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Commentary

COMPARISONS BETWEEN MAINSTREAM SOCIAL PSYCHOLOGY, "EXAMPLES" PSYCHOLOGY, AND ETHOLOGY, STIMULATED BY MICHAEL BILLIG'S BOOK: *MORE EXAMPLES, LESS THEORY*[1]

John Richer

Department of Physiology, Anatomy and Genetics, University of Oxford, UK.
Paediatric Psychology, Oxford University Hospitals NHS Trust, UK.

johnricher@oxhs.co.uk

ABSTRACT

This paper started out as a review of Michael Billig's book, *More Examples, Less Theory*, but soon the interest of the ideas in this book made it clear that it would be an ideal stimulus for a discussion about the similarities and differences between human ethology and psychology and between the "examples" psychology Billig advocates and the current mainstream social psychology.

Billig describes the thinking and circumstances of a number of students of human behaviour, starting with Locke, moving though many including little known figures and also James, Freud, Lewin and lastly Jahoda whom he sees as the best exemplar of the approach he is advocating. This approach uses vivid examples not only to communicate better to readers, but also as a way of developing a better study of people in all their complexity. The top down theory driven approach of mainstream social psychology is criticised because it does not start with getting information about people (so what is it about?), and in its experimental, group comparison, methodology with dodgy "measures", it loses much information and often comes to misleading and unhelpful conclusions. Its replicability is amongst the worst in science. The examples driven psychology is close to ethology in its insistence in looking at people in their natural habitat and getting information in great detail and in an open minded way. It differs from ethology in the types of categories it works with. Whilst ethology takes an onlooker approach and develops its behaviour categories out of direct observation, examples psychology uses ordinary language and deliberately embraces the subjective viewpoints of its subjects. Both are coherent exercises, unlike mainstream social psychology, but two different stories are being told. The story from examples psychology places it within the same category as the Arts and everyday discussions of human behaviour embracing their agency and subjectivity. Ethology by contrast is a science, using only the onlooker stance, like all sciences. It is important to stress that the two stories can and should talk to each other to their mutual benefit as long as the different epistemological status of each is understood.

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[1] More Examples, Less Theory
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INTRODUCTION

This paper started out as a review of Michael Billig's book, *More Examples, Less Theory*, but soon the interest of the ideas in this book made it clear that it would be an ideal stimulus for a discussion about the similarities and differences between human ethology and psychology and between the psychology Billig advocates and the current mainstream social psychology.

This is an excellent, gently powerful, widely researched and very readable book. Michael Billig describes it as a tour of some figures in the history of psychology which is "not concerned with the past for its own sake, as it aims to address current psychological issues by looking at figures from the past." This paper too hopes to address some of the issues which the book raises but from the perspective, not of a psychologist like Michael Billig, but of a human ethologist.

Billig is critical of mainstream social psychology, and advocates a different approach, exemplified by some of the people he discusses, and especially by Marie Jahoda. In doing so he brings the psychology he advocates closer in some ways to ethology. I shall be using the ideas in this book to discuss a three way comparison:

(1) mainstream social psychology, which Billig characterises as theory heavy
(2) The approach advocated by Billig, Jahoda and others:more examples and less theory
(3) Human ethology

At the end of this paper there is a table crudely summarising some of the similarities and differences. It will be unsurprising that, as an ethologist, I am critical of mainstream social psychology. But possibly surprising that I am very sympathetic to the "example-heavy" psychology he advocates. Whilst seeing it as clearly a different approach from that of ethology, it is a necessary one. Moreover the two approaches can and should run in parallel and compliment each other.

So on to the book itself.

MORE EXAMPLES, LESS THEORY, CHAPTER BY CHAPTER

The title of the book about psychology seems to have immediate appeal to ethologists since it could be taken as saying, more direct observation (= more examples) and fewer armchair hypotheses (= less theory). But it isn't, at least not exactly, it is more. It illuminates the struggles of psychologists to develop approaches which are coherent and testable and useful. In the process Billig gently but forensically exposes the incoherence, untestablity and uselessness of some mainstream social psychology and by extension some other branches of psychology.

Michael Billig has taught and researched in psychology all his career and is very familiar with psychology's ways of thinking. The book is first presented as a critique of the way psychology is written up. As the title implies, he thinks that there is too much theory in psychology writing and not enough examples. Psychologists, he says, "often consider their discipline rests on twin pillars: theory and methodology"(page 3). He goes on to say, "Psychology requires a third element that tends to be overlooked and certainly is not treated as equivalent to the broad-backed twins" (page 3) – ethologists like me wait with eager anticipation for direct observation to be mentioned, but he continues – "psychology needs to be written" (page 3). Written?! Cue my groan of (temporary) slight disappointment.

Picking myself up metaphorically, I remind myself that Billig's agenda might be different from what I was anticipating, or this might not be his only or final conclusion (and it isn't). The explicit focus on writing should have been apparent to me when he wrote earlier in his introduction, "what is it about their [various psychologist's] writing that makes them so appealing… as I will be suggesting throughout the book, good psychological writers use well chosen examples" (page 1). I myself could add, so did Jesus, he told parables, so does any good communicator, they tell their stories with particulars, often giving the first person perspective. We are a storytelling species, the stories are accounts of actions and feelings and aspirations of individuals and their relationships with one another. In telling stories we pass on values, abilities, knowledge, and the deeds of heroes and heroines, their setbacks, courage and triumphs. Stories give a topic life and colour. We are intersubjective, and therefore receptive to absorbing the particular perspective of others. On the other hand, generalities (theories) longer than a few aphoristic phrases tend not to get absorbed.

But still I wondered whether Billig was edging towards a real science of real behaviour. He drops hints: talking about journal articles he says "Basically, the standard experimental report is empty of humans." (page 4) And later, "Example free psychology was impoverished psychology, disconnected from the lives of those who were being studied. It became increasingly clear that the third pillar [of psychology] should not be 'writing' but rather '**examples**'" (page 4, *emboldening mine*).

He quotes Thomas Scheff (1990, 2006) who made the distinction between parts and wholes in the social sciences. He notes, "The parts may have a privileged status in some areas of the social sciences, for instance in ethnography, conversation analysis and history, where analysts directly examine specific examples of life rather than trying to construct general, overall theories. In psychology, it is the wholes of theory that currently have the upper hand, squeezing examples to the margins." (page 5). Later he devotes a whole chapter to Kurt Lewin (1890-1947). Lewin's teacher was Ernst Cassirer. But Billig notes, Lewin "overlooked Cassirer's warnings that theories impoverish reality and science needs good descriptions" (page 10). Amen to that. So, I asked myself, was Billig slowly inserting a critique of psychology's dearth of direct observation of the natural phenomena under the guise of talking about the desirability of giving examples in reports? This may be an effect of his book.

