these criticisms are marginal. Readers seeking insight in the modern development of thinking about nature – and about philosophical reflection inside physics – can gain much from working with this book.

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Margaret Morrison, Critical Discussion of *Unifying Scientific Theories. Physical Concepts and Mathematical Structures*, Cambridge University Press, Cambridge, 2000; 272 pages, with index; ISBN: 0-521-65216-2.

1. SUMMARY

Recently the topics of *unity* and *unification* have attracted the attention of philosophers of science. As far as I am aware, Margaret Morrison (Professor of Philosophy at the University of Toronto) is the first to have written an entire book about these two related topics. She finds "some of the arguments" in favour of a generic disunity in science, notably when we look at scientific practice, "extremely persuasive" (p. 1), although she does not share "some of these arguments" with us. She however believes it to be a mistake to deny that "science has produced unified theories", for she takes this to be a matter of established historical fact. She does not want to engage in a fruitless debate whether there is unity or disunity in science by "trying to counter examples of disunity with ones of unity". Her book has two goals.

She first wants to show that "once we have some understanding of (1) how unity is produced, (2) its implications for a metaphysics of nature, and (3) its role in theory construction and confirmation, it will cease to occupy the undesirable role attributed to it by the advocates of disunity" (Goal 1. p. 1) – she leaves "the undesirable role" unspecified, so it is a mystery how to assess whether Goal 1 will have been reached. She has found no common denominator for the instances of unity and unification in the case-studies she has undertaken and she therefore renders a general philosophical theory of unification to be "impossible" (p. 1, 232). Besides to provide "some understanding" of unity and unification, Morrison's other goal is to defend the two-fold thesis that "unification should not be understood as a form of explanatory power"; and that unification has "in some instances very few, if any, implications for a reductionist metaphysics and an ontological unity of nature" (Goal 2, p. 5).

The body of the book consists of five historical case-studies of unity and unification in science which are jointly intended to achieve Goal 1: three about physics (classical electrodynamics, quantum electro-weak dynamics and the special theory of relativity), and two about biology (the theory of evolution and genetic population statistics). These case-studies cover together five of the seven chapters and almost 70% of the text. The two opening chapters and a small closing section with conclusions cover the other 30%. In the first, introductory chapter Morrison expounds "the many faces of unity in the history of science and philosophy": Kepler's mathematico-metaphysical unity, Kant's methodological unity, Whewell's consilience of inductions leading to certainty and the logical-positivist's idea of the unity of science. In the second, critical chapter Morrison argues against other philosophers of science, *viz.* M. Friedman, P. Kitcher and C. Glymour, who have argued in favour of a connexion between unification, explanation and even realism. Here, but also in the chapters of the case-studies, she intends to achieve Goal 2.

2. CASE-STUDIES

The case-study chapters of unifications are mainly expository and summarise Morrison's reading of the work of physicists, biologists and their historians with a keen eye for the topics of unity and unification; they provide informative reading for philosophers and natural scientists alike who know little about the historical development of the theories under consideration. (The prominent presence of biology makes the subtitle of the book odd.) For this reason alone, no philosopher of science interested in the topics of unity and unification can afford to leave this book closed. If anyone ever doubted that unifications have taken place in the history of science, here is material to demolish those doubts. She also declares there is nothing but disunity in these unifications.

In these case-study chapters Morrison presupposes a thorough familiarity with the theories under consideration, e.g., Chapter 4 on quantum electro-weak unification will be incomprehensible in the most literal sense of the word for those who have not taken a course in elementary particle physics or applied quantum field theory – the fact that in a footnote she writes down the definition of a group (p. 249, footnote 13) shows that Morrison has a subtle sense of humour. This brings me however to the first criticism of the book: for an initiated audience the expository parts could and should have been shorter. A crispy summary of the essentials would have sufficed; many of the details that Morrison provides about the historical development seem not relevant in comparison to the meager philosophical yield that Morrison is able to harvest from her extensive sowing-expeditions. I shall shortly provide support for this criticism, mainly on the basis of Chapter 3 on 'Maxwell's Electro-magnetism and Optics', by focussing on the five conclusions she draws from this case-study (next Section).

