psychology among the sciences" (p. 26). He began by noting that many people did not want psychology to be or become a science: "While yearning, perhaps, for a science of psychology, men really hope that it will never become a science. The weight of a portion of love or the orbits of the particles of a sentiment would be an intolerable sight!" (p. 29). Griffith argued that psychology *could* be viewed as a science, like any other, because—like other sciences—psychology could define and circumscribe a unique subject matter; however, he noted that cultural prejudices toward viewing the established natural sciences as sciences and psychology as a second-rate discipline were hard to overcome.

McGeoch's (1933) article on systematic psychology was entitled "The formal criteria of a systematic psychology". In introducing his article, he admitted that, for "some critics the term 'system' implies a closed, finished, somewhat dogmatic body of doctrine and the present vitality of system-making is to them a regrettable fact" (p. 1). In response to these critics, he argued that a system "need be neither finished nor dogmatic" (p. 1-2), and that systems were necessary for organizing and interpreting the facts of a science: "By a system is meant, not an *a priori* construct but a coherent and inclusive, yet flexible, organization and interpretation of the facts and special theories of psychology" (11-12). McGeoch concluded by outlining six formal criteria for any system of psychology: "(1)

A definition of the field; (2) a statement of the postulates [i.e., basic assumptions]; (3) determination of the character of the data to be studied; (4) a mind-body position; (5) principles of connection [i.e., how to relate facts], and (6) principles of selection [i.e., how to select which facts to relate]" (p. 12).

Finally, Guthrie (1950) surveyed "the status of systematic psychology" (p.97), and his chief concern was the rise of the science-practice tension. He argued that in his generation there had been "an enormous increase in the practice of psychology" (p. 98), and that this increase had had a major impact on the organization and operation of the American Psychological Association (APA). He argued the APA tried to include all psychologists within their organization, without being mindful of "possible fundamental differences in aims" (p. 98). Though Guthrie believed a science-based psychological practice was important, he concluded that psychology should remain a science, while leaving practice to other disciplines: "What I have been saying is that practice is one thing, science another, that practitioners ask whether a treatment works whereas scientists want to know how it works" (p. 100). As a solution to the science-practice tension, Guthrie proposed "that the practice of psychology...be left to physicians and social workers whose training shall be in practice, and that psychology take its place as one of the basic sciences" (p. 101). Guthrie concluded that if psychology did not keep the science and practice domains separate, the discipline's science would 'be diluted' and its practice would be 'unskilled'.

Integrational psychology. 'Systematic' was not the only alterative term for 'unified' employed during this time period. Leuba (1955) preferred the phrase 'integrational

psychology'. He lamented the lack of unity in psychology and argued: "Psychology needs facts; it needs specialization and experimentation on strictly limited topics; facts are the building stones of a science. But it also needs mortar. It needs to supplement the prevailing approaches by more numerous and significant efforts of an integrating and unifying sort" (p. 858). He argued that either specialists needed to stop from time to time to address general concerns or else "full-time, general psychologists" could be trained "who would not need to apologize for their failure to have specialized" (p. 858). Leuba concluded that, although "unifying principles are not yet clearly discernible", it was evident that such principles would have to "consider man both as a biological organism and as a product of culture" (p. 858).

Prescriptive and General Writings about Unity

During this time period, there were also a handful of articles which specifically addressed the unification of psychology. Gladin (1961), for example, argued that psychology's subject matter was dualistic (i.e., subjective and objective, based on the body-mind dualism), but argued that psychology could become a unified science:

> ...the time seems ripe for an earnest attempt at the unification of psychology into a comprehensive science. This is not a task to be accomplished by a mere compilation of yesterday's papers like so many chapters of holy writ, chaff and wheat unsorted; nor is it a task for a collection of humans with axes to grind, as so often symposia turn out to be. It is a monumental task involving the dispassionate examination of various positions without regard for the persons espousing them and in the full light of available—if not always empirical—knowledge. Also required is the critical

> examination and sorting of the appalling accumulation of insenate data whose periodical issuance is ever on the increase in an inexorable, chaotic flood...This is the imperative, the inevitable goal of a scientific psychology—one to be accomplished later if by trial and error or sooner if pursued with conscious purpose and in good faith. (p. 420)

In contrast, Dashiell (1939) argued that psychology was *already* experiencing some unifying trends or 'rapprochements'. He argued that within various research areas of psychology, convergences were taking place; in other words, phenomena were being studied from various perspectives and some cooperation and integration were beginning to take place. He also argued that there were preliminary convergences between experimental and clinical[29] psychology, which had been divided primarily due to differences in 'attitude' or goal and secondarily due to differences in method or approach. For example, he argued that experimental research was being conducted on clinical topics and populations around the United States and that the National Research Council was also organizing round-table discussions, which featured scientists and clinicians. He also argued that psychology's movement toward researching child development featured an integration of scientific and clinical perspectives.

Poffenberger and Bryan (1944) argued that unity was taking place on a professional association level. They argued that World War II was changing the landscape of psychology. One major change, which they predicted would blossom following the War,

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[29] Though it is beyond the scope of this history, it should be noted that prior to World War II, 'clinical' psychology was quite different than post-War clinical psychology.

was a drastic increase in applied psychology. Due to this nation-wide change, they explained that professional associations needed to become more unified. Although there were concerns about having a single association representing all psychologists—including whether or not it could meet the needs of applied and academic psychologists concurrently—Poffenberger and Bryan explained that the American Psychological Association was chosen to serve as a unifying association during the War, absorbing other associations. They argued that psychologists had for too long "been preoccupied with our internal schisms and semantic controversies" (p. 257) and that it was "high time, now, for the profession to come of age, mobilize its energies and resources through a unified national organization and fulfill its social responsibilities as an integrated scientific and professional group dedicated to the promotion of human welfare" (p. 257).

Ericksen (1941) defined unity as "a condition whereby the different results and theories of psychologists can show a meaningful relation to one another", and he argued that there is little to be said for "a science that dissipates its energy in fruitless controversy arising out of mutual misunderstanding" (p. 74). In terms of a 'unifying principle', he argued that psychologists focused primarily on methodology, and he noted that psychologists tended to "let their method define their subject matter" (p. 74). He argued this practice was problematic, as was the belief that quantification would somehow lead to unity: "If there is any unity to come from the use of mathematics in psychology it is certainly not from the numbers themselves but from the interpretations and use that are made of them. The quantitative method is not, therefore, in itself, any assurance of unity in psychology as a complete science" (p. 76).

Although he devoted space to critique, Ericksen also provided a positive vision of unity in psychology: "Unity in regard to the fundamental theoretical problems of the science is the only real unity that is important. For unity in this basic relation does not necessarily exclude diversity and complexity...Lack of unity results when we fail to see the inherent relations between the various approaches to the study of human behaviour" (p. 78). He concluded: "We shall find unity only insofar as we can integrate the results of the different methods used in psychology. As a matter of specific procedure each problem of research must be conceived, directed, and interpreted in terms of some fundamental frame of reference" (p. 82).

Page (1956) observed: "Is [psychology] the Babylon of the sciences? Almost, the immensity, variety and vitality suggest rather the terrifying proliferation of some uncontrolled and pathological growth than the ordered and coherent development of a rational discipline" (p. 12). However, she went on to argue that the disunity of psychology in terms of content was only an outward appearance: "[the] real unity or disunity of psychology can better be discerned from a consideration of its fundamental assumptions and its methods of enquiry than from a superficial inspection of its areas of investigation" (p. 13).

Page also argued that the identity of psychology as a science was problematic, since it reduced human nature to something that would fit within the traditional scope of natural science. She argued that a fully human psychology would be "scientific only in a much less rigorous sense" (p. 16). She then suggested that the question to which these issues led was: "Can the unity of psychology be preserved only at the expense of its humanity?"

In response to her own question, Page argued that unity would *not* be achieved "by any externally applied patching process, by a public relations approach, by a diplomatically arranged compromise, by suggestions of a dialectically creative state of uneasy coexistence based on antagonistic cooperation. The rift is too deep" (p. 17). She then offered two suggestions for approaching unity in psychology. First, psychologists should obtain a more sophisticated understanding of "the real implications of scientific methodology in psychology" (p. 17)—especially the domains within which such methodology was limited. Secondly, psychologists should acknowledge the limitations of 'psychological truth', which were also the limitations of 'scientific truth'. Page concluded that psychological knowledge represented only one aspect of what constituted a person: "Man as psychology studies him is always a fragment, an abstraction, something less than the whole man, and psychology, while it may be true psychology, is never the whole truth" (p. 19).

Narain (1952) argued it was no surprise that, when confronted with the extreme diversity of data from various schools of psychology, "the student of psychology is more at a loss with the field of his so-called science than the student of any other subject" (p. 128). He added that the fragmentation of psychology took away from the discipline's credibility:

> The fact that psychologists, like philosophers, espouse systems and belong to schools, detracts from their scientific status. It is an open secret that psychologists are looked askance in scientific gatherings, and treated as half-castes. If psychologists,

> therefore, aspire to command greater prestige in the scientific fraternity, they should sink their sectarian differences, and present a unified front. (p. 129)

He argued such unity would include educational curricula and training practices.

Narain proposed that disunity was caused by at least five main factors: "personal ambition" (p. 130); the "youth of psychology" (p. 130); a "partisan spirit" (p. 130); the "exploitation of a principle" (p. 130) (i.e., a certain finding is made, which is then made to serve as the basis for a new school of thought); and "philosophical predilections" (p. 131) (i.e., psychology had only recently separated from philosophy and, therefore, many psychologists still received training in philosophy and, therefore, were prone to becoming concerned with philosophical issues). He also proposed that disunity could be curbed by adopting three controls: insisting upon the use of rigorous operationism for the introduction of new terms; making use of "an authoritative dictionary of psychology" (p. 131); and increasing the amount of 'laboratory training' while minimizing the amount of training in philosophy. He concluded: "The antidote to partisan loyalty is loyalty to facts" (p. 131).

1970-2005: The Explosion of the Crisis and Unification Literature

Overview

The crisis and unification literature exploded after 1970. Twelve major figures are featured in the final time period, and a plethora of other authors contributed their thoughts on the topic as well. However, many of the same themes from the previous time periods continued to be discussed; and, specifically, the theme of 'two cultures' (i.e., scientific versus humanistic) or 'two psychologies' (i.e., objective versus subjective)

continued to be prominent during this time period. Some new themes did emerge though, including concern with 'preconditions' (e.g., a common definition of unity) and 'levels' (e.g., methodology, theory, subject matter) of unification, as well as with social and culture factors related to unity.[30] Some of the language used during this time period also changed slightly; for example, the phrase 'crisis of psychology' was revitalized after some stagnation in the previous time period, while the phrase 'systematic psychology' was largely forgotten. Finally, although the explosion of crisis and unification literature suggests an increased concern with the topic, authors were beginning to note an ironic state of affairs: the literature on fragmentation was *itself* becoming extremely fragmented (Drob, 2003; Koch, 1993; Teo, 1999; Yanchar & Slife, 1997a).

Introduction

The final time period of this history featured a dramatic increase in the quantity of crisis and unification writings. This increase in literature was produced by twelve major figures and a substantial amount of other writers. One notable major figure from this time period was Arthur Staats (1970, 1983) who published more crisis and unification writings than any other major figure featured in this descriptive account of the literature. With his frequently-cited book *Psychology's Crisis of Disunity*, Staats (1983) was also one of the writers who helped revitalize the phrase 'crisis of psychology' during this time period. I will discuss Staats' contributions shortly, but will begin with Kantor's (1979,

[30] Although, in general, this was a theme which only received attention in the third time period, it should be pointed out that Vygotsky (1997) discussed the importance of socio-cultural factors as early as 1927.

1983) writings since his first contribution to the crisis and unification literature was featured in the first time period (Kantor, 1922).

Major Figures

Kantor (1888-1984): Science, Psychology, and Unity

Jacob Kantor (1888-1984) lived through much of psychology's struggles to legitimate itself as an independent science; thus, although I mentioned his first contribution (Kantor, 1922) to the crisis and unification literature in the first time period, I am also able to discuss his final contributions (Kantor, 1979, 1983, 1984; Observer, 1971[31], 1982) in this final time period. One of his contributions (Observer, 1971) featured an examination of "some of the favourable and unfavourable conditions and consequences of the unity and disunity in science" (p. 565). In this article, he argued that disunity in science due to subject matter—in a division-of-labour sense—was "potentially fruitful" (p. 565); however, he argued this was not the kind of disunity that existed in psychology. Rather, Kantor argued psychology still retained its 'cultural traditions'—stemming from 'religious roots'—which included mentalism and metaphysics; and he argued these approaches to psychology introduced a problematic disunity to the discipline. He further argued that psychology had tried to attach itself to physiology in the interest of acquiring scientific status, but that it had absorbed from physiology mentalism and metaphysics (i.e., the mind-body problem): "Instead of aiding psychology to be scientific, physiology perpetuates spiritistic thinking" (p. 565). According to Kantor, psychology needed to

[31] Kantor founded the journal *The Psychological Record* and published numerous 'comments and queries' in it under the pseudonym 'Observer' (Mountjoy & Hansor, 1989).

purge itself of mentalism and metaphysics in order to establish itself as a true science, replete with general laws, theories, and explanations. He argued this would also result in greater unity for the discipline.

Kantor (1979) also argued that a science examined 'things and events' which existed in space and time. This examination included the origins and evolution of, and relationships between, these 'things and events'. Following this definition of science, Kantor concluded psychology was not a science. He argued that the discipline permitted too many "transcendental presuppositions" (p. 159), and that it viewed the behaviour of organisms as an independent, as opposed to a dependent, variable—thus endorsing "a discredited ontology of cause" (p. 159). He concluded that, by understanding its subject matter to be the 'interbehaviour' of organisms (i.e., an organism's behaviour in response to other organisms and the environment), psychology could be a science: "What is required for psychology to be a science is to throw off the spiritistic shroud which conceals what is actually occurring, because of the reverence for outworn dogmas, and to abide by the investigation of confrontable events" (p. 161).

Kantor (1983) also addressed the importance of "system analysis in science and in psychology" (p. 301). He argued that any event or object, from the atomic level up to the social, involved an organized system. According to Kantor, systems were made up of components in relationship to each other, and he argued that, since phenomena existed in systems, science in general, and psychology in particular, should take a system analysis approach to the study of these phenomena. However, he chastised psychology for being 'indifferent or ignorant' regarding the importance of seeing phenomena as existing in

systems. Furthermore, Kantor stressed that, in order for a systems approach to work, psychology needed to agree on "valid basic assumptions" (p. 301)—which could not include mentalism and metaphysics. He argued the subject matter of psychology should be defined as: "no other than systems of interbehavior, that is, fields of interaction between organisms, stimulus objects, media of contact, and conditions favoring or restraining the interbehavior" (p. 307). Furthermore, he argued the methodology of psychology should conform to the assumptions and subject matter thus defined. In short, he argued experimentation would be the prized method. He concluded that the use of system analysis in psychology could finally separate it from metaphysics, establish it as a science, and provide it with unity.

Despite this degree of optimism, Kantor (Observer, 1982) argued: "In 1922, Kantor (1922) (see also Observer, 1971) made a strong appeal for unity in psychology, but that was simply a lonely cry in the wilderness. The mythicism of mentality rather than the activities of organisms has shown no sign of retreat" (p. 292). He then went on to argue that nature did not exhibit any disunity, so neither should science; and he added that, for psychology to be a science, its subject matter had to be observable in nature. He concluded that psychology would not be a unified science "until students of interbehavior join into a unity of realization that no science can tolerate the belief in supernormal happenings which are nothing other than verbal assertions that have been institutionalized as cultural traditions" (p. 295).

Dixon (1983), however, took exception to Kantor's arguments and presented a defense of disunity in psychology. His primary argument was that pluralism (ontological and

epistemological) was "an important precondition of scientific progress" (p. 337). He argued that psychology's subject matter was complex and—due to limitations on human reason—difficult to pin down; as a result, pluralism in psychology was necessary for scientific growth—and he cited writings by Karl Popper (1902-1994) and Paul Feyerabend (1924-1994) in support of his argument. Also, in response to Kantor's attack on mentalism, Dixon argued: "That mentalism has 'shown no sign of retreat' is not unexpected; similarly, one should not marvel that in the mentalism camp partisans declaim the intransigence of naturalistic psychologists" (p. 338). Finally, Dixon concluded that the ultimate benefit of Kantor's work would come through being one perspective amongst many, and through stimulating both supporting and contrasting research. He called this process "the essential tension of scientific development" (p. 339).

Kantor (1984) responded vehemently to Dixon's article. He began by pointing out that he was fine with disunity in human life in general, but what was in question was the aim of a science and, therefore, unity was necessary. He then argued that Dixon was simply providing a 'smokescreen' for reintroducing metaphysics into psychology: "He is basically interested only in transcendentalism. This is evidenced by basing himself on the fully discredited, irrational, and antiscientific writers such as Popper and Feyerabend who teach that there are no positive reasons for believing in any scientific theory or indeed that science ever leads to any knowledge about the natural world" (p. 70). He called this 'tactic' by Dixon a "pitiful means" (p. 70) for trying to salvage metaphysics in psychology. He also referred to Popper, Feyerabend, Kuhn (1962), and others as being

"metaphysicians" who included "mentalisms" which "exist only in the fictional verbalisms of the person who utters them" (p. 70). Kantor concluded by referring to Dixon's argument as a "scandal of polluting science, philosophy, and psychology with superstition and rhetorical legerdemain" (p. 71).

Lee (1985)[32] responded to the Dixon-Kantor exchange with an attempt to find a middle ground between the two positions. She agreed with Dixon that psychology was pluralistic and she attributed the pluralism to psychology being "preparadigmatic" (p. 288) or "protoscientific" (p. 287). However, she agreed with Kantor in that the pluralism was not guaranteed to remain indefinitely—especially with respect to the "fundamental matters" (p. 290) of the discipline (i.e., subject matter, methodology, and direction). She argued that psychology could not hide behind its diverse subject matter with respect to the fundamentals of the discipline since, when compared to other disciplines' diversity of subject matter, psychology's was "unextraordinary" (p. 290). She concluded that the unity of fundamentals in psychology was tied to psychology being recognized as a science, but that diversity on non-fundamental issues could remain. She added that if psychology could accomplish this goal, it could move from having the "trappings of science" (p. 290) (e.g., journals, conferences, etc.) to actually being one.

Koch (1917-1996): The Psychological Studies

In commentating on the wealth of crisis and unification literature that had been written, Sigmund Koch (1917-1996) argued: "The integration, integratability, coherence,

[32] It is worth noting that this article was published following Kantor's death. If he had still been alive it is likely that he would have responded. Unfortunately, Lee's article represents the end of this series of exchanges.

or unity of psychology—whether as a scientific or some kind of sui generis discipline—has been questioned in so many ways that one might raise second-order questions concerning the integratability of the critiques" (Koch, 1993, p. 903). However, this observation on the wealth of material that had been produced did not stop him from continuing to add to it a number of important arguments.

For example, Koch (1976) argued that psychology's crisis was perennial and that it was a necessary consequence of trying to unify an extremely diverse range of subject matter and theories. He admitted that the crisis intensified again following World War II in response to both a dissatisfaction with pre-War theories and the need for different theories in order to operate in the applied domains in which psychologists were beginning to engage. However, Koch argued that psychology's crisis had a long history and was tied to three premature commitments psychology had made: choosing methods before subject matter, institution before content, and a commitment to science before human phenomena.

Koch (1976) also argued psychology in general, and behaviourism in particular, continued to endorse an outdated philosophy of science, including operationism, which the natural sciences had already abandoned. In discussing psychology's primary commitment to being viewed as a science, Koch (1971) argued: "We have thus been given—for more than 100 years—a vast a varied set of prolegomena to action [for overcoming the crisis of psychology]. But very little action. Only one element has remained broadly constant in this rich tradition of Babel: the practice of decorating (and justifying) each formulation with the iconology of science" (p. 671).

Koch (1971, 1974, 1981) further argued that psychology had failed to capitalize on developments in the philosophy of science community that could have provided important directions for the discipline (e.g., post-Kuhnian philosophy of science). Instead, he argued psychology continued to engage in a process of "ameaningful thinking", which involved a strict "method-fetishism" and "a-ontologism" (Koch, 1981, p. 260). Essentially, this meant psychologists believed they could gain knowledge automatically from applying methods instead of thinking critically about—and *interpreting*—their subject matter and data.

Koch (1973) was particularly critical of behaviourism and he argued that psychology needed to realize that 'facts' could no longer be thought of as existing independently of values, interpretations, and contexts. Koch (1974) even went so far as to argue that—after critiquing it for a quarter of a century—he had delivered the death-blow to behaviourism: "In my humble opinion, behaviourism is finished. If there is residual motility, it is only that the corpse does not understand my arguments" (p. 4).

Despite being intensely critical of behaviourism, Koch (1992a) laid a portion of the blame for psychology's desire to be seen as a science at the feet of 20th century American culture:

> ...America's founding heritage of Enlightenment values, its almost explicitly "experimentalist" ideology in respect to the translation of such values into social practice, the practical and activist strain of national sensibility induced by the opportunities and challenges of a dynamically expanding and indefinitely expansible

frontier—all these were among the broad conditions favoring high receptivity to a "science" that seemed to promise prediction and control of human affairs. (p. 22)

However, Koch remained quite sceptical about whether or not psychology could fulfill this desire to be a science.

In fact, one of Koch's (1971, 1974, 1978, 1981, 1992b, 1992c, 1993) central arguments was that *psychology could not be an independent discipline—especially from philosophy—nor a true science*.[33] He argued that some aspects of psychology were scientific, but others were distinctly more akin to the humanities. He primarily advocated for psychology to reorganize itself as 'the psychological studies'—a move which would necessitate changes in pedagogy that would result in psychology being presented as a collection of disciplines, as opposed to a single discipline.

Koch (1981) also argued that psychologists needed to accept the 'antinomalities' of their subject matter: "Antinomality, in sum, is at the basis of the endemic human need for crawling into cozy conceptual boxes—any box, so long as it gives promise of relieving the pains of cognitive uncertainty or easing problematic tension" (p. 264). He further argued that 'crawling into cozy conceptual boxes' had an isolating effect on psychologists who chose to do so: "...having climbed into our conceptual box...we are prepared to defend our happy domicile to the death—meaning, in the typical instance, *your* death. It is not that we don't want you to join us inside...it's just that we don't want

[33] Farrell (1978) disagreed with Koch (1974) and argued that psychology was simply preparadigmatic and that the possibility of a paradigm was not impossible *in principle* as Koch suggested; however, Koch (1976) had examined Kuhn's writings on paradigms—particularly Kuhn's later writings—and argued that Kuhn was being misinterpreted by many psychologists and that, in fact, a paradigm was not necessarily in psychology's future.

you tampering with our box or suggesting—by your location in another one—that there are other places in which to live" (p. 264-265, italics in original). Koch argued that the solution to the conceptual-box problem was to engage in *meaningful thinking*, which meant confronting the subject matter as it existed instead of attempting to force it into conceptual boxes.

Koch (1976) concluded that psychology needed many different 'language communities' (i.e., groups of researchers, investigating a set of phenomena, who develop their own language for describing, understanding, and explaining the phenomena). He argued that a language community could be as small as two people and that it needed to be this way to ensure that no important meanings were lost. He admitted that the various language communities eventually became incommensurable, isolated 'search cells'; however, he rejected a relativist epistemology (1976, 1992c, 1993). That is, he argued that not all positions were equal and evaluation was still important. However, he argued that there were no immediately-given criteria for evaluation and that psychologists would have to simply do the best they could. He concluded that psychologists needed to become *humans* first (as opposed to specialized psychologists) and utilize *meaningful thinking* when evaluating different theories.

Staats (born 1924): Psychology's Crisis of Disunity

Arthur Staats (born 1924), a professor emeritus of psychology at the University of Hawaii at Manoa, consistently argued that psychology perennially struggled with a crisis of disunity/fragmentation (Staats, 1981, 1983, 1985b, 1987c, 1996a, 1999). He argued that psychology was a science and—like other sciences—it moved from an initial state of

disunity toward a state of unity;[34] however—unlike other, more paradigmatic, sciences—psychology did not develop when very few people were working within the discipline. In contrast, Staats (1983, 1985b, 1986b, 1987c, 1988c, 1991, 1995, 2005) argued psychology was a 'modern disunified science' and, as a result, the discipline faced unique challenges (e.g., trying to cope with many researchers using sophisticated technology to generate a massive amount of literature in a comparatively short period of time).[35] Staats (1970, 1988b) admitted that psychology had the 'accoutrements' (i.e., equipment, modes of production) of science, but argued the discipline had no sense of direction, which led to a general research strategy characterized by "empirically-centered aimlessness" (Staats, 1987c, p. 297). Staats (1993b, 1995, 2005) argued that, to be a 'full' science, psychology needed to become unified; and Staats (1993a) argued that unification would also resolve psychology's science-practice schism.

Staats (1985b, 1999) argued that one of the central *causes* of psychology's fragmentation was the massive amount of diverse knowledge which had been produced in a very short period of time. He argued that the result was chaos; and the challenge of integrating this knowledge and producing unity grew exponentially with each passing year (Staats, 1999). Staats (1981, 1983, 1993b) added that compounding this problem was the issue that the discipline was characterized by 'separatism', which meant a general sense of alienation and isolation experienced amongst its members and specialized areas.

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[34] Staats (1991, 1995, 1999, 2004, 2005) called this the 'disunity-unity dimension'.

[35] McNally (1992) disagreed with Staats' 'crisis of disunity' in psychology; instead, he argued Staats' 'disunity' was actually healthy diversity or 'speciation' and a sign of scientific growth. He cited Kuhn's more contemporary writings (e.g., Kuhn, 1991, as cited in McNally, 1992) in support of his argument.

However, Staats (1999) argued there were other causes of fragmentation as well. For example, psychology's subject matter was complex, unobservable, numerous, and dynamic. It also covered a broad scope. Also, Staats (1989, 1999) argued psychology artificially produced a 'disunified mode of operation'. In other words, Staats (1999, 2004) argued psychology did not possess a framework for unity—in fact, unity was not even a goal for psychology (Staats, 1985b, 1987c, 1988c, 1995, 1999)—and, as a result, chaotic diversity was allowed to flourish (Staats, 1988c, 1995, 1999). Staats (1987c, 1989) added that this chaotic diversity left psychology susceptible to 'fads' while, in contrast, he argued that other disciplines, such as physics, would not tolerate such faddishness nor would it tolerate multiple terms for the same phenomenon as psychology did (Staats, 1999). Finally, Staats (1987e, 1999) argued that psychology's focus on novelty and experimentation—to the exclusion of integrative theoretical work—also contributed to psychology's fragmentation, as did various social and organizational forces which reinforced these primary emphases.[36]

Staats' (1991) summarized his stance with respect to the nature of psychology's fragmentation quite clearly:

> One thing is clear at this point: Chaotic knowledge—inconsistent, non-consensual, faddish, disorganized, unrelated, redundant—is not effective scientific knowledge. Its disunification is an embarrassment to both the science and the profession. It is

[36] In terms of causes of disunity, Schneider (1992) added that a pervasive mutual misunderstanding of each others' work also contributed to disunity in psychology. Along these lines, she argued Staats (1991) had misrepresented much of Skinner's (e.g., 1953, as cited in Schneider, 1992) work in his article and that this kind of misrepresentation was responsible for continued disputes. She also noted that not all psychologists agreed that psychology should utilize natural scientific methods and perspectives, although she felt psychology should.

