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# Conceptual Patchworks and Conceptual Housekeeping

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**Abstract.**

Recent work on scientific concepts has established that they often have a patchwork structure, in which use is regimented into distinct patches of application associated with distinct size- and/or time-scales, measurement techniques, and licensed inferences. Patchworks thus inherently involve structured polysemy. Why tolerate such conceptual complexity? Why not use distinct terms for each patch to avoid the threat of equivocation? At the very least, an account is owed about when such complexity goes too far: how and when do patchwork concepts fail? We address these questions by considering two cases of conceptual housekeeping: cases where the relevant scientists themselves judged a patchwork concept to have gone too far and took steps to clean up the mess. On the basis of these case studies (plus supporting normative arguments), we defend two theses. We argue, first, that such housekeeping efforts are context-sensitive: concept deviance cannot be read off concept structure alone. Second, we defend minimalism about such housekeeping: tolerance for conceptual complexity is an appropriate default attitude.

# Conceptual Patchworks and Conceptual Housekeeping

> "But names do have a fixed standard of what makes them good names: when they are direct and easy, and do not drift, they are good names."
>
> – Xunzi (quoted in Ziporyn 2012, 204)

## 1. Introduction

Recent work on scientific concepts establishes that they often have a patchwork structure (Bursten 2018; Haueis 2018; 2020; Novick 2018; Novick and Doolittle 2021; Wilson 2006; 2018). Much of this work focuses on particular concepts, spanning a wide range of scientific disciplines, e.g., "temperature", "gold", "homology", "hierarchy", and "cortical column". Given their prevalence, it is worth asking what may be said about patchwork structures in general, a question that is only beginning to be addressed (Haueis forthcoming). This paper contributes to that project, with a focus on *conceptual housekeeping*: the explicit, conscious efforts to scientists to regulate their terminology. Under what conditions do patchwork concepts require such efforts?

Patchwork concepts have two key features (§2.1). First, they involve multiple localized patches of use for some particular term. Second, these patches do not share core features which are individually necessary and jointly sufficient conditions of application. Rather, what connects the patches into a single structure may vary by patch. Connections between patches are local, not global. In this sense, patchwork concepts are characterized by a complex internal structure.

This very complexity invites a skeptical line of questioning: why keep patchworks around at all? Perhaps they are more trouble than they are worth, generating confusion and pointless debate where we were seeking clarity of understanding. Eliminativist responses to conceptual complexity have been raised both for particular concepts (Ereshefsky 1992; 1998; 2010; Machery 2009; Mariscal and Doolittle 2020) and as a general default view (Taylor and Vickers 2017). Eliminativism poses a general challenge for any philosopher sympathetic to a patchwork approach. Less skeptically, one might reasonably ask about the bounds to this complexity. Even if patchworks are to be tolerated, the open-ended nature of the processes that generate them (§§2.2-2.3) threaten to permit their complexity to become unruly. What, if anything, limits this?

Our aim here is to address these questions, albeit in a roundabout way. Direct defenses of the value of patchwork concepts are offered in many of the above-cited case studies. We focus instead on the ways in which patchwork concepts can *fail*, investigating the conditions that precipitate these failures. By investigating two cases where scientists themselves were sufficiently concerned to engage in conceptual housekeeping, we develop a taxonomy of three ways in which patchwork concepts can fail, which vary in kind and severity (§§3-4, §5.3).

From this taxonomy, we draw two general lessons (§5). The first lesson is that failure or unruliness is not intrinsic to patchwork concepts, but instead depends on the contexts in which various patches are used. Patches that might, in principle, cause issues requiring housekeeping efforts can coexist if suitably sequestered (this will be seen most clearly in §4). The second, related lesson is a kind of minimalist directive about conceptual housekeeping: do nothing until problems arise. Humans in general are quite adept at managing polysemy, i.e. words with multiple related meanings (Falkum 2015), and scientists are no exception. Even leaving aside the benefits of messy language (Wilson 2006), why insist on neatness where messiness is doing harm? We argue that this minimalist attitude is both manifested in our case studies and, from a normative vantage point, good policy.

## 2. Conceptual patchworks

### 2.1. Patchwork concepts

Conceptual patchwork structures consist of *patches* and their *relations* to each other. A patch is a scale-dependent, technique-involving, domain-specific and property-targeting way of using a word. These features can be formalized as follows:

$$P^{s} < t, d\,(\theta) >$$

Here, a patch $P$ is characterized by four parameters (Haueis forthcoming). Many scientific concepts characterize properties or behaviors of entities that occur at particular spatial, temporal, or energetic length scales (Bursten 2018; Wilson 2018, chap. 5); this is captured in the scale parameter $s$. Scientific concepts are also connected to particular measurement and modeling techniques (e.g., the use of kinetic gas theory equations to apply “temperature” to gases); this is captured by the technique parameter $t$. Particular uses of scientific concepts also apply within particular domains where they often pick out different properties. Thus, each patch specifies a class of entities (domain $d$) for which a property $\theta$ assigns members of that class to the extension of the concept. The scope and number of domains must be determined empirically (Novick 2018).

To illustrate how this general format works, consider how it applies to "temperature":

(1) *Temperature*$^{\mathit{Molecules}}$< *kinetic gas theory*, *gases* (mean kinetic energy) >

(2) *Temperature*$^{\mathit{Polymer}}$ < *restricted ensemble approach*, *solid materials* (frozen order) >

Patch (1) shows that when physicists apply "temperature" to the domain of gases, they use equations from kinetic gas theory to calculate the mean kinetic energy at the scale of molecules (Chapman, Cowling, and Burnett 1990, 37). Patch (2) reveals that when physicists apply "temperature" to the domain of solids, they use equations from statistical mechanics to calculate the metastable equilibrium of a restricted set of material configurations, such as the frozen order at the scale of polymer chains (Tadmor and Miller 2011, 554).