The second criticism I would advance concerns Morrison's most philosophical chapter, which is about unification, realism and inference (chapter 2). Several of the criticisms she levels against Friedman's concept of explanation I found unconvincing. The reasons why are expounded below, in Section 4. This review closes with some conclusions (Section 5) and an Appendix with errors (Section 6).

3. MAXWELL'S UNIFICATION

After a journey through Maxwell's famous papers 'On Faraday's Lines of Force' (1856), 'On Physical Lines of Force' (1861–1862), 'A Dynamical Theory of the Electro-magnetic Field' (1865) and a look at his *Treatise on Electricity and Magnetism* (1873), Morrison arrives at Section 3.4.1 (which perhaps should have been Section 3.5), called 'Philosophical Conclusions'. First she concludes that her case study "at the very least shows that theory unification is a rather complex process that integrates mathematical techniques and broad-ranging physical principles" (p. 105). Well, that in physics mathematics and physical principles are integrated, is that supposed to be a 'philosophical' conclusion, let alone a surprising one?

After several such remarks, which I would classify – with all due respect – as textbook wisdom, Morrison makes an interesting distinction between *reductive unification* and *synthetic unification*. Reductive unification is ontological in nature and obtains when a theory shows that two (or more) distinct classes of phenomena can be considered to belong to the same *natural kind* of phenomena (whether Morrison, who elsewhere displays empiricist sympathies, wants to commit herself to the existence of natural kinds, a typically realist delight, remains in the dark). Synthetic unification obtains when two (or more) distinct classes of phenomena are described by in a single theoretical framework. It seems that reductive implies synthetic unity but not conversely. I call attention to the surprising fact, that unification is in neither of these two senses a triadic inter-theoretical relation, as in 'theory $\mathbf{T}$ unifies theories $\mathbf{T}_1$ and $\mathbf{T}_2$'. When Maxwell showed in 'On Physical Lines of Force' that the electromagnetic aether and the luminiferous (= optical) aether were one and the same mechanical aether, he achieved both types of unification. Morrison claims that Maxwell's more abstract theory of the electro-magnetic field of the *Treatise*, wherein the aether was pushed out of the limelight and did no longer occur in what we now call 'the Maxwell equations', also achieved both types of unification (p. 107). But then she goes on to argue that "we cannot assume that Maxwell's electro-magnetic field reduces electricity and magnetism to one force" (ibid.), because both the electric and the magnetic field occur as separate vectors **E** and **B** in Maxwell's equations. These field equations merely relate **E** and **B** to each other, just as Newton's law of motion $\mathbf{F} = m\mathbf{a}$, say, relates force **F** to acceleration **a**, but the field equations do not show that **E** and **B** belong to the same natural kind – baptising $\langle \mathbf{E}, \mathbf{B} \rangle$ the 'electro-magnetic field' would only be a 'verbal move', without any theoretical substance. Morrison: "Viewed this way, one calls into question the traditional relationship between unified theories and theoretical reduction. The former need not, even in the relatively straightforward case of Maxwell's theory, imply the latter." This conclusion has left me puzzled, because earlier she claimed that Maxwell's theory achieved both kinds of unification: synthetic and reductive, whereas now she claims that Maxwell's theory illustrates that synthetic unification does not imply reductive unification.

The reductive unification of **E** and **B** was achieved by Einstein in 1905 and mathematically codified by Minkowski in 1908, when he merged space and time into a "synthetical unity" we now call *Minkowski spacetime* $\mathcal{M}$, and introduced the electro-magnetic field tensor $F^{\mu\nu} : \mathcal{M} \to \mathbb{R}^{16}$. That we now can truly speak of a reductive unification is manifest in the fact that whether electric or magnetic components appear in $F^{\mu\nu}$ has become an inertial-frame dependent phenomenon (p. 190). This is one of the conclusions of Morrison's in the beautiful Chapter 5 on the special theory of relativity (a natural sequel to Chapter 3 but placed behind the chapter on quantum electro-weak dynamics). We turn back to Chapter 3.