> clear that no matter how many well-conducted experiments psychology produces, no matter the refinement of methods of data production and analysis, no matter how sophisticated the specialized apparatus and theory construction, as long as psychology's products are inconsistent, unrelated, and mutually discrediting, psychology will be considered "a would-be scientific discipline". (p. 910)

To combat this disunity, Staats (1983, 1985b, 1988c, 1995, 1999) argued that psychology needed to develop a framework for unity, and that the starting point for this framework would be to make unity a primary goal for the discipline. From there, he argued psychology needed to devote programs, training opportunities, funding, journals, and overall general support for unification efforts; and he added that the divisions of general, historical, and theoretical psychology of the American Psychological Association should be responsible for overseeing unification efforts. Furthermore, Staats (1987c, 1988a, 1988c) argued that psychology needed to cultivate a new philosophy of science to deal with issues which were indigenous to psychology. Staats (1988a) concluded there was no single solution that would provide unity; instead, there was a need to tackle the problem from a variety of angles simultaneously.[37]

One angle which Staats' (1981, 1983, 1985b, 1985d, 1986a, 1986b, 1987c, 1988c, 1989, 1991, 1993a, 1999) consistently encouraged was for a great deal of theoretical

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[37] Green (1992) disagreed and argued that unity should not be sought *a priori*. He noted: "Unification has been the outcome of centuries of work in the physical sciences" (p. 1057) and he argued that this success in the natural sciences resulted in psychologists' desiring unity for its own sake. However, he argued that this *a priori* striving for unity was a mistake and psychologists should instead focus on "having better psychological science" (p. 1057). He concluded unity will come for psychology as a natural product of its research efforts or not at all.

work to be conducted.[38] This work included the theoretical unification of: concepts, to eliminate redundancy; subject matter, to establish relationships amongst phenomena; methodologies, to eliminate methodological disputes; schisms (e.g., nature-nurture), to eliminate philosophical disputes; general theoretical frameworks (i.e., research paradigms), to integrate psychology's specialized areas; and citations, to integrate psychology's literature. He added that intensive research reviews and 'bridging (i.e., unifying) theory' would be needed to achieve these theory goals. Staats (1987a) concluded that, when developing criteria for evaluating competing theories, the goal of unity should be given primary consideration.[39]

Staats (1983, 1991) also argued that psychology should not give up on pursuing grand unified theory, despite the discipline's history of failed attempts—and, he led by example in this regard. He proposed what he referred to as a bidirectional, multilevel unified theory (Staats, 1975, 1981, 1983, 1996a), and in the course of his writing, he interchangeably referred to this theory as 'social behaviourism' (Staats, 1970, 1975), 'unified positivism' (Staats, 1986b, 1987c, 1991, 1995), and 'psychological behaviourism' (Staats, 1995, 1996a, 1996b, 1996c).[40] Staats (1996a, 1998) argued that

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[38] Kukla (1992) argued that Staats' theoretical strategies in general and his unified positivism in particular were too limited for scientific progress. He argued that sciences needed to use "diverse theoretical strategies" (p. 1055) to accomplish theoretical unifications. He cited the unification strategy—which did *not* follow unified positivism—that created the kinetic-molecular theory in support of his argument.

[39] In a response to Staats' (1991) article, Kunkel argued that theories were too "complex and quite amorphous in form and content" (p. 1058) and, therefore, the 'units of unification' should be individual propositions.

[40] In reviewing Staats' (1983) *Psychology's crisis of disunity*, Baars (1984, 1985a, 1985b) argued that cognitive science was already providing a unifying framework for psychology. He criticized Staats' (1983) approach, calling it simply another behaviourism. He argued that contemporary psychologists should reject Staats' "atheoretical eclecticism" (1985b, p. 421). Staats (1985a, 1985c) responded that Baars was simply

psychological behaviourism (PB)[41] was a suitable name for this unified model since the fundamental level was the basic learning level (i.e., behaviourism) but each of the other areas were linked to this basic level,[42] thus making it more than *just* a behaviourism (i.e., psychological).[43] Staats (1996a, 1998) argued that previous behaviourisms had failed because they failed to do justice to the complexities of human psychological subject matter. Furthermore, previous behaviourisms were based on a model of logical positivism, which Staats (1987c, 1996a, 1998) argued was problematic since psychology's preparadigmatic nature prevented it from fitting a logical positivist axiomatic framework. However, Staats argued psychology should retain the strengths of positivism (e.g., experimentation, observation)—hence the name unified *positivism*—and he also argued that both classical and operant behaviourism represented fundamental

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responding from his own specialized viewpoint and was not taking the generalist perspective necessary for addressing unity issues.

[41] Yanchar (1998) published a review of Staats' (1996a) Psychological behaviorism. He argued that Staats assumed a three-part thesis: 1) the central task of science was to reduce and simplify diversity; 2) unified positivism is the philosophy of science best suited to perform this task in psychology; and 3) psychological behaviourism was the overarching theory that could unify psychology. Yanchar argued that many would be willing to support the goal of unity, but not necessarily Staats' own attempt. He argued that Staats began with a behaviouristic starting point which excluded many important perspectives, including hermeneutic psychology. He concluded that a hermeneutical approach was superior to Staats' search for basic principles.

[42] In this theory, he argued that the various research areas of psychology could be aligned in the following hierarchy (from bottom-up): biological bases, basic learning theory, human learning, child development, personality and emotion, psychological measurement, abnormal psychology, social psychology, and applied psychology.

[43] Throughout all of his writings, Staats was vague as to whether or not higher levels of his hierarchical model were to be reduced to the basic learning level. At times he argued each subsequent level enhanced the basic learning level, but Staats (1970, 1975) had also argued for reductionism: "Theoretical level by theoretical level, the elementary principles are elaborated to deal with human behavior. Conversely, the behaviors of humans are reduced to the more elementary principles of animal learning" (1975, p. 566; see also, Staats, 1970).

insights that could be extended to serve as an explanatory basis for the other levels in his model.

Staats (1975, 1981, 1983, 1987c, 1998) also argued that his model had the potential to reconcile psychology's various schisms, including: subjectivism-objectivism; holism-atomism; naturalistic observation-laboratory observation; ideographism-nomotheticism; qualitativism-quantitativism; understanding-prediction/control; indeterminism-determinism; agency-mechanism; self development-conditioning; values in science-valueless science; applied research-basic research; purposive behaviour-prior and present causation; self-awareness-conditioning; and nature-nurture (see Staats, 1975, p. 462) .[44] Staats (1987b) argued these schisms were at the basis of the divide between humanism and behaviourism, and to overcome this divide, he argued each side needed to cease its dichotomozing and instead try to build bridges that would bring them together.

In general, Staats (1996a) argued psychology had the elements for a unified science, but that these elements needed to be woven together. He also argued that psychology needed to cultivate a balance between new, diversifying research findings and synthetic, unifying theoretical work (Staats, 1986b). Finally, Staats (1983, 1987c) argued that psychology had previously had only one revolution: its separation from philosophy. He concluded that the second revolution in psychology would be the revolution to unity.

Giorgi (born 1931): Psychology as a Human Science

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[44] It should be noted, however, that Staats (1983, 1988b) defined personality as a set of 'behavioural repertoires' which were established through 'prior conditioning' and he defined intentionality/free will as 'unconscious prior conditioning'. Furthermore, Staats (1983) conveniently avoided explaining why the basic learning level was to be primary in his hierarchical model. He argued that the explanation was too extensive to discuss in his book (which was over 300 pages).

Amedeo Giorgi (born 1931), a psychology faculty member at the Saybrook Graduate School and Research Center, argued that psychology's strict emulation of the natural sciences was responsible for the crisis of psychology (Giorgi, 1970, 1984). He argued that the natural science model was insufficient for explaining the entire scope of psychological subject matter and, as a result, its strict emulation set up a split between researchers who made a primary commitment to the methods of natural science and those who wanted to address the entire range of subject matter. Giorgi (1970, 2000) further argued that the natural science model should be abandoned in favour of a human science model, which would feature an expanded philosophy of science based on phenomenological-psychological principles,[45] as well as broadened definitions of science and objectivity. Giorgi (1974) concluded that Maurice Merleau-Ponty's (1908-1961) metapsychology (i.e., a phenomenological psychology) could provide the basis for unity in psychology and he argued it could reconstruct and unify Gestalt psychology, psychoanalysis, behaviourism, and social psychology.[46]

Giorgi (1976) also argued that the crisis of psychology was a *theoretical* crisis and that it had three main components: disunity/fragmentation; psychological research lacking relevance to human life; and psychology emulating an outdated natural science model. He suggested that if psychology was *really* going to emulate the natural sciences,

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[45] MacLeod (1970) argued that "a careful and rigorous descriptive analysis of experience should precede any attempt to develop a psychological system" (p. 261). Like Giorgi, he also believed that phenomenological principles and perspectives were important for psychology as a human science.

[46] It is worth noting that Giorgi did not include physiological psychology or similar natural scientific psychologies. He made it implicitly or explicitly clear, throughout his writings, that his version of a unified psychology would not include these natural scientifically-oriented psychologies.

it should emulate the *development*—and not the *outcome/contemporary status*—of the natural sciences. However, Giorgi (1970, 1990, 1992) stressed that, to be a science, psychology didn't have to emulate the natural sciences at all; it was sufficient to engage in methodical, systematic, and critical research. Furthermore, he argued that the subject matter of psychology—if explicitly outlined/circumscribed—could define the scope of the discipline and provide a basis for unity. However, in terms of subject matter, Giorgi (1982) argued that behaviour, consciousness, and experience were insufficient descriptors; instead, he offered the term 'expressiveness', which he argued implicated internal consciousness and experience, as well as external behaviour. Giorgi (1990, 1992) concluded that psychology should focus on the fundamental issues related to its subject matter, which he argued it had suppressed in favour of a primary commitment to the methods of natural science.

Specifically with respect to unity, Giorgi (1985) examined two contrasting definitions: uniformity and concinnity. He argued that psychologists in favour of unity tended to mean concinnity (i.e., diversity in service of a common goal) while those against unity tended to mean uniformity, which corresponded to their fear that diversity would be lost if psychology was to become unified. Giorgi further distinguished between concinnous and disconcinnous diversity, and he argued that concinnous diversity (i.e., diversity in service of a common goal) was beneficial for psychology but that disconcinnous diversity (i.e., unrelated diversity) was essentially problematic fragmentation.

Royce (1921-1989): At the Crossroads between the Sciences and Humanities

Joseph Royce (1921-1989), a former psychology faculty member at the University of Alberta, argued that psychology had operated under the belief that it was a single, independent natural science; however, he argued that, following World War II, the natural scientific model could not account for many psychological phenomena which were of primary concern (e.g., psychological conditions of returning veterans). He further argued that, combined with the rise of clinical and applied psychology, this inability of the natural science model to account for certain phenomena was leading to numerous conflicts which were beginning to take place about the nature of psychology. In response to these tensions, Royce proposed that psychology existed 'between the sciences and the humanities' and, as a result, psychology would have to allow for different kinds of epistemological criteria to co-exist if it wanted to do justice to the totality of human psychological subject matter (Royce, 1965[47]).

Royce (1970b) edited *Toward Unification in Psychology*,[48] which contained the proceedings of the founding conference of the theoretical psychology program which he founded at the University of Alberta.[49] In this volume, Royce (1970a) provided a chapter in which he argued there were three paths to knowledge in psychology: rationalism (logical), empiricism (perceptual), and metaphorism (symbolic). He also surveyed

[47] I have included Royce's (1965) article in this 1970-2005 section since it was deemed too short of an article to warrant creating a whole section on Royce as a major figure in the 1931-1969 section.

[48] In his epilogue to this volume, Krech (1970) noted that the initial optimism surrounding the conference had diminished. He argued that the unification of psychology was impossible—he referred to psychology as a "potpourri" (p. 300)—but admitted that integration efforts could still benefit the discipline by increasing the *degree* of unity.

[49] In a review of the volume, Shaw (1972) argued that "the collection of papers assembled do seem to reflect the disunity of our science more than any supposed unity in either purpose or methods" (p. 75). He concluded: "Ignore the title, and read each article for its own worth" (p. 75).

approaches to theory construction in psychology and came to three conclusions: theories with a strong empirical base and high degree of formalism were the most powerful and desirable; descriptive and correlational approaches had a good empirical base, but had a low degree of formalism and, therefore, were less powerful; and phenomenological-speculative approaches were low on empiricism and formalism and therefore were the least powerful and desirable (see also, Royce, 1977). He concluded that psychology should focus on area-specific unifications first and then look to develop bridges between areas in the future.

Royce (1976) went on to argue: "Psychology is multi-: methodological, variate, epistemic, worldview, systemic, paradigmatic, theoretic, and disciplinary". He argued that psychology had been blind to its own multidimensional nature and that it continued to place mistaken trust in bivariate approaches. He suggested that the natural sciences had had success with bivariate approaches—despite dealing with multivariate systems—because the systems they were dealing with were relatively closed and nomothetic; however, in contrast, psychology, in aspiring to emulate the natural sciences' success, was blind to the fact that psychological systems were relatively dynamic and idiographic. He concluded that psychology needed to develop an indigenous philosophy, which would locate psychology between the humanities and sciences and do justice to its multiplicities.

Royce (1977) also argued that psychology needed to repair its ties with philosophy since post-Kuhnian philosophy of science had demonstrated that facts could not be understood apart from theoretical and contextual frameworks. He criticized psychology's

dearth of theoretical and philosophical programs and training, and argued psychology should develop these areas to address the discipline's theoretical fragmentation. However, he admitted that psychology's fragmentation was partly generated by social history and psychology having lacked "an articulated theory of man" (p. 23). For example, he argued that historical movements in American psychology in the 1960s had introduced humanistic psychology to the discipline, and he argued that this counter-culture movement had introduced a challenge to the previous mechanistic theory of human nature, which the behaviourists had utilized. The result was dissolution of the discipline's theory of human nature and an increase in fragmentation more generally.

Royce (1977, 1985a) proposed "constructive dialectics" (p. 29) as a solution to psychology's fragmentation. This approach entailed retaining opposing tensions (dialectics) and making a sustained effort to resolve the tensions by introducing new perspectives, findings, theories, etc. (constructive). Royce (1977) concluded that this approach would contribute to increasing unity in psychology, although he admitted that "[w]hether psychology continues as a single discipline or fractionates is partly a political issue" (p. 30).

Royce (1978) went on to argue in favour of five assumptions: sciences progressed through stages; the ultimate goal of science was to develop powerful theory; psychology existed in an empirical stage; psychology was theoretically immature; and scientific advancement in psychology was contingent upon developments in theory construction and evaluation. He concluded that psychology needed to address its immature theoretical pluralism.

Royce (1985a) then examined this theoretical pluralism in detail. He argued such pluralism was to be expected in immature sciences like psychology, and that it did not indicate psychology had failed. He admitted that theoretical pluralism was always necessary for a science, but he argued that preparadigmatic sciences possessed *simultaneous* theoretical pluralism while paradigmatic sciences proceeded through *sequential* pluralism. He also distinguished between complementary and incommensurable theories, and he argued that complementarity could address much of the theoretical pluralism in psychology; however, he admitted that incommensurability remained a serious issue. Royce concluded though that incommensurability could be addressed through the adoption of constructive dialectics.[50]

In one of Royce's (1987a) final contributions[51] he reiterated his arguments for a 'bottom-up' approach to theoretical unity (i.e., starting with smaller theoretical unifications and then moving to larger unifications), and for moving toward sequential, as opposed to, simultaneous pluralism. He added that psychology should take time to mine

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[50] Three critics responded to Royce's article. Hyland (1985b) urged Royce to distinguish between two kinds of complementarity: explanans and explanandum. According to Hyland, explanans complementarity was when theories overlapped conceptually while explanandum complementarity was when theories did not overlap, but instead both served to explain a domain which encompassed both of them. Lindholm (1985) argued that pluralism and fragmentation were only seen as problematic when one tried to develop a totalizing or singular perspective and urged Royce to be more open to pluralism in general. Finally, Kitchener (1985) argued Royce failed to address to what degree unification is possible and what degree of pluralism would be left over following unification. He also argued Royce failed to be explicit as to how theories were to be evaluated, rejected or accepted, and how they would be eventually integrated through constructive dialectics. Royce (1985b) responded generally to his critics and argued that all theory was tentative, science adopted the best available theories, and that science developed by constructing better theories. He concluded that psychology should retain the tension between theoretical pluralism and theoretical singularism "at a high pitch" (p. 341).

[51] Royce's (1987b) other final contribution was a response to an article by Krantz (1987). However, to avoid repetition, I direct the reader to a footnote following the description of Krantz's article which is discussed in the section entitled *Preconditions for Unity*.

the existing 'mountains' of literature for empirical generalizations before engaging in more 'mindless' empirical fact-gathering. He concluded: "We must recognize the value of the conceptual aspect of science and increase our commitment to theoretical and metatheoretical analysis" (p. 275).

Kendler (born 1937): A Good Divorce is better than a Bad Marriage

Howard Kendler (born 1937), a professor emeritus of psychology at the University of California at Santa Barbara, argued: "a unified psychology does not, and may never, exist" (Kendler, 1970, p. 30). In support of his statement, he argued three distinct and irreducible subject matter domains existed within psychology: behaviour, neurophysiology, and phenomenal experience. He added that problems of disunity tended to be magnified when psychologists tried to determine or deny the relationships between these three domains. Secondly, Kendler argued two main explanatory frameworks existed within psychology: epistemological (i.e., based on deduction) and psychological (i.e., based on intuitive understanding). He argued that epistemological understanding could be straightforwardly applied to behaviour and neurophysiology, but not to phenomenal experience—since phenomenal experience was not publicly observable. He further argued that the two explanatory frameworks could not be reconciled and that, to achieve unity, psychology would have to select one of them. He concluded that the epistemological/deductive framework was the most likely candidate for unifying psychology, although he acknowledged that this would require researchers of phenomenal experience to find a way to reconcile their subject matter with this

framework; if such reconciliation was not possible, he argued a split within psychology would be necessary.

However, Kendler (1981) later reflected that he had been too optimistic in his hopes for a unified psychology via a commitment to the epistemological/deductive framework:

> In retrospect I must confess to being naïve and unrealistic. I harboured the optimistic belief that the natural-science method in psychology would ultimately achieve a level of success that would encourage the abandonment of competing methodological approaches...My optimism was unjustified for two reasons. (p. 305)

The first reason, Kendler argued, was that natural science methodology had not achieved a sufficient degree of success in psychology, particularly from the public's perspective; for example, he argued that the natural science perspective should be more visible on psychology shelves in bookstores. The second reason was that the schisms within psychology were too difficult to overcome: "Recognizing and accepting the fundamental differences in contemporary psychology may be a wiser alternative than attempting to combine artistic and scientific traditions and 'methods' that at best could yield a homogenized product without the positive attributes of either" (p. 313). Kendler concluded:

> The unity of psychology has all but collapsed...different segments [employ] irreconcilable orientations. As a result bitter disputes have occurred concerning the proper methodological position that psychology should adopt...The best that can be hoped for within psychology is a mutual understanding of the competing

> methodological positions and an appreciation of the decisions that led to their adoption. (p. 371)

Though Kendler (1981) argued that a 'mutual understanding' was necessary between the competing perspectives in psychology, his next contribution (Kendler, 1987) was entitled "a good divorce is better than a bad marriage", and he introduced it by admitting that, he had *again* been too optimistic about the prospects for cohesion in psychology:

> The achievement of "a mutual understanding of the competing methodological positions and an appreciation of the decisions that led to their adoption" is beyond realistic expectations. The reason is simple. Many psychologists are so dominated by ideological commitments that they cannot understand competing conceptions of psychology, much less tolerate them. Consequently, the profession of psychology inevitably will be divided into warring camps that cannot achieve any real peace or even an armistice. To search for unity in psychology is to pursue a romantic illusion, as unreal as the fountain of youth...[Psychology] should be divided into independent disciplines, all of which share common methodologies and social aims. (p. 56)

Kendler added that the mind-body problem was a major contributor to the disunity in psychology and that it would always be a dividing line. He argued that psychology could be the science of consciousness or the science of behaviour, that the two were incommensurable, and that one had to have priority in order to provide direction for the discipline. He argued that the "incompatibility of subject matter is grounds for a divorce between a mentally based and a behaviorally oriented psychology" (p. 70). Furthermore, with respect to values in psychology, he argued: "The issues of subject matter and modes

of understanding pale in comparison to the emotional antagonisms—sufficient grounds for divorce—generated among psychologists by the conflicting views of the role of psychology in society" (p. 81). He argued that one camp in psychology wanted to avoid social involvement and play a descriptive role only (i.e., provide empirical evaluations of social policies) while another camp wanted to engage in social involvement and play a prescriptive role (i.e., provide arguments for and against social policies). He concluded that the two were "irreconcilable" and that a "general distrust of all of psychology is created within society by the actions of those who perceive psychology as a form of political activity" (p. 86).

Finally,[52] Kendler argued that "the decisions that face psychologists in regard to subject matter, criterion of truth, and ethical foundations...lead to different kinds of knowledge and social application. The decisions themselves cannot be judged in terms of their truth character; they are choices, basically, of alternative life styles within psychology" (p. 87). He admitted that this freedom of choice might prompt some psychologists to conclude that natural and human scientific psychology were "equally justified" (p. 87); however, he argued "[this] conclusion is flagrantly false when it implies that the knowledge claims of different kinds of psychology are equally well justified and useful" (p. 87). He argued that natural scientific psychology was the only approach which could "yield reliable knowledge that is free from individual biases, ethical commitments, and social goals" (p. 87). However, he also admitted that there

[52] Kendler's (2002) final contribution was a response to Sternberg and Grigorenko (2001). He primarily argued that their conception of a unified psychology overlooked the substantial epistemological rifts within psychology. To avoid repetition, I direct the reader to a footnote in the section on Sternberg for a short description of Kendler's comment.

remained many psychologists who were in favour of humanistic psychology, social activism, and political involvement. As a result, Kendler concluded: "A divorce among irreconcilable segments of the community of psychologists is therefore *not only desirable but imperative*" (p. 87, italics added).

Sarason (born 1919): Psychology Misdirected

Seymour Sarason (born 1919), a professor emeritus of psychology at Yale University, argued that psychology found itself in a state of crisis, following an initial period of post-World War II optimism, due to having conducted its research and applications in an ahistorical and asocial manner (Sarason, 1981). He argued that World War II had provided psychology with a rise in socio-political status, which led to the optimistic belief that psychology could make substantial contributions to society. He argued the GI bill, population boom, and "desire to build a new and better world" (Sarason, 1981, p. 2) had all contributed to the growth and optimistic character of psychology during the period immediately following World War II; and, to achieve its goals, psychology had emulated the natural sciences (e.g., quantification, atomism) in the hope of attaining a similar level of success.

However, Sarason argued there were three factors related to the optimism that sowed the seeds for its collapse: psychologists believed that hard knowledge achieved by emulating the natural sciences was what was most necessary for social applications; the government encouraged this belief through funding and recognition; and the first two points led to a lack of reflection on fundamental assumptions, which were shaped by the social and historical context of the time. He further argued that one of the primary

fundamental assumptions of American psychology was the superiority of the capitalist model (i.e., individualism, competition, etc.). And he argued that a negative outcome of capitalism was a feeling of alienation amongst its participants, especially those who were not the substantial beneficiaries of the model.

Sarason concluded that the optimism within psychology had collapsed and that the social sciences were "in disarray" (p. 13). He explained that he came to this conclusion, not through an examination of the literature—which was still being produced in abundance—but through talking to psychologists who had become increasingly dissatisfied with the social sciences in general and psychology in particular. He argued that this feeling of dissatisfaction stemmed from the fact that the "relationship between social science and central government has been a very mixed blessing, a relationship far more transient and problematic than social scientists once imagined" (p. 14). He argued that the social sciences had maintained the status quo and done nothing to counter the widespread feelings of alienation—both amongst individuals within the United States and between the United States and other countries. Sarason argued that by dichotomizing the social and individual, and by focusing on the individual only, psychology had become an unconscious victim to the social forces of its own society.

As a result, Sarason argued post-World War II psychology found itself in a state of crisis: "when I talk about the crisis in American psychology I refer to a state of affairs about which many psychologists are perplexed, compelling them to seek to understand how an era of prosperity and optimism led to the current malaise and a dysphoric sense of the future" (p. 18). However, he admitted: "Crisis, like beauty, is in the eye of the

beholder" (p. 19), and he argued the crisis in psychology could easily be viewed as a specific case of a more general crisis in American society:

> Psychologists live in the same society [as everyone else] and they, like others, will be affected by its features that produce the sense of crisis in or beyond work roles. What I am saying here is that if you have concluded that many people in our society experience a sense of personal crisis, you do not have to know anything about psychology and psychologists to assume that they, too, are having similar experiences. (p. 21)

Also with respect to the crisis, Sarason discussed the importance of psychology's worldview, which he labelled "Weltanschauung" (p. 46). He defined this concept as "a comprehensive conception or apprehension of the world, especially from a specific viewpoint" (p. 46) and added: "A weltanschauung is not motivated; it is received, imbibed, a kind of given, a basic outline within which motivation gets direction" (p. 47). He argued that "every psychologist has a weltanschauung" (p. 57), and that psychology, as a discipline, also had one. He further argued that psychology's weltanschauung—which was based strongly on individualism, capitalism, and natural scientific values—had served the discipline poorly when it had ventured into applied realms following World War II. Following this failure, there was a growing dissatisfaction amongst psychologists.

Sarason concluded that "a new psychology need be born" (p. 174). And his conception of a new psychology included a greater focus on society, history, social history, and worldviews. He added that the discipline should include, but not become,

history. Furthermore, the discipline should emphasize understanding as opposed to measurement. He argued that as long as psychology was creating its own subject matter, independent of society, it would simply be measuring an artificial world it had created—which was "a mammoth waste of time" (p. 183).

Sarason's (1989) other major contribution was an article focusing on "the lack of an overarching conception in psychology" (p. 263). In this article, he argued that psychology was becoming "increasingly molecular", had no 'overarching conception', and was in the grips of many "centrifugal forces" (p. 263). He also argued that the fragmentation and dichotomization of psychology were problematic precisely because humans did not *experience* reality as 'mind *and* brain', 'individual *and* social', etc.; in contrast, humans experienced reality as a totality and, therefore, psychology should approach a similar integration. He added that dichotomization did "violence to the truly seamless web of our social existence" (p. 269). Sarason concluded:

> If we have learned that facts should not be confused with the truth, it is because we know that it is only by understanding the complexity of contexts that we can make sense of facts. We have a surfeit of facts. What we do not have, and most of us in the quiet of our nights know it, is an overarching conception of context into which we can put these facts and, having done so, the truth then stands a chance of emerging. (p. 279)

Kimble (born 1917): Functional Behaviourism

Although Koch (1961, 1964) may have been the first to use C. P. Snow's (1905-1980) description of 'two cultures' in academia in understanding the two psychologies within

psychology, Gregory Kimble (born 1917), a professor emeritus of psychology at Duke University, devoted a whole article to precisely this task (Kimble, 1984). In this article, he argued that Snow's identification of two cultures in academia (scientific and humanistic) could be used to understand the conflicts within psychology. He further argued that the 'two cultures' within psychology were divided by at least six key schisms: scientific vs. humanistic values; determinism vs. indeterminism; observation vs. intuition; laboratory studies vs. naturalistic observation; nomothetic vs. idiographic explanation; and elementism vs. holism. Kimble argued that the two cultures were a product of individuals having basic assumptions, which directed them to join certain groups or divisions, which then reinforced the basic assumptions; eventually, the individual and group became mutually reinforcing and increasingly separated from individuals and groups with contrary assumptions. As a result, Kimble concluded the prospects for unity between the two cultures were not good.