What unites these scale-dependent, technique-involving, domain-specific uses of a term into a single patchwork concept are the relations between patches. We consider four types of relation. First, novel patches of a concept are often created by extending a reasoning strategy to a novel domain. For example, the thermodynamic reasoning strategy for "temperature" instructs researchers to find the energetic equilibrium state of a system and calculate how increases in kinetic energy relate to decreases in degrees of freedom. Both kinetic gas theory and the restricted ensemble approach can realize this strategy. But extending the strategy from gases to solids shifts "temperature" from encoding information about the mean kinetic energy of molecules to the frozen order of polymer chains, generating a novel yet related patch. Second, researchers can *reuse* techniques from one patch to characterize properties described by another patch (Neto 2020). In the case of “temperature”, researchers can use mathematical techniques from statistical mechanics and general techniques of thermodynamic analysis (e.g., calculating thermal equilibria) in both patches (see also below, §3).

Third, the domains of distinct patches may overlap. This occurs when $\theta$ from $d_1$ or $\psi$ from $d_2$ can be used equivalently to assign entities in the overlap region to the extension of a concept. For example: under certain conditions, modeling techniques from statistical mechanics and continuum thermodynamics can be used equivalently to calculate the temperature of a heterogeneous medium, (Tadmor and Miller 2011, 83f.). Nonetheless each technique has a significant domain of systems to which it applies and the other does not.

Finally, researchers can sometimes draw on multiple patches to describe, classify, or explain the behavior of an entity. This occurs especially in cases where processes at different scales (and captured by different patches) interact to produce a phenomenon of interest. Adequately describing the thermal behavior of a steel bar, for example, requires combining descriptions of physical behaviors of the bar at different length scales (e.g., the rigid behavior of the molecular lattice structure and the elastic behavior of grain boundaries at a higher spatial scale).

Each of these types of relation may change over time. In the case of “sequence homology” (§4), for instance, we will see how once-healthy inter-patch relations became toxic (leading to an episode of conceptual housekeeping). In the following two subsections, we consider, respectively, how patchwork concepts arise, and the concerns relevant to assessing their legitimacy.

### *2.2. The open texture of patchwork concepts*

Patchwork concepts are polysemous, i.e., they have multiple related meanings (Haueis forthcoming). They are akin to inherently polysemous words in everyday language, such as "newspaper". "Newspaper" has multiple, related meanings: it can refer to an institution (e.g., *The New York Times*) or to a physical object (e.g., a paper copy thereof). However, it lacks a core meaning: none of the meanings is primary (Dölling 2020, 7). Similarly, the unity of patchwork concepts does not derive from a core meaning to which all of their uses are related, but rather from the network of local relations between patches (Wilson 2006).

Patchwork concepts arise in part due to the *open texture* of empirical concepts. Empirical concepts (e.g., "temperature") cannot be exhaustively defined: new and unforeseen circumstances may arise in which it is uncertain whether or not the concept applies (Waismann 1968, chap. 2; cf. Taylor and Vickers 2017). A definition might capture existing usage, but cannot fully anticipate how novel applications will reshape the term (Wilson 1982). Such applications may generate a patchwork structure.

For example, as noted above (§2.1), "temperature" captures different physical properties in its gases patch and its solids patch (Wilson 2018, 177). The temperature of a gas is the mean kinetic energy of its molecules, but mean kinetic energy is not informative about the thermal behavior of solids, whose molecules are not in molecular energetic equilibrium. In the solids patch, therefore, "temperature" refers to the frozen order of polymer chains. When, e.g., a rubber band is expanded, the frozen order restricts the ability of its polymer chains to wiggle freely, increasing its temperature. Neither referent is primary; each is informative in its particular patch.

The appropriateness of a novel extension is often indeterminate. This is distinct from the indeterminacy affecting the application of vague predicates (e.g., "is red") to borderline cases (e.g., a color patch intermediate between red and orange). With vague predicates, indeterminacy is a *permanent*, *predictable* feature of the terms, and is not "due to the ignorance of the facts" (Grice 1989, 177). By contrast, the indeterminacy of open-textured concepts is *unexpected* (Wilson 1982, n. 5) and *impermanent*. It arises where the use of a term is shaped by multiple factors that come apart in unforeseen ways. In the case of temperature, solids are both unlike gases (because not in molecular energetic equilibrium) and like gases (because similar reasoning strategies are applicable). Applying "temperature" to gases neither requires nor forbids its application to solids—an element of choice is involved. Subsequent research can then clarify the referent of the term in its new use. In extending "temperature" to solids, physicists did not initially know the physical correlate of "temperature" in solids. It was a significant discovery that the frozen order of polymer chains plays this role.

### *2.3. The open-ended normativity of patchwork concepts*

The open texture of empirical concepts allows them to acquire multiple related meanings. However, it also raises a worry: it seems there is no principled limit of extending a term to novel cases and adapting its meaning. Can open texture be pushed too far? For example, some economists have applied mathematical techniques from equilibrium thermodynamics to model supply and demand equilibria (Georgescu-Roegen 1971; Burley and Foster 1994). Can we therefore speak of the "temperature" of economic systems? What criteria determine whether this extension is appropriate or over the line?

Here, we draw on Joseph Rouse's *normative conception of practices* (Rouse 2007a; 2007b; Haueis and Slaby manuscript). This conception does not require determinate regularities or rules for judging particular performances of a practice (e.g., the extension of a concept to a new case). Instead, Rouse (2007b) focuses on the *issues* and *stakes* affected by such performances. Issues are problems that practitioners attempt to resolve as part of a practice, while stakes capture the wider significance of resolving an issue.

Issues are partially indeterminate: present practitioners can disagree on how to resolve them, and their normative status depends on future performances. For instance, different physicists may disagree about how to determine the temperature of a steel bar (*present disagreement*). The resolution of the issue (e.g., via introducing a novel mathematical technique for calculating energetic equilibria) may change what counts as a correct determination of temperature (*dependence on future performance*).

Determining the temperature of a steel bar matters to a range of other practices dealing with steel bars (e.g., engineering and materials science)—these are the stakes. Like issues, stakes are partially indeterminate due to both present disagreement (practitioners may disagree on the wider significance of their performances) and dependence on future performances. For example: a novel engineering project requiring heat-resistant steel bars might reconfigure the significance of particular responses to a given issue.