Morrison says that unification is not sufficient for theory-acceptance and mentions that only after Hertz's experiments of 1887, confirming the existence of free electro-magnetic waves, Maxwell's theory was accepted. But did anyone believe that unification is sufficient for theory-acceptance? Supersymmetric unification theories of the strong and the electro-weak interaction are almost as old as the standard model of elementary particles itself (of the 1970s), but none of them belongs to the accepted body of physical knowledge. Why? Because there is not a whiff of independent empirical evidence to support any of them. We need not delve into the past for a conclusion that everyone already subscribes to.

I have always understood Friedman (against whom Morrison directs this conclusion about theory-acceptance) as saying that unification plays a role in theory-acceptance along the following lines. Suppose we have two theories, $\mathbf{T}$ and $\mathbf{T}'$ say, that describe some phenomenon, call it $\mathcal{D}$, and describe also all phenomena covered jointly by theories $\mathbf{T}_1$ and $\mathbf{T}_2$ (assuming that not one of them describes $\mathcal{D}$); suppose further that $\mathbf{T}$ unifies $\mathbf{T}_1$ and $\mathbf{T}_2$, and that $\mathbf{T}'$ does not unify them. Then the scientific community will accept $\mathbf{T}$ rather than $\mathbf{T}'$ all other things being equal. Not one of the case-studies of Morrison's speaks against this, including Maxwell's theory, which simply had no rival theory in this sense. So it seems that unification, although *of course* insufficient for theory-acceptance, is a scientific virtue that can play a role in theory-acceptance.

Another point of Morrison's is that Maxwell's unified theory was not *explanatory*; this observation is then supposed to make trouble for Friedman's idea that unification is a provider of explanations. Does it make such trouble? Not if one *defines* explanation in terms of reductive unification, as Friedman does. But then what is Morrison driving at? She holds that Maxwell's theory does make trouble for Friedman, because Maxwell did never embrace some specific aether theory and apparently a *mechanical* explanation based on the aether is the only explanatory story to be had for Morrison. Although Maxwell believed in the existence of an aether in order to explain the propagation of light mechanically and to sustain electric and magnetic fields mechanically, he held there was insufficient evidence to decide which of the mechanical aether theories was correct; therefore he suspended his judgment and by implication he had no basis for a mechanical explanation. In a nut-shell, I can only see a disagreement as a consequence of an equivocation about explanation: unifying vs. mechanical.

There also was insufficient evidence to choose between the different theories about the luminiferous aether and theories about the electric aether in the pre-Maxwellian age. Maxwell claimed that a single mechanical aether was sufficient for his unified theory of electromagnetism (reductive unification), but inherited the problem of how to choose from the theories of electric aether and of the luminiferous aether: the differences between the various theories of the mechanical aether were too beyond empirical reach – this changed however with the Michelson-Morely experiment, which ruled out empirically theories according to which the Earth is at rest with respect to the aether. The last-mentioned fact is the reason why Morrison rejects Maxwell's theory as explanatory – there were in fact too many explanations. But on that account, the earlier, separate theories about the aether were not explanatory either. All explanations of the same phenomenon that involve unobservables are experimentally indistinguishable. If that is unacceptable for Morrison, then few theories would qualify as explanatory (think of the Quine–Duhem thesis). It raises the question rather urgently what a theory must look like in order to be explanatory according to Morrison. In the Conclusions of the book she says on explanation: “I have remained deliberately silent on the subject primarily because it is, in itself, the topic of a separate book and one that many philosophers of science have already addressed in detail” (p. 236). This reminded me of a remark in a book review of Pauli’s about the book on matrix mechanics by Max Born and Pascual Jordan who said the reviewed book belongs to a series wherein the contents of book n will be explained in book $n + 1$.