From the pessimistic conclusion found within this first article, one might think Kimble would have gone on to argue *against* the possibility of unity—but in fact the opposite was true. First, Kimble (1989) published an article that featured ten basic tenets for psychology.[53] The ten tenets were: determinism, empiricism, elementism, nature *and* nurture, trait *and* state, logical empiricism, nomothetic *and* idiographic, behaviourism, hypothetical-deductivism, and scientific *and* humanistic values. Kimble argued that behaviour was determined by a combination of heredity and the environment

[53] Following this article, Kimble (1994, 1995, 1996, 1999, 2000, 2005) also produced a series of publications in which he articulated his explicit proposal for unity in psychology. This series included his book *Psychology: The Hope of a Science* (Kimble, 1996), which was originally tentatively titled *Toward Unity in Psychology*.

(determinism); that data of psychology must be publicly observable (empiricism); that psychology must be analytic (elementism); that nature set limits and nurture determined how far one went toward reaching those limits (nature *and* nurture); that behaviour was a product of stable traits and temporary states (trait *and* state); that mentalistic concepts could only be included in psychology as intervening variables (logical empiricism); that individuals were unique since they were the product of idiographic effects of nomothetic laws (nomothetic *and* idiographic); that concepts in psychology must be publicly observable and have a relationship to behaviour (behaviourism); that theories were a collection of concepts and laws in a structure that facilitated the deduction of behavioural consequences (hypothetical-deductivism); and that scientific values operated in the research domain while humanistic values operated in the domain of psychologists' behaviour and in applications of psychological research (scientific *and* humanistic values) (see p. 491-499). Kimble concluded that these ten tenets could unify the discipline and that most psychologists could find them acceptable.[54]

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[54] Five critiques followed Kimble's article. First, Snyder (1990) argued that, to be a science, psychology need not subscribe to a strong determinism since, for example, quantum mechanics was a probabilistic, not deterministic, scientific theory. Rozeboom (1990) rejected Kimble's argument that psychology needed to adopt hypothetical deductivism since the only alternative was a radical empiricism; he argued that a rigorous statistical induction was a viable alternative. Green and Powell (1990) argued that Kimble ignored developments in philosophy of science in the latter half of the 20th century, as well as important lessons learned from the failed attempt to institute psychology as a natural science in the first half of the century. Branch (1990) argued that Kimble had misrepresented radical behaviourists; and, finally, McGraw (1990) argued that Kimble had misinterpreted the heritability coefficient.

In a short response to these criticisms, Kimble (1990) basically dismissed Snyder's criticisms, but surrendered a little bit to Rozeboom and Green and Powell's arguments, admitting that he was not well-read in the contemporary philosophy of science literature. Finally, Kimble simply clarified his arguments with respect to McGraw and Branch and it appeared these conflicts were simply based on misinterpretations. Kimble concluded that he believed psychology was much the same, in terms of philosophy of science, as it was around the time of World War II and that he should not be criticized for simply pointing out the current state of affairs in psychology.

Kimble (1994, 1996, 1999) also consistently argued that psychology's best hope for unity was through conforming to the methods of natural science. He argued that, to do so, psychology had to play by the rules of natural science, which included studying observable phenomena. As a result, he argued psychology as a natural science had to be some form of behaviourism—by definition—since behaviourism was the only form of psychology which explicitly focused on stimuli and responses—both of which met the criterion of public observation. However, Kimble (1999) separated himself from previous behaviourisms and offered *functional* behaviourism, which included evolutionary theory. He concluded that the functional behaviourist approach implied that: psychology's laws must be compatible, though not reducible to, physiological laws; behaviour evolved in response to the environment; and evolution created levels of complex behaviour ranging from simple (found in animals and children) to very complex (adult humans).

While expounding his model of functional behaviourism, Kimble (1994, 1995) proposed five fundamental laws for psychology, which he argued emulated the Newtonian model of physics: behaviour was the product of basic potentials and instigations for action; humans had two basic strategies for dealing with the environment—coping and adaptation; behaviour was simultaneously affected by excitatory and inhibitory stimuli; behaviour occurred when an instigation for action

raised a potential above a certain threshold; and behaviours were ordered in a hierarchical organization.[55] After presenting these five fundamental laws, Kimble (1995) mused:

> As I review these comments, I hear the voice of caution asking, "Aren't these axioms and applications based on Newton surely wrong for psychology? After all, behavior is not mechanical? And, as everybody knows, the flaws in Newton's theory forced modern physics to progress beyond it?" But then I hear another voice responding with what is usually a good question: "What are the alternatives? Would psychology rather have the scattered truths belonging to the discipline today *or a coherent science that is wrong* like that of Isaac Newton? (p. 37, italics added)

Kimble later (1999, 2000, 2005) reduced his five laws down to just three: potential-instigation, excitation-inhibition, and adaptation-coping; however, he still discussed the importance of thresholds and hierarchical organization.

Finally, in the epilogue of his book, Kimble (1996)—following George A. Miller (born 1920)—also argued that the goal of psychologists should be "to give psychology away" (p. 131). He argued that psychologists should attempt to educate the public about psychology so that people could make use of what psychology had to offer. However, Kimble admitted that the task would not be an easy one, since psychology had a poor public image; he also admitted that the task was further complicated by the disunity which existed within psychology. He concluded that his proposed solution to the

[55] This basically means that all behaviours possess an organization and this organization is learned and can change over the course of development in a series of stages.

problem of fragmentation could unify psychology and also provide a framework of knowledge which the public could accept.[56]

Tolman (born 1935): The General Crisis of Theoretical Indeterminacy

Charles W. Tolman (born 1935), a professor emeritus of psychology at the University of Victoria, argued in favour of dialectical materialism as a solution to psychology's disunity crisis (Tolman, 1987, 1988). However, he argued that materialism was *not* the same as positivism—though he admitted the two philosophies shared some assumptions. He cited positivism's reliance on intersubjective validation—and subsequent inability to assume an objective reality, independent of such validation (i.e., solipsism)—as a key difference from materialism (see 1987, p. 213-214 and 1988, p. 29). In contrast, he argued materialism involved three key assumptions: the world was material and, therefore, everything had a material cause; matter existed independently of the mind, though the mind was a product of material processes; and the world and its laws were knowable, though our knowledge could be partial or incorrect at any given time. He added that materialism was also based on an assumption of direct realism, but argued there was no good reason to, for example, humour someone who insisted an elephant was an umbrella after a group of people had determined the object was, in fact, an elephant (see Tolman, 1988, p. 30). Tolman (1987) concluded: "The object must have independent existence and must be accessible to our knowing. This is what dialectical

[56] Throughout all of his writings, Kimble argued that his proposal was the best possibility for establishing unity in psychology (cf., Kimble, 1994).

materialism asserts...What would be the point of going through these [research] motions if we cannot, in principle, gain an objective account of the process?" (p. 223).

In terms of *dialectical* materialism, Tolman argued that all matter evolved through stages of increasing complexity. He also argued at each stage of increasing complexity, emergent properties could occur thus qualitatively differentiating each stage from previous ones. He argued this process of emergence, which occurred between stages, constituted a Hegelian dialectic. Tolman also argued that the mind was an example of a qualitative property emerging from a previous level of complexity (i.e., the brain, biology); and he argued that emergent properties could not be reduced to previous levels of complexity. As a result, he concluded that psychology's subject matter were developmentally linked to, but qualitatively distinct from, physics, biology, etc.

To achieve unity in psychology, Tolman argued that materialism, time, and effort were all necessary, but not sufficient. He argued that 'conclusive thinking' (which he admitted was somewhat vague) and a broader theoretical perspective were necessary to evaluate theories and determine which contained the most relevant pieces. He explained that, without a broader perspective and conclusive thinking, two researchers could gain empirical support for two contradictory theories and eventually conclude the theories were incommensurable; with these two additional aspects, however, he argued the two theories could be commensurable.

Tolman then examined what made a theory relevant or not, and he began by presenting three criteria of *irrelevancy*: 1) theories which had unclear conceptual boundaries; 2) theories which falsely extended findings from one level of complexity to

another; and 3) theories which universalized something that was only particular. He then examined what was *relevant* and, for Tolman, it was what contributed to survival and the evolution of the species; and he argued it was the survival of the society, not the individual, which was of primary concern, since human evolution depended on the development of societies in contrast to animal communities which depended more on the strength of individual members for survival. Tolman concluded this evolutionary criterion, combined with a dialectical materialist philosophy, could provide a basis for unity in psychology; he also concluded that unity should be viewed as a process and not an end state since unifications also produced or uncovered new problems.

Tolman (1989) also wrote an explicit proposal for resolving "the general psychological crisis" (p. 197), and the proposal primarily involved employing principles of comparative psychology. Tolman argued psychology's crisis was a crisis of disunity caused by theoretical indeterminacy and that this indeterminacy was caused by abstract (e.g., a 'common factors' or classification approach)—as opposed to concrete (i.e., focusing on the material evolution or genesis of a phenomenon)—generalizing (see also Tolman, 1987, 1988): "There are no constraints within the [abstract generalization] process by which to decide what is to be abstracted. Any class of objects is likely to yield any number of equally 'good' abstractions" (Tolman, 1989, p. 203). He added that abstract generalization also contained 'a circularity' in that "[it] cannot actually discover the characters of any particular category because it must first define the category in order to decide what is general to it" (p. 203).

Tolman then reiterated his argument in favour of an evolutionary or developmental perspective, which could serve as the basis for a *concrete* resolution to the theoretical indeterminacy. He argued that, by focusing on the evolutionary history of a phenomenon, generalizations became concrete and indeterminacy was eliminated; and he added that concrete generalizations "need not be empirically, statistically universal. There are humans who do not make tools. What makes toolmaking universal for the human species is that even what is done by those who do not make tools is understandable in terms of our toolmaking origins" (p. 204). Tolman concluded that comparative psychology already possessed, and was employing, this concrete generalization approach with a developmental perspective and, therefore, was the best, and possibly the only, approach for a unified and scientific psychology.

Tolman and Lemery (1990) added that the crisis of psychology would *not* be solved either by the top-down imposition of any philosophical idealism or by the simple accumulation of more empirical data. Instead, they reiterated arguments for a developmental perspective which would facilitate concrete, as opposed to abstract, generalizations. They also reiterated the need for 'conclusive thinking', and concluded that theoretical psychologists were best suited for developing the methods and approaches which would combat psychology's crisis.

In his final contribution, Tolman (1991) distinguished between metaphysical and explicative pluralism. He argued metaphysical pluralism was ontological while explicative pluralism was epistemological—but "ultimately metaphysically monist" (p. 147), since the pluralism corresponded to different *levels* of material complexity. Tolman

rejected the pluralistic relativists' (e.g., Gergen, 1985) argument that theoretical indeterminacy was caused by metaphysical pluralism. He also rejected the relativists' subsequent argument in favour of eclecticism. He concluded that theoretical indeterminacy in psychology was *not* caused by ontological incommensurability, but rather it was caused by abstract generalization; he argued theoretical indeterminacy could be resolved through the adoption of a concrete developmental perspective which appreciated how qualitatively distinct elements emerged from previous levels of complexity.

Sternberg (born 1949) (with Grigorenko and Kalmar): Unified Psychology

Robert J. Sternberg (born 1949),[57] dean of the School of Arts and Sciences at Tufts University, presented a model of 'unified psychology', initially with Elena Grigorenko, a psychology faculty member at Yale University (Sternberg & Grigorenko, 2001a, 2001b): "*Unified psychology* is the multiparadigmatic, multidisciplinary, and integrated study of psychological phenomena through converging operations" (Sternberg & Grigorenko, 2001a, p. 1069, italics in original).[58] Converging operations, for Sternberg and Grigorenko, involved using multiple perspectives, methods, and theories to conduct

[57] Sternberg's (1992) first contribution to the literature was a response to Gardner's (1992) article. However, to avoid repetition, I direct the reader to a footnote in the section on Gardner's article which is under the heading *The Crisis and Fragmentation of Psychology*.

[58] In a personal communication (July 5, 2004), I asked Sternberg about the apparent lack of an "indigenous epistemology" (Yanchar, 1997a) in his proposal of unified psychology. He responded by arguing that pragmatism was his underlying epistemology and that methodologies and theories were evaluated through the "self-correcting" nature of science in a "survival of the fittest" manner. Sternberg was adamant that "armchair speculation" can only support, not replace, empirical research and he said he tended to "dislike moralists" who tried to impose certain methods, theories, or subject matter on psychology since they were usually "full of hot air and do not practice what they preach".

coordinated research on psychological phenomena. They argued that unified psychology would supplement, not replace, traditional areas of research.

For unified psychology to develop, Sternberg and Grigorenko argued that three "bad habits" (p. 1069) in psychology had to be given up: an over-reliance on single methods; a primary identification/affiliation with subdisciplines (e.g., social psychology) instead of the subject matter being studied; and a primary identification/affiliation with individual paradigms instead of being multiparadigmatic. They argued the first bad habit was caused and reinforced by three factors: psychologists were trained to only use one method and learning more methods took time, which was not always available; psychologists came to view a method as a 'cure-all' for studying certain subject matter; and single methods were the norm—journal articles emphasized single methods and other researchers expected single methods. They further argued the second and third bad habits were caused and reinforced by three main factors: the role of tradition (i.e., psychology departments, and universities in general, were set up a certain way and designed to stay that way); psychologists spent a lot of time doing things in the standard way and thus had a "vested interest" (p. 1073) in towing the line; and there was simply the pragmatic need to specialize—"no one can specialize in everything" (p. 1073).

Despite all the reinforcers to the contrary, however, Sternberg and Grigorenko argued that psychology could, and should, change. They argued that, in the existing model, phenomena were being inadequately studied, and that their proposal would help bring researchers together who studied the same phenomena, but from different perspectives; furthermore, they argued that their proposal for a unified psychology would help avoid

marginalizing certain phenomena which fell outside the scope of individual research areas or which did not lend themselves to specific methods. They also argued "false oppositions" (p. 1074) between research areas would be overcome, and that their proposal would encourage, rather than prevent the development of, novel research methods and perspectives. Finally, they argued their proposal would help avoid evoking the blind-men-and-the-elephant parable[59] which so frequently occurred in psychology. They concluded that "psychology will only fragment if psychologists wish it to" (p. 1077), and argued that psychologists could be proactive and train succeeding generations of scholars to embrace unified psychology as part of the discipline; alternatively, they admitted psychologists could let the discipline fragment and ultimately split apart. However, Sternberg and Grigorenko argued that psychology "needs all its parts—integrated in a unified way" (p. 1077).[60]

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[59] The blind-men-and-the-elephant parable is generally described as follows: Some blind men are studying an elephant. One grabs his trunk and explains to the others that the elephant resembles a snake. Another grabs his leg and says, no it is like a tree trunk. Another grabs its ear and exclaims, no it is like a huge leaf. And so on. The argument behind the parable is that the human quest for knowledge is similar: We are all blind and grasping onto different aspects of reality and we should avoid confusing our aspect with the totality.

[60] Five replies followed Sternberg and Grigorenko's article. First, Kendler (2002) argued that Sternberg and Grigorenko had presented a "romantic notion of psychology" (p. 1125). He further argued that they had "ignored fundamental methodological differences within the discipline" and that they had used 'converging operations' and 'paradigm' in ways unintended by the authors who had originally put forward those ideas. Kendler concluded that a "unified discipline cannot emerge from conflicting methodologies any more than the games of bridge and poker can be played simultaneously with the same deck of cards" (p. 1125).

Lau (2002) examined postmodernism in psychology parallel to Sternberg and Grigorenko's unified psychology. He argued there were a number of important overlaps between the two perspectives, including a key argument that "scientific theories and methodologies provide only a partial and always incomplete picture of reality" (p. 1126). As a corollary to this point, he agreed that multiple methodologies and theories were necessary for psychology. However, Lau's criticism of Sternberg and Grigorenko was twofold: 1) though they shared with postmodernism (e.g., Gergen, 2001, as cited in Lau, 2002) an emphasis on pragmatism, they failed to discuss exactly how pragmatism factored into their unified psychology; and 2) they did not explicitly consider the role of values within unified psychology. In other words, science was not value-free and, therefore, what role would values play in unified psychology? Lau argued that a

Kalmar and Sternberg (1988) presented an "explicit theory of theory development" (p. 161) called 'theory-knitting'. They argued that, historically, psychology had engaged in a "segregative approach to theory development" (p. 153), which was based on a logical positivist philosophy of science. They proposed that this approach was problematic for at

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unified psychology "would merely have replaced one meaningless configuration with another if concerns for social values and scientific assumptions are ignored" (p. 1126). In general, however, Lau appeared optimistic about unified psychology, if it was to be a *postmodern* unified psychology.

Kassinove (2002) argued that "unification is inevitable" and that there was no "need to be proactive in the process of unification" (p. 1127). He argued that converging operations and a multidisciplinary approach were the norm in psychology already and that journals already encouraged them. Furthermore, once "less entrenched scholars enter the field" (p. 1127), who represented the next generation of researchers, unity would occur even more rapidly.

Chovan (2002) argued that Sternberg and Grigorenko presented team work within psychology as a multidisciplinary effort instead of an interdisciplinary one. According to Chovan: "a multidisciplinary team is one that is composed of members from several disciplines working independently of each other. On the other hand, in the interdisciplinary team structure, 'Leadership functions are shared among members...everyone must be equally committed to...work together' [Zeiss & Steffen, 1996, p. 427, as cited in Chovan, 2002]" (p. 1128). He argued that when individuals came together from different perspectives to work on a problem, they brought with them different assumptions which needed to be integrated in order for the work to proceed smoothly. He criticized Sternberg and Grigorenko for not addressing how this integration of assumptions would occur within their model of unified psychology.

Finally, Chao (2002) praised Sternberg and Grigorenko for "a marvelous model of psychological prose" (p. 1128), but suggested that unified psychology would simply be another specialization—thus increasing, rather than reducing, the fragmentation in psychology. Chao further argued that the various "paradigms and perspectives [in psychology] conflict with one another" (p. 1128) and asked how they could be reconciled when each individual involved represented but one of the perspectives and could not be expected to grasp the others in sufficient detail to render an integration possible. Chao added that without this integration, fragmentation would remain. She concluded that psychologists should take Sternberg and Grigorenko's call for unified psychology as precisely that—a call, not a separate discipline. This would avoid the creation of yet another specialty within psychology.

Sternberg and Grigorenko (2002) then responded to the five comments. With respect to Kassinove, they simply agreed to disagree. They argued that "every generation thinks it is the changing of the guard...None quite does" (p. 1129). Furthermore, though Kassinove argued programmatic research was an example of the normative nature of converging operations in psychology, they argued: "Research can be programmatic and use only one type of operation" (p. 1129). To Lau, they agreed that science was never value-free, but argued it was self-correcting as well and therefore avoided the potentials of relativism that postmodernism could elicit through arguing everything was value-laden. To Chao, they argued unified psychology was not another specialty because "it does not have specialized content" (p. 1130). They explained that unified psychology was 'an umbrella framework' within with numerous specialized researchers could work. To Chovan, they simply argued he was incorrect in conceptualizing their position as multidisciplinary instead of interdisciplinary; they argued they were using the latter definition in their writing. Finally, to Kendler, they responded that they "embrace methodological differences" (p. 1130) and they argued: "Contemporary psychologists may indeed have different criteria for truth, as Kendler argued. But a wonderful characteristic of science is that it does not care what the criteria are. Ultimately, because science is self-correcting, the truth will out" (p. 1130).

least four reasons: it led to competing theories being generated for the sake of competition when, in fact, theories were often insufficiently different in that they did not always produce qualitatively different predictions; competing theories led researchers to focus on different aspects of the same phenomena and thus they often ended up arguing about two different things; competing theories resulted in researchers becoming accustomed to viewing a phenomenon in an incomplete manner; and it led to an emphasis on competition instead of cooperation, which was counterproductive for the growth of the science. What was needed, they argued, was an alternative approach—theory knitting—which was based on post-Kuhnian philosophy of science.

Kalmar and Sternberg explained that the emphases of theory knitting were on explanation over prediction and creating higher-order theories instead of refining individual theories (since individual theories were likely to represent only one perspective and, therefore, be incomplete). Following these emphases, they argued the goal of theory knitting was not to reconcile competing theories, but rather to "grasp in enough sophistication the guiding assumptions underlying the competing theories" (p. 165) thus allowing for a higher-order theory to be constructed; in doing so, the scope of the theory's predictions was widened qualitatively, as well as quantitatively. They concluded that theory knitting involved retaining the empirically supportable elements of the individual theories in question while adding new elements to unify these elements;[61] they called this the "creative thrust of theory knitting" (p. 164).

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[61] In a personal communication (July 5, 2004) I asked Sternberg what happened regarding theory knitting when empirical data supported two contradictory theories. He argued that this happened in psychology since "main effects are rare" and that it also happened when questions were misformulated. He argued, as research was refined, questions were better formulated and the contradictions disappeared. I also asked

Kalmar and Sternberg[62] argued there were at least four advantages and two disadvantages to theory knitting. The first advantage was that, since "the thrust of theory knitting [was] not empirical predictive competition, but conceptual integration" (p. 165), it had a lesser probability of steering research in the wrong direction. Secondly, they argued "theory knitting recognises the role of guiding assumptions in research" (p. 165). The third benefit was that theory knitting integrated those guiding assumptions "into a new framework" (p. 165). Finally, they argued that "theory knitting assists the theorist in explicitly identifying the theoretical constructs that constitute the new theory" (p. 165). They argued the disadvantages were: "the crazy-quilt phenomenon"—theory knitting could be done poorly (p. 165); and theory knitting was only appropriate toward the latter stages of the theory construction process; at the beginning stages "there is not enough 'yarn'" (p. 166).

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him about whether or not psychology needed an "explicit ontology" (Yanchar & Hill, 2003) and he argued that the subject matter of psychology changed over time and that it was up to individual researchers to convince the broader community that their subject matter was psychological. He did not think that anyone should proclaim the subject matter of psychology and force other psychologists to only study those phenomena. In my own thinking, I have come to view this explanation as 'negotiated ontology' in contrast to 'explicit ontology'.

[62] Kalmar and Sternberg's article on theory-knitting was deemed relevant for inclusion in this descriptive account due to the fact that it formed the basis for a subsequent article. In this article, Sternberg, Grigorenko, and Kalmar (2001) added the theory knitting conception to Sternberg and Grigorenko's (2001a; 2001b) unified psychology. The purpose of this addition, they argued, was to address their previous lack of attention given to "the role of theory in unified psychology" (p. 99). However, there is not much more that can be said about this article, since it was essentially a merger of the previous articles and did not contain any new material; therefore, this article is not discussed in any detail within the main body of this descriptive account.

In his final contributions, Sternberg (2002, 2003, 2005[63]) focused on some causes and costs of fragmentation, as well as reasons why psychology should unify; and he began with the causes and costs. First, he argued there were 'three main reasons' why psychology was fragmenting: training in psychology focused on "extreme specialization" (2005, p. 4) and, as a result, students came to devalue the work of other areas; students simply became ignorant of, and separate from, the work of other areas due to the process of extreme specialization; and psychologists were often in competition with each other for grants, clients, etc. and, therefore, tended to devalue the work of their 'competitors'. Second, he argued there were three costs of fragmentation: "a house divided is full of unhappy people" (2005, p. 4); time and energy was wasted fighting when it could have been spent in cooperation doing research; finally, infighting and not being able to present a common front reduced psychology's credibility in the eyes of others.

Sternberg (2002) then surveyed four broad tensions in psychology in order to illustrate why they were "foolish" (p. 10). The first tension was between basic and applied research. Sternberg (2005) attributed this split to "elitism", "short-term economic gains", and "false perceptions" (p. 12-13); however, he argued the two domains were interdependent in that basic research formed the basis of future applied research while applied research stimulated ideas for basic research (Sternberg, 2002, 2005). The second

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[63] One of Sternberg's main contributions to the literature was his edited book: *Unity in Psychology: Possibility or Pipedream?* (Sternberg, 2005). Morris (2005) reviewed the book and suggested that it underachieved in four ways: 1) it assumed unity in psychology was possible in principle; 2) it did not address the fact that if unity was indeed possible, the nature of the unification would set limits on 'unity in practice'; 3) it lacked *comparison* between the contributors' contributions, which suggested that unity was only a pipedream; and 4) it lacked *integration* of the contributor's contributions, which also suggested that unity was only a pipedream.

tension was between methodologies (e.g., qualitative-quantitative). Sternberg argued there was no one correct method and psychologists should stop debating about methods and simply employ the use of multiple methods (i.e., converging operations). The third tension was between teaching and research. Sternberg (2002) again argued in favour of interdependence: teaching provided ideas, participants, and an opportunity for "trying out many of the ideas that arise from research" (p. 10) while research provided the content, experience, and passion for teaching. Finally, the fourth tension was between science and practice; and again Sternberg (2005) argued for interdependence: science needed practice because it attracted students, funding, and applied outlets for research while practice needed science because it provided theories, assessment tools, and "adequate tests of therapies" (p. 6). Sternberg (2005) admitted there had been controversy surrounding what constituted 'adequate tests of therapies' and whether such tests did, or could ever, exist; however, he pointed out that science had provided a basis for rejecting "tarot cards, crystals, orgone boxes, and the like", and he added: "We can only hope we are well beyond this ilk of practice" (p. 7).

Finally, Sternberg (2003) urged psychology to unify and suggested seven benefits to doing so: "conserving resources" (p. 5), increasing psychology's credibility, "profiting from each other" (p. 5), recognizing the interdependence of psychology's research areas, developing better research questions, recognizing that "we're all the same at heart" (p. 5), and creating a more positive atmosphere. Sternberg (2005) concluded: "The fragmentation of psychology is a mistake. We (Sternberg & Grigorenko, 2001) have

suggested a 'cure'—namely, unified psychology, an approach that emphasizes studying psychological phenomena from a variety of perspectives" (p. 13).

Yanchar (born 1969) (with Slife, Kristensen, and Hill): Hermeneutic Unity

Stephen C. Yanchar (born 1969), an instructional psychology and technology faculty member at Brigham Young University, published his first two contributions to the crisis and unification literature with Brent Slife, who is the chair of the doctoral programme in Theoretical and Philosophical Psychology programme at Brigham Young University (Yanchar & Slife, 1996a, 1996b). These publications were part of an exchange with Hoshmand and Martin (1994, 1996), who argued that post-Kuhnian philosophy in psychology had led to a state of pluralism which shaded into relativism and a growing concern about incommensurability. However, they argued that the evaluation and integration of competing theories was desirable for psychology, and that the discipline needed a consensual epistemology (i.e., convergent pluralism, as opposed to divergent pluralism)—even if this degree of unity was contingent and ever-evolving. For this consensual epistemology, they argued that a combination of pragmatism and critical (as opposed to naïve) realism could lead to the development of a heuristic set of evaluation criteria which could be accepted by most researchers. Finally, they argued that empirical and theoretical contributions were both important for psychology, and that the discipline's professional reward structure encouraged fragmentation.

Yanchar and Slife (1996a) argued that Hoshmand and Martin's approach could be beneficial to psychology, but *only* if it was preceded by an extensive critical analysis of the theoretical assumptions upon which psychology was based. They agreed that an

indigenous epistemology was desirable for psychology, but argued that Hoshmand and Martin did not provide a critical examination of their own assumptions (e.g., pragmatism). They argued that any evaluation in psychology necessarily involved privileging certain assumptions—that it could not be a view from nowhere. They further argued that such a theoretical examination would have to be conducted since empirical methods also relied on certain assumptions; and, also, the theoretical examination needed to start from what psychology 'ought' to be instead of what it was currently doing. They proposed that psychology should consider adopting morality and intellectual adequacy as criteria for evaluating apparently incommensurable theories, and concluded that psychology should return to considering the fundamental philosophical questions which had been ignored by psychology for most of the 20th century.