Following his own dictum of specificity, the rest of the book is devoted to discussing individual psychologists or, before the word was coined in English, philosophers, starting with John **Locke** (1632-1704) and Anthony Cooper, the third Earl of **Shaftesbury** (1671-1713) who regarded Locke, his good friend, as his foster father. Despite widely divergent approaches they never criticised each other by name. This duo is a good starting point for two reasons. It reminds us of psychology's roots, not in zoology, but in philosophy with its acceptance of introspection and everyday mental concepts. Secondly in contrasting the approaches of these two friends, it allows him to describe two very different approaches in psychology, which have echoes even today.

Billig contrasts Locke's atomistic approach with Shaftesbury's holistic one, and notes the impact which that has on their writing styles. Locke is trying to break down mental phenomena into their basic non divisible components, his "clear and distinct ideas", which are the building blocks of more complex ideas. Perhaps his most enduring contribution, the association of ideas, did not appear until the fourth edition of his *Essay Concerning Human Understanding*. In his writing style he made a deliberate attempt to be plain and simple and avoid what he called the "artificial ignorance and learned gibberish" of his philosopher contemporaries. His book had numbered chapters and numbered sections within chapters to give the appearance of an organised structure building up a theory.

Shaftesbury's approach, in *Characteristicks of Man, Manners, Opinions and Times,* was entirely different: discursive, using stories and metaphor, imaginary dialogues and other devices to try and convey the underlying larger meanings which are to be intuited from the patterns of accumulated meanings. So Shaftesbury's approach was closer to the approach of most people when trying to understand something in everyday life. And it is closer to that of poets, clerics, artists, novelists and the like[2].

Shaftesbury imagines a dialogue between a Descartes-type rationalist and a Lockean-type empiricist. Both are searching for a point of certainty (The Cartesian, the certainty of his own existence, the Lockean, the certainty of clear ideas). But Shaftesbury's response to both is "And what, say I, if in the whole matter there is no certainty at all". In fact, as we shall see, Shaftesbury was nearer the truth when it comes to mental phenomena.

Shaftesbury makes no attempt to define terms, but wants to let the meaning emerge from the prose. Locke does want to define his terms. In a telling paragraph, Billig writes, on page 28 (emboldening mine):

> "Locke used the word 'consciousness' in the important section on personal identity that he added to the second edition of the *Essay* (1694). As has been pointed out, Locke was not entirely clear in his use of 'consciousness' because he did not stick to his own definition of the word (Balibar, 2014). **He was doing something similar to what many modern psychologists and social scientists do. They will often define their technical concepts with great care; but when they come to use these concepts, they will employ them in ways that burst through the restrictions of their own formal definitions** (Billig, 2013).

In another passage (page 31) Billig writes:

> "To this day, in the work of psychologists, there is a continuing tension between specialist and ordinary language. Psychologists today want to investigate psychological phenomena scientifically and they will use **special terminology** to do so. Nevertheless, they also use **ordinary terms, particularly to describe their broad topics of inquiry**. For example, today's cognitive scientists, just like Locke many years earlier, will claim to be studying 'memory', 'beliefs', 'ideas', etc., thereby using plain words to describe the subject matter of their investigations (Jacobson, 2007). However, in the course of studying such ordinarily named phenomena, psychologists will find themselves wanting to make distinctions for which there are no ready terms in everyday use. Then, they will devise specialist terms, distinguishing between different types of memory, beliefs or ideas - just as Locke had done more than three centuries ago.
>
> Locke was showing how analysts of the mind can have an uneasy relationship with plain language. Those who claim to have discovered new aspects of the world, including new aspects of the human mind, may need to be linguistic innovators if they want their discoveries to be accepted as discoveries."

I have been be less forgiving than Michael Billig. In 1974 I wrote:

> "…behaviourists study things like emotions whose identification in everyday life is a matter of negotiation, and because of the way they do it they end up with one of two absurdities. Either (i) they say such and such a behaviour is a 'measure' of the emotion,

[2] There is notion of at least one aspect of beauty is that we find something beautiful and stare at (or listen to, or taste) it at length as a way of trying to intuit the underlying meanings and patterns in the apparent chaos, in other words, the function finding something beautiful is that it gets us observing it at length and thereby helps us discern its deep structure and underlying truth.

> without, of course, being able separately to identify the emotion, or (ii) they operationally define the emotion in terms of a very narrow range of behaviours and situations and this way either lose all relevance and generality, but they craftily slip back into talking about the emotion in general when all they have observed is a change in rate of bar-pressing, so the behaviour just becomes a 'measure' of the emotion again".

This has been one of the central errors in the approach of large swathes of psychology. Like Locke these psychologists wish to study various mental phenomena in a "scientific" way, part of which is trying to find underlying units (atoms, ideas) and explore the mechanisms of which they are part. It is a well intentioned approach, they would say they are following the Popperian falsifiability criterion for science and contrast that with the untestable vague notions of, say, analytic psychology. The trouble is, as ethologists know only too well, the exercise is not founded on good direct observation of publicly observable behaviour with hypotheses emerging from that observation of the natural phenomena. Instead the hypotheses, and the justifications for pursuing a particular line of enquiry, come from everyday cultural and mentalistic concepts. This is the path which Locke, with the best of intentions, set psychology on. As Michael Billig notes there is a clear unbroken chain of explicit influence of Locke's work to some modern psychology.

By contrast, Shaftesbury has been virtually forgotten, he is hardly ever referenced. And yet his work is truer to the ephemeral nature of mental concepts. To create an analogy, he would realise it was pointless to try and carefully measure the shape of each cloud in the sky, but would argue that by observing a multitude of clouds, patterns emerge (which they do).

As Michael Billig writes (page 22, emboldening mine)

> "Shaftesbury was not attempting to construct an alternative, more rigorous theory of the mind, as Hume was to. He was following the stoic philosophers of the ancient world in believing that philosophy should aim at **practical** self-improvement. Looking in detail at the way that the mind forms concepts by combining ideas was not a stoic aim. However, as Shaftesbury presented ways to improve thinking in practice, almost as a by-product he outlined a theory of thinking that seems to be an alternative to Locke's painstaking atomism."