The appropriateness of extending a concept to novel cases depends on the issues and stakes affected by that extension. Extending “temperature” to steel bars (and developing techniques for measuring their temperature) resolved an issue that matters to practices that deal with the thermal behavior of steel bars. In judging the extension of temperature” to economic processes, we must likewise consider the issues it aims to resolve (e.g. describing the thermal effects of economic systems on their environment, by extracting natural resources and transforming it into waste and emissions) and how its attempted resolution affects practices that deal with the behavior of economic systems (e.g., how to tax waste and emissions to minimize environmental pollution). On this matter, the jury remains out (Baumgartner 2004; Burley and Foster 1994; Burness et al. 1980; Schwartzman 2008; Young 1991).

In the most interesting cases, such judgments are non-obvious: while it may be clear that we should not extend “rabbit” to apply to kangaroos, extending “temperature” to economic systems is a thornier matter, as evidenced by the decades of disagreement about its legitimacy. Furthermore, historical developments can change the appropriateness of particular extensions: at one time, applying “temperature” to economic systems may have seemed like applying “rabbit” to kangaroos, but developments in nonequilibrium thermodynamics and thermoeconomics have at least rendered it worthy of discussion. In this manner, conceptual housekeeping is contextual and historically contingent, which is why structure *alone* is insufficient to determine whether a conceptual patchwork is healthy.

With these considerations on the table, we turn to considering two ways in which patchwork concepts can break down (a third will be considered later; §5.3).

## 3. Failure 1: internal patch breakdown

The first kind of failure concerns a conceptual extension which violates one or more of the normative constraints on parameters of a patch: either an extension fails to resolve any issue, or its resolution of an issue is unsuitable for the relevant stakes (or both). To expand on this, a patch of a patchwork concept is normatively legitimate if the technique used to apply it produces *reliable* results, the domain to which it is applied is *homogenous*, and the properties to which it refers are *significant* to describe, classify or explain the behavior of entities in the concepts' extension (Haueis forthcoming; these will be described in more detail below). Reliability and homogeneity concern the potential of the concept to resolve the relevant issue; significance concerns how the resolution of an issue serves the relevant stakes. Extending a concept to novel cases can fail if the technique produces *unreliable* results, the domain contains a *heterogenous* set of entities, or the property it refers to fails to be significant for achieving descriptive, classificatory or explanatory goals associated with the concept. The result of all three types of failure is the *internal breakdown* of the problem patch.

The patchwork concept "cortical column" from neuroscience (Haueis 2020) illustrates this form of failure. This concept can refer to different kinds of *vertical* structures in the neocortex by describing a functional or structural property at a characteristic length scale. We can distinguish three patches of this concept:

*(1) "Hypercolumn"*$^{2\text{-}3mm}$ < *tangential recordings, V1, V2, MT* (sequence regularity) >
*(2) "Column"*$^{0.5mm}$ < *vertical/tangential recordings, primary sensory areas* (uniform responses) >
*(3) "Minicolumn"*$^{30\text{-}80\mu m}$ < *golgi stain, neocortex* (vertical connections > horizontal connections) >

At the microscale, “cortical column” picks out *minicolumns*, i.e. cell bands whose vertical, internal connections are stronger than its horizontal, external connections (Mountcastle 1997). Identifying this structural property involves anatomical staining techniques such as golgi staining. At the mesoscale, “cortical column” refers to circuits which all respond to the same sensory stimulus. Neurons in an orientation column, for example, all respond uniformly to a bar of a certain angle of orientation (Hubel and Wiesel 1977). At the macroscale, many columns form one *hypercolumn* if their uniform responses are *sequence regular*, i.e. they progress in an orderly manner across the cortex. Identifying these functional properties involves electrophysiological recording techniques. These scale-dependent and technique-involving uses of “cortical column” are domain-specific because each of the three properties are found in a restricted set of areas.

The patches of “cortical column” are related. The techniques listed in patches (1)-(3) all realize the same reasoning strategy, which instructs researchers to search for vertical structures with similar functional properties in the neocortex. In addition, patches (1) and (2) are related because they use the technique of tangential electrode recordings both to identify the boundaries of individual columns and to identify the sequence regularity of hypercolumns. Lastly, descriptions of scale-dependent properties can be combined in a multiscale model (Fig. 1) which describes the primary visual cortex composed of orientation minicolumns which are sequence regular at the hypercolumn scale and which orthogonally intersect with two ocular dominance columns.

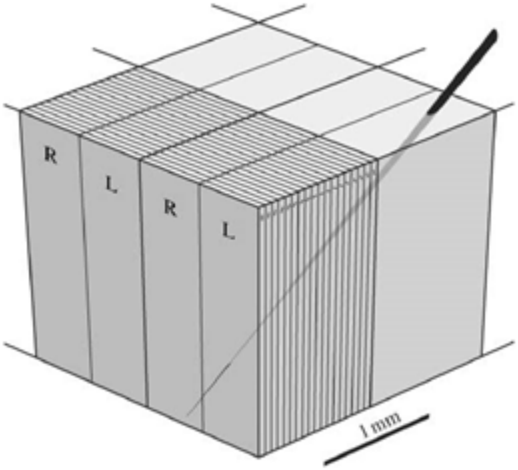

**Fig. 1:** The ice-cube model of primary visual cortex (adopted from Horton and Adams 2005).

The history of the column concept illustrates all three types of internal patch failure discussed at the beginning of the section. First, extending patch (1) from V1 to middle temporal area MT illustrates technique unreliability. A technique *t* is reliable if it can be used to detect the same property θ in entity $e_1$ and entity $e_2$. Consequently, *t* is unreliable if using it *permanently* and *systematically* fails to detect the same θ in $e_1$ and $e_2$. For example: Albright et al. (1984) tried to extend the column concept to MT by measuring how neurons in this area respond to the axis or direction in which stimuli move. The curvature of MT, however, prevented them to record uniform responses via vertical electrode recordings. This failure is *permanent* – it occurs repeatedly when using *t* – but it is not systematic, because curvature of MT is an intervening factor which prevents researchers from applying vertical electrode recordings. By contrast, when the researchers performed tangential recordings they found that sequence regularity in MT ranged from perfect to chaotic (ibid., Fig. 5). This failure is systematic because tangential recordings are not designed to deal with *noncolumnar* responses. Reliability failure is thus an issue which requires conceptual housekeeping: it requires researchers to restrict the *application range* of a technique (Wilson 1982). Cortical areas without sequence regular responses lie outside the proper application range of tangential recordings and thus, the hypercolumn patch of “cortical column”.