Remaining silent on the topic of explanation while continuously condemning theories for their lack of explanatory power is not the hall mark of cogency. Anyway, it seems unreasonable to demand that a theory must be explanatory in order to be unifying. Nonetheless Morrison holds that Friedman holds this. But all that Friedman holds is the converse, namely that explanation involves *reduction* in the technical sense as he has defined it (which is not necessarily identical to Morrison’s ‘reductive unification’). Morrison does not go into these technical details. Thus not only do Friedman and Morrison have different notions of what it is for a theory to be explanatory, but Morrison seems to have an unreasonably strong yet unexplained notion of explanation.

Finally Morrison addresses the inference from a unified theory to some ontological unity of nature. She does not find it “irrational to put faith in” the unified theory, even if there is no evidential support in favour of it (for Maxwell’s theory this came with Hertz’s discovery), but she is “simply cautioning against seeing a successful phenomenological theory as evidence for an ontological interpretation of theoretical parameters” (p. 108). Again, with all due respect, empiricist philosophers from centuries past have been cautioning us for such inferences. But why should we be cautious if it is rational not to be cautious? Or should we behave irrationally? Moreover, about twenty years ago B. C. van Fraassen has driven it home safely that abductive inferences in science (from observables to unobservables, from observed explanandum to unobserved explanans) are not necessarily sound (in the model-theoretic sense of transmitting truth), in contrast to deductive inferences – this is, to emphasise, not to say that it is irrational to make such inferences, only that such inferences are not necessarily sound. Frequently we have to recognise that we have accepted an inference to a false explanation. Think for example of inferring the existence of an aether from a variety of optical phenomena. Do we really need a forty-page exposition of Maxwell's intellectual development to hear *this* cautioning remark once more?

To repeat, the philosophical yield of Morrison's case studies is meager.

4. UNIFICATION, REALISM AND INFERENCE

M. Friedman requires that a philosophical theory of scientific explanation makes it an objective notion, i.e., "explanations should not depend on the idiosyncrasies and changing tastes of scientists and historical periods", as Morrison puts it (p. 25). She then claims that mechanical explanations, conceived by physicists as genuinely explanatory, violate Friedman's requirement because the very fact they are *mechanical* means they depend on what was once fashionable: mechanical theories. This is awkward. During any historical epoch every explanation will always be framed in the then-accepted, relevant theory. If *that* counts as 'taste' and 'fashion', then every single accepted theory counts as 'taste' and 'fashion'. Such an all-encompassing notion of 'taste' and 'fashion' not only makes the notion uninteresting, if not spurious, but it is very likely not what Friednian had in mind when talking about "idiosyncrasies and changing tastes". Equivocation revisited.

On pages 39–40, Morrison discusses Friedman's notion of theory-reduction. She proceeds abstractly and we tread in her footsteps. We notice it is a missed opportunity for Morrison *not* to have illustrated these abstract notions by means of here case-studies. On top of p. 40 comes a mathematical expression that is supposed to be an inference facilitated by "a reduction". She does not say what is being reduced to what. The mathematical expression says the following (below I have changed Morrison's cumbersome notation into a more transparent one).

Let $\mathcal{T} = \langle T, T_1, \ldots, T_n \rangle$ be a 'theoretical structure', where T is some set and T_j some relation on T; $\mathcal{T}$ is a 'model' from the class Δ of models that characterises the theory under consideration, **T** say. Let $\mathcal{D} = \langle D, D_1, \ldots, D_m \rangle$ be a 'data structure' ($m \leqslant n$). The Suppes–Van Fraassen construal of 'theory **T** saves phenomenon $\mathcal{D}$' is to embed $\mathcal{D}$ into some $\mathcal{T} \in \Delta$. Friedman wants to consider the identity only – for reasons that escape me, because in Suppe's Measurement Theory any structure $\mathcal{D}'$ isomorphic to $\mathcal{D}$ will do an equally good job in representing exactly the same experimental data that $\mathcal{D}$ represents, so that for every data structure embeddable in $\mathcal{T}$ *not* by the identity, there is another one representing the experimental data equally well that is embeddable into $\mathcal{T}$ by means of the identity. I leave this now. The mentioned inference reads that for "reduction" we must have that restriction $T_{1|D} = D_1$, from $\langle D, D_1 \rangle$, for