Yanchar (1997a) also provided a critical examination of methodological pluralism in psychology. He primarily argued that, in order for methodological pluralism to be effective, it had to be accompanied by an indigenous epistemology; without an accompanying epistemology, the threat of fragmentation remained because there were no criteria for evaluating contrasting methodologies or findings. He also argued that methodological pluralism was increasing in popularity due to reactions against the received view of science (e.g., single-method, naïve realism); however, he argued that uncritical pluralism could ultimately lead to incommensurability when different methods produced contradictory findings. As a result, Yanchar argued that psychology needed to develop two types of coherence; weak coherence involved epistemological and

ontological unity *within* the specializations of psychology, while strong coherence involved unity *across* specializations.

Yanchar argued that coherence avoided eclecticism, but still allowed for a healthy diversity; however, he also argued that it required an epistemology that could provide evaluative criteria for determining which methods to include or exclude, and for dealing with contradictory findings. He admitted though that a constant problem was which epistemology to adopt, and he argued that which assumptions to privilege and which evaluative criteria should be used were two related issues which needed to be addressed. He did not provide answers, but instead argued that psychology should examine various assumptions and trace the assumptions to their logical conclusions; after doing so, psychologists could engage in dialogue about which conclusions were most desirable. Yanchar concluded by admitting that many psychologists might not even want strong coherence (i.e., the unification of psychology); however, he argued that anyone who did want the unification of psychology needed to address the concerns he raised regarding weak and strong coherence, as well as the need for an indigenous epistemology.

Yanchar (1997b) then explicitly addressed the problem of fragmentation in psychology. He argued that fragmentation was a problem, and that it was tied to psychology's status as a science and an independent discipline. He also argued that unity could not be achieved without a consideration of fundamental ontological, epistemological, and axiological issues, and he argued that a critical examination of these issues was preferable to integration attempts that tried to fit together incommensurable pieces. He argued that when this latter approach was taken, certain parts were altered in

order to fit and this alteration negatively distorted those parts. He concluded that psychology needed to engage in a continually-evolving evaluation of various perspectives, and that this evaluation necessitated certain evaluation criteria; however, he admitted that the search for such evaluation criteria could lead to an infinite regress of criteria selection (i.e., once criteria were selected, critics could ask why the criteria were selected thus necessitating further criteria to justify the original criteria; this process could then continue *ad infinitum*).

Yanchar and Slife (1997a)[64] also explicitly addressed the problem of fragmentation, and they began by arguing that the proposals for curing psychology's fragmentation were themselves quite fragmented: "It is our thesis...that the proposed solutions to fragmentation are as fragmented as the discipline they are attempting to unify. As a result, current unification efforts have failed to reverse trends toward fragmentation" (p. 235). They then examined five causes of fragmentation: the professional reward structure; the science-practice tension; diverse theoretical positions; diverse theoretical languages; and diverse methods. They argued that psychology had never been unified, but that the fragmentation was continuing to increase. They also argued that the fragmentation was taking place on a number of levels (e.g., theoretical, methodological). Instead of providing resolutions, however, they offered three primary questions which

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[64] Yanchar and Slife (1997b) also argued there were important parallels between multiculturalism and fragmentation in psychology. They argued that both concerns required addressing a great deal of diversity while at the same time recognizing that no objective grounds were available from which to evaluate the diversity. Furthermore, both concerns required openness to various truth claims and moralities, as well as space for dialogue about these differences. Finally, there was the need to be self-aware and self-critical of one's own assumptions and to engage in critical dialogue about different assumptions. They concluded that psychology needed to engage in this kind of self-critical reflection and dialogue.

they argued should be addressed by future researchers. Was commensurability even possible? If so, how was it possible? Finally, what did the manner of commensurability suggest for the unification and future direction of psychology?

Yanchar and Slife (2000a, 2000b) also guest-edited a special issue on unification for the *Journal of Mind and Behavior*, which featured a detailed discussion of their hermeneutical approach to unification. The special issue featured six articles. First, Yanchar and Slife (2000b) introduced the special issue by arguing that fragmentation was a problem for psychology. They argued that common critical responses to the pursuit of unity as a cure to fragmentation included: diversity was necessary (so unity was not); psychology should be ruptured not unified; and psychology should be rejected in favour of other disciplines, such as cognitive science. They noted that the various solutions were themselves fragmented, but they argued that, despite its fragmentation, the fragmentation literature served four important purposes: raising awareness about the issues; highlighting key issues; illustrating the many levels of fragmentation; and indicating the need for evaluative criteria for competing theories. They concluded that psychology needed an overarching framework, but admitted there was the fear of an infinite regress of criteria selection that could occur while pursuing such a framework.[65]

Kristensen, Slife, and Yanchar (2000) provided an article building on previous arguments made by Yanchar and Slife (1997a, 1997b, 2000a). They argued that the starting point for pursuing unity in psychology should be moral dialogue. They further argued that, from this starting point, a 'nonlocal moral order' could develop (i.e., a

[65] Yanchar (2000) then provided an article, which basically summarized arguments he and Slife had previously made (see Yanchar & Slife, 1997a).

general, moral perspective capable of providing the basis for discussion and evaluation of various competing research findings, theories, perspectives, etc.).[66] They provided four characteristics of such a nonlocal moral order: it would emerge from one or more local perspectives, but be able to serve as a nonlocal moral order; it would need to be sensitive to incommensurabilities; psychologists would have to develop it with caution and even humility; and certain standards would have to be upheld, particularly consideration for the consequences of psychological research and applications for human beings in general.

Richardson (2000) then provided an article which basically reiterated Yanchar and Slife's call for a hermeneutic resolution to the fragmentation of psychology. He argued that fragmentation had plagued psychology "since its inception" (p. 289) and that 'scientistic'—i.e., using the scientific method and controlled observation as a unifying framework—and 'constructionistic'—i.e., endorsing a "radical heterogeneity" (p. 289)—solutions to the problem of fragmentation had previously been put forward. Richardson rejected such solutions and seconded Yanchar and Slife's call for a hermeneutic unity.

Martin (2000) was the only author to provide a critique of the possibility of a hermeneutic unification. He argued there were two main barriers to the proposal. First, the hermeneutic resolution could not resolve metaphysical or ontological disagreements—some competing views were based on two different theories of reality.

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[66] In a personal communication (July 13, 2004), Yanchar explained to me that a non-local moral order would essentially be a shared moral framework that would serve as an overarching identifying background for psychology. However, apart from this shared moral framework, methodologies, theories, etc. would be "wide open". The goal of a non-local moral order was to demonstrate that "we are not doomed to constructionism, relativism, skepticism, nihilism, and so on". In response to my argument that morals may change over time, Yanchar argued that, that "merely means that the work of critical reflection must continually occur".

Secondly, in situations where hermeneutic unity could be pursued, some psychologists simply did not possess the openness and critical understanding necessary to engage in the required tasks. He concluded that if the proposal was to be implemented, it would require a substantial reorganization of the discipline's operations, including reorienting the curriculum according to a liberal arts model—which he argued psychology had always firmly avoided.

Yanchar and Slife (2000a) wrote the concluding article, which was written as a summary and also a response to Martin's concerns. They started by addressing Martin's first concern about differences in ontologies. They argued this schism could in fact exist or such incommensurabilities could actually be commensurable at a higher level of abstraction. For example, a mechanist and mentalist were both presenting *theories of human nature* and it was on this higher level of abstraction that dialogue could occur. With respect to the second concern about psychologists' ability or willingness to engage in hermeneutic tasks, they argued that it might in fact be the reason why differences in views on reality were seen as incommensurable in the first place. They admitted they were aware of the possible resistance to this approach, but they argued that it was possible. They also argued that theoretical psychologists should be responsible for overseeing the dialogue and that this responsibility would result in an increased role for theoretical psychology more generally. They concluded that some perspectives might need to be eliminated from psychology if they were antithetical to dialogue; however, they hoped to minimize the need for such exclusionary approaches, and they emphasized

that the goal of hermeneutic dialogue was to retain apparently incommensurable perspectives in the hope that a resolution could be found.

Finally,[67] Yanchar and Hill (2003) discussed the prospects of "an explicit ontology" (p. 11) for psychology. In this article, they argued that psychology, historically, had privileged epistemological—and, specifically, methodological—issues "to the virtual exclusion of other theoretical and philosophical concerns" (p. 15), including ontological concerns. Furthermore, they argued the epistemological commitments psychology had made were based on positivist and traditional empiricist frameworks. They argued that psychology's overemphasis on epistemology, and subsequent de-emphasis on ontology, resulted in four main problems: a discipline-wide identity crisis based on not possessing a "genuinely indigenous" subject matter (p. 16); psychology uncritically adopting an implicit ontology (e.g., materialism), which could potentially have a negative impact on humans (e.g., denying or devaluing "intentionality, agency, morality, spirituality...and so forth", p. 18); methodological disputes (e.g., qualitative-quantitative) arising from some researchers rejecting the "default epistemology" (p. 19) of positivism based on differing "ontological commitments" (p. 19); and fragmentation: "it would seem that the absence of an explicit ontology has worked to our disadvantage by compromising our ability to remain a coherent, independent discipline" (p. 19).

Yanchar and Hill argued that an explicit ontology would be a major contribution to unity in psychology: "Unity of the sort we envision is similar to a form of coherence,

[67] Yanchar's (2004) final contribution to the literature was in fact a response to an article by Henriques (2004). However, to avoid repetition, I direct the reader to the section on Henriques' writings for a description of this comment.

wherein the manifold aspects of the discipline fit theoretically and consistently into a larger ontological picture" (p. 20). They argued that this unity could "entail multiple levels of causation", but that it would provide "a common ontological framework for making sense of the variegated claims of research and theory [in psychology]" (p. 20). They concluded that such a 'common ontological framework' would increase the prospects for overall unity in psychology.

Yanchar and Hill then presented their attempt at an explicit ontology. It involved three assumptions. The first was that "research and theory must begin with the givens of our experience" (p. 20). The second was that "any ontological commitment that a priori contradicts the givens of our experience, including mental life [should] be rejected [since it violates the first assumption]" (p. 21). Finally, they argued: "Our ontology must be open to continual clarification, re-examination, and reinterpretation" (p. 22). They then argued that their proposal was closely aligned with "hermeneutics...phenomenology...and some forms of pragmatism" (p. 22). They concluded that, in calling for an explicit ontology, they did not wish to suggest that ontology could ever be fully separated from epistemology; however, in psychology, epistemology had been given considerable attention, and "it is now ontology's turn" (p. 27).

Henriques (born 1970): The Tree of Knowledge (ToK) System

Gregg Henriques (born 1970), a psychology faculty member at James Madison University, provided a proposal for theoretically unifying psychology (Henriques, 2003, 2004, 2005b), which he called "the tree of knowledge system (ToK)" (Henriques, 2003,

p. 150). He argued that all life was based on an evolution of complexity, and that this evolution began with the Big Bang—which produced matter—continued to the emergence of life and biology, then to mind, and finally to culture. He argued that the modern synthesis of natural selection and genetics provided the 'theoretical joint point' between matter and life.[68] He then argued that psychology's task was to develop the theoretical joint points between life-and-mind and mind-and-culture. He concluded that the completion of these two tasks could provide theoretical unity for psychology.

For the life-to-mind joint point, Henriques (2003) proposed the "behavioural investment theory (BIT)" (p. 158), which consisted of an integration of cognitive science, neuroscience, behavioural science, evolutionary theory (including genetics), and systems theory. He argued that cognitive science studied the mind, which was what the brain did (neuroscience), namely coordinate behaviour (behavioural science). In terms of the evolutionary component, he explained the BIT was based on four basic postulates: the nervous system evolved as a "conceptual control center that coordinates the behaviour of the animal as a whole" (p. 160); phylogenetic evolution (i.e., genes that produced "neurobehavioural selectors that expended behavioural energy in a way that increased overall evolutionary fitness were selected" (p. 160), while those that did not were not selected); ontogenetic evolution (i.e., behavioural investments that enhanced the relationship of organism-environment relationships to ancestral inclusive fitness were selected, those that did not were not selected); and current behaviour was a product of

[68] Henriques explicitly stated that his proposal was contingent upon the 'Big Bang' theory and the 'modern synthesis' of evolutionary theory and genetics. He admitted that his proposal should be rejected if those more basic theories were disproved. However, he argued it was a strength, not a weakness, of his theory since scientific theories *should* be falsifiable.

phylogenetic and ontogenetic evolution. He argued that B. F. Skinner (1904-1990) was essentially correct in developing a theory of behavioural evolution, but that Skinner had not included the important role of the neural impulse; he argued this oversight was analogous to Charles Darwin (1809-1882) having the theory of evolution but not the mechanism of genetics. Finally, he argued the systems theory component of the BIT featured the addition of the theories of complexity (e.g., biological creatures were more complex in their evolution than physical matter) and emergence (e.g., life and its corresponding behaviour emerged from the complex organization and evolution of matter). He concluded that the BIT was sufficient for explaining animal psychology (i.e., basic mental life and social organization); however, he admitted it was not sufficient for explaining human behaviour because humans possessed culture and language (i.e., complex mental life and social organization). Therefore, he explained that a theory for the emergence of culture was needed, and he offered the "justification hypothesis (JH)" (p. 166).

Henriques based the JH on an interpretation of Sigmund Freud's (1856-1939) work. He argued that the BIT was analogous to Freud's id. He further argued that culture could be seen as representing Freud's superego. He argued that the ego evolved due to the adaptive problem of having to justify behaviours to other humans; therefore, the ego served as a justification filter for inhibiting the id's impulses and for justifying actions to society. He argued that humans did not possess self-consciousness prior to having other humans ask why they did something; and it was at this point that humans were forced to

self-reflect on their actions to come up with a justification. He argued it was through this process that the ego evolved as a self-conscious-justification-system.

Henriques (2003) concluded:

> The absence of a large-scale meta-theoretical framework that could effectively incorporate physical, biological, psychological, and social causation in explaining human behavior has resulted in the rift between the two cultures [i.e., subjective/humanistic and objective/scientific psychology]. The ToK system, with its depiction of the correspondence between the four layers of complexity and the four domains of science [i.e., physical, biological, psychological, social], provides the meta-theoretical framework necessary for consilience between the natural and social sciences to be achieved. (p. 176)[69]

Henriques (2005a; Henriques & Cobb, 2004) also served as guest editor for two special issues on his ToK system for the *Journal of Clinical Psychology*. In the two issues, Henriques (2004) wrote a target article, which was followed by twenty responses, and finally a concluding article by Henriques (2005b), which was both a conclusion and response to his critics and supporters.

In the target article, Henriques (2004) primarily summarized his previous article; however, he added a few new elements. First, he explained that the ToK system basically

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[69] In a personal communication (May 14, 2005), I asked Henriques whether he felt his proposal really 'unified' the two cultures in psychology. In response, he argued:

> Perhaps the best way to think about what I am proposing in terms of the institution is to look at biology, neuroscience and medicine. I would argue there is theoretical cogency and conceptual harmony between these domains…I am essentially proposing that the institution of psychology needs to be arranged in the same manner: the core discipline of psychology, the crucial subdiscipline of human psychology, and the profession of psychology. I see this as effectively organizing the discipline. However, some may not see it exactly as unifying the institution. Does it reinforce the two culture split? I see it as disentangling the split.

rejected mentalism. He argued that psychology could be defined as the science of the mind, as long as the mind was defined as a type of behaviour. In this way, it was only the complexity of behaviour that separated psychology from biology and physics. Furthermore, this approach eliminated the mind-body problem of a metaphysical substance affecting a physical substance (see p. 1213). Secondly, he argued that, if the ToK system was accepted, psychology would have two main domains: scientific psychology or psychological formalism (i.e., a complex discipline comprised of the disciplines integrated in the BIT: cognitive science, neuroscience, evolutionary theory and genetics, behavioural science, and systems theory) and human psychology, which was a combination of psychological formalism and the social sciences. In other words, he argued that psychological formalism was "one of the four fundamental sciences [i.e., physical, biological, psychological, social]" while human psychology was a "hybrid discipline" (p. 1218) that existed between the psychological and social sciences.[70] Finally, Henriques argued that the BIT linked humans to animals and that the JH made humans distinct; in this way psychology represented a distinct area of study, but one which was linked to other sciences that addressed more basic levels of complexity. As a corollary to this point, he concluded that psychology's subject matter should be defined as animal behaviour; however, he explained that humans, being animals, were not excluded from study.

[70] Following this distinction, he argued that the American Psychological Association should be renamed the American Human Psychological Association to better reflect its primary commitments to the human psychology domain.

Twenty responses were written to Henriques' (2004) target article. In order to provide a loose organization, I have separated them into two groups: primarily supportive and primarily critical. I will present the supportive responses first, followed by the critical, and then conclude with Henriques' (2005b) final contribution.

Calhoun (2004) suggested the unification of psychology in general, and Henriques' attempt in particular, was "a noble quest" (p. 1283) and—although he cautioned that the proposal seemed to: 1) exclude postmodern perspectives; 2) not address the influence of cultural and economic forces; and 3) necessitate disciplines remaining in their prescribed boundaries—Calhoun argued: "The proposed unified theory for psychology clearly offers the possibility of significant heuristic value" (p. 1284).

Haaga (2004) posed the rhetorical question: "If it is so difficult to define terms such as 'psychology' with precision, why bother?" (p. 1228). He then argued that a key strength of Henriques' article was that it demonstrated how psychology made sense, or could be defined, in relation to neighbouring disciplines. However, he concluded: "Whether this sort of intriguing insight actually generates new ideas for research; squashes disharmony between behaviorists and cognitivists or transpersonalists and conditioning researchers; or is crucial to deriving an optimal, consensual definition of psychology, will become clearer with time" (p. 1229).

Gilbert (2004) began his response by pointing out that economics had also historically questioned its identity as a science; however, he argued economics had something psychology did not—an awareness that it needed a micro (e.g., "money markets, labor markets, or resource availability", p. 1223) *and* a macro approach (e.g., "how all these

[money markets, etc.] work together at various levels of complexity and emergence of self-regulation and de-regulation", p. 1223). Gilbert explained that he was shocked when he discovered that psychology did not have an equivalent macro approach, and he concluded that Henriques' proposal provided such an approach.

Stanovich (2004) began his response by stating that he agreed with much of what Henriques had presented, and he argued that the BIT and JH were particularly intriguing. His one disagreement was that he felt "metarepresentational abilities" (p. 1263) were what distinguished humans from animals; however, he admitted that the JH could have been responsible for the development of these abilities.

Rand and Ilardi (2005) agreed with Henriques that psychology was in a fragmented state and they accepted much of his proposal. However, they rejected the argument that the fragmentation was because of the discipline's youth. Instead, they proposed that the reason for psychology's fragmentation was more a result of the complexity of its subject matter, which they argued was greater than for the subject matter of the natural sciences. Although they admitted that it would take time for Henriques' proposal to be properly evaluated, they suggested that a pragmatic criterion (i.e., the degree to which it makes a positive impact on science and society) would be used in the evaluation process since it was becoming more popular within the academic community.

Kihlstrom (2004) agreed with Henriques about "the unifying centrality of mind" (p. 1244) and argued: "psychologists seek to understand the cognitive, emotional, and motivational processes that underlie human experience, thought, and action. Psychology would be unified instantly and permanently, and distinguished from all other disciplines,

if everyone would embrace this simple definition" (p. 1244). Kihlstrom added: "The unique identity of psychology is that it is the science of mental life, and it is with the mind, and with science, that psychology finds its unity" (p. 1246). However, Kihlstrom admitted that even this unity would be subject to tension between the two cultures since, as Henriques identified, psychology was linked to both the biological and social sciences and, therefore, some psychologists would be focussed on moving psychology toward the biological sciences and some to the social sciences.

Quackenbush (2005) agreed with most of Henriques' assumptions and argued that his proposal provided a theoretical framework within which many psychologists could work. He concluded that Henriques' ToK system "does not mask over the tensions between naturalism and social constructionism"; instead, he argued that it resolved such tensions.

Geary (2005), Shaffer (2005), and Shealy (2005)[71] explored and expanded upon various aspects of Henriques' proposal. Geary outlined a "motivation-to-control model" (p. 21), which he argued expanded on Henriques' BIT. This model was an evolutionary model which addressed how organisms attempted to control "social (e.g., mates), biological (e.g., prey), and physical (e.g., nesting spots) resources" (p. 21). Shaffer focussed on providing evidence for the JH, which she proposed could be found in the sociological writings of Mead (1930, 1934, as cited in Shaffer, 2005) and Cooley (1902, as cited in Shaffer, 2005) in particular. Finally, Shealy also addressed the JH. He argued that the JH "needs greater specification (i.e., justification) regarding what it is, how it

[71] Providing a detailed examination of these three articles is beyond the scope of this history since they were more focussed on various aspects of Henriques' proposal than on the unification of psychology *per se*. I simply mentioned them here since they agreed with, and expanded upon, Henriques' proposal. I direct the interested reader to the articles themselves if more information is desired.

might be operationalized and measured, and what it does and does not predict in the real world" (p. 81).

There were ten articles which were primarily critical. The title of Viney's (2004) article summarized his position: "Pluralism in the sciences is not easily dismissed" (p. 1275). He argued that contemporary researchers were beginning to question whether or not the natural sciences were really as unified as most people believed and he suggested that "the disunities in psychology need not result in a sense of disciplinary inferiority" (p. 1275). Viney concluded that "a pluralistic perspective just may be more empirical and more scientific than any of the varieties of monism that are always too quick to tell how to count and what to count" (p. 1277).

Vazire and Robins (2004) challenged Henriques' JH and argued: "his explanation for the evolution of self-consciousness is overly narrow and the evolutionary sequence of events is backwards" (p. 1271). They argued that in order to be motivated to ask 'why are you doing that?' an individual would *already have to possess self-awareness.* They concluded that the evolutionary sequence Henriques had proposed was "implausible" (p. 1272).

Stricker (2004) argued that Henriques had not addressed political and power issues (i.e., degree of influence held by individuals and organizations), which could prevent the implementation of *any* metatheory. As an example, he suggested that power and politics were responsible for the scientist-practitioner model having only received 'lip-service' in clinical psychology. He concluded that Henriques needed to address the impact of such issues.

Stam (2004) suggested that the unification of psychology was merely a "disciplinary maneuver" (p. 1259). He argued: "The unification of psychology is largely a disciplinary maneuver and not primarily an epistemological act. It is concerned with the nature of institutional psychology even if that institutional entity is not divorced from its practices and problems" (p. 1259-1260). He added that psychology was already 'unified' by methodological commitments: "Institutional psychology has, by default, been unified for some time around a series of methodological and functional categories that have served to support its institutional projects while disallowing an inquiry into those problems it might otherwise pursue" (p. 1260).

Katzko (2004) provided three main criticisms. First, Henriques needed to spend more time fleshing out the implications of the distinction between psychology the discipline and psychology the subject matter. Secondly, unification should be a goal, but not necessarily "a precondition for progress" (p. 1238). Finally, Henriques' proposal seemed to have been too strongly influenced by American psychology: "There are many historical routes to be followed through the epistemic terrain of psychology. Some of these routes can bypass entirely the specific highlights picked out [by Henriques]" (p. 1240).

Presbury (2004) provided an argument for why psychology, not physics, was the fundamental science:

> Models are always problematic when it comes to accurately portraying anything. I believe the ToK model is as useful as any. However, if it is to be a tree, I would prefer one on which the branches of mind and culture bend downward toward the

> nourishing ground, more a willow than an oak. The Big Bang and the dinosaurs may have been here long before we humans were, but if we hadn't come along, they might as well have not existed, because nobody would know about it. The basic epistemological issue is that nothing could be known if we humans did not have the capacity to know. Everything is psychology. It is psychology—not physics—that is the mother of all sciences. (p. 1257)

Lilienfeld (2004) provided three arguments. First, the concept 'psychology' was "inherently fuzzy" and "resists precise definition" (p. 1249). Secondly, defining psychology was actually detrimental to interdisciplinary consilience because a definition of psychology would promote "turf warfare" (p. 1251) between psychology and closely related disciplines (e.g., sociology). Finally, the science-practice schism was a product of "different approaches to acquiring knowledge" (p. 1249) and *not* 'definitional disputes'.

Hayes (2004) argued that Henriques' proposal "reflects an underlying philosophy of science that emphasizes coherence as its truth criterion" (p. 1231). He further argued that, when viewed from other perspectives (e.g., "functional contextualism", which called for strong empirical support, p. 1231), the proposal "has no known value" (p. 1231). He suggested that clinical psychology in particular relied more on perspectives like functional contextualism and less on coherence and, therefore, he argued clincians would "have few current empirical reasons to be attracted to this taxonomy" (p. 1231).

Slife (2005) suggested that Henriques' proposal had certain 'limits', which placed certain psychological approaches (e.g., "qualitative research and spiritual therapy strategies", p. 107) outside of the proposed unified psychology. Following Ludwig

Wittgenstein (1889-1951), Slife argued that "one cannot unite the various language games of a discipline's discourse communities through common overarching features" (p. 107). Instead, Slife reiterated the proposal for hermeneutic unity he had developed with Yanchar (Yanchar & Slife, 2000a, 2000b).

Finally, Yanchar (2004) argued that "theoretical unification should not be pursued for its own sake" and that "many psychologists are unlikely to endorse the specific unifying principles of the Tree of Knowledge System" (p. 1279). With respect to Henriques naming Freud and Skinner as psychology's fundamental theorists, Yanchar argued:

> Historical prominence does not necessarily imply truthfulness, theoretical cogency, or even optimal practical utility. Indeed, many psychologists with diverse perspectives—humanistic, phenomenological, existential, hermeneutic, feminist, and others—are unlikely to endorse Skinnerian, Freudian, or neurocognitive principles; and it is equally unlikely that the precepts from these diverse theoretical perspectives can be coherently subsumed within Henriques' "human psychology"...without altering them fundamentally. (p. 1280)

Yanchar suggested that "the pursuit of truth in view of other relevant ethical and ontological concerns" and that "the need for continual dialogue among psychologists from diverse research communities...overshadows [the need for] unification" (p. 1280). Yanchar concluded: "If one clear message has emerged from the vast literature of fragmentation, it is that the topic of human nature is controversial, perhaps even more controversial than it is complex" (p. 1280).

In his concluding article, Henriques (2005b) summarized his position while responding to his critics and supporters. He observed that one of the most frequent concerns was that his proposal could be too dogmatic and, thus, potentially detrimental to the development of the discipline. In response, he explained that if his model was too dogmatic, "it will have done a tremendous disservice to scientific inquiry" (p. 123). He argued, however, that many of the comments were supportive of his model and seemed "to open, rather than close, lines of inquiry" (p. 123).

He emphasized that his goal was to develop a 'shared worldview' similar to the one developed within biology thanks to the modern synthesis of evolutionary theory and genetics—and he pointed out that diversity still existed within biology, as it would still exist in psychology.

Although Henriques argued his proposal was inclusive of many areas of psychology, he admitted it did exclude some perspectives. He argued that psychology was a science and, as a science, it excluded religious, spiritual, and ethical perspectives. Psychological formalism, in particular, would not embrace these domains, although he suggested human psychology could deal with them since it was a hybrid discipline which included social science perspectives. He also explained that, although some of his critics had implied otherwise, he was aware of the issues which faced the practice of psychology. He admitted that his proposal primarily focussed on academic psychology, but argued that his proposal had implications for practice. For example, he suggested that, although power and political issues *were* problematic in the science-practice divide, a clarification of epistemological and theoretical issues could diminish the intensity of the power

struggles. He concluded by admitting that there were factors beyond a definition of psychology which needed to be addressed with respect to the science-practice divide.