Second, domain heterogeneity occurred when researchers tried to extend “cortical column” patch (2) to the somatosensory cortex of rodents. A domain *d* of entities is homogenous if scientists can use the same property θ to assign each member in d to the extension of the concept. By contrast, a domain *d* is heterogenous if for some members of d, researchers use ψ but not θ to assign them to the concept’s extension. Recall that for patch (2), researchers use the property of functionally uniform responses to describe sensory areas as composed of columns which process stimulus features other than topography. In the 1970s, researchers argued to extend this domain to *cortical barrels* because they show vertical uniform responses, namely to tactile stimuli of the same whisker hair (Mountcastle 1978; Woolsey and Van der Loos 1970; see Haueis 2020). But although the techniques remained reliable, the extension fails the homogeneity constraint because uniform responses in barrels only represent sensory topography, and not the columnar organization of other stimulus parameters. Homogeneity failures are issues which require conceptual housekeeping because researchers would otherwise lump distinct phenomena under the same patch.

Third, extending patch (3) to all areas of the mammalian neocortex illustrates property insignificance. A property θ is likely significant if (S1) it is the property detected by reliable instances of *t*, (S2) it can be used to assign entities in *d c*oncept, and (S3) scientists can use θ to achieve their descriptive, classificatory or explanatory goals. Consequently, θ is likely insignificant if it fails to meet one or more of (S1)–(S3). There are both synchronic failures of significance (at time $t_1$, θ is significant in $d_1$ but not $d_2$) and diachronic failures of significance (θ is significant in $d_1$ at $t_1$ but insignificant at $t_2$).

The property *vertical connections* > *horizontal connections* from column patch (3) failed to be diachronically significant. Initially this property was evaluated to be significant: it was (S1) detected by reliable instances of the best anatomical techniques available, such as golgi and myelin staining. Consequently (S2), Mountcastle used *vertical connections* > *horizontal connections* to assign all members of the class *vertical cell band* to the extension of "cortical column". The property also contributed to descriptive and explanatory goals (S3). First, strong vertical connections allow minicolumns to process inputs independently of neighboring minicolumns. Thus, neuroscientists described minicolumns as computing the same *intrinsic function* which underlies sensory and higher cognitive functions across brain areas and species. Second, horizontal connections in V1 only connect orientation minicolumns with the same stimulus preference. Neuroscientists used this finding to explain how minicolumns implement orientation-selective responses in V1. Recent quantitative analyses, however, show that vertical connections are equal to or weaker than horizontal connections. This means that earlier anatomical techniques did not reliably detect the ratio between these connections (S1). Thus, *vertical connections* > *horizontal connections* cannot be used to assign vertical cell bands to the extension of "cortical column" (S2). Resolving these issues implies that *vertical connections* > *horizontal connections* can no longer serve the relevant stakes, namely to describe intrinsic function of minicolumns or explain orientation selectivity at the microscale. This shows that significance failures require conceptual housekeeping because they prompt researchers to realign their concepts with properties which contribute to the epistemic goals they pursue.

In sum, the column example shows that conceptual extensions can fail when the internal structure of a patch breaks down. There are multiple types of internal breakdown. Some are independent, e.g., failures of homogeneity can occur without failures of reliability. Other types are interconnected, e.g., failures of reliability or homogeneity can indicate failures of significance.

## 4. Failure 2: patch expulsion

### *4.1. The rise of "sequence homology"*

A second type of failure of a patchwork concept occurs when a particular patch, though internally in order, is connected to the rest of the patchwork in problematic ways. This can result in the patch being expelled from the patchwork. The "sequence homology" concept illustrates this second type of failure. While "sequence homology" maintained a healthy connection to the main "homology" patchwork for a time, this connection was eventually severed and the patch expelled from the patchwork.

"Sequence homology" refers to the degree to which two DNA (or protein) sequences are similar. For instance, two DNA sequences that are 70% similar (in terms of nucleotide sequence) can be referred to as "70% homologous", e.g.:

> They concluded that when 16S rRNA percent homology values are less than 97%, strains will not exceed 70% DNA–DNA similarity and thus it can be

> safely inferred that they do *not* belong to the same species. Yet, in the same paper, they concluded that having a 16S rRNA percent homology *greater* than 97% was *not* sufficient to conclude that two strains belonged to the same species. (Huss 2014, 396)

This is strange: "homology" generally applies to parts that are the *same* (due to common ancestry), even if they are *dissimilar* (Novick 2018). "sequence homology", by contrast, refers to a pure similarity relation, without regard to the source of that similarity. It is no surprise, then, to find biologists complaining that the usage is deviant and should be abolished (e.g., Fitch 2000). Less polemically, it may be regarded "merely as a homonym" (Griffiths 2006, 11; cf. Brigandt 2003).

The actual history, however, is more interesting than this quick summary suggests (Egel 2000). "Sequence homology" emerged in three major steps. First, "homology" was applied to chromosomes; second, it was recognized that chromosome homology could be partial; third, sequence similarity was recognized as the causal explanation of chromosome homology.

Walter Sutton (1902; 1903) was the first to speak of "homologous" chromosomes. At issue was the question: do chromosomes maintain their individuality during meiosis? At stake was the relationship between chromosomes and heredity. Sutton attempted to show that chromosomes do maintain their individuality, and argued that this allowed one to explain many otherwise puzzling features of Mendelian inheritance (Sutton 1903). Because they maintained their individuality, correspondence relations could be established between maternal and paternal chromosomes; Sutton (1902) dubbed this relation "homology".