In this Shaftesbury was dead right, mentalistic concepts are judged by their usefulness not their truth (Richer, 1974, 2016). A connected point is made by the late John Shotter, whom Michael Billig mentions with affection in his preface. In his 1990 paper "The Myth of Mind and the Mistake of Psychology" Shotter argues that psychology is not a *natural* science, but a *moral* science whose aim is gaining practical-moral knowledge.

The fact that Shaftesbury is hardly referenced (we must thank Billig for remedying this), and that there is no unbroken chain of explicit influence, is actually consistent with his approach. It is of the moment, specific to a time and place, and to the matrix of shared meanings that existed there and then. It is like sand paintings, or street art. Ephemeral and of their times and places. But, as Billig also notes, the ideas embodied in Shaftesbury's work repeatedly surface down the centuries. Again this is consistent with Shaftesbury's approach of trying to reveal underlying, possibly more enduring, meanings, the deep structure. Insofar as those meanings are part of the "Characteristicks of Man", they will repeatedly resurface. Cultural anthropologists will be aware of this, as will ethologists observing the different manifestations in different cultures of the same underlying human needs and motivations.

To state the obvious, science is different from this. It accumulates knowledge and understanding, building on the shoulders of giants, as Newton said. But it is built on fairly solid foundations of publicly observable and agreed facts (I leave aside disputes about terminology,

definition, and Kuhnian paradigm shifts, they do not alter the basic point). Lockean style psychology is built on the shifting sands of mentalistic concepts both for its data and ideas, yet it pretends to be a science, and, like some contemporary psychology, wraps itself in the methodological paraphernalia of science, like the Emperor with his new clothes.

Shaftesbury is interesting in another area when he gives priority to the social / dialogical nature of language and thought. On page 22, Michael Billig writes:

> " For Shaftesbury, on the other hand, language was not the means of communicating thoughts that had already been formulated [Locke's view], but it was the means by which we think, whether we are engaged in actual dialogue with others or dialogue with ourselves. We think because we can speak rather than speaking because we have already thought."

Both have a point. Locke is of course right in saying that we can communicate information to others, e.g. when we witness something and then later relate what happened. But Shaftesbury's position points to a deeper truth which is that any discussion of subjective phenomena (thoughts, etc.) can only, *a priori* take place when these ideas get into the public (social) realm. The questions then become, how does that happen, what is the context? The answer is that we negotiate shared meanings, and part of the meaning must be the connected with behaviour and situation. As Wittgenstein (1953) said, "private states stand in need of public criteria".

The next chapter is about William **James** (1842-1910) possibly the best known of nineteenth century psychologists, and Abraham **Tucker** (1705-1774) whose magnum opus *The Light of Nature Pursued*, had some followers and was abridged by William Hazlitt in 1807 but it has largely been forgotten, not helped by being published under the name of Edward Search. Tucker also used Cuthbert Comment as a pseudonym elsewhere. Tucker was a man of independent means and bought Betchworth Castle near Dorking, South of London, when he was 22 and enjoyed the rest of his time quietly there, living the life of a country gentleman, albeit one well educated, but not one moving in the intellectual circles of the day. On the other hand, James, also born to wealthy parents and who trained as a doctor, although he never practiced, taught at Harvard, and had many colleagues and students as a ready readership of his books. Billig focuses on James' *The Principles of Psychology*.

Tucker "sought understanding through 'experience and observation of ourselves' …examining the workings of the mind 'in the common occurrences of life'"… "'familiar to every one's and every day's experience'"(page 60). James too, more than 100 years later, focussed on the everyday. He included in the scope of his psychology "the habitual actions such as 'standing, walking, buttoning and unbuttoning'". Both gave examples from their own lives. Tucker is relaxed about simply "gawking[3]" at people, "' a philosopher can loll out of a window like Miss Gawky, to see a wheelbarrow trundle, or the butcher's dog carry the tray'"(page 60). It was typical of Tucker's modesty to use a word like "gawk" but his observation was anything but stupid, and closer perhaps to Tinbergen's "watching and wondering" (enjoying the wonder, but also wondering, why?). It allowed him to draw attention to usually unnoticed everyday behaviour. As Billig says, "it takes a special talent to notice the unnoticed". Billig quotes Clifford Geertz who pointed out that when reading 'accounts of other cultures, we can often then become aware of our own unnoticed assumptions, customs and everyday rituals"(page 63).

[3] **Gawk** (*verb*) stare openly and stupidly (OED)

Again there are parallels with ethological direct observation, the simplest being that the observer looks at some everyday behaviour and says to themselves, "how strange, why do they do that". This is often necessary when observing all too familiar human behaviour, in a way that it is not when observing much animal behaviour which is already unfamiliar and strange. The ethological approach was developed observing "strange" animal behaviour.

Billig brings out how Tucker differed from Locke in another respect. He argued that behaviour can only be understood as embedded in sequences and situations and to try and gain understanding by isolating discrete items, Locke's clear and distinct ideas, was simply not going to achieve understanding.

Continuing this line of thought, Tucker doubted whether the present moment, in this context, actually existed. James too argued that we have no consciousness of the present, it is "'gone in the instant of becoming'" (page 67). Both used the metaphor of a river, James famously talking about a "stream of consciousness". They were emphasising the continuous nature of experience and behaviour, in contrast to Locke.

Billig notes Tucker's idea that the "mind is both active and passive. We cannot completely control our thoughts", "'thoughts introduce one another successively'", (page 67). Both saw the effort to control thoughts as something we should try to do, but something we shall never completely succeed in doing. Both of which points clinicians treating psychiatric disorders are daily well aware, namely the desirability of the effort and difficulty of success.

Billig comes close to the theme of the book when he writes (page 74),

> "Tucker and James both viewed concrete examples as more than stylistic adornments to amuse their readers. They were also a means for discovering how the mind operates. … Tucker wrote that he was not going "'to hunt after abstract notions' but, instead, he was going to examine humans in terms of their actions and thoughts: we may discover our own nature 'by diligently observing what we do, how we come to act in such or such manner, together with the consequences or effects of our actions' …
> It was, Tucker wrote, the way of science that the ' abstract must be learned from the concrete'".

I asked myself, is he edging towards direct observation being the starting point of a science of human behaviour? This foundation in concrete examples would have helped to avoid, as Tinbergen (1963) put it, the situation where "psychology skipped the preliminary descriptive stage other natural sciences had gone through, and was soon losing touch with the natural phenomena".