In discussing the potential curriculum benefits provided by the ToK System, Henriques also touched on how his model addressed the two cultures:

> ...many psychology departments...currently divide the teaching of psychology into two broad domains: (a) psychology as a natural science and (b) psychology as a social science. Obviously, there are clear parallels between this split and the organization I am advocating. However, unlike the division between natural science and social science, which is fraught with ambiguities (e.g., are social phenomena unnatural?), the ToK System spells out clearly and specifically why the lines are drawn where they are. (p. 132-133)

He concluded that the issue of the two cultures in psychology would be resolved if his model were to be adopted.

In response to Vazire and Robin's (2004) critique that he had the 'evolutionary sequence' for the JH backwards, Henriques responded that he "was not convinced that self-reflective awareness is required to access the thoughts of others via language" (p. 133).[72] He added that he viewed "the JH as a more sophisticated evolutionary analysis to self-consciousness because it specifically matches a particular and unique adaptive problem to the design features of the self-consciousness system" (p. 133). He also

[72] In a personal communication (May 17, 2005), Henriques admitted that instead of mind developing as a result of the need for justification, mind and justification evolved 'in concert'. So, instead of the mind evolving from justification, he argued that the mind and justification co-evolved. Thus, some degree of mind/self-awareness was present in order to perceive and understand the need for justification, but through justification, the mind evolved to greater complexity. This likely could have assuaged the concerns of Vazire and Robins (2004).

argued, however, that there was "much overlap" (p. 133) between their positions and, therefore, their "differences could be unnecessarily magnified" (p. 133).

Henriques concluded by agreeing with Stam that psychology was not in a state of crisis, but he argued that it was also not meeting its full potential:

> Stam (2004) makes the point that psychology has long been seen to be in crisis and that, in many ways, the institution of psychology is functioning adequately. I certainly agree that psychology is not in "imminent danger" of falling apart. The issue isn't so much about an institutional crisis as it is about potential—I believe strongly that the potential of our discipline to shape society in a constructive way has not been realized. (p. 136)

He suggested that his proposal for a theoretically unified psychology would help the discipline meet its full potential.

Other Crisis and Unification Literature

The Two Cultures and Psychology's Schisms

The major figures (e.g., Kimble, 1984) were not the only ones who continued discussing 'the two cultures' within psychology during the 1970-2005 time period. For example, Wertheimer (1972) organized his book *Fundamental Issues in Psychology* according to dichotomous tensions in psychology, which he claimed corresponded to the two cultures: "...we mentioned William D. Hitt's (1969) 'Two models of man.' His dimensions or 'contrasting views'...were classed as either phenomenological or existential on the one hand, or behavioristic on the other. Perhaps these same labels could be applied to the two sides of our table of the issues" (p. 260). The table he

mentioned included the following dichotomies: richness (i.e., depth of understanding) vs. precision (i.e., accuracy of understanding); theory vs. data; man as master (i.e., agency) vs. man as victim (i.e., mechanism); man as good vs. man as evil; transsum (i.e., holism) vs. andsum (i.e., atomism); mind vs. body; subjective vs. objective; present vs. past; nature vs. nurture; and complexity vs. simplicity (see p. 260). The former aspect of each dichotomy was aligned with the phenomenological/existential/subjective psychology and the latter with behaviouristic/objective psychology. Wertheimer hoped that the future of psychology would include "more fusion of yin and yang" (p. 260).

Fuchs and Kawash (1974; Kawash & Fuchs, 1974) empirically examined Watson's (1967) dichotomous prescriptions in two articles. In the first article, they explained the purpose of their study: "...the present study was designed to determine the position of each of five schools of psychology on Watson's (1967) prescriptions by obtaining ratings of the schools on the prescriptions from knowledgeable judges" (p. 352). The five schools were behaviourism, Gestalt, functionalism, psychoanalysis, and structuralism. The raters were 68 members of Division 26 (history of psychology) of the American Psychological Association (APA). And, after discussing their results, they concluded:

> Watson's (1967) prescriptive dimensions would seem to provide a useful framework within which to summarize and characterize the systematic positions within psychology. Judges were able to assign ratings to schools on each of thirty-six dimensions with a satisfactory degree of reliability. The resulting scores provided a characterization of each school and enabled comparisons among schools to be made

in terms of relative emphasis on aspects of the subject matter, methods, theory, and aims of psychology and its general scientific character. (p. 365)

Of more relevance for this history, however, was Kawash and Fuchs' (1974) factor analysis of Watson's prescriptions. They found that seven factors accounted for many of Watson's prescriptions: naturalism; dynamicism; inductivism; peripheralism; dualism; idiographicism; and molarism. Factor one included: naturalism, nomotheticism, mechanism, contentual subjectivism, vitalism (negative factor loading), and supernaturalism (negative factor loading). Factor two included: dynamicism, developmentalism, staticism$_1$, and staticism$_2$. Factor three included: inductivism, deductivism (negative factor loading), and empiricism. Factor four included: functionalism, peripheralism, and structuralism. Factor five included: dualism, contentual subjectivism, and monism (negative factor loading). Factor six included: idiographicism, nativism, deductivism, and utilitarianism. Finally, factor seven included: molarism, staticism$_2$, and nomotheticism.

Kawash and Fuchs admitted the following prescriptions did not load on their factor analysis: centralism, contentual objectivism, molecularism, purism, conscious mentalism, unconscious mentalism, indeterminism, determinism, irrationalism, rationalism, methodological objectivism, methodological subjectivism, qualitativism, and quantitativism. However, they suggested: "It is possible that the themes would have emerged if the rotations had not been done in a blind fashion; thus it is not clear that the failure of these themes to emerge in the factor structure can be interpreted as an indication of their relative significance for the schools" (p. 436). Finally, in the first

article they also admitted their research had a number of important limitations, which could have ultimately affected their factor analysis (see Fuchs and Kawash, 1974 for a complete description).

Cronbach (1975) re-evaluated his previous article (Cronbach, 1957) in light of nearly twenty years of psychology's growth and admitted: "...the line of investigation I advocated in 1957 no longer seems sufficient. Interactions are not confined to the first order; the dimensions of the situation and of the person enter into complex interactions. This complexity forces us to ask once again, Should social science aspire to reduce behavior to laws?" (p. 116). Although Cronbach still insisted correlational and experimental psychology should work together, he argued: "Social scientists generally, and psychologists in particular, have modeled their work on physical science, aspiring to amass empirical generalizations, to restructure them into more general laws, and to weld scattered laws into coherent theory. That lofty aspiration is far from realization" (p. 125). He concluded that, although psychologists should make use of natural scientific methodologies, the discipline should avoid having "too narrow an identification with science" (p. 126).

Fiske (1979) examined "two worlds of psychological phenomena" (p. 733)—behaviour and "dispositions or tendencies (e.g., attitudes, traits, values)" (p. 734). He argued that studying behaviour was more objective, suffered fewer threats to reliability and validity due to investigators' subjectivity, and had made more empirical discoveries and generated more scientific laws than studying the other world of phenomena. However, he admitted that studying subjective phenomena was "of central importance in

everyday affairs" (p. 738). He concluded that—instead of being overly concerned with psychology per se—psychologists should limit themselves to investigating "one world of phenomena, some particular set of events or behavioral products" in order to "progress toward a 'mini-science' for that world of phenomena" (p. 739).

Eysenck (1987) argued: "There seems to be no doubt that psychology is plagued by contradictions so profound as to threaten its integrity as a unified discipline" (p. 95), and he was explicit in his prescription for resolving psychology's crisis. He called the separation of science and humanism "a necessary disunification" (p. 100), and he argued:

> If we wish to have a single, unified science of psychology, then we must slough off the unscientific humanist-subjective approach completely; psychology cannot live as half science, half art. In saying this I wish to make it clear that I have the highest possible regard for art, for religion, and for the many other aspects of life that are not part of science. Science certainly is not all of life, and we would all be the poorer if it were. But this fact should not lead us to discount the importance of science or its need for purity. (p. 101)

Echoing Kendler's (1987) solution, Eysenck concluded: "Psychology will never become a unified science unless we terminate the ordeal by quackery through an amicable divorce, separating the scientific from the humanist part and concentrating all our energies on the former" (p. 101).

Fishman (1986, 1987, 1988b, 1990a) presented an argument for dividing psychology between three paradigms: experimental, technological, and hermeneutic. The experimental paradigm [similar to objective psychology] was based on logical positivism,

engaged in quantitative and nomothetic research, desired formal theories, worked in laboratory settings, emulated the natural sciences, and published in 'highly technical journals'. The hermeneutic paradigm [similar to subjective psychology] was based on social constructionism, engaged in qualitative and idiographic research, desired understanding of phenomena, worked in natural settings, emulated the humanities, and published in both academic journals and "intellectual media" (e.g., New York Times) (p. 7). The technological paradigm was a blend of the other two paradigms and was based on social constructionism, engaged in quantitative and idiographic research, desired solutions to practical problems, worked in natural settings, emulated statistical research organizations, and published primarily technical reports intended to guide practical decision-making.

Fishman argued these three paradigms could be understood as three 'factors', which constituted "the basic underlying dimensions and categories for rationally reducing the multiplicity and fragmentation of contemporary psychology" (1985, p. 4). With respect to unity, he suggested that psychology limit itself to unifying efforts within each paradigm. He added that psychologists should explicitly state their underlying assumptions so that the assumptions could be evaluated in terms of their ultimate consequences (Fishman, 1988a).

Conway (1992) posed the rhetorical question: "How is it that we psychologists come to hold such contrasting metatheoretical positions about the discipline?" (p. 1). He then examined the two cultures within psychology under the heading: "The Grand Dimension in Metaphysical Values: Science versus Humanism" (p. 1). In this section, he primarily

discussed Kimble's (1984) article on the two cultures and, like Kimble, he was pessimistic about the possibilities of unification the two cultures:

> ...most of us seem content to go our separate ways, and some others seem able to struggle along in dialectical fashion with what remain as competing and incommensurable metaphysical values. To ask which set of values, Scientific or Humanistic, best represents knowledge about humans is to ask a question that most philosophers consider to be undecidable. Each set of values depicts reality and may lead to truth, the polarities can neither contradict nor support one another. (p. 4)

In addition, Conway argued that personality differences amongst psychologists strongly contributed to differences in worldviews held within psychology.

Teo (1999) added a third culture to the mix. He proposed three "functions of knowledge in psychology": SCIENTIA, CULTURA, and CRITICA (p. 1). For Teo, SCIENTIA referred to "practices that produce primarily knowledge about a psychological object or event, or details of this object or event. The cultural meanings of knowledge for a subject are secondary" (p. 2). CULTURA "produces meaning-knowledge primarily about a subject for a subject, where the subject may be a single individual, a community, or a whole culture" (p. 3). And CRITICA "has a monitoring and controlling function over SCIENTIA, CULTURA, and even over itself by being self-critical. CRITICA's status is different from the other knowledge functions, as its level of research is mostly meta-psychological" (p. 3). Teo added that CRITICA may be more common in psychology (as opposed to other disciplines) due to the discipline's complex subject matter.

With respect to the fragmentation of the crisis and unification literature, Teo argued: "How could the majority of psychologists be convinced that unification is productive, when proponents of unification were not able to unify the small community of unification advocates or to commit them to a few unification goals?" (p. 8). Instead of unification, he suggested that "it seems again more adequate to propose a pragmatic approach that focuses on actual knowledge functions in psychology instead of demanding an ideological commitment to unification" (p. 8); and he added that any unification which favoured one domain within psychology or which "does no justice to the factual work of psychologists" (p. 8) would not be endorsed by the discipline's members.

Teo proposed an 'equilibrium' for the three knowledge functions in psychology. He argued that when the three became unbalanced, problematic situations arose. For example, when SCIENTIA was emphasized, the relevance of psychological research to society declined; in contrast, when CULTURA was emphasized, "a successful and important dimension of knowledge production is abandoned" (p. 10). Finally, when CRITICA was de-emphasized, Teo argued it resulted "in psychology being an unconscious discipline that is prone to many self-misunderstandings" (p. 10). Teo concluded that all three knowledge functions should be taught to students of psychology and publication venues should be maintained on an equal level for all of them as well; furthermore, criteria for evaluating knowledge within a given knowledge function should be those which were developed from within that knowledge function (i.e., criteria from one knowledge function should not be used to evaluate and/or dismiss another knowledge function).

Denmark and Krauss (2005) argued that it was possible to pursue "unification through diversity" (p. 15), but that the two cultures would have to be bridged: "We are convinced that psychology's diverse specialties contain, in different proportion, elements of the natural sciences and the humanities. If it is indeed possible to unify psychology under the umbrella of a single paradigm, that paradigm must be a synthesis of these two great streams of the western intellectual tradition" (p. 34-35). However, even its current state, they argued psychology was functioning well amidst its diversity: "As a discipline, psychology is successful by any standard of production applied to it. It is well established and well thought of. It is productive, influential, and popular. Each leg of the tripod on which it rests—knowledge creation, knowledge dissemination, and knowledge application—are strong and of proven durability" (p. 15).

Finally, Drob (2003) provided a "dialectical solution" (p. 102) to the fragmentation of psychology,[73] which he argued was based on the following dichotomous schisms: determinism vs. free will; objectivism vs. constructivism; elementism vs. holism; public vs. private mental criteria (i.e., observability vs. introspection); individualism vs. collectivism; factualism vs. hermeneutics (i.e., realism vs. interpretivism); and complete knowability vs. an essential unknown (p. 113). Based on his analysis of these schisms, he argued that the multiple paradigms in psychology were each necessary but not sufficient for developing a "synoptic view of the human mind (p. 115). However, Drob ultimately concluded: "It is my hope that the analogies and arguments set forth in this

[73] Like Koch (1993) and Yanchar and Slife (1997a), Drob argued that the fragmentation of the fragmentation literature was ironic: "...a cursory review of [the fragmentation] literature reveals there to be nearly as much fragmentation among those who propose solutions to psychology's disunity as there is within the field itself" (p. 103).

article have provided a certain justification for liberalism and multiperspectivism in psychology. Psychology unified under a single, dogmatic paradigm is an impossible and unlaudable ideal" (p. 122).

The Crisis and Fragmentation of Psychology

Fragmentation. Drob (2003) addressed the fragmentation of psychology and argued that a dialectical understanding would be the best 'solution' psychology could manage—unity was impossible. Gardner (1992b, 2005) provided a similar argument. In response to the endemic fragmentation of psychology, he suggested psychology could take one of four options: ignore the problem; argue psychology *was* unified or *was* a success; await a grand theorist who could fix the problem; or argue that other disciplines were in similar predicaments. However, Gardner admitted he preferred a fifth alternative, which was to allow various parts of psychology to be absorbed by other, rapidly-developing disciplines (e.g., cognitive psychology absorbed by cognitive science).[74]

Specifically, Gardner argued neuroscience, cognitive science, cultural studies, and various applied areas[75] would absorb the majority of psychology. What would remain would be "the person-centered quartet" of "personality, self, will, and consciousness" (p.

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[74] Spence (1987, 1990) took a less optimistic outlook regarding this option: "In my worst nightmares I foresee a decimation of institutional psychology as we know it. Human experimental psychologists desert to the emerging discipline of cognitive science; physiological psychologists go happily to departments of biology and neuroscience...[etc.]...In universities with doctoral programs, departments of psychology would be pale shadows of their former selves, their members outnumbered and outclassed by the natural sciences...and the humanities" (1987, p. 103).

[75] While reflecting on the fifteen years of development in applied psychology that had occurred since he published his initial article, Gardner (2005) admitted: "At least in the case that I know best, educational psychology, [my] prediction [about absorption into other disciplines] was wide of the mark. Indeed, I have become quite deeply involved in a new concentration...called 'mind, brain, and education' (MBE)" (p. 82).

185).[76] Though he admitted these areas, historically, had been "a slight embarrassment" (p. 185) due to a lack of progress in terms of convincing research, he argued scientific research could do only so much since the subjective self was largely the domain of the humanities. He concluded that psychology should draw on the sciences but especially the humanities in the pursuit of knowledge in this remaining area of study. Gardner (1992a) concluded: "it may be time to bury scientific psychology, at least as a single coherent undertaking" and recommended that future students and researchers draw from a variety of specializations instead of pursuing a discipline of 'psychology'.[77]

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[76] In his second work, Gardner (2005) presented personality, self, and will—"the person-centered trio" (p. 83). He noted that in the fifteen years that had elapsed, consciousness had become a hot topic in various disciplines. However, Gardner altered his overarching argument somewhat and admitted that consciousness—*not* the person- centered trio—could become the basis for *integrating* the various disciplines interested in psychological phenomena: "Perhaps consciousness—rather than personality, self, or will—will emerge as the topic around which the psychologically oriented sciences will ultimately coalesce" (p. 86).

[77] Five comments were published on Gardner's (1992a) article. Sternberg (1992) argued that scientific psychology should not be abandoned. His reasoning went as follows: scientific psychology was young—in its childhood only, when compared to the more established sciences. Science proceeded developmentally, in stages, and in its earlier stages—which was where psychology was at—abundant diversity was expected and should be welcomed and encouraged; diversity enriched the discipline, it did not destroy it. Sternberg argued that psychologists should tolerate ambiguity and he concluded that all fields of study had unclear boundaries and psychologists simply should not worry so much.

Marková (1992) presented two main criticisms: Gardner's person-centered quartet ignored the socio-cultural dimension of human life; and it also failed to include language and communication as an important domain of psychological research.

Oatley (1992) suggested that it was important to distinguish between psychology the subject matter and Psychology the discipline; he argued that the former had an optimistic future, but the latter had a pessimistic future. He also argued that there were two kinds of truth in psychology—consistency (i.e., a criterion from the arts/humanities) and correspondence (i.e., a criterion from the sciences)—and he argued that both had to be recognized.

Woodward (1992) argued that feminists had broadened the definitions of 'empiricism' and 'objectivity' and, in so doing, had provided a basis for psychology to continue to recognize itself as a complex science. In response to the standard criticism that these expanded definitions ultimately ended in extreme relativism, he argued that "we must seek more subtle analyses" and not submit to a relativistic fate.

Finally, Potter and Wetherell (1992) suggested that a constructionist, "discourse-based approach" could "take on cognitive scientists in their heartland" and "raise fundamental questions about the very format of psychological discourse itself" (p. 226). They concluded: "Whether this is seen as psychology being absorbed by something else ultimately depends on how seriously one takes psychology's past fictions" (p. 226).

Finally, Miller (1992) described psychology as an "intellectual zoo" within which parts were "clearly scientific" while others were "pure moonshine" (p. 40-41). He added that one could "make psychology and psychologists out to be almost anything that pleases your fancy at the moment" (p. 41). He argued that much of the fragmentation within psychology was attributable to two opposing poles within psychology: "biotropic and sociotropic" (p. 43). He concluded that psychology should focus on becoming a 'science of consciousness', which "could not be cannibalized by either biology or sociology" (p. 44).

Crisis, crises, malaise. As I mentioned in the introduction to this 1970-2005 section, the phrase 'crisis of psychology' experienced a renaissance during this time period. For example, Nazarro (1976) presented psychology as having an "identity crisis" (p. 44)—which, he argued, had specific implications for pedagogy. More significantly, the *Journal of Social Distress and the Homeless* devoted a special issue to the crisis of psychology in 1996.[78] Bakan (1996) provided the first article—entitled "The crisis in

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In response to his commentators, Gardner (1992a) argued: "While I find the five commentaries interesting and constructive, I am not convinced that my analysis requires rethinking" (p. 229). For Oatley (1992), Gardner simply agreed: "I find little to quarrel with in Keith Oatley's presentation" (p. 230). In response to Sternberg (1992), he suggested: "our disagreement cannot be joined because it reflects different intuitions; only time will tell who is right" (p. 230). For Marková (1992), Gardner admitted that a cultural perspective could be admitted into psychology, but then psychology could not be seen as a science—"at least in the way that [the term] has been utilized in established sciences" (p. 230). In response to Potter and Wetherell (1992), Gardner admitted that a literary, discursive approach could be beneficial, but that this approach would also not be a *science*; therefore, his initial prognosis of the demise of scientific psychology still held. Finally, Gardner suggested that Woodward (1992) was "challenging the very notion of a *discipline*" but he argued that "as researchers and scholars...we do need the constraints and the models which have evolved" (p. 230, italics in original). Gardner concluded that his critics' use of the term 'science' was "very different from that now canonized by physical and natural scientists, by mainstream psychologists, and by the general public" (p. 231). Instead of science, he suggested that his critics should develop a new name so as to avoid confusion.

[78] From 1992-2005, special issues on the crisis and unification of psychology were published by: the *Journal of Social Distress and the Homeless* (see Bakan, 1996), the *Journal of Mind and Behavior* (see

psychology"—in which he argued that the crisis in psychology was not the result of a *lack* of psychologists or literature being produced; instead, he argued the crisis was a result of three 'losses': 1) a loss of understanding of psychology's subject matter: "the self-evident fact that mentation, at least in the form of decisions, plans and goals, is essential to human conduct" (p. 335); 2) a loss of understanding of psychology's method: "the uncritical acceptance of the assumption that the psychological is readily revealed by applying statistical methods to aggregated measures of behavior" (p. 335); and 3) a loss of understanding of psychology's mission: psychology having separated itself from politics "and the task of designing the social order so that it is in harmony with human nature" (p. 335).

O'Connell (1996) argued that there was no such thing as a *crisis of psychology per se*. Instead, he cited Bühler's (1927) book *Die Krise der Psychologie* and argued:

> If there was a crisis in psychology in 1927, the currently alleged crisis must be considered either a continuation or a recurrence. I wish to argue that it is, in fact, a continuation. But the very concept of a continuing crisis is oxymoronic. A crisis is by its very nature an acute condition, not a chronic one. And so, I would prefer some other expression—perhaps *malaise*. A glance at Webster's definition of the word *malaise* provides the rationale for my preference: "An indefinite feeling of debility or lack of health often indicative of or accompanying the onset of an illness (1981, p. 689). (p. 343, italics in original)

Yanchar & Slife, 2000a, 2000b), the *International Journal of Psychology* (see Lévy-Leboyer, 1992), and two by the *Journal of Clinical Psychology* (see Henriques, 2005a; Henriques & Cobb, 2004).

He added: "To consider the condition of this patient [psychology] as critical is not only melodramatic, it is ahistorical. There is very little new or different in the current situation to warrant ICU or ER terminology" (p. 343).

Salzinger (1996) agreed with Bakan (1996) that psychology was in a state of crisis and he attributed it to psychology's reward system: "I see a crisis brought on by psychologists' need to publish, not so much because they have made an important original discovery, but rather because they need to get grants and salary increases. Psychologists cannot, because of those materialistic needs, engage in what is supposed to be the tradition in academic research, to think before publishing" (p. 354). However, Salzinger disagreed with some aspects of Bakan's diagnosis and recommendations as well. Specifically, he suggested psychology should not only study mental life, and he also argued that the solution to the crisis should be the result of "more science rather than less" (p. 356).

Finally, Mos (1996) suggested there were two meanings of crisis which were used by authors who wrote on the topic: 1) a crisis of psychology as "a unified, or autonomous, discipline" (p. 359); and 2) "a set of symptoms or purported criteria that would suggest that psychology is not yet a science, or if a science a dysfunctional one" (p. 359). He added that he agreed with Bakan's (1996) three losses and he suggested that they had originated "in the founding of psychology as an autonomous discipline" (p. 368), which had bought its 'disciplinary autonomy and scientific unity' at the expense of divorcing itself from various human psychological subject matter. Mos concluded the special issue

on an optimistic note, however, arguing that the human capacity for self-reflection provided hope that the crisis of psychology could be overcome.

Rieber and Wollock (1997) suggested that 'the crisis of psychology' was historically discontinuous and that Vygotsky's (1997) work had been already addressing a *second* crisis.[79] Furthermore, they argued the *contemporary* crisis of psychology was also distinct from Vygotsky's crisis. They argued that he had been addressing a crisis of psychology where "the profession was everywhere undeveloped" while the contemporary crisis arose because "the field is overdeveloped and its general level is mediocre" (p. x). They suggested that, to address its contemporary crisis, psychology needed to address the validity of its presuppositions, as well as of the "presently recognized boundaries of knowledge" (p. xi).

In his book *Current Crises of Psychology*, Westland (1978) argued there were numerous simultaneous cris*es* within psychology. In his introduction, he explained his reasoning for the use of the plural form of 'crisis': "There have always been those prepared to bandy the singular form about, but is has become more and more apparent that an opening sentence such as 'Psychology is in a state of crisis' tells the reader very little about what is to follow. Different commentators see many different crises, and there is little consistency among them" (p. vii). Specifically, he argued psychology experienced the following crises: a usefulness crisis (i.e., psychology's research was too divorced from human life/application); a 'laboratory' crisis (i.e., psychology's research

[79] The first, they argued, was the debate between Wilhelm Dilthey (1833-1911) and Hermann Ebbinghaus (1850-1909) which occurred toward the end of the 19th century (see Rieber & Wollock, 1997, p. vii; see also Teo, 2005).

tried to be too experimental and overlooked the subjectivity of the experiment's participants); a statistical crisis (i.e., statistics were misused, inappropriate, or logically fallacious when applied to psychology's subject matter); a science crisis (i.e., the status of 'a science' was premature or inappropriate for psychology); a philosophical crisis (i.e., numerous foundational philosophical issues plagued psychology; for example, the self-reflexive nature of humans or debates surrounding psychology's philosophy of science); a professional crisis (i.e., a crisis of psychology's identity; how were psychologists to be defined relative to other professionals?); a publication crisis (i.e., selection biases of journal editors and statistically insignificant findings not being published); an ethical crisis (i.e., was psychological research ethical, particularly in cases where deception was necessary?); and a resolution-of-crises crisis (i.e., 'a second-order crisis' regarding whether the crises which faced psychology could be resolved).

In his prescriptive concluding chapter, Westland made two primary arguments: psychology should cease with its support of false dichotomies; and psychology should lower its sights in terms of aspiring to be a completely coherent science. With respect to the first point, he rejected Hitt's (1969) dichotomous presentation of 'two models of man' (i.e., behaviouristic and phenomenological): "I believe that not only are most such dichotomies false in their implications, and therefore that arguments about them are largely sterile, but that they might cease to distort the perspective if we focused instead on epistemological considerations, if that is not too pretentious and frightening a phrase" (p. 161). Finally, with respect to the second point, Westland argued: "If I have a positive thesis to present, it is that the hope of a unified discipline of psychology is a chimera, in

the foreseeable future if at all, but that that is in the nature of things and does not amount to condemnation" (p. 163).

Products and sustaining factors. Fishman (1987) argued: "The incentive conditions for *individual* psychologists today actually appear to encourage the continuation of this disunity. Rewards generally come from innovation in very specialized, frequently highly esoteric, subfields—as long as the work is new and meets the particular methodological criteria within the subfield" (p. 25). Kunkel (1985), Wittig (1985), Wachtel (1985), Maher (1985), Eifert (1985), and MacIntyre (1985) also argued that the professional reward system of academia encouraged novelty and proliferation at the expense of (more time-consuming) integration efforts. Overmeier (1990) added that the graduate training model was designed with this reward system in mind and, therefore, contributed to the perpetuation of the fragmentation of psychology by emphasizing extreme specialization and frequent publication.