Two features of Sutton's use of the term bear note. First, homologizing chromosomes was connected, from the start, to explanations of Mendelian patterns of heredity. Second, the evidence used to homologize particular chromosomes came from observation of chromosomal behavior during meiosis, as revealed by then-current imaging techniques. These features were specific to this particular patch of "homology", illustrating how terms adapt themselves to local features of new domains.

Under the influence of these patch-specific pressures, cytologists and geneticists in the 1920s and 1930s began to speak of 'partial homology' (e.g., Muller 1930; Creighton and McClintock 1931; Lawrence 1931; Dobzhansky 1934). Unequal crossing over generated partially homologous chromosomes: those that share homologous parts but are not homologous along their entire lengths. This development was tied to the goal of explaining Mendelian inheritance patterns. Not only could homology be identified by examining chromosomal behavior, it could also explain this behavior: pairing occurs between homologous genes.

With the discovery that chromosomes possess a linear sequence of base pairs, sequence similarity stepped into the explanatory role occupied by partial chromosomal homology (e.g., Comings and Okada 1970; see Greene 2016 for a recent review), and "sequence homology" was born (Neurath, Walsh, and Winter 1967). The following glossary entry (Britten 1967, 69, emphasis added) reveals the tight connection between these developments:

> **homology** — The degree of similarity between the nucleic acid sequences of

> different species, as in: *The homology between two species is measured by the capability of their DNA to form interspecies pairs at a given criterion* and by the thermal stability of the resulting pairs. Numerical specification is difficult or impossible because more than one parameter enters.

Within a few years, the caveat of the last sentence no longer held, as methods were developed to estimate percent sequence homology (e.g., Dutta and Ojha 1972; Harris and Teller 1973; Dedman, Gracy, and Harris 1974).

Thus far, "sequence homology" seems like a mere deviant usage, unconnected to genealogical patches of the term. However, identifying highly homologous *sequences* helped solve the issue of identifying (genealogically) homologous *genes* and *proteins* (de Ley 1968), and this played an important role in bacterial classification (Sapp 2009, chap. 10). Noteworthily, this use of "sequence homology" was put to explicitly phylogenetic ends in Carl Woese's pathbreaking work with 16S rRNA sequence data (e.g., Fox, Pechman, and Woese 1977; Woese and Fox 1977; Gupta, Lanter, and Woese 1983).

### *4.2. The fall of "sequence homology"*

Critics, however, saw this use of "homology" (rather than 'similarity' or 'identity') as dangerously confused. This came to a head with a brief letter published in *Cell* in 1987 (Reeck et al. 1987), though the basic criticisms were as old the usage itself. Why the twenty-year delay? Understanding this will throw light on scientists' efforts at conceptual housekeeping.

"Sequence homology" was scarcely born before it was criticized. After Neurath et al.

(1967) defined the term, Nolan and Margoliash (1968, 2) countered that, when genealogy was not strictly meant, "similarity" should be used, not "homology". Neurath and colleagues responded that this restriction rendered the term unusable in molecular biology (Winter, Walsh, and Neurath 1968). In molecular contexts, "homology" must conform to the experimental techniques and data streams at hand, and "cannot be allowed to become inflexible" (Winter, Walsh, and Neurath 1968). Winter et al.'s reply appears to have won the day, as the usage caught on.

This changed in 1987, when eleven eminent biologists published a brief letter in *Cell* critiquing "sequence homology" (Reeck et al. 1987). Their protest made it to the pages of *Science* (Lewin 1987; Kimelberg 1987), and soon began to be cited as the source of "current convention" (Brooks, Weir, and Schaffer 1988; cf. Mishler et al. 1988) and as a handy source for distinguishing true homology from mere similarity (Weir 1988; Bledsoe and Sheldon 1989; Goldsmith 1990; Petrella and Yokoyama 1990). *Cell* published three responses to Reeck et al. (Aboitiz 1987; Dover 1987; Wegnez 1987); among these, only Dover defended the embattled usage. Based on a Google Scholar search of papers citing Reeck et al.'s letter between 1987 and 1990, Dover's letter appears to be the only defense of the term in this period.

The core of the 1987 critique matched that of 1968: "homology" refers to common ancestry and should not be used to refer to mere similarity. However, Reeck et al. (1987) could further allege that "sequence homology" generated confusion (a matter of *stakes*):

> If using "homology" loosely did not interfere with our thinking about evolutionary relationships, the way in which we use the term would be a rather

> unimportant semantic issue. The fact is, however, that loose usage in sequence comparison papers often makes it difficult to know the author's intent and can lead to confusion for the reader (and even for the author).

In insisting that such housekeeping was worthwhile only because the co-existence of strict and loose senses of the term generated confusion, Reeck et al. endorsed the *minimalism* that we will discuss below (§5.1).

Reeck et al. identified three problematic scenarios, in increasing order of troublesomeness: (1) the use of "homology" while denying common ancestry, (2) the use of "homology" while not addressing common ancestry one way or the other, and (3) the use of similarities to "support a hypothesis of evolutionary homology". In the third case, they argued that, while similarity can be a "fully documented, simple fact", claims of common evolutionary origin remain hypothetical. By referring to mere similarity as "homology", the risk is that "we can deceive ourselves into thinking we have proved something substantial (evolutionary homology) when, in actuality, we have merely established a simple fact (similarity, mislabeled as homology)." They thus challenged whether "sequence homology" really resolved the issue of identifying homologous genes and proteins.

Dover's (1987) reply addressed this point directly: "Most sequence… similarity, statistically above the trivial level of coincidence, surely reflects homology in its true sense." Moreover, given then-current methods, "homology and similarity are operationally synonymous." This matched Winter et al.'s (1968) old defense of the usage, albeit mitigated by Dover's acknowledgment that sequence homology was not "true" homology. Regardless, Dover's defense did not catch on: the use of "sequence homology" declined, and even those who were willing to infer common ancestry from similarity were frequently content to restrict "homology" to the inferred ancestry (e.g., Green 1989; White and Wilson 1989).