Billig writes that James warned of a related danger which he called the "psychologist's fallacy"…"This 'great snare of the psychologist' occurs when psychologists confuse their 'own standpoint with the mental fact' that they are reporting (1890, Vol. 1, page 196)." (page 77). Billig sees it as one the strengths of both Tucker and James that "they could empty their minds of pre-held theories and look directly at their own experiences". This is perhaps overstating the strength, since we are all prey to preconceptions and categories with which we describe experience, as Billig discussed in relation to Freud. The same criticism has been levelled at direct observation, namely that it can never be completely free of preconceptions (Cooper et al, 1974), to which the reply is, Yes but one tries to minimise them and not commit to a single hypothesis beforehand, observation is not the same as an experiment (Richer, 1974). Part of this is doing what Tucker and James, and indeed Shaftesbury advocate, namely using ordinary simple language free of jargon.

The next chapter is on **Freud**, with the subtitle, "Writing to reveal and to conceal himself". As Billig writes," The quality of his writing is evident not just in the stories he tells from his case histories, but in the way that he constructs arguments, formulates striking metaphors and uses examples to convince his readers." Harold Bloom claimed "that such were Freud's persuasive skills that he is a difficult writer to resist, …" (page 79). So there is no shortage of examples.

Consistent with the story so far, Billig focusses, not on Freud's psychiatric writings such as *Studies on Hysteria,* but on *The Psychopathology of Everyday Life (1901)*. Central, of course, to Freud's ideas is that people are not actually thinking and feeling what they say they are, but their repressed unconscious motives can be revealed by, for instance, psychoanalysis. The analyst must always to be suspicious, including when the object is himself. Luckily Freud's writings about himself are sufficiently vivid and detailed to allow alternative interpretations from the ones he offered. His method of suspicion can thus be turned back on him, and in a nice turn of phrase. Billig writes, "we are faced with a paradox: to follow Freud, and to accept his word at face value, is not to follow Freud." (page 80).

Billig embarks on a long and detailed detective hunt about an example of self observation which Freud uses at the beginning of the *The Psychopathology of Everyday Life* where he related an incident when, on a holiday journey through the Balkans and in conversation with someone on a train, he had a momentary lapse of memory and could not remember the name of the artist who painted the picture of the Last Judgement in Orvieto. (It was Signorelli.) He wrote three slightly different versions of this, one private in a letter almost immediately on his return, the next published was in a journal and the third in the book. Much controversy has swirled around this; there has been speculation as to whether or not he was having an affair just before the forgetting incident and also speculation about associations of the word Signorelli might be stressful and so the name repressed and there was the question of why none of these entered Freud's analysis of the memory lapse. As Billig remarks,

> "The question is not, How do these connections contribute to Freud's temporary loss of memory? It is, Why did Freud fail to make such obvious connections in his analysis? Something must have been stopping him. And then, there is a further question: What would Freud have thought if one of his patients had denied that there were such associations with the forgotten Italian name? Freud's own ideas about resistance and repression would have alerted him to suspect that something was being concealed." (page 105).

Billig wonders, not only about repression in the stories, but also whether Freud's writing about this incident was itself an act of repression (I would slightly rephrase it and say it was part of Freud's repression of ideas about his putative affair). He reminds us that Freud was very concerned about his reputation and that the description and analysis of some of his examples may have been distorted by that. By seeming to reveal himself, he in fact conceals.

Billig also reminds us that people tend to go easy on themselves and he quotes Wittgenstein (1980) who wrote, "'nothing is so difficult as not deceiving oneself'" (page109).

So, as Billig says, (i) Freud's descriptions, his examples, were vivid and compelling, and they were instrumental in spreading his ideas and they were the fuel for much subsequent debate and analysis. But also (ii) Freud's descriptions, like anyone's, contain interpretations, a point already made above in relation to direct observation. This latter point has been used by some psychologists to argue for the weakness of using any examples, but this, Billig points out, is naïve.

The next chapter is on Jacques **Lacan** (1901-1981) and subtitled "An Ego in Pursuit of the Ego". Lacan is the odd one out, Billig writes, because all the others have given him interest, pleasure and stimulus, even when he disagreed with an idea of theirs. What shines through in his

description of Lacan is not only the vacuity of his ideas, but something approaching moral disgust at egomania of the man himself. He paints a picture of a bully, an egoist, a narcissist, a speaker of pompous, sometimes dishonest, nonsense wrapped up in a style of opaque prose which led the gullible to think he was saying something really profound, even though, or perhaps because, they could not understand what he was saying. He was behaving like a cult leader and trading in mystery. (In passing, the book, Prophets, Cults and Madness by Anthony Stevens and John Price (2000) – who often came to ISHE meetings – is relevant to this story of Lacan).

The contrast is with Shaftesbury and Tucker, modest men who did not seek to promote themselves but instead put forward their ideas modestly and clearly, even pseudonymously in Tucker's case. Locke and James, though famous, are on the same side of this fence. Character matters. When someone acts in such a way that they themselves are more important than the truth (like the present (2020) incumbents of the White House and 10 Downing Street), then it is very likely that what they say and do will not serve the public good, or, in Lacan's case, will not serve the furtherance of their "discipline" (a word which is a misnomer in their case). But then that's not what they are trying to do. The way that Lacan is described is the embodiment of the adage, "never let a fact spoil a great idea". One of Lacan's "big ideas" was the importance of children's recognition of themselves in a mirror for the development of a sense of Self. This is forensically destroyed and discredited by Billig pointing to lack of evidence, lack of clarity and coining of neologisms whose meaning is unclear, misquoting or ignoring the findings of others, dearth of references, belittling others and at times being spiteful. At one point he mentions Henri Wallon, a major figure of inter-war French psychology. Wallon published about children and mirrors some years before Lacan. Lacan later published similar ideas with no reference to Wallon, and some writers have suggested he plagiarised Wallon's work, which would be all of a piece with the other characteristics Billig describes. Character matters.

Billig ends the chapter contrasting Lacan with Tucker and Shaftesbury. The latter's self deprecating humour, attachment to reality and respect for the reader is contrasted with Lacan's arrogance and assumption that he does not have to explain himself or give evidence. As he says, "We do not have to take the masters at their word, especially when that word is so difficult to understand but all too tempting to follow." (page 151) Lacan would almost certainly not been admitted to the Royal Society in London, whose motto is "Nullius in Verba" - take nobody's word for it.

The two chapters on psychoanalysts, on Freud and Lacan, are the only ones that become slightly tortuous in their forensic examination of facts and texts and their detailed exposition. But then, one might say, that's psychoanalysis. Its "virtue" is that it can explain anything and thus is a splendid tool allowing clinicians to justify whatever their professional intuition tells them is their patient's problem and what they should do about it (Mandel, 1963; Richer, 2014) It does not need to be tied down to reality because it needs to embrace any reality. Because of that, it is also a gift to literary criticism. But it is the antithesis of science.