Thorngate (1990a, 1990b; see also Wittig, 1985) argued that the proliferation of psychology's literature, when combined with human limitations on attentional capacity, constituted a major problem for psychology. Thorngate (1990b) noted: "Psychologists are now collectively publishing articles at the rate of about 100 per day, about one every 15 minutes" and he argued that "the cumulative effect is remarkable" (p. 262). He further argued that some psychologists saw this 'cumulative effect' as a healthy sign of growth and that they supported their position with the 'publish-or-perish' mantra. However, he argued that the limits of humans' attentional capacity made the proliferation of psychology literature a substantial problem, and he listed four axioms related to

attentional capacity. First, "*we must pay attention to be informed*" (p. 263, italics in original). Secondly, "*attention is a fixed asset*" (p. 263, italics in original). Third, "*attention can be divided among people as well as across time*" (p. 263, italics in original). Finally, "*attention is invested in expectation of emotional returns*" (p. 264, italics in original).

Thorngate then went on to argue that "information tends to proliferate, but attention does not" (p. 264). He argued that "information tends to expand in *length*...*breadth*...[and] *depth*" (p. 264, italics in original), and he added that the information in psychology was far past the point where any individual could read and digest it all. Though he admitted that arguments in favour of increased specialization (i.e., reducing the *breadth* axis) and focussing only on new information (i.e., reducing the *length* axis) had historically been put forward to deal with this broad problem, he argued: "Topical concentration and historical truncation are defensible means for selecting the information we attend, but their collective effects can undermine our discipline. A discipline is defined by a shared history, a common core of information, and the mutual interests of its members" (p. 265). Thorngate concluded that his third axiom needed to be utilized much more extensively within psychology in order for the discipline to grow as a science; however, he admitted the prospects looked grim: "...with no common experience save one course in statistics, how can we expect to communicate? And if we cannot communicate, why should we expect our discipline to evolve or even survive?" (p. 266).

Thorngate then argued that devoting more time to teaching and learning, and using computer technology advances (e.g., search engines and citation indices), synopses, and selection criteria (e.g., 'scientific standards') could all be employed to combat the broad problem. However, he also argued each had its limits. First, "few of us are told to teach or perish" (p. 166). In other words, the emphasis in academia for promotion and tenure was on research and publication. Secondly, search engines and citation indices were imperfect and important articles could go unnoticed if specific (and sometimes uncommon) key terms were not entered. Third, synopses were "not used without cost. The success of a synopsis depends very much on its fidelity, its ability to capture the essence of the original...[also] synopses omit detail, and usually the detail they omit is important in judging the quality of the information they summarize" (p. 267). Finally, selection criteria, such as scientific standards, still required the user to read articles to determine whether or not they met the criteria; alternatively, he admitted the user could rely on editors' judgments as to whether articles met certain criteria, but he argued: "Good editors and reviewers are hard to find because most candidates cannot afford the attentional investments required to do their jobs well" (p. 267-268).

In terms of a prognosis, Thorngate suggested that both an increase in the fragmentation of psychology and a decline of scholarship would be the products of the broad problem he presented. First, he argued that rapid specialization would continue to parallel the combination of literature production and attentional limitations; this rapid specialization would accelerate psychology's fragmentation. Secondly, in terms of a decline in scholarship (see also Maher, 1985), he argued: "Within 20 years the growth of

our discipline will virtually guarantee that the psychology we are now producing, often at great personal sacrifice, will never be read, cited, or otherwise consumed by anyone" (p. 270). He added: "Our Deans tell us to publish or perish, so we all produce more and flood the market with information. In response, our Deans stop reading and start counting. In turn, we do what counts. Sooner or later, 'Everybody lies; but it doesn't matter, since nobody listens'" (p. 270). Thorngate concluded: "To stimulate a reduction we must change the structure of academic rewards. In particular, our current publish-or-perish system must go. It has been the source of far too much suffering and injustice, and it has never been shown to increase the importance of our questions, the quality of our answers, or the size of our audience" (p. 270).[80]

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[80] Seven responses were made to Thorngate's (1990b) article. Tees (1990) agreed with Thorngate that psychology was going to dissolve, but argued that the emergence of 'psychology' as a specialization within other fields was a more likely centrifugal force for pulling apart the discipline. In short, he argued: "The information explosion is an effect rather than a cause of any dilemma" (p. 272). He also disagreed that administrators ignored the quality of publications and further argued that once specialized journals reached a critical mass, the competition would produce the need for a resurgence of integrative journals.

Adair (1990) argued that Thorngate failed to provide any empirical evidence for his arguments. As a result, Adair presented some of his own research:

> I counted the citations in psychological research articles over a number of years. The number of articles cited by Canadian researchers in the *Canadian Journal of Behavioural Science* increased from a mean of 15.81 in 1972 to 21.81 in 1978-1980 to 26.01 in 1984-1986...These data suggest that there is an increased literature; [but] we also seem to have developed methods for accessing, processing, and reporting it...[also] when we empirically examined one of his arguments, that the new is not being read at the expense of the old, we found the opposite trend. The citation in *CJBS* of "old" (20+ years old) literature increased monotonically—a 67% increase in 1984-1986 from what it was in 1972-1974. (p. 274)

Adair concluded that a lack of empirical evidence was a major shortcoming of Thorngate's article.

Grusec (1990) argued against increasing publication standards. Instead, she suggested that psychology should increase the relative importance of review articles and also increase the size of research teams to promote working on more extensive projects, which would result in higher quality articles. Also, after surveying a convenience sample of biologists, Grusec concluded that psychology was not so unique in worrying about Thorngate's issues. But, she did suggest that psychology was more preoccupied with the issues since the biologists seemed confident that their students could gain a breadth of information more informally (e.g., by attending conferences).

Danziger (1990) agreed with much of Thorngate's article, but said he wished Thorngate had avoided putting the argument in a 'scarce-resources', 'impending doom' rhetorical frame. Danziger suggested that the problems Thorngate had illustrated were less the result of mass publication and more the result of the low percentage of quality articles being produced amongst the mass of literature. He concluded that the

social nature of science—Thorngate's third axiom—could assist psychologists in dealing with the rapid development of the discipline's literature; informal social networks could help alert individuals to publications within the broader literature base which were salient to their research topics.

Mos (1990) was critical of Thorngate's argument. He argued: "It is in our professing of the science of the psychological that we must find the reasons for both the nature and extent of our specializations and the fragmentation of the discipline. It is not the limitation on our attentional resources which is responsible for specialization or fragmentation" (p. 282). He also argued that if quantity of publications was eliminated as a criterion, it could become more difficult to evaluate grant proposals and thus to distinguish who should be granted the limited resources which were available. He also suggested that radical proposals for changing a discipline's reward system could locate an individual outside of that discipline; instead, Mos argued changes had to come from within the discipline (and thus were likely to occur gradually as opposed to radically).

Stam (1990) agreed with much of Thorngate's article, but suggested that 'Thorngate's problem' was a sub-issue of a larger problem: the relationship between data and theory/knowledge more generally. Stam outlined two possible counter-arguments to Thorngate's argument: "One argument (the 'who cares' argument) might be that psychology was artificially patched together in the first place under intense pressure to be relevant to a new age...Its dissolution is therefore, if not inevitable, at least not surprising" (p. 282-283); and "the 'so what' argument states...that psychology is a unified discipline, if only in what it takes to be its starting point...If the various segments of the discipline do not talk to one another, that is too bad but no great loss" (p. 283). However, he rejected both of these arguments. For the 'who cares' argument, Stam argued that—even if psychology had historically developed from a variety of different paths—"having established itself as a discipline, it became a cultural and intellectual force in its own right, and...it certainly appears that the discipline will remain so in the short run" (p. 283). Finally, for the 'so what' argument, Stam argued: "Research articles make sense only within the context of follow-up research to answer the many questions left unanswered by any single study. Without conceptual direction, the amount of data that can be collected for any given problem is, in principle, unlimited" (p. 283-284).

Vikis-Freilbergs (1990) agreed with much of Thorngate's argument and added to it that the turnout for many presentations at large conferences was also decidedly low. She argued that more was not always better and that there was a need to ease up on external pressures in order to allow researchers to produce quality, as well as quantity of, publications. She added that there was also a need to ask better research questions and for more of a general psychological perspective in the psychology curriculum.

In response to his commentators, Thorngate (1990a) stated that, to conserve space, he would focus on the negative reactions instead of the positive. To Tees, Thorngate admitted that other disciplines experienced the broad problem on an even broader scale than psychology; however, he countered that these disciplines also shared common models and theories, which allowed them to enjoy a unity that psychology lacked. In terms of the potential for a self-correcting turn to more integrative journals in the future, Thorngate suggested that this could simply be an infinite regress of the problem since researchers would still have to read contrary reviews on the same topic or entrust to 'experts' that the reviews did justice to the original material.

In response to Adair, Thorngate wryly noted:

> Perhaps I should have considered [Adair's] data before writing my previous article, but I didn't know they existed. I tried one of Adair's 'constructive strategies devised to enable us to cope with the increasing amounts of literature' in order to see if the three papers he authored and cited could be found. My 'computerized literature search' of PsychLIT uncovered none of these articles. Nor were any of the articles in my university's library. (p. 289)

Thorngate then went on to challenge Adair's empirical data. His challenge was quite acute and further illustrated his primary arguments:

> Adair notes that the number of citations in the *Canadian Journal of Behavioural Science* increased from a mean of 15.8 in 1972-74 to 26.0 in 1984-86. Apparently he views this mean increase of about 62% in 12 years as a refutation of my argument that our time for consuming psychology is limited...Alas, Adair makes no logical or empirical case for the relation between the number of citations in published articles and the amount of time we have to read them. We thus do not know, for

Rieber (2001) argued that psychosocial and socio-cultural influences had a major impact on the recurrent crises which psychology experienced: "These critical periods or crises are directly related to the psychological, political and economic conditions of the times. For example, peak periods of crises may be observed just before, during and after major wars, depressions, and/or inflations" (p. 111). Specifically, he concluded that psychosocial distress and resulting 'psychopathy'—which, he argued, were becoming normative and were being 'sustained' by society's institutions—were major contributing factors to psychology's crises.

Finally, the science-practice tension was cited as a major product and sustaining factor of psychology's fragmentation. For example, Fowler and Bullock (2005) argued that the

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example, whether the 62% increase is due to an equivalent increase in time available for reading or a 62% decrease in the care taken to read. Note, however, that the 15.8 articles cited in 1972-74 represent about 0.12% of the mean number of articles published during those years (12, 672 as estimated from *Psychological Abstracts* counts). The 26.0 articles cited in 1984-86 represent 0.08% of the mean number of articles published during those years (32, 312). Thus, the proportion of literature cited in each article has decreased, in part I suggest, because we have not been able to increase the amount of our attention or the speed of our reading to keep up with the growth of our discipline.

Incidentally, it is instructive to project how many citations we can expect in an average *CJBS* article given the 12-year, 62% growth rate Adair found. If his trend continues, then by the year 2020 each article will contain about 105 references. By the year 2090, each will contain about 1,750 references. I am not sure where future psychologists will find the time to ingest, much less digest, these numbers of references. Perhaps someone will invent more methods of speed teaching, speed meeting, speed writing, and speed living that will give future generations of academics more reading time. (p. 289)

In response to some of the other commentaries, Thorngate agreed that an emphasis on theory was important and that it was necessary to distinguish between psychology the subject matter and Psychology the discipline. However, he argued there were potential problems for relying on informal methods or communities (i.e., "invisible colleges", p. 290) for attending to knowledge:

> At the moment, these invisible colleges do indeed appear to be a wise attentional investment. Yet as information proliferates, invisible colleges multiply (note the growth of specialty conferences) and so, too, do the conditions of their demise. Students, for example, enrol in visible colleges, pay their tuition fees to visible colleges, and expect an education from visible colleges in return. Visible colleges are supposed to house visible colleagues, who are supposed to communicate with each other, co-ordinate course offerings, and generally act as members of a visible academic community. As we increasingly affiliate with our invisible colleagues, however, we have less time to spend with our visible ones and less attention to pay to students and colleagues therein...when should our visible colleges stop paying our visible salaries? (p. 290)

Thorngate concluded that the dilemma he had presented would result in a 'survival of the most interesting' contributions to the literature, as opposed to the most scholarly or most scientific.

science-practice tension was a product of philosophical and conceptual disagreements and resulted in professional fragmentation[81] (e.g., tensions within the American Psychological Association (APA) between scientists and practitioners, as well as the establishment of the American Psychological Society (APS) in response to the perceived practitioner-dominance of the APA). Also, Wand (1993) argued that scientists and practitioners experienced the split differently—scientists experienced it as a tension between the two camps while practitioners experienced it as "a degree of disconnection and alienation from the scientific pursuits of mainstream psychology" (p. 131). Wand concluded that developing "new ways to be scientific may help to bring the two groups together" (p. 131) and she noted that, meanwhile, practitioners would maintain their emphasis on humanism and being 'useful'.[82]

No crisis. Although the phrase 'the crisis of psychology' was used more frequently in this time period, it was not always invoked in support of the view that psychology was, in fact, in a state of crisis. Some authors argued that 'the crisis of psychology' was an incorrect label given to a discipline which was either: progressing just fine; experiencing healthy specialization; or not worth retaining as a single area of study. For example, Kassinove (2002) argued that psychology was already unified, Baars (1994, 1995a, 1995b) argued a specialization of psychology (cognitive) was unifying the discipline, and

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[81] Harari and Peters (1987) also argued that professional fragmentation was "a by-product of...the fragmentation of the discipline of psychology" (p. 822). They also surveyed 249 members of APA and over half of them responded that adding new divisions to the association was "not at all important" (p. 824).

[82] Boneau (1988) also argued that unity could be pursued through "making psychology useful" (p. 1) while Levant (2005) argued that psychology could be unified "through social relevance" (p. 107).

Marková (1992) argued: "Psychology is a subject with many potentials, one which could freely form allegiances with a variety of subjects and contribute to the formation of new subdisciplines within its very broad field. Perhaps when this happens we shall finally disarm complaints about 'crises' within our heterogeneous field and stop worrying whether psychology is or is not a science" (p. 220).

Matarazzo (1987, 1992) argued that there was a 'single core' of subject matter within psychology, which all students were exposed to, regardless of specialization, and which provided psychology with its unity. This single core included education in: "(a) the biological bases of behaviour; (b) cognitive-affective bases of behaviour; (c) social bases of behaviour; and (d) individual differences in behaviour" (Matarazzo, 1992, p. 328). He also argued that psychology did not have any *official* specialties, which were recognized by society. Matarazzo (1987) concluded that "there is only one psychology, no specialties, but many applications" (p. 893), and he added that there was more unity in psychology than commonly believed; he also added that his analysis extended across cultures.

Church (1990) argued that disciplines were evolving within academia and that psychology would take part in this evolutionary process. He argued that psychology would continue to integrate with linguistics, anthropology, and computer science to form cognitive science and to integrate with anatomy, pharmacology, and physiology to form neuroscience. He also suggested that cognitive science and neuroscience would continue to evolve and become cognitive neuroscience.

Overmeier (1989) argued that the search for unity in psychology was often conducted under the rhetoric that other sciences were more unified than psychology; however, he argued other sciences were more fragmented/specialized than psychology acknowledged yet these other sciences did not experience a 'crisis' like psychology. He did point out though that other sciences had more mutual respect amongst their various specializations than psychology and he argued: "We in psychology often—very often—lack this mutual respect, and we hurt ourselves and our science that way" (p. 13). He concluded that developing mutual respect and conducting better scientific research were more important than the pursuit of unity.

Scott (1991) argued that psychology, historically, had been "a federation of often-unrelated disciplines placed in one administrative category" (p. 975). He further argued that once these disciplines matured, it was inevitable that they would break apart. He suggested that it was a positive development for academia and that psychologists should not lament the dissolution of administrative boundaries, which had been largely arbitrary to begin with.

Bower (1993) argued that healthy specialization was being interpreted as fragmentation: "Rather than being viewed as disintegration, the apparent fragmentation of psychology is interpreted positively as an inevitable consequence of increasing specialization of knowledge as our science matures and our range of applications expands" (p. 905). He further argued that psychology dealt with three distinct domains of subject matter—behaviour, neurobiology, and phenomenal experience—and that numerous specializations were precisely required to address this broad disciplinary scope.

He acknowledged that national 'umbrella' organizations were necessary for advocacy and addressing other common interests; however, he stressed that healthy specialization should not be termed fragmentation, which had too many "negative connotations" (p. 906).

With respect to the discipline of psychology, Neisser (1995) presented an argument similar to Bower's: "In general, the history of science is never one of growing unity but of ever-increasing specialization and fractionation. And the reason for that relentless trend is not at all deplorable: it is simply the growth of knowledge itself" (p. 6). However, he did argue that psychology should avoid letting its specialization distort the unity of its *subject matter*: persons. He argued: "the coherence of the self in action can be perceived directly, and from the beginning of life. For that reason, the ecological analysis of self-perception and prospective control may occupy an increasingly central position in psychology in the years to come" (p. 6).

Viney (1996) contrasted the "nineteenth-century metaphor of a block universe in which science is regarded as a structure consisting of basic building blocks resting on firm foundations...with the contemporary metaphor of science as a network of relations" (p. 31). He argued that, in terms of the network metaphor, "psychology is no more disunified than biology" (p. 31). He further argued that psychology would continue to study a diversity of subject matter, with a diversity of methodologies, which would lead to a diversity of academic disciplines and professional organizations representing psychology. However, he suggested that this diversity put psychology in common with, as opposed to in contrast to, other sciences. He concluded: "There is no basis for the

development of a disciplinary inferiority complex based on the belief that the other sciences are unified while psychology remains in the intellectual backwaters of plurality" (p. 31).

Gruber and Gruber (1996)[83] argued that there was a crisis, but that psychology had falsely attributed the cause to internal factors. In contrast, they argued that the crisis existed at a societal level—that society was in a state of crisis, not psychology (they argued psychology was developing nicely). They suggested that pollution, nuclear weapons, and topsoil erosion constituted a far greater crisis than any experienced within psychology. They further suggested that society's industrial processes were not sustainable and that it was important to establish equilibrium with the Earth. They concluded that psychology, as a discipline, needed to contribute to addressing this greater, global crisis.

Kelly (1998)[84] argued that the crisis of psychology "may be understood not as an expression of weakness or deficiencies in the discipline's condition and stature but as evidence of diversity and productivity" (p. 216). He explained: "What may be wrong, paradoxically, may be that the profession, at least in intellectual terms, is healthy...There may be no end...just an ever-expanding horizon of new projects, new perspectives, and new specializations which may be distressing for some not only because such trends threaten to further fragment an enterprise whose coherence they believe to be in danger

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[83] Gruber and Gruber's (1996) article was also a part of the special issue by the *Journal of Social Distress and the Homeless.* However, it is featured under 'no crisis', since their argument was more relevant for this section.

[84] Kelly's (1998) article was a commentary on the special issue on the crisis of psychology in the *Journal of Social Distress and the Homeless.*

but because these developments will disperse power and authority that was once centralized" (p. 221). Kelly concluded that instead of fretting over an apparent crisis, psychologists should address the rhetorical question: "In the service of what values and with the hope of what consequences do we want to see the life of the profession arranged?" (p. 222).

Preconditions for Unity

De Groot (1990a, 1990b) suggested that, before unity in psychology could be pursued, certain *preconditions* needed to be addressed. For example, psychologists interested in unification had to raise awareness of, and interest in, unity-related problems; furthermore, unificationists had to determine how to unify the perspectives on unification: "Proponents of unifying psychology had better agree, first, on the fragmentation *diagnosis*, and second, on the *therapy* they invisage—including the arguments for necessity and feasibility" (p. 1, italics in original). Fundamentally, de Groot argued unificationists had to reach agreement on a working definition of unity: "what do we *mean* when we *say* that we want to work for 'unifying psychology', for studying 'unity issues', or for the ideal of a 'uninomic psychology'? This must be specified in a formulation" (p. 4, italics in original).

De Groot suggested that a working definition of unity could be "seeking, strengthening, and enlarging a common basis" (p. 5). Following this definition, de Groot suggested that one key strategy for unification efforts would be 'consensus groups' of psychologists meeting to analyze and define key concepts to establish this 'common basis'. de Groot further suggested that scientists as well as humanists had to be part of

these consensus groups so that the scientist-humanist schism could be replaced by a commitment to 'scientia'.[85] This would mean that a common solution to the demarcation problem (i.e., what constituted psychological science and what constituted 'pseudoscience') needed to be agreed upon by scientists and humanists. This would also mean that psychologists needed to 'dedogmatize' the discipline and its specializations. Finally, de Groot argued that psychology needed to support these efforts toward addressing preconditions of unity in general and, more specifically, with funding.

Bartley (1974) also argued psychology should address its basic definitions and orientations, which he referred to as 'rubrics': "it is often found that the rubrics of a discipline such as psychology are in need of scrutiny. The material both of fact and of interpretation has often become rather poorly structured and inconsistencies abound" (p. 42). Bartley added that psychology dealt with three kinds of subject matter: "human experience (consciousness, introspection),...body process, [and] overt behavior" (p. 34). However, he argued that defining psychology in terms of its subject matter was problematic, since other disciplines also studied parts of psychology's subject matter. Instead, he argued that psychology should take a 'functional' approach to its definition and define itself in terms of the problems it addressed. He concluded that such an approach would also facilitate increased unity since: "These problems, rightly envisaged, are problems that no other science-discipline poses" (p. 35).

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[85] de Groot used 'scientia' differently than Teo (1999). de Groot meant roughly an expanded philosophy of science which incorporated humanist perspectives and values (but which remained committed to scientific practices).

Hyland (1985a) suggested that an important precondition for unity was a critical examination of the research questions asked by psychology. Specifically, he argued psychology lacked a general framework which would provide a sense of coherence for psychology's research questions: "In physics there is unifying theory...In psychology, on the other hand, questions are usually of a simple empirical nature, relating to one or a few variables. There is no general framework in which these questions are asked" (p. 22). He concluded that psychologists "ought to have some plan of the questions even if we cannot agree on the answers" (p. 22).

Rappard (1987) began his article by arguing that 'systematic psychology'[86] was a better descriptor than 'paradigmatic' or 'uninomic' psychology: "the 'uni' in [uninomic] seems to me to imply a bit too much uniformity" (p. 11). He then argued that it was important to determine on what "grounds" (see p. 12-13) psychology could be defined. Rappard defined 'grounds' as 'constituting rules and internal rules', where constituting rules were what distinguished the system from a different system while internal rules were what defined the parameters within the system. He then argued: "Grounds, however, entail *ontological* rather than epistemological foundations" (p. 13, italics in original). As a result, he argued that it was the subject matter of psychology which would provide the platform for the discipline's definition and unity. Rappard concluded: "'Grounding' definitions can certainly not be arrived at by way of empirical research. Rather,...definitions of the object of psychology...require the kind of historically

[86] Rappard failed to cite the literature on 'systematic psychology' from the 1920s-1950s; this was a good example of the lack of cohesion and ahistorical nature of the crisis and unification literature.

informed theoretical work that could best be carried out, it seems to me, by interested members in Div. 1, 24, and 26 [of the American Psychological Association]" (p. 15).

Krantz (1987) presented a number of preconditions for unity. For example, he argued that psychologists had to determine the extent to which they were able and willing to communicate with one another. He suspected that many psychologists were content to communicate with other psychologists within their specialization and were not overly concerned with cross-specialization dialogue. Also, Krantz argued that unificationists had to identify what *level* of unification was desired: intradisciplinary, interdisciplinary, or some ultimate unification of the two cultures more generally. He also suggested that the sociocultural *Zeitgeist* was one where postmodern relativism was dominant, which negatively impacted the potential for sympathy towards unification efforts. Furthermore, changes to psychology's structure (e.g., changing the professional reward system) would likely only occur *after* similar changes had been implemented at the broader, socio-cultural level. Finally, he suggested that unificationists had to settle on a model for unity. He contrasted 'family' and 'federation' models and concluded that a federation model—which depicted specializations as distinct nations attempting to cooperate—was more realistic than, and preferable to, a family model—which depicted specializations as family members who existed in a more uniform harmony (he argued the family model eliminated important diversities). In terms of developing a federation model for unity,

Krantz suggested that psychologists should examine their moral interests to determine the motivations behind various unification proposals.[87]

Bevan (1991; Bevan & Kessel, 1994) argued that psychology's fragmentation was problematic, and that the discipline needed to develop new, indigenous perspectives and methods to address this problem. He also argued that the quality of psychology's scholarship suffered due to the impact of 'extreme specialization'. He further argued that this decline in scholarship was compounded by psychology having avoided addressing

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[87] Five responses were made to Krantz' article. First, Baer (1987) agreed with Krantz that psychology was multiparadigmatic and likely would not even be open to unity. He also agreed that society's fragmentation had a detrimental impact on psychology's fragmentation. And he added that, outside of rhetoric, there was very little value given to unification. However, he argued that, if unification was possible, it would be the product of developments made within scientific psychology, which he argued was the preferable paradigm within psychology.

Bakan (1987) suggested that psychologists needed to become more comfortable with accepting their ignorance of psychological subject matter; he cited the mind-body problem as still being a problem, although he noted that most psychologists either ignored or denied the problem. He also argued that psychological research was necessarily tied to, or judged by, common sense; he suggested that if psychology took results which strayed too far from common sense into the social sphere, people would reject the results as being nonsensical. Finally, Bakan argued that psychology needed to focus on science as a *process* of combating knowledge gaps instead of as a *product* or outcome.

Royce (1987b) argued that psychology was "not unified because of inadequacies of knowledge, not because of social or political disharmony as his analogy seems to suggest" (p. 341). He then reiterated his previous arguments (see Royce, 1976, 1977, 1978, 1987a) in favour of a 'part-to-whole' approach to unity, which began with smaller, more local syntheses and then moved to broader syntheses. He stressed that theoretical integration was extremely important for unity in psychology and that empirical work was not sufficient. However, he admitted that, after years of trying to train students in theory integration that these kinds of psychologists might be "born, not made" (p. 342). He concluded that, after they had been identified, these kinds of psychologists needed to be supported and given an environment where they could engage in the complex theoretical work which would help promote unity more generally.

Toulmin (1987) noted: "my only complaint is that [Krantz] does not pursue his critique further or deeper" (p. 351). Specifically, Toulmin suggested there were historical reasons for psychology's emulation of physics, but also historical alternatives to the natural science model which could be examined. He explained that if these historical contexts were examined, psychologists might find that they were more similar to biologists than to physicists. He then cited biology's lack of total theoretical unification and argued that "psychologists need demand no more than biologists" (p. 353); in other words, psychologists needed a degree of mutual understanding, but did not need "a single comprehensive conceptual system, to provide a theoretical vocabulary for all its branches" (p. 353). He concluded that psychologists should not try to 'over-unify' their discipline.

Finally, Gergen (1988) argued that psychology needed diversity in order to evolve as a discipline and, therefore, unity was problematic. He also argued that psychological theory was interrelated with culture and, therefore, unity would end up 'impoverishing' culture; as a corollary, he argued that unity would diminish the range of evaluative criteria available for assessing what was good or bad within society.

"fundamental metaphysical issues that make psychology, in Toulmin's phrase, a 'would-be-discipline'" (1991, p. 475). Instead of addressing these issues, Bevan argued, psychology had emulated the natural sciences and adopted definitions of 'progress' and 'objectivity' which were problematic for psychology's research process and subject matter.