Given the similarity of the arguments in 1968 and 1987, why the difference in outcome? Here it is impossible to give a definitive answer, but a few plausible (and compatible) hypotheses may be voiced. First, there are relevant sociological factors: the Reeck et al. letter included numerous prominent authors (e.g., Emile Zuckerkandl) and was published in a major venue (*Cell*, plus a summary in *Science*). Second, it is plausible that the letter itself marked the articulation of a growing discontent that stretched beyond its authors. This is suggested both by the sheer number of authors as well as by the swiftness with which the letter was taken to document "current convention".

Third, Dover's claim that, in the case of molecular sequences, 'similarity' and "homology" were "operationally synonymous" was overly simple. Already in 1970, Walter Fitch (1970), an author on the 1987 letter, had distinguished orthology from paralogy. While both are forms of true homology *at the gene level*, only the former could be used to construct phylogenies of *taxa*. Even if sequence similarity could reliably indicate orthology-*or*-paralogy, it could not disambiguate the two (Patterson 1988). In this light, "sequence homology" could not resolve the issue of identifying genealogical homology *in a manner suited to the relevant stakes.*

Furthermore, the same basic tests for disambiguating homology from non-homology applied to both morphology and molecules (Patterson 1988). This was a conceptual point, and early uses of "sequence homology" were tied to the practical inability to distinguish homology from analogy (Sapp 2009, 227). However, in the lead-up to the Reeck et al. letter, the situation was rapidly changing, with DNA sequencing appearing in 1977 and PCR in 1983 (Sapp 2009, 244). What was an acceptable resolution of the issue in 1968 became, in the eyes of many, an unacceptable resolution in 1987.

What was the resolution of this late-80s debate? Use of “sequence homology” declined, but did not vanish—indeed, Fitch (2000) complained about it again a decade later, and it persists to the present day (e.g., Grifoni et al. 2020). It is noteworthy, however, that Grifoni et al. use the term in a mechanistic, not phylogenetic context (the identification of immune response targets to SARS-CoV-2). In this sense, while the deviant usage persists, *it no longer exists as a patch of the main “homology” concept*: the expulsion has been successful. Meanwhile, there also exists the more proper use of “sequence homology” to refer to the *inferred* homology of sequences, not their similarity (Fujimoto et al. 2016).

### *4.3. A brief conclusion*

In the case of various extensions of “cortical column”, we saw how patches break down under internal pressures: the unreliability of techniques, the heterogeneity of domains, and/or the insignificance of properties. By contrast, “sequence homology” suffered from none of these problems. Sequence similarity is an important notion and requires *some* name. The problem lay in the deviant patch’s *relationship* to the remainder of the patchwork. Accordingly, the conceptual housekeeping in this case involved dissociating the deviant patch from the remainder of the patchwork, promoting replacement terms that would make the distance clear.

## 5. Two general lessons about conceptual housekeeping

### *5.1 Minimalism*

The first general lesson is that scientists (considered collectively; individuals will of course vary) are often reticent to make interventions into terminology, even when such interventions would increase precision and reduce polysemy. This can hold both (a) when a new usage is recognized as clearly distinct from the old (“sequence homology”) and even (b) when a patchwork concept is failing (“cortical column”). Philosophical discussions of scientific concepts sometimes assume that simplicity and precision are the default and complexity requires special justification. Minimalism can be understood as the inverse assumption: tolerance of conceptual complexity is the default, and active intervention requires special justification. We contend that minimalism is good policy.

In the case of “sequence homology”, the deviation of the new usage from the old was recognized and acknowledged by all parties from the start. Indeed, Winter et al. (1968) were open that their usage was novel and marked the adaptation of the term to a new domain. Some individuals, to be sure, took umbrage: in addition to Nolan and Margoliash (1968), cited above, we may add Fitch (1970), though it should be noted that Fitch focused on Winter et al.’s underlying assumptions; he did *not* contest their terminology directly. It was only later, *after* the new usage had started generating problems, that criticisms of the term itself took hold. And, even then, the authors of the *Cell* letter explicitly apologized for focusing on the use of words, defending themselves by citing the problems caused by “sequence homology”. Were it not for these problems, “the way in which we use the term would be a rather unimportant semantic issue" (Reeck et al. 1987). This is a clear statement of minimalism. Even in science, unambiguous terminology is not valuable for its own sake, and interventions into the uses of words requires special justification.

The point is further brought home by considering a closely related term: "homology arm". Homology arms are crucial to gene editing technology. Suppose you wish to replace a DNA sequence S in an organism with a new sequence S′. In the organism, S will be surrounded by flanking sequences $F_1$ and $F_2$. To insert S′, biologists design a sequence $F_1$-S′-$F_2$, allowing them to take advantage of homologous recombination. These designed flanking sequences are the "homology arms", and the "homology" in question is just sequence similarity. (Note also the relation to chromosome homology: the origin of the term follows the same path as "sequence homology".) What is important here is that 'homology arm'—a term that is deviant in *exactly* the same way as "sequence homology"—persists without any controversy. Why? Because it is sequestered: it applies only in gene editing contexts where the question of phylogeny *can't* arise, and thus the issues that motivated the authors of the *Cell* letter are a non-factor.

The case of "cortical column" goes further, showing that scientists may be reticent to intervene even where a patchwork is failing. For instance, despite partial failures of reliability, neuroscientists continued to apply the column concept and the ice-cube model to area MT (Haueis 2020). They also knew early on that cortical columns in mammals and cortical barrels in rodents may not form a homogeneous domain, but nonetheless subsumed barrels under "cortical column" (Haueis 2016). And they discovered strong horizontal connections between vertical cell bands but did not deem it as a challenge to the minicolumn patch. They also discovered microstructural details which questioned that ocular dominance columns had sharp boundaries, but used previously successful strategies to explain why these findings did not question the anatomical modularity of columns (Katz, Gilbert, and Wiesel 1989). Mountcastle, who introduced the column concept initially, also discovered that neural responses in unanesthesized animals have noncolumnar organization (Mountcastle et al. 1975), yet he continued to argue for two decades that "cortical column" refers to the basic functional unit in the neocortex (Mountcastle 1997).

At least in the two cases just discussed, minimalism appears to operative within the relevant scientific communities, but this leaves open the possibility that it is bad policy, and that scientists really ought to devote more energy to ensuring they keep their concepts precise and clear. We contend, however, that minimalism is good policy, for three reasons.