$\mathcal{T} \in \Delta_1$ and restriction $T_{2|D} = D_2$, from $\langle D, D_2 \rangle$, for $\mathcal{T} \in \Delta_2$ together imply that $T_{1|D} = D_1$ and $T_{2|D} = D_2$ and $\mathcal{T} \in \Delta_1 \cap \Delta_2$ from $\langle D, D_1, D_2 \rangle$ (presumably with an overall existential quantification over $\mathcal{T}$). Evidently this always holds and I therefore fail to see how it can be an interesting requirement.

Next Morrison considers the weaker demand of embeddability, where the embeddability mapping need not be the identity. This should then license a similar inference: $\exists \mathcal{T} \in \Delta_1$, where $\mathcal{T} = \langle T, T_1, \ldots T_n \rangle$, such that $T_{1|D} \cong D_1$ (isomorphic) and $\exists \mathcal{T}' \in \Delta_2$, where $\mathcal{T}' = \langle T', T_1', \ldots T_n' \rangle$, such that $T_2'|_D \cong D_2$ together imply that $\exists \mathcal{T}'' \in \Delta_1 \cap \Delta_2$ such that $T_1''|_D \cong D_1$ and $T_2''|_D \cong D_2$. Morrison calls the embeddability mappings $\phi : D \to T$ (preserving D_1 into T_1) and $\psi : D \to T'$ (preserving D_1 into T_1'). She then comments: "This latter inference is invalid, because $\mathcal{T}$ and $\mathcal{T}'$ are different models, but even if they were the same, we would require some guarantee that the mappings ϕ and ψ had the [embeddability] mapping χ [from D to T''] in common" (p. 40).

Well, let us then consider the case $\mathcal{T} = \mathcal{T}'$. If $\phi[D] \cap \psi[D] = \varnothing$, then $\chi = \phi \cup \psi$ qualifies to make the inference valid. If $\psi[D] \cap \psi[D] \neq \varnothing$, then we can still choose ϕ or ψ as long as we end up outside the intersection $\phi[D] \cap \psi[D]$ pending on whether we come in either $\psi[D]$ or $\psi[D]$, respectively, and for members of D that are sent by ϕ and ψ to $\phi[D] \cap \psi[D]$, we can choose for χ the mapping $f \circ \psi^{-1} \circ \phi$, where $f = \phi$ for those pairs $x, y \in D$ such that xD_1y, and $f = \psi$ for those pairs $x, y \in D$ such that xD_2y. This is always possible when the relations D_1 and D_2 are of a different sort (one binary and the other ternary, for example); this is not possible whenever the relations D_1 and D_2 are both *total* orderings of same sort, but if they are not, it might be possible; we then might try a composition with a relation-preserving permutation on D_1, etc. This all goes to show that little can be said in general; much hinges on what the classes of models Δ_1 and Δ_2 contain. Morrison's claim that the inference is always invalid is not correct.

Furthermore, the fact that the validity of the inference depends on the theories $\mathbf{T}_1$ and $\mathbf{T}_2$ is *good*, because if the inference were always valid, requiring it to be valid would pose no restrictions on the notion of 'reducibility' and hence would turn it into a spurious notion; and if the inference were always invalid, it would become an impossible notion.