The next chapter is on Kurt **Lewin** (1890-1947). The subtitle is "Is There Nothing as Practical as a Good Example?" which is a play on Lewin's motto, "There is nothing as practical as a good theory", itself probably dating from 18th century German.

Billig puts in two words of warning. He is crediting Lewin with inventing the sort of "real-life" social experiment, where some perturbation was introduced into an environment not otherwise interfered with by the psychologist, and the results are observed (page154). Harold Garfinkel (1967) advocated a similar methodology, breaching experiments, sometimes known as "Garfinkeling" where a stooge violates some everyday social convention and the result is observed. It bears similarities to what in Perceptual Control Theory is called the "Test for the Controlled Variable", again the experimenter alters the feedback to the person and observes the result, as a way of finding out what variable (the perceived environmental condition) the person is

trying to achieve (Mansell, 2019). Lewin's approach will also be familiar to ethologists and was classically used Lorenz and by Tinbergen as early as the 1930s (e.g. Ter Pelkwijk and Tinbergen (1937), Tinbergen, (1948)).

Billig's second word of warning (page 155) is that Lewin's "social experiments are not the tightly controlled experiments that have come to predominate within social psychology". To which I for one would say, all the better for that. There is a group delusion within psychology that by extracting people from their ordinary environments and putting them in some highly researcher-prescribed and researcher-controlled environment and then changing one variable, is actually interesting. Why it is interesting to know how someone behaves in an environment far removed from everyday life whose effects (of the "controlled" environment) are often neither known nor even wondered about? It is caricature of science, often done, one fears, more to look good and seriously "scientific" than to find interesting information about the natural world.

Lewin attacked this "controlled environment" research for other reasons. One was that the aggregation of each person's data into group scores. He argued that each trial must be treated as a separate event (an example). Instead psychologists should "study specific, concrete events unfolding over time". Here clinicians have a clear head start thanks to the privilege of seeing their patient's progress over time. The clinician conducts an intervention (a.k.a. treatments[4]), akin to Lewinian real-life experiment, in the patient's otherwise uncontrolled everyday environment and is able to observe the results. Another of Lewin's objections was that the standard statistical procedures obscure what is going on with each individual and often exaggerate small differences. The search for such differences encourages what Freud "called 'the narcissism of minor differences'" (page 165).

Billig quotes Lewin's formula: B= f (PE) which means **B**ehaviour is a function of the **P**erson and their **E**nvironment. To this an ethologist, and many others, might ironically reply, "Oh, really?" The idea was there in Darwin a century before and almost certainly centuries before. Would anyone ever have doubted it? My old friend Glen McBride, Professor of Ethology at the University of Queensland, who was a master of the telling aphorism, pointed out we are all "social chameleons"[5].

In the search for what makes for a good theory Lewin advocated, copying physics, that we use more mathematics in theory formulation. He criticised the psychologists' use of "valuative concepts" and "binary concepts", an instance of both errors being "normal vs pathological". The concepts used in theories should be non valuative and "continuous gradations". (A small voice might ask, "can one be slightly pregnant"? - sometimes binary is appropriate.) Lewin's philosophy teacher, Ernst Cassirer, to whose views Lewin claimed to adhere, actually had very different views and seemed essentially to be saying that each discipline will develop its own terminology and methodology. In particular, biology and psychology need both binary and continuous concepts.

Billig goes on to report that Lewin's work did not always conform to these ideals. One set of studies compared autocratic and democratic, and, later, laissez faire, styles of an adult leading groups of children brought into the campus and given a task to do. The goal was to show the superiority of democratic styles over autocratic, which Billig points out offends Lewin's own criteria by being both valuative and binary. The reports were also value laden in their terminology

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[4] Of course the treatments are done in the patient's best interests to achieve a positive result for them, that is the overriding consideration, plus the likelihood of the anticipated outcome is usually known. The experiments are not done to satisfy some curiosity of the clinician, unless that is no well researched options are available then the patient enters that "experiment" having been fully informed.

[5] Another time he was describing the results of studies looking at what environmental features best promoted egg laying in domestic poultry, there needed to be and upright rough surface and an overhang. Having described the research, he concluded, with characteristic modesty and humour, "We had discovered the bush".

and interpretation, and often tautological in their "explanations". An example of the latter would be: the boys were aggressive because they were tense, how do we know they were tense? Because they were aggressive. Such tautologies are common and useful in everyday speech, a teacher might say, he can concentrate when he's interested. How do we know he's interested? Because he concentrates. It is unfortunate that psychologists have sometimes failed to break out of this everyday way of thinking, whilst still posing a scientists.

Towards the end of the chapter Billig discusses the contribution of Cassirer and how that contrasts with that of his pupil, Lewin. In particular he looks at ideas about description and interpretation. The thread here seems to get a bit lost (or rather, to be accurate, I lost it). There is Cassirer's point (made by many others) that, 'science means abstraction and abstraction is always an impoverishment of reality' (page 182) in contrast to art which "can achieve 'an intensification of reality'". So the route to understanding is to describe carefully the details of behaviour. Cassirer reports Robert Mayer, the founder of thermodynamics, as saying that a full description of the facts involves explaining them – "Once we have described something in sufficient detail – …- we have explained it." What Billig does not bring in at this point is the thorny problem of the terms in which descriptions are couched. At the end of the section he quotes Goethe: "all that is factual is already theory" (page184), which I (I am certainly not a Goethe scholar) would take to mean that the choice of the terms in which phenomena are described has implications for how the phenomena are understood, and so inclines towards certain explanations. But that "biassed" description is not itself an explanation.

Returning to Lewin's leadership experiments, Billig shows how proper detailed description of the boys' behaviour and the situation and events which surround it, which minimises abstractions and interpretations, gets to very different conclusions from those Lewin et al arrived at. He also shows how the experimental conditions themselves, were far from models of what they purported to be (autocratic etc.) not least in the actual power structure within the experiment.

But for all his forensic criticism, supported by detailed background research, of methodology, terminology and conclusions and of Lewin's failure to adhere to his own maxims, Billig nevertheless ends on a positive note about Lewin, his optimism, care for his students, his boundless curiosity, his willingness to engage in research with possible practical outcomes and his desire to further psychology as a science. All of this recommends him to Billig as someone to be kept in mind in difficult times, even though his style of social psychology is hardly followed now.