The first reason concerns the mechanism through which patchwork concepts acquire new meanings. New uses of terms often occur *silently*, appearing like business as usual (Wilson 1982). Scientists can follow established general reasoning strategies into new domains, as when Sutton extended morphological reasoning about corresponding individuated parts to chromosomes (§4.1), or when neuroscientists searched for vertical structures with similar functional responses in novel brain areas and species (§3). Intrinsic to this activity of extending established resources to novel domains is an element of unpredictability: it can't be known *in advance* how closely the new domain will match the old. What initially appears as "business as usual" might prove to be anything but. In this regard, conceptual complexity is just a natural result of exploring new and poorly understood areas: we use our established conceptual resources to get an initial grip on a domain, and, as we come to better understand that domain, we modify those resources to address the particularities of the domain (Rouse 2015, chaps. 9–10).

But, even if the emergence of such complexity is inevitable, this does not mean that it should be tolerated once recognized. This brings us to our second consideration in favor of minimalism: the open texture of patchwork concepts (§2.2), which implies that the apparent failure of a patch may be *impermanent* and resolvable by further research. For example: one can reasonably argue that reliability failures make it indeterminate whether "hypercolumn" is applicable to area MT. But one could also insist that better methods will eventually resolve the indeterminacy by producing reliable results for all members of the hypercolumn domain. In the case of "sequence homology", in 1967, it was difficult if not impossible to separate sequence homology from genealogical homology—only as such methods improved did the need to sharply disambiguate the two become pressing. Why insist on the sharp separation of an inoperable difference? In light of this, it is prudent to delay conceptual housekeeping until failure turns out to be persistent, *despite* methodological improvements ("hypercolumn"), or until conceptual differences become significant to practice ("sequence homology").

A third reason to favor minimalism about conceptual housekeeping is that, as linguists have noted, polysemy rarely poses a problem for successful communication, since ordinary speakers are generally able to decode the relevant meaning from a conversational context (Falkum 2015). Similarly, scientists typically do not face problems when using polysemous patchwork concepts because they are trained to use them in a technique-involving, scale-dependent and domain-specific manner. For example, in the case of MT, the success of using the columnar reasoning strategy made failures of reliability tolerable; modeling the functional architecture as columnar was permissible as long as one did not take certain features of the model literally (DeAngelis and Newsome 1999, 1418). Furthermore, horizontal connections did not threaten the significance of vertical connections because researchers had strategies to explain how these connections fit into the received view of columnar architecture (Haueis 2020, sec. 2.3). By contrast, in the case of "sequence homology", the co-presence of both genealogical and non-genealogical use of "homology" *in contexts where the difference both (a) mattered and (b) was difficult to discern without explicit specification* created a situation where polysemy was difficult to manage. Under those circumstances, conceptual housekeeping reasonably came to seem an important activity. So long as researchers have empirical or methodological means to deal with the issues raised by polysemy, however, conceptual housekeeping is otiose.

Philosophers who favor eliminativism often point to threats of miscommunication or confusion to motivate more rigorous conceptual housekeeping in the face of polysemy (Ereshefsky 1992; Machery 2009; Taylor and Vickers 2017; Mariscal and Doolittle 2020). What our analysis here shows—and what patchwork analyses more generally show—is that these threats may be bigger for non-specialists or philosophers using patchwork concepts than for practitioners themselves (Haueis 2020; forthcoming). The general ability of competent speakers to tolerate polysemy is thus a point in favor of scientists' pragmatic attitude towards housekeeping of conceptual patchworks.

### *5.2 Contextuality of conceptual housekeeping*

The second general lesson is that conceptual housekeeping is thoroughly dependent on historical context, and thus that conceptual structure alone is insufficient to decide whether or not a particular extension of a concept is appropriate. The open-ended normativity of patchwork concepts (§2.3) is helpful to analyse the contextual factors involved in conceptual housekeeping.

Consider the case of extending "cortical column" to barrels in the rodent somatosensory cortex. Prominent researchers such as Hubel and Wiesel already noted that the extension would fail the homogeneity constraint: isomorphic representations of the sensory periphery are not themselves columnar systems because columnar systems map *additional* features besides sensory field position (e.g., orientation, ocular dominance) onto the cortical surface (cf. Haueis 2016, 22). Applying the notions of "issues" and "stakes" to this case explains why other neuroscientists subsumed barrels under the column concept despite this criticism. The issue in this case was whether rodent primary somatosensory cortex exhibits a columnar architecture, which was partially indeterminate because researchers disagreed whether to count isomorphic representations as columns (Mountcastle said 'yes', whereas Hubel and Wiesel said 'no'). What was at stake in the extension was whether column concept described a species-invariant building block of the neocortex with discrete anatomical boundaries, rather than just a form of functional organization of some areas in particular species. These stakes were relevant to a number of disciplines dealing with cortical architecture, such as developmental neuroscience and neuroanatomy. If the cortex was made of the same building blocks everywhere, cortical organization would be much easier to understand and knowledge about it would be easily transferable between different subdisciplines which use the column concept as a common framework. The discovery that rodent somatosensory cortex contained anatomically discrete vertical structures with uniform functional responses spoke in favor of this 'building block picture' of the column concept (Haueis 2020). Given these stakes, it was reasonable to initially resolve the issue in favor of including barrels. By contrast, when later researchers argued to exclude barrels from the column concept (Horton and Adams 2005), the tide had shifted against the building block picture, in part because of the reliability and significance failures in other parts of the column patchwork. What researchers formerly saw as a justified extension of the column concept now appeared as an unmotivated bend to a different phenomenon. These historically variable judgments underline that the normativity of patchwork concepts is open-ended, and that conceptual housekeeping of their extension is thoroughly contextual.