On p. 39, Morrison claims there is a "difficulty" with regarding embeddability as the proper relation between theory and phenomena, because we might have two embeddings, say $\phi : D \to T$ and $\psi : D \to T$ such that for some T_j in $\mathcal{T}$, $j > m$, ϕ or ψ will not preserve T_j. Suppose T_j is a binary relation; we then might have that for some $x, y \in D$:

$\phi(x)T_j\phi(y)$ but that *not* $\psi(x)T_j\psi(y)$. Well, so what? If the embedding is not an isomorphism between $\mathcal{T}$ and $\mathcal{D}$, then what? It was never supposed to be one: the relations between the measurements in the data structure $\mathcal{D}$ were supposed to be isomorphic to relations in the theoretical structure $\mathcal{T}$. That *suffices* to render the relation with theory and phenomenon precise.

In light of the remarks above, Morrison's judgment that "the apparatus of formal model theory is simply too rigid to capture the rather messy relations that are part of the modeling of scientific phenomena" (p. 52) carries little weight, quite apart from the widespread confusion between vagueness and generality which underlies such, in some circles popular, but ill-founded judgments.

On pages 44–45, Morrison levels a peculiar argument against taking mathematical structures as objects of "literally true identification" with physical systems 'out there' in the world, as Friedman would have it: "if we literally identify the [kinetic] energy of a particular molecule with the mathematical representation $E = m(v_x^2+v_y^2+v_z^2)/2$, then there are aspects of the latter that seem to lack any materially isolatable counterparts having physical significance". Peculiar here is the unwarranted identification between physical subsystem, or "materially isolatable counterpart", and the velocity-component pertaining to the composite system, which ought to have been regarded as corresponding to a 'sub-property' of the property of velocity v of the whole molecule.

To round up, I am not saying that Friedman has spoken the last words on explanation and reduction, because Morrison also levels good arguments against Friedman c.s., e.g., her argument related to what Van der Waals law accomplishes (pp. 47–51) and her criticisms of Kitcher's theory of explanation by means of the theory of evolution (Chapter 6), but I am saying that not all her criticisms are cogent.

With regard to arguing in favour of the thesis that unification does "*not* in all instances" have implications for a "reductionist metaphysics and an ontological unity of nature" (Goal 2), I resubmit that anno 2001 this is not exactly a surprising thesis. The interest of philosophers of science goes, or should go, in the first place to the instances where there are such implications; and then still conditional on being a realist of sorts, because an anti-realist who believes that science has little metaphysical and less ontological implications anyway, will not change his mind after witnessing that certain inter-theoretic relations obtain, e.g., that classical electro-dynamics unifies wave optics and the theories of electricity and magnetism. In contrast, a realist with a taste for a parsimonious ontology will choose a unifying theory, all other things being equal, and will believe to have thusly set one step closer to the truth.

5. CONCLUSIONS

In her historical case-studies Morrison has found a bewildering variety of uses of the words 'unity' and 'unification': theoretical unity, synthetic unity, methodological unity, formal unity, structural unity, mathematical unity, material unity, interconnective unity, reductive unity, and more. These uses are different and from this observation she concludes that a general theory of unification and unity is "impossible": "no single account of theory unification can be given" (p. 232). Yet she announced to provide "some understanding" of unity and unification (Goal 1). I detect some tension here, unless we put an enormous amount of weight on the "some". Understanding unity and unification in science only comes about when we are able to see what the mentioned uses have in common. Morrison focuses on the differences rather than on the similarities. There certainly must be some similarities, otherwise the same words would not have been used in these different contexts of physics and biology. Waving the white flag for the project of finding these similarities in the very first book devoted to the subject I find premature. In fact, Morrison does mention a few similarities, such as the presence of a 'unifying parameter' in the examples of unification in physics (displacement current, γ-factor, Weinberg's weak mixing angle, field tensor), which is why I do not understand why she so happily waves the white flag. The case-studies from physics suggest a definition of unification as a triadic inter-theoretic relation along the following lines: theory $\mathbf{T}$ *unifies* theories $\mathbf{T}_1$ and $\mathbf{T}_2$, construed as classes of 'models' Δ, Δ_1 and Δ_2 respectively, iff (i) for every model $M_1 \in \Delta_1$ there is a model $M_2 \in \Delta_2$ and there is a model $M \in \Delta$ such that both M_1 and M_2 are embeddable in M, and the number of free parameters in M_1 and M_2 together exceed those of M, and (ii) *mutatis mutandis* with 1 and 2 interchanged. Whether this encompasses all cases of unification in physics, I dare not say, but it looks appealingly simple and seems in full agreement with Morrison's case studies from physics, from which this definition emerges. Needless to say, this should be worked out in detail and tested explicitly by case-studies. If Morrison had done this, we would have had some understanding of unity because then we would have laid the finger on the unity in unity.