Bevan then suggested that psychology needed to understand itself as a human science and, as a result, it therefore needed to address sociology and psychology of science issues; he added that, if it addressed these issues, psychology would realize science did not differ substantially from other techniques of meaning-making. He argued psychology could then make use of some of those other techniques (e.g., perspectives from the humanities). However, Bevan also pointed out that psychologists would come to realize that worldviews were inescapable and would always impact on the research process: "[worldviews] are like sand at a picnic: They get into everything" (Bevan & Kessel, 1994, p. 506).

Bevan argued that psychologists needed to obtain more training in existing psychological methods, obtain training in other disciplines' methods which were relevant for psychology, and develop new methods which were necessary for addressing psychological subject matter. He added that psychologists should engage in more complex research aimed at integration (e.g., between the sciences and humanities) and more significant research outcomes. He concluded: "Our science will hit its stride when we understand that our reality is formidably complex, dominated by asymmetries, and forever challenged by the unpredicted" (p. 482); he added that 'Complex Systems

Theory' (cf., Frank, 1970; von Bertalanffy, 1970) could potentially help make sense of psychology's complexity.

Lincoln (1994) argued that the crisis in psychology was a result of a historical commitment to a positivist philosophy of science. She argued this commitment had resulted in a tension between scientists and humanists, as well as between researchers and practitioners. She further argued it had also been largely responsible for psychology's 'faddishness' with respect to research topics. As a precondition for unity, Lincoln suggested that psychology needed to heed the postmodern critiques and work on developing a new philosophy of science, which would include at least five elements: a move away from narrow definitions of 'empiricism'; a return to considering psychology's fundamental metaphysical questions; focusing on the social structures responsible for producing 'norms' and 'deviance' instead of just on 'deviants'; a return to the inside of 'the black box' and thus to a study of experience, mind, and qualitative richness; and a critical/emancipatory approach to research, similar to that in which practitioners were already beginning to engage.

Messer (1988) proposed three "philosophical obstacles to the unification of psychology" (p. 22): whether reality was "discovered or invented" (p. 22); the inherent value-laden nature of language; and the division between empirical and interpretive methodologies. He stressed that unification was contingent upon overcoming these three obstacles.

Changing the professional reward and graduate training systems in academia was also proposed as a major precondition for unity (Eifert, 1985; Kunkel, 1985; MacIntyre, 1985;

Maher, 1985; Thorngate, 1990a, 1990b; Wachtel, 1985; Wittig, 1985). Forsyth (1994), Cahill (1994), and Masia, Beach, and Hock (1994) added that psychology needed to develop a more unified training model for its graduate students.[88]

Finally, I argued that, before the rest of psychology should be expected to take unification proposals seriously (and, therefore, assist in unification efforts), the unification literature itself should be unified (Goertzen, 2005c), and common definitions of key terms should be developed (Goertzen, 2005b). As a first step in attempting to unify part of the crisis and unification literature, I offered a theoretical integration of the work of Staats (1983), Giorgi (1970), Koch (1981), Yanchar and Slife (1997a), and Sternberg and Grigorenko (2001a, 2001b) (Goertzen, 2005c). I concluded—following Vygotsky (1997)—that a *theory of the crisis of psychology* was also a necessary precondition for unity (Goertzen, 2005a).

Understanding the Terms 'Unity,' 'Disunity,' and 'Diversity'

Ballantyne (1992) argued: "Despite the efforts of unificationists such as A. W. Staats, J. R. Royce, A. de Groot, and C. W. Tolman who suggest possible criteria or preconditions for a unified psychological science, the task of specifying exactly what is meant by the term unification has received relatively little attention in the literature" (Introduction section, para. 1).[89] Indeed de Groot (1990a, 1990b) did argue that precisely

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[88] Interestingly, McGovern & Brewer (2005) argued that *less* unity was needed for undergraduate training models. They suggested that it was beneficial for students to experience "courses and programs with fuzzier boundaries" and they concluded: "Experience has taught us that less unity can be more fun!" (p. 140).

[89] Specifically, Ballantyne argued that unificationists needed to distinguish between interdisciplinary, subject matter, and theoretical unity.

what was meant by 'unity' needed to be better articulated and that this was one important precondition for unity in psychology. Ritchie and Sabourin (1992) agreed: "At a conceptual level, let alone operationally, the problem [of fragmentation] is exacerbated even by the absence of common definitions about what constitutes unity" (p. 312).

Altman (1987) suggested that 'centripetal' (i.e., unifying) and 'centrifugal' (i.e., diversifying) forces operated within psychology simultaneously, "though one or the other pole may be momentarily stronger" (p. 1058). He added: "one must avoid attributing universally positive or negative features to either polar opposite; they are both essential to the coherence of the whole" (p. 1058); however, he admitted: "the danger of total centrifugality is valid, but...By the same token, total centripetality is unhealthy because it can reflect insularity and stagnation" (p. 1069). He then argued: "we should recognize that dialectical unities are dynamic 'open systems'...engaged in continual exchange with their environment...Although interchange with the many contexts around us can be stressful, open systems must accommodate and interact with their environments or they will wither" (p. 1069). Altman concluded that psychologists should actively seek out centripetal trends to balance the centrifugal trends and should "struggle to shape the directions of the discipline in an assertive and constructive way" (p. 1069).

Rosenzweig (1992) presented an argument similar to Altman's: "Factors both internal and external to psychology are working for both unity and diversity" (p. 283). In terms of internal unifying factors, he argued that a commitment to "the canons of science" (p. 283), "broad international consensus about the main areas of psychological research and application" (p. 284), "a common history" (p. 285), international professional

organizations and publications, and (in some countries) "a common basic curriculum" (p. 284) were leading candidates. For internal diversifying factors, he argued psychology's fragmentation of research areas, discontinuous aspects of the discipline's history, theoretical differences, the science-practice tension, and psychologists changing their primary affiliation to other disciplines were all problematic. In terms of external unifying factors, he suggested that the public perception of psychologists as being able to address social problems and the formal status afforded psychologists by educational and legal systems increased psychology's unity. For external diversifying factors, he suggested that the diversity of national political and economic systems, degree of recognition given to psychologists, and indigenous psychologies, as well as the impact of novel developments in other disciplines or technology, all centrifugally impacted psychology.

Rosenzweig also argued it was important to survey the benefits and costs of both unity and diversity in psychology. He argued that unity allowed psychologists to speak with 'a single voice' on advocacy initiatives and also served to attract more substantial funds for research; however, he argued unity could mask diversity, which was necessary for disciplinary growth. He argued that diversity "allows for creative experimentation" (p. 288) and could promote "more effective research on certain questions than a unified science would allow" (p. 289); however, he also argued: "Diversity may cause fragmentation of effort or even costly debates among psychologists. It may also make it harder for psychology to establish itself as a science and profession, especially when it faces challenges from other groups or from older institutions" (p. 289). He concluded

that a diversity which stopped short of fragmentation was probably the most beneficial for psychology.

Ritchie and Sabourin (1992) surveyed unity-diversity within Canadian psychology and they made use of Rosenzweig's 'unity-diversity, internal-external' model in their article. In terms of internal unifying factors, they argued that "a philosophy of science anchored within the modern empirical tradition", a core undergraduate curriculum, and the Canadian Psychological Association's (CPA) accreditation and standards of practice guidelines could all be included. For internal diversifying factors, they cited the science-practice tension, increased identification of psychologists with other disciplines, and the natural-human science division. In terms of external unifying factors, they cited the public's support for psychology, general advocacy efforts, and political ties to the federal government. For external diversifying factors, they cited the growth of new specialized disciplines, the French-English tension in Canada, and "the federal government's arbitrary retrenchment of its historic commitments to the financing of educational, health and social programmes" (p. 321). They concluded by explaining that the CPA had decided to place an emphasis on addressing functional, political goals (e.g., advocacy efforts) to attempt to unify psychologists on more pragmatic grounds; they called this "Canada's functional-structural approach to the unity of psychology" (p. 311).

Fishman (1988b, 1990a) also argued that there were both intrinsic and extrinsic causes of psychology's fragmentation. Intrinsically, he suggested psychology could be understood to possess three epistemological frameworks: experimental, technological, and hermeneutic; furthermore, psychology traditionally emphasized novelty over

cooperation. Extrinsically, he suggested there were socio-cultural influences, such as wars and funding bodies, which contributed to psychology's disunity.

Furedy (1990) preferred to focus on psychology's intrinsic centrifugal forces. He argued that "Alice-in-Wonderland (AW) terminological usage [which was]...characterized by a tolerance for internal inconsistency, and by the employment of basic terms that have an infinitely wide extension, so that their meaning is purely emotive rather than informational" was a particularly strong intrinsic centrifugal force (p. 4). He concluded that even "when external centrifugal forces are more evident", it was important to address "the internal centrifugal forces that come from Alice-in-Wonderland terminological usage" (p. 8).

Diaz-Guerrero (1992) argued that unity and diversity could impact on various *domains* of psychology.[90] For example, he argued that curriculum requirements tended to be unifying across cultures. However, levels of training required to practice psychology was diverse across cultures. Also, socio-historical contexts could produce "'centrifugal forces' that threaten the unity of psychology" (p. 294). In terms of the political domain, he argued that the American Psychological Association (APA) was a unifying force in America, but this kind of centripetal influence varied cross-culturally. Finally, with

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[90] In a review of *Annals of Theoretical Psychology, Volume 5* (which was devoted to the issue of unification), Rychlak (1989) also stressed that it was important to determine which domains were in question. For example, he explained he was in agreement with Staats (1987d) when it came to Staats' proposals for methodological unification, but he was not in agreement with Staats' proposals for theoretical unification. He added that psychologists needed to distinguish between Reichenbach's (1938, as cited in Rychlak, 1989) "'context of discovery' (i.e., theorizing about things) and...'context of justification' (i.e., proving one's theoretical claims)" (p. 1000). Specifically, in response to Kendler's (1987) chapter, Rychlak concluded: "I do not favor a divorce at this point in our history...What we need is a less selfish marriage" (p. 1001).

respect to the theoretical domain, Diaz-Guerrero advocated for: "*Unity in the midst of diversity*" (p. 297, italics in original). He cited the fact that, with quantum, relativity, and Newtonian theories, physics retained a 'unity in the midst of diversity' which psychology could emulate.

Ballantyne (1993) argued that unity and pluralism could also differ as to whether they were addressing ontological or epistemological domains. Specifically, he argued that 'ontological plurality' was necessary since psychological subject matter were complex, developmental, and, at times, contradictory; however, he argued 'epistemological pluralism' was problematic since it ultimately led to extreme relativism and nihilism. In other words, he argued that psychology could tolerate disunities related to subject matter, but not to theories. Following Tolman (1987, 1988, 1989, 1991), Ballantyne referred to his position as 'pluralistic monism'.

Viney (1989) examined James' (1892/1983) philosophical writings on pluralism and monism in comparison with the contemporary unity-disunity debate. He noted that, if he were alive today, James would likely ask: "Is the unity to be linguistic, philosophical, methodological, axiological, or aesthetic? If there are to be several unities, will they fit within a larger coherent framework? If not, there could still be gains in modest or limited unities" (p. 1264). Viney added: "James's major concern with unity was whether it could be so instituted as to assure that alternative viewpoints receive a fair hearing. The problem is to guarantee that any intellectual unity be established on pragmatic and provisional grounds so as to assure that it does not slip into totality" (p. 1264). Viney concluded that there was sufficient doubt about "the possibility of any *large* or *enduring*

unity. Nevertheless, we psychologists can have confidence that within the stream of experience there are real workable unities and truths to be discovered" (p. 1265, italics added).[91]

Mos (1987) posed the rhetorical question of whether psychology should pursue unity or integrity. He cited Holton's (1986, as cited in Mos, 1987) discussion of 'the integrity of science', which involved four principles: 1) "try to get it right at any cost, sparing no effort" (p. 345); 2) "try to be a scientist first and a specialist second" (p. 346); 3) "science is and must be part of the total world view of our time" (p. 346); and 4) scientists had a "special obligation...to exercise sound citizenship each in his own way" (p. 346). Mos concluded that he was sceptical about the prospects for unity in psychology, but that he was more hopeful about the potential for "some sense of concinnity" (p. 347) or integrity.

Derksen (2005) argued specifically against evolutionary theory as a unifying force for the social sciences and "against integration" (p. 139) for psychology and the social sciences in general. Of interest for this descriptive account was his treatment of the term 'integration', which he defined as "unified, homogenous" and "unified, seamless" (p. 141). Based on this definition, he argued: "pluralism is to be preferred over unification. The attempt to erase all divisions and tensions between disciplines is misguided" (p.

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[91] In response to this article, Brožek (1990) took issue with Viney's occasional use of the term 'disunity'—particularly with respect to characterizing the American Psychological Association's multiple divisions. He stated that he preferred the term 'diversity'. Brožek then cited Henning's (1932, as cited in Brožek, 1990) conceptual model of professional psychology, which located experimental psychology in the middle and applied psychology surrounding the experimental. He was in support of Henning's model, but suggested that the middle of the model would have to be updated to include other general psychological areas, such as history of psychology.

141). He concluded that instead of integration/unification, "both interdisciplinary collaboration and polemical confrontation are needed to manage disunity" (p. 158).

Furedy (1988) discussed "the Socratic/Sophist dichotomy" (p. 3) within psychology. He rhetorically asked whether the goal for unity was to persuade and gain mutual support for a position in a political manner (Sophist) or if it was to sharpen differences and develop logical unifications and differentiations in a scientific manner (Socratic). He argued the former implicated a uniform label of, and rhetorical commitment to, unity (though with the potential for underlying disunities), while the latter implicated a potentially disunified, but more scientific, approach to the development of theories, perspectives, etc. Furedy concluded he was in favour of the latter, though he admitted the dichotomy had been "shelved but not solved" (p. 3).

Finally, I explored the definitions of 'crisis', 'unity', 'disunity', and 'psychology' (Goertzen, 2005b) and argued that differences in definition were responsible for a substantial amount of confusion in the crisis and unification literature. I particularly emphasized—following Giorgi (1985)—that advocates of unity tended to define it as convergent pluralism while dissenters against unity tended to define it as uniformity. I concluded that authors needed to be more clear on their definitions of terms, particularly when using the terms 'unity' and 'disunity'.

The Role of Theory and Theoretical Unification

Many crisis and unification authors addressed either the role of theory with respect to the unification of psychology more generally or the possibilities surrounding theoretical unification more specifically. For example, Rappard (1985) distinguished between "Meta

(Theoretical) Psychology and General (Theoretical) Psychology" and argued that "the constructive aspect of Theoretical Psychology should again be aimed at the integration of the field...the development of General Psychologies in the sense of systems built on explicit—and hence ultimately philosophical—foundations" (p. 6).

Kunkel (1987a) suggested that psychologists should "want to work only with those theories that have considerable empirical support" (p. 36). He also argued that integration would probably not even be needed if "theories were: a) structured into logically related sets of propositions...b) testable and refutable...[and] c) evaluated solely in terms of empirical evidence" (p. 35-36).

Wertheimer (1987, 1988a, 1988b) argued that psychology was a young, complex discipline and, therefore, should retain apparent incommensurabilities instead of allowing one or both of the opposing perspectives to be abandoned. With respect to theoretical integration, he argued that at least four outcomes were possible for apparent incommensurabilities:

> ...they may (a) turn out to be intertranslateable [sic], in which case they are not really competing...(b) be truly contradictory, and therefore logically impossible to integrate...(c) be apparently but only superficially mutually translateable [sic], but with the translation doing violence to at least one of the theories;[92] or...(d) be

[92] Franks (1988) argued that the integration of incommensurable theories would result in distortion of one or both of the theories. He then posed the rhetorical question: "How can one achieve unification between two quite alien models which cannot even agree about such a basic notion as what constitutes acceptable data?" (p. 26). He concluded that psychology was in an eclectic phase and that general unification was premature.

> mutually irrelevant, so that there is little point in trying to integrate them. (Wertheimer, 1988b, p. 131)

He also argued that the metatheoretical literature was little help in dealing with incommensurabilities: "We do not want for schemes of dimensions along which theories should be, or could be, compared; but one comes away from this literature with the impression that rather than a clean, useable single analytical tool, it yields little more than a *profusion of confusion*" (1988b, p. 133, italics added). He concluded that psychology's problems of theoretical incommensurability were multiply-determined (e.g., by "an emphasis on empiricism...on novelty...the pressure to establish a unique position...as well as other methodological, organisational, and social factors", p. 132) and would likely require "an awesomely massive social program...if there is to be any chance of changing them in such a way as to produce unifying rather than the current fragmentary efforts" (p. 133).

Rozeboom (1970) proposed a similar argument: "There are three primary categories into which a pair of discrepant psychological assertions *P* and *Q* may fall. First of all, *P* and *Q* may actually *agree* with one another in that they express essentially the same thought albeit in different words. Secondly, *P* and *Q* may *contradict* one another. And thirdly, *P* and *Q* may *complement* each other in that they say different but compatible things" (p. 157). For the first scenario, he argued that "we have unity already and it only remains to make this explicit" (p. 157). For the second scenario, he argued that "unification requires resolution of the points of disagreement". Finally, for the third scenario, he argued that "unification would consist in finding some significant connection

between them" (p. 157). However, Rozeboom argued that actually attempting this theoretical work was not so straightforward: "It is often *very* uncertain whether [psychological theories] agree, disagree or complement each other, and if the last, whether they are talking about the same or different things" (p. 157). He concluded that what was needed was 'conceptual analysis', which could clarify the meaning and 'logical relations' of competing theories.

Drob (2003) made a similar argument, echoing Wertheimer and Rozeboom:

> I also outlined six options for the resolution of psychology's factionalized state. One could (a) opt for a form of *reductionism*, attempting to translate or reduce the propositions of the other theories to one's own, (b) hold that the various theories were commensurable with one another and therefore subject to selection through scientific tests, (c) hold that, similar to so many different religions, they were relative to one's point of view and, therefore, incommensurable, (d) choose some form of eclecticism, (e) hold that the different theories were meant to explain different states and phenomena, and were therefore referentially distinct, or (f) hold out for an ultimate synthesis, a super theory. (p. 103)

However, he did not discuss these approaches in detail.

Darden (1988), a biologist, discussed four theoretical interrelationships for addressing apparent incommensurabilities which biologists used and which psychologists could potentially adopt: "identity under differing descriptions" (p. 6); "part-whole" (p. 6); "structure-function" (p. 6); and "providing a mechanism at a different level of

organization" (p. 6).[93] Darden's 'identity under differing descriptions' had the same meaning as Wertheimer's 'turn out to be intertranslateable'; 'part-whole relationship' meant that one theory was addressing an element which was a part of the whole which the other theory was addressing; finally, she explained that acknowledging structural and functional differences in subject matter, as well as multiple levels of complexity/organization, could assist in generating useful hypotheses.

Rychlak (1988, 1993, 2005) addressed theory in relation to method and the resulting possibilities of theoretical and methodological unity. In short, he argued that methodological unity was possible and desirable while theoretical unity was impossible and undesirable. In terms of methodological disunity, he argued: "[The] tendency to dismiss traditional scientific research in favor of what becomes a procedurally-tested line of argument, a plausible or 'convincing' argument advanced by a clever 'analyst,' is a clear and present danger to the unification of psychology" (Rychlak, 1988, p. 13). In terms of theoretical unity, he argued: "I could never support an effort to unify psychology under one theoretical outlook. Such unification is deadly" (Rychlak, 1988, p. 13). However, Rychlak (1993) suggested four 'groundings' for theoretical *complementarity* for psychology: physikos (i.e., inanimate matter); bios (i.e., biological organisms); logos (i.e., meaning); and socius (i.e., culture). He argued that psychology should cease its quest for a single theoretical paradigm and instead acknowledge that its subject matter

[93] Minke (1988) agreed with Darden and argued that psychology should particularly seek to identify identity and part-whole relationships. He also argued that psychology needed to recognize the hierarchical nature of the levels of complexity inherent in its subject matter, as well as the importance of the 'principles' of unified knowledge—i.e., "unified knowledge is a fundamental goal of science" (p. 34)—and progressive development—i.e., science is always changing and evolving.

was sufficiently complex to warrant four complementary paradigms. He concluded that unity in psychology would come through "understanding and tolerance of opposition" (Rychlak, 1988, p. 13), and through "an appreciation of the complementary diversity of our [epistemological] groundings" (Rychlak, 1993, p. 939).

Gosling (1986) presented an argument for pursuing theoretical unity in psychology. He began by defining theoretical unity "as a state in which all theoretical statements made in a science will be interpretable within a *common* theoretical framework, generally accepted within that science" (p. 13, italics in original). He argued that, without such a framework, the discipline could have no "strategy for acquiring knowledge" (p. 13). Specifically, he suggested that theoretical unity could be pursued in three ways: analyzing "the specific knowledge requirements of psychology" as dictated by the discipline's subject matter (p. 15); analyzing "the types of knowledge currently represented in existing psychological theory" (p. 15); and analyzing "the nature of the theoretical devices by which knowledge is represented in current psychological theory and a consideration of their limitations" (p. 16). From this deconstructive process, Gosling argued psychology could reconstruct a specific 'epistemic strategy', which would include recommendations for overcoming differences in theoretical languages, as well as for expressing the complementarity that existed between theories.

Van Strien (1987) also examined "the feasibility and the limits of a theoretical unification of psychology" (p. 333). In introducing the article, van Strien argued that other, 'model' disciplines continually influenced psychology and that the "intellectual appeal of specific disciplines as a model for other disciplines depends on the general

intellectual climate in the culture of a given time" (p. 336). van Strien suggested that this influence of model disciplines was likely responsible for a large proportion of the variability as to why research topics were adopted or abandoned within psychology.

With respect to unity, van Strien argued it was necessary to distinguish between unity "within the same research tradition...[and] unification between different research traditions" (p. 340-341). In terms of unity within a research tradition, van Strien argued "there is a strong need for bridging concepts and theories that organize the results of paradigmatic research into a more coherent whole" (p. 340). However, van Strien also argued: "The striving for unification within a research tradition does not mean that competing paradigms should be completely eliminated...A precondition, however, is that researchers who work along the lines of different paradigms should be aware of each other's work, and that a platform for theoretical discussion should exist" (p. 340). In terms of unity between research traditions, van Strien argued: "In this case there is much more difficulty. The divergence is not...the result of diversity of problems...but of fundamental differences in theoretical and methodological suppositions" (p. 341). van Strien added that the divide between seeing a human as "a biological organism, and as an intentional cultural being" (p. 341) also contributed to the difficulties in producing a more general theoretical unity.

The Role of Method and Methodological Unification

Some crisis and unification authors also addressed either the role of method with respect to the unification of psychology more generally or the possibilities surrounding methodological unification more specifically. For example, Wertz (1999) examined the

use of multiple methods with respect to unification. He argued that the positivist commitment to experimental methodologies in psychology was "no longer tenable in light of criticisms by philosophers of science and psychologists" (p. 131). He also discussed problems related to eclecticism, constructionism, and fragmentation that could occur when psychologists tried to make use of methodological pluralism. He argued that psychology needed "an indigenous epistemological foundation" (p. 131) to support its use of multiple methodologies; and he concluded: "as Yanchar says...without a clear ontology and epistemology, [multiple] methods can yield incoherent and even irrelevant or not properly psychological results" (p. 151).

Minke (1987) argued that methodological disunity in psychology was problematic and contributed to overall disunity in the discipline. He argued that a hallmark of science was the use of unified methods to study unified subject matter with the goal of producing unified theories. He concluded that more work needed to go into linking psychology's methods with its theoretical frameworks and ensuring that methods were selected in response to subject matter and not vice versa.

Fishman (1990b, 1993; Fishman & Messer, 2005) argued that the 'research case study' had the potential to significantly unify psychology. He argued: "While there are certain intrinsic complexities, ambiguities, and subjectivities in the study of psychological phenomena which prevent complete unification of our field, there are intellectually legitimate ways for substantially reducing the disunity that does now exist" (Fishman, 1990b, p. 6). Specifically, he argued that taking a case study approach to research—which addressed aspects of all three of the paradigms he (Fishman, 1986,

1987) had previously outlined—would promote unity within the discipline. He added that this approach was based on a postmodern-pragmatist epistemology, which emphasized perspectivism, but which employed pragmatism in service of avoiding the pitfalls of relativism. Instead of strictly positivist or postmodernist approaches, he (Fishman & Messer, 2005) concluded that there was a third way to approach unity in psychology: "This is the pragmatic case study method, which proposes the creation of peer-reviewed journal-databases of systematic case studies in applied psychology areas such as psychotherapy, allowing for unifying themes within areas to emerge inductively through cross-case analysis" (p. 56-57).[94]

Cattell (1995) argued that unity could only be pursued "through a multivariate approach" (p. 23), and that psychology was engaged in "three misdirections of valuable effort" (p. 24). The first was "a predominant, unconscious following of weak bivariate designs" (p. 24). The second was "an escape into narrowly cognitive rather than dynamic explanations" (p. 24). Finally, he argued that psychology possessed "a satisfied preoccupation with applied details" (p. 24). He concluded that all three of the misdirections "involve a studied neglect of powerful multivariate methods—factor analysis, canonical correlation, and so forth that can alone open many doors we otherwise beat upon in vain" (p. 24). In failing to move toward multivariate methodologies, Cattell

[94] Peterson (1993) and Cherniss (1993) responded to Fishman's (1993) article. Peterson agreed with the bulk of Fishman's analysis, except that he took issue with Fishman's critique of positivism. He argued that adopting a critical realist—as opposed to naïve realist—epistemology could assist in avoiding many of the negative pitfalls of positivism while retaining many of the strengths. Cherniss argued: "In rejecting the absolutes of positivism, psychologists...might open a methodological Pandora's box if they do not address the problem of verification" (p. 23). She concluded that relativism could be avoided if researchers remained 'grounded' in their data; this grounding process would limit the seeming arbitrariness of interpretation (e.g., it would allow for interrater reliability checks).

concluded that "contemporary psychology is...in my opinion rushing gaily down a steep place to a sea of futility" (p. 25).

Social and Cultural Considerations

Gilgen (1984) argued that a 'cross-cultural perspective' was necessary for producing unity in psychology.[95] Kameoka (1991) agreed: "If we are to continue in our search for principles underlying human behavior, we must be prepared to cross cultural boundaries and put our theories to the test among populations in all parts of the world" (p. 11). She also argued: "In transporting psychological measures to other cultures, researchers virtually ignore indigenous meanings and perspectives that are inextricably imbedded in [a] sociocultural and linguistic system that differs from the system in which the measures were originally developed" (p. 13-14). She concluded that if researchers were going to continue to use Western measures cross-culturally, at the very least, the psychometric properties of translations needed to be checked against the original scales. Alternatively, researchers could develop indigenous measures for each culture, although she acknowledged that this led to questions of whether the results would be comparable across cultures; she concluded that if this second approach was employed, "cross cultural comparisons should involve comparisons of construct interpretations (i.e., validities)" (p. 14).

Brislin (1991) also agreed that culture was important for psychology in general and unity in particular. With respect to unity, he argued that a bias of Western culture—"a

[95] Kunkel (1987b) argued that culture linked psychology to sociology and anthropology. As a result, he argued the unification of psychology (or lack thereof) would have substantial implications for these other social sciences.

tremendously high value...placed upon originality" (p. 23)—negatively impacted its potential:

> To achieve a universal paradigm, researchers will have to accept certain concepts and will have to cease the constant search for new constructs that might eventually have their names attached to them...Rather than the 5 or 6 articles and chapters per year that informally mark 'a real producer', tenure review committees may call for one per year that is widely read and used because of its centrality to the unifying paradigm. (p. 23)

Finally, Lévy-Leboyer (1992) argued that culture was important because cultural differences could serve as disciplinary fault lines. Specifically, cultures could experience differences in: 1) theoretical and methodological preferences; 2) kinds of social needs; 3) university curricula; 4) validity criteria for methods, therapies, etc.; and 5) psychologists' "missions and role in society" (p. 282). However, he did not see these fault lines as problematic; instead, he argued: "Psychologists must accept their diversity as a potential asset to build on and not use it as a basis for useless struggles and clashes of interest" (p. 282).