Henri **Tajfel** (1919-1982) is the next psychologist to be considered, although he came to psychology quite late. Born in Poland, he was studying in France when war broke out, he enlisted in the French army, became a prisoner of war, but survived despite being Jewish, unlike most of his family and friends in Poland. He worked for some time trying to rehabilitate surviving victims of Nazi antisemitism. It was not until 1951 when he was in his early 30s that he began to study psychology. This was in London, whence he went to Durham then Oxford then to a Chair at Bristol. It was at Bristol that Michael Billig was a student of his and was cajoled and inspired into an academic career in psychology. So, unlike the other authors he discusses, Billig actually knew Tajfel personally, which made choosing a text to discuss in this book difficult.

A theme Billig chooses to bring out is the interesting one of "a familiar psychological trope" (page 193) where psychologists tend to write in universalist ways, as if assuming that the findings of some study in one time and place are generalisable to all humanity. Billig does not mention the even more ludicrous assumption that the behaviour of laboratory reared rats or pigeons can be uncritically extrapolated to tell us anything interesting about human behaviour. (which is not to say there are not many interesting similarities between species but these need to be demonstrated not assumed).

Billig cites the example of Fritz **Bernstein** who moved to Palestine in 1936, thereby escaping the holocaust which claimed so many of his family and friends. In 1926, though not University

educated, he wrote a book in German entitled "Anti-Semitism as a Group Phenomenon: Exploration of a Sociology of Jew-Hatred". After the war he became active in Israeli politics and became a minister of trade in its first government. He was asked to publish an updated version of his book in English but he was too busy to rewrite the original so his son translated it and he wrote an epilogue which then became a prologue. Bernstein had tried in the 1926 book to propound a theory of group enmity, centred around frustration leading to displaced aggression at a group level (ethologists call it re-directed aggression) and then see hatred of Jews as one example of that. In his prologue to the 1951 translation, Bernstein argued that the enormity of the holocaust made it impossible for him, a Jew, then to be dispassionate and detached about Jew hatred. Even in 1923, Billig wrote, " Even in 1923, Bernstein had been aware of the dangers of explaining the unforgiveable. He wrote that if one tries to enumerate the causal factors behind a crime, one risks diminishing the guilt that should be attached to the criminal for '*tout comprendre c'est tout pardonner'* page 98). After the war, that had become unthinkable" (page 210). Billig argues "It would be inappropriate to 'explain' the Holocaust, using the same set of terms that might be used for understanding other types of group identification, especially those that can lead to positive social actions."(page 209). This may be true in the sense that theories may be inadequate and insufficient to explain the holocaust, but Billig, Tajfel and Bernstein seem to be in danger of committing a variant of the moralistic fallacy – the fallacy which says that because something ought not to be the case, it is not the case. (This fallacy, of erroneously deriving "is" from "ought", is the reverse of the naturalistic fallacy – cannot derive "ought" from "is"). All three are Jewish so their feelings are totally understandable and legitimate. If this (quasi moralistic fallacy) is what he is arguing, and he seems to skirt close to it throughout the chapter, then I would part company on this. To explain something is not make any moral judgement nor to excuse it, although the Courts are sometimes invited to do precisely that. Again there is the muddling of scientific explanation with moral judgement.

But Billig may not be implying this as he goes on to quote Tajfel as arguing that "should try to study social psychological factors within particular cultural and historical contexts."(page 211). This cannot be false. He argues that this is ignored by many social psychologists who seem to think that the phenomena in one place and time can be a model for all places and times. Failure to study the particular in its own right, risks not only ending up with "bland generalities" but missing out on rich data sources. By prioritising theories, social psychologists have generated less not more understanding. A parallel thought came from the ethologist Nick Blurton Jones (1975, page 72) when he succinctly wrote, "the lateral thinking inductive approach of ethology may be contrasted with the deductive approach of psychology and its disdain for facts for their own sake". It is perhaps telling to note that when a natural scientist is asked what s/he does, the reply is usually couched in terms of the phenomena studied – carbon compounds, foraging behaviour of Bonobos, hurricanes, and so on. With a psychologist the answer may well be," I'm taking a social constructionist/ psychoanalytic / behaviourist/ … approach", i.e. the defining characteristic is the theoretical approach.

It is important to put these ideas in perspective. Billig quotes James: "As William James had realised in much gentler times, theory, by its simplifications, mutilates reality" (page 215). This is certainly true of the premature theorising in social psychology which Billig describes. What the premature, one might say impulsive, theorising in social psychology does is to cause the subject to bounce around like a ball in an arcade pinball machine ending up back in the hole in the bottom rolling on to be the plunger ready for another round. But, in defence of good theory, would anyone say that Newtonian Laws of motion mutilated reality when they predict most motion

phenomena[6] in a good enough way? Newtonian Laws do not seek to embrace the beauty of the dance of a ballerina, or of the flight of a swallow, although those movements are subject to those laws. Essentially, Billig is implying, and as Tinbergen (1963) and Blurton Jones (1975) said, by failing to describe the richness of a multitude of examples, psychology was rushing into premature theorising in an effort to look "scientific". Ethologists have long held to the mantra when deciding behaviour categories, "when in doubt, split don't lump".

The next chapter is on Marie **Jahoda** (1907 – 2001). Billig writes: "Marie Jahoda, … claimed that 'the major merit' of Tajfel's position was that he put 'theory in its place' – and its place was not at the top of social psychological tree (page212)". Jahoda was born to a middle class Viennese family, studied teaching then psychology at Vienna University whilst at the same time being active in left wing politics. Her goal had been to move into government and she saw her studies in psychology as a good training for working for change in Austria. Her attitude to research generally was to see it as pointless unless it had a practical goal. In this sense she could be seen as more of an applied scientist. She seems to have seen the alternative to being driven by practical problems as being driven by theory, but this is not the only alternative. Another alternative is to have simple curiosity about everyday behaviour, like Abraham Tucker or the Earl of Shaftesbury, or like any ethologist who does Tinbergen's "Watching and Wondering" with the implicit mindset, "how strange", to make the familiar unfamiliar.

Jahoda's study of the unemployed in Marienthal, begun in 1930, proved a groundbreaking and classic study. It is perhaps noteworthy that her then husband and co author Paul Lazarsfeldt was the director of her Institute's research centre which did much market research, i.e. applied research with short term practical goals. But soon afterwards, the rise of right wing authoritarianism in Austria led to her being imprisoned and then released after eight months on condition she leave the country. She went to the UK and, after the war to the USA, before returning to the UK eventually to take up a Chair at the then new University of Sussex.