In the case of "sequence homology", the same point holds. As noted, the deviation of "sequence homology" from standard uses of "homology" was apparent to all (defenders and critics alike) from the start. It was a change in context that rendered this deviation sufficiently problematic to warrant its elimination. This change in context affected both issues and stakes. Improving techniques for identifying genealogical homology of genes and proteins, and for distinguishing different types of homology (orthology, paralogy, and xenology; Fitch 1970; Gray and Fitch 1983) reduced simple inferences from high sequence similarity to genealogical homology to the status of an overly crude resolution of the issue. This crudity affected the broader stakes for which identifying homologous molecules mattered, namely, the reconstruction of organismal phylogeny (Reeck et al. 1987). The structure of the "homology" patchwork did not change between 1967 and 1987; the context did.

### *5.3. Global collapse*

In the cases of "cortical column" and "sequence homology", we encountered cases of local breakdown: a particular patch of a patchwork either collapsed in on itself (various extensions of "cortical column") or was expelled from the patchwork but permitted to retain an independent existence under a new name ("sequence homology"). But there is a third possibility: a patchwork may collapse globally. We expect such cases to be rare, though not impossible. Lacking space for a third case study, we here restrict ourselves to a few brief, speculative comments.

In keeping with the two forms of local collapse, we might identify two forms of global collapse as well: global implosion and global fragmentation. In global implosion, each patch of a patchwork fails in itself—as would happen if every patch of "cortical column" suffered the same fate as the various problematic extensions. In global fragmentation, each individual patch is preserved, but the connections between them are severed. Glibly, the former is a collapse of content, the latter of form. Lankester's (1870) failed attempt to eliminate "homology" from post-Darwinian morphology is an example housekeeping aimed at internal implosion; as is the criticism of nearly every use of "cortical column" by Horton and Adams (2005), which other neuroscientists called a "premature" announcement of the "death" of the column (da Costa and Martin 2010, 3). By contrast, defenses of eliminative pluralism (Ereshefsky 1992; Machery 2009) are examples of housekeeping aimed at global fragmentation. So conceived, it is obvious that hybrid forms of global collapse are possible, in which some patches implode while others fragment. Indeed, where global collapse *does* occur, it may well primarily occur in a hybrid manner.

Why should global fragmentation be rare? There are a few related reasons. The first ties back to our discussion of minimalism (§5.1). Given that people are actually rather adept at managing conceptual complexity, resistance to top-down change is reasonable. While calls to fragment "species" (Ereshefsky 1992) and "concept" (Machery 2009) have found some support among scientists, others have stressed the productive interactions between different patches (Machery 2010, open peer commentary; Novick and Doolittle 2021 and references therein). Compounding this is a second reason: there are simply no set conditions determining when it is appropriate to split a patchwork and when it is not. Insisting on a strict link between concepts and single operations, as in extreme forms of Bridgman's operationalism (see Chang 2019), would allow for this, but at a high cost. Failing that, we are left with the task of managing complex, difficult to assess trade-offs, and it is no surprise that conservatism tends to hold.

What about global implosion? Here it is useful to recall how patchworks commonly form (§2): a concept is applied to multiple domains, whether all at once or sequentially, then adapts to local features of those domains (often with the help of domain-specific techniques), generating distinct patches of use. While it is possible for a concept to fail to gain purchase in some one or other domain to which it is applied (as occurred with “cortical column”), the existence of a patchwork generally implies that adaptation has been reasonably successful within some domains The column concept allowed neuroscientists to formulate a set of generalizations about brain architecture and sensory processing which remain true even though researchers gave up on stronger commitments like a common building block in all cortical area (Haueis 2020). The total implosion of all patches may well require the sort of radical theory change that dispenses with a whole body of terms and the properties they describe. Thus, for instance, if one could make the case that ‘phlogiston’ was a patchwork, it might be a candidate case of global implosion.

One final reason applies to both global implosion and global fragmentation. Patchwork concepts tend to circulate among multiple distinct communities and subdisciplines. Conceptual housekeeping is difficult enough within one subdiscipline; how much more when it must be coordinated among many!

Thus, while certainly not impossible, we expect the global collapse of patchwork concepts to be a rare event. Yet the reasons we have offered here are suggestive only, and the issue requires further study.

## 6. Conclusion

Philosophers who work on complex scientific concepts can be roughly classified into a few camps. There are those who seek to find the single correct analysis that subsumes all legitimate uses of a term (e.g., De Queiroz 2007). Then there are those who recognize distinct legitimate uses that cannot be so unified, and recommend sharply disambiguating them, dispensing with the overarching concept on the grounds that it gives a false sense of unity and encourages equivocation (e.g., Ereshefsky 1992; Machery 2009). There are pluralists, who also recognize distinct legitimate uses, but see active benefits in this plurality (e.g., Jamniczky 2005; Neto 2019). And then there are those—such as the present authors—who see such complexity as being structured into patchworks: patches of use tied together by local connections, but not admitting of any global, overarching unity. (One reason this categorization is rough is that individuals can—and most will—adopt different approaches to different concepts.) The first two may be grouped (again, roughly) as foes of conceptual complexity; the latter two as complexity's friends.

The foes appear, on first glance, to have the high ground: complexity implies polysemy, which suggests imprecision, ambiguity, and the risk of equivocation and other confusions. Let everyday language go to the dogs if it will, but surely it would be best if *scientific* language were precise and simple. And the friends are accordingly on the defensive, searching for benefits of complexity that justify tolerating it (e.g., Neto 2020). This defensive posture has certainly had some good effects, as friends have uncovered various ways in which conceptual complexity may be a boon to scientific research.

If we are right, however, this defensive posture is unnecessary. To the question, "why tolerate complexity?", the answer "why *not*?" is sufficient. Through our analysis of the partial failures of two patchwork concepts, we have tried to show that it is actually the *elimination* of complexity that requires special justification, not the tolerance thereof. Complex conceptual structure alone is not sufficient to warrant conceptual housekeeping: context—what is at issue and what is at stake—matters.

Among philosophers of science are so many unwitting followers of Xunzi, insisting that names have a fixed standard, that they be direct and easy, that they not drift. Against this, we offer this nugget from the *Zhuangzi* (Ziporyn 2020, 13):

> "But human speech is not just a blowing of air. Speech has something *of which* it speaks, something it refers to."
>
> Yes, but what it refers to is peculiarly unfixed.

Very much so. And this is a very good thing indeed.

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