Prescriptive Writings about Unity

There was no shortage of prescriptive writings about unity during this time period. For example, Bunge and Ardila (1987) argued: "Because the fragmentation into rival schools derives from rival philosophies, it can only be overcome by adopting *a single underlying philosophy*—preferably one closest to the 'scientific spirit'" (p. 31, italics in original). More specifically, they argued that "adopting a philosophy containing the

psychophysical identity hypothesis...entails that every item of psychological interest be viewed as being controlled by the nervous system (the case of behavior) or as a particular function of that system (the case of mental processes)" (p. 31). However, they argued that they did not want to reduce psychology to physiological psychology; instead they argued that "no matter what level of analysis or description be chosen, it be kept in mind (or rather in brain) that the process happens to be neural or under the control of some neural system" (p. 31).

Hebb (1974) presented a similar argument: "*Psychology is a biological science*" (p. 72, italics in original). He argued that scientific psychology might be incomplete with respect to informing people "about how to live wisely and well" (p. 74), but he argued that the solution was not to eliminate the science from psychology: "Humanistic psychology, I think, confuses two very different ways of knowing human beings and knowing how to live with self-respect. One is science; the other is literature...Science is the servant of humanism, not part of it. Combining the two ruins both" (p. 74). He concluded that psychology should be the scientific study of the mind, which was primarily the product of neural activity, and therefore psychology should involve an intense scientific study of the brain.

Eysenck (1980) also presented a similar argument. He argued that biology accounted for approximately eighty per cent of human psychology while 'social conditioning' accounted for the remaining variance. He concluded psychology could be a unified discipline under the framework of the scientific study of the mind, which was simply neural activity under the modest influence of social conditioning.

Bartley (1970, 1982) also argued that psychology was a biological science. He reiterated his previous argument (Bartley, 1974) that research problems, not subject matter, should define a science. In this way, he explained psychology and physiology could share subject matter, but be distinguished based on research problems. He then argued that psychology's research problems centered around: "(1) "*The problem of action and its direction*...(2) *Contact of the organism with the surrounds*...(3) *The kinds of changes that occur in the human*...(4) *Internal organization and conflict*...[and] (5) *Demand and the ability* [of organisms] *to meet it*" (Bartley, 1982, p. 122, italics in original).

In contrast, Pribram (1995) argued that psychology existed between biology and sociology. He argued that 'boundary disciplines' helped define and shape disciplines, and that both biology and sociology had an impact on psychology. He concluded: "Psychology, the science of mental processes, may well depend for its maturity on the development of interfaces with the social and biological sciences" (p. 19).

Ardila (1992) argued that psychology had started with eight schools (i.e., structuralism, functionalism, reflexology, behaviourism, Gestalt, topology, psychoanalysis, and existentialism), moved to four systems (i.e., neo-behaviourism, neo-psychoanalysis, dialectical-materialism, and humanistic psychology), and was headed toward a single paradigm: "The experimental synthesis of behaviour" (p. 299). Ardila argued that this synthesis was most similar to operant conditioning and he suggested that psychology should: use experimentation as its fundamental research methodology; use mathematical formalism as its fundamental approach to theory development; emphasize

the subject matter of behaviour and learning; eliminate dogmatism and emphasize 'behavioural humanism'; acknowledge the importance of both nature and nurture, while emphasizing that behavioural laws were not reducible to biological or sociological laws; and use the 'technologies of behavioural science' to modify humans in adaptive ways. He concluded that this model was unifying—not eclectic.

Gilgen (1970, 1985, 1987, 1988) argued that the study of behaviour was not sufficient for a paradigm within psychology. He argued that a true paradigm would only come from looking at neurophysiology, psychic functioning, and behaviour. However, he noted that neurophysiology and behaviour, as well as environmental influences, were "not in and of themselves psychological" (Gilgen, 1987, p. 179); he argued that the 'psychological domain' consisted of "*mind, consciousness, perception, cognition, affect,* and *personality*" (Gilgen, 1987, p. 179, italics in original). He explained that it was important for psychology to explicate the intra- and interdisciplinary interrelationships involved in the study of its subject matter. He added that conceptual fragmentation was problematic in psychology and its reduction was essential for the development of unity. He concluded that psychology should continue to operate in applied domains, but suggested that the discipline would need changes to its curriculum based on his recommendations.

Wapner (1988a, 1988b; Wapner & Demick, 1989) argued in favour of "a holistic, developmental, systems perspective" (Wapner,1988b, p. 1) with respect to the unity of psychology, and he argued that psychology should study "person-in-environment system[s]" (Wapner, 1988b, p. 5). He further argued that "persons are characterized by

three aspects, sociocultural (e.g., role), psychological/intrapersonal (e.g., self-concept), and physical/biological (e.g., health)" while "environments are characterized by physical features (e.g., natural and made objects), interpersonal (e.g., some other people in this room), and sociocultural features (e.g., school, country, prison)" (Wapner, 1988b, p. 5). He then argued: "Person-in-environment systems are characterized as operating in a dynamic equilibrium" (p. 5). He also argued that humans showed development over time and that this development occurred within environmental contexts. Finally, in terms of the unity of psychology, Wapner concluded that his approach implicated the need for: an emphasis on understanding human contexts; holism in the study of human life; appreciating the developmental levels of organization humans experienced throughout their lives; the use of human scientific and natural scientific methods, with particular emphasis on 'system units' of psychological subject matter, and an awareness that "the experimenter is part of the environmental context" (Wapner, 1988b, p. 10); and breaking down the basic-applied research schism.[96]

Maiers (1987) was less optimistic about the possibilities of unity within psychology: "Achieving paradigmaticity throughout the discipline...is beyond the scope of an intra-scientific revolution of the prevailing disciplinary worldview. It is contingent upon which of the conflicting contemporary ideological main forces wins the hegemony in society" (p. 185). He concluded that if a new system could not be foreseen to overcome

[96] Yamamoto (1988) posed to Wapner (1988b) the rhetorical question "How far can we go towards unifying psychology?" He argued that 'a grand integration of psychology' was probably impossible and that a more realistic goal would be to focus on smaller, area-specific integrations. He also argued that the science-practice tension was a schism that perhaps could not be overcome. Wapner (1988a) responded that the goal of unification was a good one, even if it was only an ideal: "Striving toward integration, even if not accomplished, will have a healthy affect on psychology of the future" (p. 19).

the 'variable-psychology' which was currently entrenched, then the prospects for correcting the 'dysfunctional mainstream' were bleak.[97]

Fraisse (1987) argued:

> Psychology is not a science! But there is a growing body of scientific psychology distinguishable from the stable and invarying folk psychology which is the domain of novelists and moralists, both ancient and modern. Today, if we were to exclude, for example, phenomenology, psychoanalysis, and clinical psychology...many of the arguments for the disunity of psychology...would be groundless. (p. 235)

He added that these other perspectives could be included within scientific psychology "if shortsighted positivism is avoided" (p. 235). However, he concluded "that science is not the only way to understand man" (p. 238) and he suggested that psychology needed to develop multiple theories for understanding the various levels of complexity inherent within its subject matter. He concluded that theoretical pluralism was not antithetical to disciplinary unity.

Yela (1987) argued: "Psychology is a plethoric, frustrating, and divided science, partially, because of its youth and complexity; fundamentally, because of a basic dualism of data: private experience and public behavior" (p. 241). However, he maintained: "Unity is possible!" (p. 241). He suggested that it was possible with respect to subject matter by defining psychological subject matter as "physical action that is biologically and/or personally meaningful" and with respect to methodology by a commitment to the

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[97] Maiers (1987) admitted, however, that "the Marxist historical approach" had the potential for a paradigm in psychology since it avoided "reductionism as well as the blind alley of the seemingly more concrete conceptual pluralism" (p. 186).

"verification of public behavior" (p. 241). However, although he argued unity was possible, he concluded that it was improbable since "the actual strategies of research, teaching, intervention, and organization...favor a reinforcing system for fragmentation and disunity" (p. 241).

Diaz-Guerrero (1989) argued in favour of "an ecosystemic psychology" (p. 229). In terms of unity in psychology, he concluded "that the best opportunity for a scientific and comprehensive psychology, is to embrace a psycho-bio-ecological, ecosystemic type of thinking" (p. 229). In terms of research in psychology, he argued that six findings from physics should be heeded by psychology. First, "a dialectical approach is indispensable to gain knowledge in many areas of psychology" (p. 233). Secondly, "if light or electricity are many-sided the psychological phenomenon should be even more multiform" (p. 233). Third, "different methods of inquiry reveal contrastive, but not necessarily contravening, aspects of the psychological reality" (p. 233). Fourth, "inevitably, the method of inquiry interacts with the phenomenon under study" (p. 234). Fifth, "the objective, what in cognitive terms is the final purpose of the psychologist, determines his choice of facet of the psychological reality to be studied" (p. 234). Finally, "the method of inquiry should be chosen to try to fully embrace and highlight the chosen facet" (p. 234). He concluded that a unified approach to research in psychology was possible, but "to determine for specific behaviors and environments, the variables that intervene, the psychologist will have to imitate the ecologist and ponder meticulously the methodological and content principles previously derived from the physicists' insights" (p. 238).

Fowler (1990) argued in favour of unity and suggested that it would benefit psychology's advocacy and public image, as well as the public welfare of society. He also argued that the science-practice tension was a by-product of psychology's subject matter disunities (which he argued could eventually be overcome through the development of 'grand unifying principles'). Specifically, he argued that psychology needed to maintain a 'reciprocal relationship' between science and practice and argued that psychology could not shirk its relationship to society because psychology was a 'core discipline' which generated knowledge which many other (particularly applied) disciplines used. He concluded that unity was possible and that, if psychologists worked together, they could "make our second century a golden age" (p. 6).

Lee (1994) argued that psychology's fragmentation was a product of an inability to define its subject matter, which in turn was the result of psychology's separation of research from practice. Specifically, she argued:

> Attempts to sanitize psychology's fragmentation will postpone the inevitable task of identifying and conceptualizing the particulars represented by psychological data. Psychological data represent things done, changes brought about by one or more organisms. Things done comprise a vast, densely populated, and always changing domain of events. Things done depend on organisms but are conceivable apart from organisms. The domain of things done contains particulars (i.e., content) and universals (i.e., patterns). (p. 7)

Lee concluded that neither eclecticism nor 'guild membership' could overcome psychology's fragmentation. Instead, psychologists needed to commit to studying 'things

done', which would involve moving away from studying individual organisms and toward studying the systems in which 'things' were 'done'.

Katzko (2002) argued that rhetoric was more problematic for unity in psychology than the technological mass-production of research literature (which he argued "does not constitute a serious crisis for the discipline", p. 263). He argued that, because of a discipline-wide emphasis on 'uniqueness', researchers ended up conflating data with theory by treating data as evidence in favour of a broader theory; he argued that this process resulted in a theory acquiring "a complexity that does not match the data that spawned it" (p. 264). He explained:

> The method, data, and the theory are treated as equivalent to each other and on an experimentwise basis. Anything not explicitly in the design does not have a role in the presented theory. Another experimenter, manipulating a different set of variables and using the uniqueness assumption to explain the data, will by definition create a theory different from the first. The seed is now sown for a proliferation of mutually exclusive theoretical terminologies. The procedure, practiced by dozens if not hundreds of independent researchers, results in an overload of theoretical terms...and creates the disorganization in the published record so often lamented as a lack of unity. (p. 265)

Katzko further explained that this conflation resulted in a 'throwing out of the baby with the bathwater':

> Whenever a method or a theory is rejected for some reason, the data are dismissed along with the associated method or theory. The practitioners reject each others'

> theories and habitually discard the data in the process. The critics discard method, and this also tends to devalue both theory and data. However, if the data are rejected, so is an important link to the phenomena, with the consequence that psychology often seems to exist in a phenomenal vacuum. (p. 266)

Katzko argued that the simpler solutions to psychology's technical mass-production of literature had not been implemented because of the systemic nature of the emphasis on uniqueness. He introduced Kerr's (1998, as cited in Katzko, 2002) notion of "HARKing (Hypothesizing After the Results are Known)" (p. 267) as another variant on psychologists' use of rhetoric. Katzko explained:

> Articles are written not only to conform to a rhetorical model of hypothesis testing but also to optimize the relationship between a set of results and some set of predictions...As Kerr suggested, there is actually a large set of candidate hypotheses; only after the results are in is there a focus on a particular best fit, and the uniqueness assumption is then used to lend some aura of inevitability to the hypothesis-result relationship. (p. 267)

Katzko argued that psychologists should consider "replacing that common research report phrase 'our expectations were confirmed' with the phrase 'we were surprised to discover'" (p. 269) and concluded: "The final arbiter of quality is a personal 'refusal to be satisfied' in the search for greater understanding. No path is left unexplored. Precisely because of this diversity of paths to follow, unification lies in a more general consensus

of a shared motive or value: that everyone is doing things for the same reasons, to achieve the same intellectual ends" (p. 269).[98]

Conclusion: What a Descriptive Account of the Literature Provides

The crisis and unification literature is vast and the arguments which are presented in this literature are extremely diverse. There are conflicting opinions about whether the crisis began around World War II, in the 1970s, or if it was already chronic at the turn of the 19th century. There are conflicting opinions about whether the crisis was caused by a commitment to the methods of natural science, by a lingering attachment to philosophy, by permitting metaphysical assumptions, and more. There are proposals for psychology to be conceived as a natural science, a human science, as a discipline sitting between the natural sciences and the humanities, and as multiple disciplines, some of which are scientific while others are not. There are conflicting opinions about the nature of the crisis—whether 'crises', 'malaise', or 'healthy differentation' would be better terms for describing the state of affairs in psychology. There is disagreement about whether psychology is preparadigmatic due to its youth or whether it has had centuries qua philosophy to develop a paradigm. There are flexible proposals for unification offered, as well as proposals which are more rigid. Some authors stress preconditions for unity; others emphasize methodological or theoretical issues; while still others emphasize

[98] In response to Katzko's article, Chao (2003) argued that "rhetoric is not the universal cause of fragmentation" (p. 824). Furthermore, she argued: "unity is now less important than the integrity of psychology that converts/heals fragmentation into diversity" (p. 825). She concluded that psychologists needed to focus on developing ways to communicate with one another and to overcome the theoretical and linguistic barriers which currently separated them (and which were more causal for psychology's fragmentation than rhetoric).

social, cultural, and political issues. As others have noted, the irony of this literature is that it is as fragmented as psychology is!

Perhaps the only strong and consistent theme running throughout the literature is the theme of 'two cultures' or 'two psychologies'. Throughout the literature, the two cultures in psychology are described as objective versus subjective, behaviourism versus phenomenology, objectivism versus introspectionism, and scientific versus humanistic—and a plethora of dichotomous variables are discussed which can easily be categorized according to this general schema. However, even if there is a general consensus that this basic dualism is somehow central to the crisis of psychology, there is no consensus as to what should be done about it. Some authors suggest trying to reconcile them; some suggest a rupture or divorce; some suggest some form of eclecticism; still others suggest introducing a third culture; and so on.

After surveying this literature, I cannot help but agree with Vygotsky (1997), who argued that a theory of the crisis is lacking. Authors have put forward their opinions of what caused or maintains the crisis of psychology—i.e., they have proposed 'theories' of the crisis in one sense of the term 'theory'—but a *theory of the crisis* in the sense that Vygotsky was proposing—i.e., an in-depth, philosophically-based theory or perhaps meta-theory, which is based on a detailed analysis of the numerous contributions found in the literature on the topic—is still lacking, 80 years after he wrote his manuscript. If this detailed descriptive account is a first step at bringing together this fragmented body of knowledge, this kind of theory of the crisis should be the second. Such a theory will provide a basis for then going back and evaluating various contributions to the literature

in a meaningful and valuable way. From there, epistemological and practical recommendations—which are grounded in over a century of discourse, and which have a strong theoretical basis—can be proposed. Without this work to develop a theory of the crisis, and to meaningfully evaluate the various contributions to the literature, contemporary contributions risk being viewed as 'mere armchair speculation'.

In concluding this descriptive account, I would also like to devote some space to discussing its limitations and strengths. First, I believe the primary limitations were outlined in my introduction. In general, this descriptive account does not include *unpublished* material; it does not include material from *non-English languages*; it does not address how *specializations* of psychology have experienced the crisis—if at all; and it does not address *social, academic, or biographical contexts* in which the literature has been written and published. I am sure that others can point out other limitations, but I believe these are the most obvious.

In terms of strengths, I believe they relate to what a detailed account of the crisis and unification literature provides. First, it provides an initial detailed presentation of the crisis and unification literature, which can be evaluated and expanded (especially with material from other languages). Secondly, it provides a basis for developing criteria for evaluating the various crisis and unification writings. For example, do these writings address the variety of issues mentioned by various crisis and unification authors throughout the literature? Third, it provides a basis for subsequent theoretical work. Specifically, theoretical analyses can be conducted, using the material provided in this descriptive account, to address the nature of psychology's crisis and the possibility and

desirability of unity in psychology. Fourth, it provides a basis for subsequent biographical and social-historical research, which can illustrate the contexts in which these materials have been written and published. Finally, it provides the basis for future empirical research, of which there has been a negligible amount produced to date. How do members of the discipline perceive and experience the 'crisis' of psychology? What are their opinions on 'unification'? Are there differences between or within various groups of psychologists and students of psychology? These are empirical questions which, if answered, can help direct future theoretical and practical unification efforts. I am hopeful that others can point out other strengths, but I believe these are the most obvious.

General Discussion

The first section of this history featured a qualitative exploration of the contemporary phenomenology of the identity of psychology. For all the vast amount of writings which have been produced on the topic of psychology's identity, negligible empirical research has been conducted. However, it is important to assess whether or not various authors' concerns about disunity are the philosophical speculations of a select few or issues which are relevant to a wider—and contemporary—audience within psychology; and this qualitative study is a first step toward assessing contemporary perceptions of the identity of psychology. It is an initial litmus test to determine the relevance of the crisis and unification literature to the more practical and personal concerns of contemporary faculty and students of psychology.

And this litmus test has indeed supported the tentative argument that the crisis and unification literature is relevant for contemporary faculty and students. Although 'crisis' was not a phrase used in the focus groups, a number of themes clearly indicated the impact of disunity. For example, I discussed Susan's *disappointment* in psychology's inability to be a full science. I also discussed Debbie's experience of *alienation* from the rest of the discipline. I also quoted Michael, who argued that there is no unity within psychology, that a single discipline called psychology is only an historical phenomenon, and that, if 'psychology' could do things over again, it would split up into multiple disciplines. I also quoted Jill, who voiced concern about the split between basic and applied researchers within psychology; and, finally, I also quoted Diane, who took issue with psychology students having to study topics which, in her opinion, could be

assimilated into existing natural sciences. Although more research is clearly needed, these initial findings support the argument that the crisis and unification literature does have contemporary relevance.

The second section of this thesis featured a detailed descriptive account of the crisis and unification literature. Although this literature was traced back to at least the end of the 19th century—and despite the fact that it included well over 300 sources—no detailed review had ever been published prior to this thesis. As a result, this literature was extremely fragmented and none of the contemporary contributions were doing justice to the full scope of the topics involved. The account which I have produced is a first detailed treatment of this literature. For the first time, it has been brought together in one document, which can be examined, critiqued, and expanded—but also used as a basis for developing more sophisticated writings and research projects on this topic.

There were a number of important themes evident in the descriptive account, but perhaps none more evident than the theme of 'two cultures' or 'two psychologies'. Indeed, throughout the literature, it was evident that the disunity in psychology was, as Vygotsky (1997) argued, 'fundamentally dualistic': scientific versus humanistic; science versus practice; quantitative versus qualitative; objective versus subjective; behaviourism versus phenomenology; the list goes on and on. In short, although psychology's crisis has been described as a 'crisis of disunity' (Staats, 1983), it appears that this disunity is not a chaotic or random fragmentation, but rather *a meta-level dialectical tension*, which affects psychology at the level of its subject matter (e.g., physical versus metaphysical),

methodology (e.g., quantitative versus qualitative), status as a science (e.g., natural versus human science), and more.

The unification literature was the response to this and other issues related to psychology's disunity crisis. However, it is difficult, if not impossible, to provide a neat summary of the proposals which have been suggested. Some of them include: divorcing the two psychologies; integrating them; eclectically retaining both; choosing one over the other; reducing one to the other; introducing a third culture; focussing on preconditions for unity; and developing a hermeneutic unity wherein tensions can be maintained. There is little consensus within the unity literature and, following Vygotsky (1997), I suggest that a big part of the reason why there is so little consensus is that there has been no *theory of the crisis of psychology* (using Vygotsky's connotation of 'theory of the crisis'); and I believe the lack of a theory of the crisis was tied to the fact that no detailed treatment of the crisis and unification literature had been conducted. Hopefully the detailed account which I have conducted can assist in developing a theory of the crisis, which could then be used to foster increased consensus within the unification literature.

Although a complex integration of the findings from the qualitative analysis and the descriptive account will not be included in this discussion, I will still discuss some of the major themes which were evident in both sections. First, and most significantly, was the theme of 'the two cultures'. Kimble (1984), Vygotsky (1997), and many others discussed the split between an objective psychology on the one hand and a subjective psychology on the other. And this theme was echoed in the focus groups. For example, Ellen

perceived two streams within the undergraduate psychology curriculum; one that was more subjective and one that was more "sciency".

Related to the theme of the two cultures were a number of other significant themes. The first was science versus non-science. For example, Koch (1969), Royce (1965), and others argued that psychology contained elements which were both scientific and non-scientific. In the focus groups, Laura, Kathleen, and others echoed this theme from the literature, stating that they believed psychology included both kinds of approaches. The second was science versus practice. Fowler and Bullock (2005), for example, argued that this tension was problematic for psychology, and that it had resulted in professional disunity in the form of the American Psychological Society (which places heavy emphasis on science) being created in opposition to the American Psychological Association (a large proportion of its membership consists of practitioners). In the focus groups, Jill, Lisa, and Jennifer also discussed the science-practice tension, stating that practitioners did not always make a commitment to the methods of science, despite often receiving scientific training. The third was basic versus applied research. For example, Kendler (1981, 1987) and others argued that there was a schism within psychology between those who valued basic research and those who valued applied research. This tension played out in the graduate student focus group where Lisa, Kathleen, and Nicole argued in favour of applied research, while Jill and Susan argued in favour of basic research. Finally, the fourth was natural versus human science. For example, Giorgi (1970) and others discussed a split within psychology between those who valued the methods of natural science first and those who valued the study of human psychological

phenomena, which could not be studied through the use of natural science methods. Michael echoed this argument, stating that qualitative researchers were part of a movement toward human science, which developed in contrast to psychology as a natural science.

There were also a few other themes which were discussed in both the literature and the focus groups. First, with respect to methodology—aside from some tension with respect to qualitative versus quantitative methods—there seemed to be a general consensus within the literature and amongst the focus group participants that psychology should have few restrictions on which methods it uses. For example, Yanchar (1997b), Wertz (1999), and others argued in favour of multiple methods in the literature, while John, Debbie, and others also argued for multiple methods during the focus groups. Second, Sternberg (1992), Farrell (1978), and others argued that psychology was young and preparadigmatic; and this theme was echoed by Diane, who perceived psychology as a young discipline, still struggling with its identity. Third, Koch (1959a, 1959b, 1964), Giorgi (1970), and others argued that psychology had let its subject matter be defined by its methods; and this theme was echoed by Jill, who felt psychology was defined by its methods. Finally, another common theme which was evident was the impact of social, cultural, and political influences on psychology. For example, in the literature, Brislin (1991) argued that Western culture's emphasis on novelty negatively impacted the research enterprise within psychology. In the focus groups, Michael, Jennifer, and Debbie discussed the impact of funding bodies and political influences on psychological

research, while Diane, Ellen, and Carla stressed the importance of examining cultural topics and political influences on psychology.

Clearly there was a degree of overlap between the findings from both sections for certain key themes. This overlap lends additional support to the argument that the crisis and unification literature has relevance for contemporary members of the discipline. However, more research is needed to support this assertion, and I will conclude by discussing some potential directions for future research.

Directions for Future Research

A detailed descriptive account of the crisis and unification literature has been presented. A preliminary, qualitative investigation of psychology's identity has also been presented. These two components can now serve as the basis for the development of a number of future research projects. In terms of historical projects, social and biographical research could be conducted to delve deeper into: *Who were* these major figures (and other authors) who wrote about the crisis and unification of psychology? *When and where* did they write and publish their contributions? These are important historical questions which, if answered, could aid in interpreting, understanding, and explaining the literature. Also, research could be conducted which specifically targets the specializations of psychology with respect to whether or not they have experienced persistent states of 'crisis'. Furthermore, research could be conducted to include non-English or unpublished writings. Finally, historical work could be conducted to investigate the roots of the crisis and unification literature in the early 19th century (or possibly even earlier).

A number of empirical research projects could also be conducted. First, and perhaps most importantly, by questionnaire or other nomothetic means, a wider sample could be investigated with respect to perceptions of the identity of psychology. The findings from the qualitative and descriptive sections of this thesis could easily be used to develop an instrument which would yield important data that had much better generalizability. This kind of data is very important for obtaining a sense of the degree to which contemporary psychologists and students experience a crisis or experience fragmentation, whether they perceive psychology to be a science, whether or not they would be open to some form of unification, and more. However, further qualitative research could also be conducted. For example, students and faculty *outside of psychology* could be asked about their perceptions of psychology. Some crisis and unification authors were quick to refer to other disciplines, especially physics, but only a few of them backed up their claims with any kind of empirical data. What *do* members of other disciplines think of psychology? How does psychology compare to other disciplines in terms of disunity? Finally, more qualitative work could simply be done within psychology to continue exploring members' perceptions of the discipline's identity. If this intradisciplinary work is pursued, I recommend that a question about the crisis of psychology be included.

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Table 1

List of Participants

	Pseudonym	Specialization
Faculty	John	Clinical/Social-Personality
	Jennifer	Clinical
	Debbie	Neuro-bio-cognitive
	Michael	General*
	Mark	Developmental
	Scott	Clinical/General
Graduate Students	Robert	General
	Susan	Social-Personality
	Jill	General
	Lisa	Clinical
	Nicole	Developmental
	Kathleen	Neuro-bio-cognitive
Honours Students	Carla	General/Social-personality
	Ellen	Clinical/general
	Diane	General
	Chelsea	Social-personality
	Maureen	Social-personality
	Laura	Clinical

* General includes Methodology, History, Philosophy, and Theory

Appendix A: Focus Group Questions

Opening: (1) Let's start by introducing yourselves, the program areas you are or have been affiliated with, and your current research interests.

Introductory: (2) How would you, personally, define psychology?

Transition: (3) In general, what does it mean to take a psychological perspective?

Key Questions:

(4) Is psychology a science/scientific? *Follow up:* If so, why? If not, why not and what would have to happen for psychology to be a science?

(5) What kinds of subject matter should psychology study and not study?

(6) What kinds of research methodologies should psychology use and not use?

(7) Are there common features that underlie the specialized areas of psychology and/or that make psychology unique from other disciplines? *Follow up:* If so, what are they? If not, why not?

Ending Questions:

(8) How do you personally perceive and experience the identity of psychology?

(9) Is there any other information I may have overlooked asking for that you feel is important for the identity of psychology?