The Marienthal study had a number of features which would not be unfamiliar to those in market research and other features not unfamiliar to anthropologists and particularly it has much in common with ethogeny (Harré and Secord, 1972,- in which book, Jahoda is not referenced). In the Marienthal study, the researchers immersed themselves in the community. "The research team arranged for free medical consultations, organised gymnastic classes for the young girls and a pattern design course for their mothers and generally the researchers offered guidance and advice to parents. The researchers also rewarded the villagers for their cooperation with second-hand clothes" (page 226). They gathered some quantitative data, but also gathered accounts from the people. A similar approach was later used by Peter Marsh, Desmond Morris and Rom Harré and others in researching violence in football supporters and violence in pubs (bars) (Morris, 1981, Marsh et al 1978). They employed similar methodologies which involved the negotiation of accounts with the subjects of the study as well as gathering quantitative data. (In the "negotiation of accounts" the researcher and subjects discussed and discussed until there was agreement between them that the researcher understood their point of view.) It is full of verbatim examples. Much of the research had a practical point and ended with recommendations (e.g. Marsh and Fox Kibby, 1992, but Jahoda is not referenced here either);

Billig writes, "Marie Jahoda derived three lessons that would last the rest of her long, productive life: the importance of using qualitative material to understand the lives of individuals within a community; the importance of realising that people do not react to the same

[6] I leave out Einsteinian theory of Relativity, indeed a parallel could be drawn between how this is needed to explain phenomena on a cosmic scale, on the one hand and on the other, and the need for social psychology theories which may explain mild version of group prejudice, to give way to other theories to explain the enormity of the holocaust.

circumstances in identical ways; and if research is based on social problems, then there are reasons beyond theory and methodology for deciding whether or not to publish the findings."(page 229).

Billig himself (2013) has criticised, like Jahoda, the use of group data, which not only hides important and interesting individual differences but leads thereby to erroneous conclusions. Relatedly, the orthodox psychology habit divides people into parts – emotion, attitudes, beliefs, memory, cognition, etc., but Jahoda asks, "where is the person?". To an ethologist there is so much wrong with orthodox approach it is difficult to know where to begin: what makes emotion, memory, etc. good scientific categories? They are *a priori* concepts not derived from direct observation but from everyday language. How are they defined in terms of observables? (they aren't). How are they objectively and clearly related to each other in observable ways?(they aren't). Sometimes, more in cognitive than social psychology, they are given the veneer of systems theory probity by being put into boxes which have arrows linking them, the boxes are labelled attention, memory, etc.. One of the glaring errors here is that concepts which in ordinary language apply to the whole organism, are then proposed as part of the mechanism within the organism. It is an example of the homunculus fallacy. With this welter of incoherence, it is no wonder that social psychology studies have such a poor rate of replicability (Open Science Collaboration, 2015).

Billig continues, "Because people's lives are so complex, they are uniquely different". As a starting point in a research programme, this is a good one. Difference should be assumed until similarities are shown, this is much the same point as the critique above of assuming rats are like humans, or that people can be put into groups and thereby assumed to be all the same. But as a statement of fact the statement cannot be true, there are similarities between people, obviously in physiology but also in behaviour, and more so in the motivational structure of behaviour (Bowlby, 1969, 1973, 1980; Richer, 2016), and also in the underlying structures of information processing (see e.g. van de Rijt-Plooij and Plooij 1992, 1993; Mansell, 2020). Indeed without such similarities social life would be chaotic and impossible. But the quest for these underlying similarities comes from examining the complexity and variety of observable behaviour. Billig has described how this was done by the Earl of Shaftesbury onwards and explicitly states (when he was talking about the people in his book, on page 244) "it is not necessary to go to the opposite [of theory] extreme and to assert, in a burst of anti-theoretical enthusiasm, that every particular instance must be dealt with as an isolated example. There are points that can be made by looking across the various historical examples, noting some patterns of similarity and difference,…". Of course, the functioning of these putatively common underlying features will be varied as will their expression in behaviour. To ethologists this variety in behaviour is a given, without it Darwinian natural selection would not operate.

Ethologists have also advocated prolonged direct observation in the natural environment to absorb intuitively the deep structure of the animals' behaviour. I remember partially getting to that when observing autistic children, I got to the point where I could say what the child would do next and surprisingly I was quite often right! A long before that Schneirla (1950) put it well, "It is necessary intellectually to soak in the environmental complex of the animal to be studied until we have a facility with it which keeps us as it were, one move ahead and that from there onwards there is a progression from direct observation in the field to 'field experiments' to experiments in the lab." . Note too the direction of travel, lab experiments come last.

CONCLUSIONS

Michael Billig's advocacy of using ordinary language uncluttered by jargon and theory to describe human behaviour and situations should sit easily with human ethologists for reasons related to evolutionary theory. Ordinary language has evolved over tens of thousands of years largely to do just that. What we have now is there because it is fit for purpose, it succeeds in communicating with sufficient accuracy, flexibility and adaptiveness be used over and over again, i.e. to survive. And, of course, as with other memes and genes, evolution continues.

It should also sit easily with many clinicians, who need to talk with their patients and learn their symptoms and communicate with patients to offer what treatment they can. In fact to be a clinician is to be in a highly privileged position, akin to a bird watcher in their hide, privy to the sorts of descriptions and the range of described (and observed) behaviour not so readily available to others. Billig himself advocates gathering a wide range of observations in everyday situations, the better to contextualise and understand any specific behaviour.

A word of caution. When advocating descriptions and examples we need to remember that we use language not just to describe. As the philosopher John Austin (1962) pointed out, we do things with words. His example was the "I do" in the (English Christian) wedding ceremony. But the scope of so called "performative utterances" is very wide. Even in our descriptions we are sending messages about ourselves, our social positions and our wishes, Billig more than hints at this in many places. Words are political as well as descriptive. Our everyday "theories" are practical not scientific. Sometimes they help make decisions but also they are often *post hoc* justifications. Proverbs are exemplars of both functions ("Many hands make light work." "Too many cooks spoils the broth"). As already mentioned, like many clinical theories they have the virtue of supporting our decision making and/or then justifying it. Billig criticised Lewin for doing just that but pretending to be "scientific". Science, by contrast, tries to purge the political/ performative aspects from its descriptions. The use of the awkward passive tense is a characteristic of this approach. Locke and the thousands of psychologists after him were no doubt trying to create a purely descriptive basis, with their operational definitions, basic units, etc.. From that they hoped to construct "scientific" theories of behaviour. But they never succeeded in purging the performative and mentalistic (Richer, 2017) from their terms, so their house was built on sand.

Billig, from a different angle, is making a similar point, from within psychology, in his advocacy of examples, plainly described. His is the route to a much better, more useful psychology. But it would not be a science in the strict use of the term. In the terms coined by the anthropologist and linguist Kenneth Pike, it is an "emic" study, one which describes the culture in its own terms, it is not an "etic" study like the physical and biological sciences, which describe the phenomena in the scientists' terms.

Billig's late friend John Shotter (1990) strongly argued that "1) that psychology is not a *natural* but a *moral* science; 2) that instead of what might be called a theoretical/explanatory approach, aimed at producing theoretical knowledge, it must use a practical/ descriptive approach, aimed at gaining practical-moral knowledge". In this Shotter hits the nail on the head. To emphasise this point, "practical-moral knowledge" is about guiding and justifying our individual actions. But we then ask, how does this psychology fundamentally differ from the activity of folk like journalists, novelists, poets, painters, musicians or market researcher? The answer is that fundamentally it does not. It is another evolving facet in our cultures using the language of our cultures, albeit with its own style and approach, just like poetry or novels or painting or music are different in their approaches. Part of that approach can be counting, using statistics, being careful what is counted and what may be concluded and so on, but that does not

make it a science. Billig says as much after quoting Jahoda's vivid and heart-rending description of a poor family struggling family in Marienthal by saying, "A great artist could have painted the father on his stool in a way that conveys within one image the suffering, the despair and the unselfish spirit. The single scene would then suggest the whole life, not just of one family but of others too." (page 235). The point lends further force to Billig's basic argument by extending it, psychology writing should engage people and be accessible. It needs to do this in order to be part of the cultural conversation[7]. But it should not pretend to be a science like a natural science.

I have in the past drawn a distinction between two types of story about human behaviour, those that embrace the perspective of the agent, and those that only embrace the perspective of the onlooker (Richer, 2016). The two are summarised in the table below adapted from that in the 2016 paper. The approach advocated by Billig and exemplified by Jahoda and others in his book yields an Agent story, ethology yields an Onlooker story. It is important to stress that the two stories can talk to each other and be mutually beneficial as long as the epistemological status of each is understood, which is not the case in the mainstream social psychology as described by Billig.

Two stories about human behaviour

Agent	**Onlooker**
Sharing Minds	Observing bodies
Agency involved, action	Onlooking only, events
I do	*He/she/it* does, or, It happens
Feelings, intentions, reasons	Causes, effects, goals
Free will	Determinism
emic (culture's own terms)	etic (observer's terms)
Useful for communication *in* a culture	For communication *about* a culture
Arts, Religions	Natural Sciences
Persuasion	Manipulation
Medical Symptoms	Medical Signs
Content of consciousness	Phenomenon of intersubjectivity
Demonstrated agreements not required	Demonstrated agreements required
Billig/Jahoda "Examples" psychology	*Ethology*
Agent Psychology	*Onlooker Psychology*

[7] Which is not to say that the physical and biological sciences should not be made accessible, as many excellent science writers do, but that input into the culture is not an integral part of, and function of, those natural sciences.

I end on a personal note. When I studied psychology as an undergraduate, the first two terms were on ethology. I found that an absorbing delight and thoroughly within the scientific approach of physics and chemistry which I had studied at school. Then psychology tutorials and lectures started. My abiding shocked thought was, "What on earth are they talking about? They are not tying their ideas down to observable reality. Their data is actually not about what they say they are studying. This is only masquerading as science". Perhaps thanks to having be inoculated by ethology and before that the physical sciences, I left thinking that much of the psychology that I was taught was both non science and nonsense. Michael Billig's book puts many basic psychological ideas and approaches in their time and place and describes the minds, personalities, situations and histories of their protagonists. It helps the reader to see where psychology had come from, and why it is like it is. At the end Billig adds a few modest but welcome recommendations for young psychologists. I hope this book becomes a standard text for psychologists early in their studies. I for one should have been greatly helped if Michael Billig's book had been available when I started out.

POSTSCRIPT

There follows a table which crudely attempts to summarise some of the differences which Billig's book suggests to me, between (1) mainstream social psychology, (2) what he advocates citing Jahoda as the paradigm example, and (3) ethology.

It can be seen that his advocated approach moves closer to ethology in its detailed attention to individuals in their natural environment, in its inductive, bottom up approach, (getting information first to discover the natural phenomena that need understanding, so that, literally, they know what they are talking about) and its assumption and appreciation of individual differences. Like ethology it is a coherent study where its methods are consistent with its purposes. But it differs from ethology in its use of mental state terms and emic language generally, (to be clear, ethologists regularly ascribe mental states to their animals, but do this as a heuristic and means of communication to others, it is not the data they measure and it is not what defines their field of study). The Billig/Jahoda Examples psychology is what Shotter called a quest for "practical/moral" knowledge, it is not a natural science aimed at theory and explanation like ethology.

A crude summary of the similarites and differences between (1) Billig's account of mainstream social psychology, (2) What Billig advocates (3) Ethology *Boxes suggest closer similarity*

	Mainstream Social Psychology according to Billig	*Better psychology according to Billig/ Jahoda/Examples*	*Ethology*
Primacy of	Theory	Examples	Direct observation
Preferred Method	Experiment	Gathering accounts Direct Observation	Direct observation Asking 4 whys? followed by field and lab experiments
Samples	Groups	Individuals	Individuals
Data	Questionnaires Text responses +	Accounts + Quantitative data	Observed behaviour sampled
Where?	Laboratory Effects unknown	Natural environment Effects are a subject of study	Natural environment Effects are a subject of study
Historical roots	Philosophy Everyday language	Philosophy, Arts practical problems Everyday language	The Science of Zoology
Source of Theory	?Armchair ideas other theories, Folk psychology	Accounts +quant data Theories often eschewed	Interaction with the natural phenomena Evolutionary theory +
De-/inductive	Too much deductive Top down	Inductive Bottom up	Inductive Bottom up
Source of Terms	Everyday language	Everyday language	Direct observation
Mental terms used?	Yes	Yes	No, except often used heuristically
Definition of Terms	Operationalised, but slip into, and are justified by, everyday meanings. -> Pseudo scientific pseudo precision	Everyday meanings	In terms of observable behaviour and context
Emic?Etic?	Emic/pseudo etic	Emic	Etic
Intellectual Status	Pseudo science Poor replicability Often less useful than everyday "folk" psychology	Moral/practical study Relevant to current issues	A Natural Science The biological study of behaviour
Coherence	Incoherent	Coherent	Coherent

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