With her historical case-studies Morrison has collected valuable data for a philosophical analysis and has made a number of valuable observations, but she has not provided an *understanding* of the concepts of unity and unification. She leaves too much disunity in unity in order to speak of a genuine understanding of unity and that I find rather a pity. On the other hand, notwithstanding the fact that several critical observations of

Morrison's cut no ice, the allied forces of unification and explanation will have their hands full of some of her observations which do cut ice.

APPENDIX: ERRORS

Has proof reading gone out of fashion at Cambridge University Press? By way of a service for future readers I list the (type-setting) errors I stumbled on. Chapter 2: on p. 38 '$\subseteq$' has the standard meaning of 'is a subset of', but on p. 38 it means 'is embeddable by the identity'; primed relations R'_j on p. 38 belong to the observational substructure, but on p. 39 they are treated as *arguments* in a function whose domain is the domain of the relation (in "$\phi(R'_i)$"), and on p. 40 the primes have suddenly dropped off; on p. 39 we see that the embedding $\phi : B \to A$ has, besides arguments in B, also relation R' and model $\mathcal{B}$ as arguments, which it is of course not supposed to have; on the same page, 4th line from above, $\mathcal{A}$ and $\mathcal{B}$ should be interchanged in the elocution "$\mathcal{A}$ is embeddable into $\mathcal{B}$"; on p. 42, x, a and χ all three denote the same thing; on p. 45, the expression of the kinetic energy is written incorrectly and correctly in alternation; on p. 40, A and B are base sets of models but also propositions, viz. "A & B", and $\phi(A)$ and $\psi(B)$ must be $\phi[A]$ and $\psi[B]$, respectively.

Chapter 3: formula (3.4) on p. 74 makes no sense and contains unexplained symbols; ψ is not the vector potential (p. 76); dw^2 should be dw^3 in the energy volume-integral on p. 87; all total derivatives on pages 88–89 should be partial derivatives (also in most other places in this Chapter) – there a world of difference between total and partial differential equations!

Chapter 4: Δ on p. 115 should everywhere be ∇, $2\psi/2t$ must be $\partial\psi/\partial t$ (p. 115); Morrison takes q to be the coupling constant of $U(1)$, instead of $g'/2$ as is standardly done, which means that most of the formulae following this convention are wrong (pp. 136–137, p. 142): for instance, M_W^2, the mass squared of the W-boson, is not $e^2/2G \sin\theta_\mathrm{w}$ but $\sqrt{2}e^2/8G \sin\theta_\mathrm{w}$ (in the formulae Morrison has copied she should have replaced g' with $2q$, not with q); from the second line on p. 137 it follows that Weinberg's weak mixing angle θ_w equals $\pi/4$ plus an integer multiple of π, which is of course nonsense; dk^2 should be dk (p. 145).

Chapter 5: on p. 171, ϕ appears for the first time but is only explained two pages further on; the sentence below (5.103) on p. 156 is wrong.

Chapter 6: replace β with γ in item 2. of the list of p. 197.

Chapter 7: not a single one of the five summation-symbols on p. 220 has a summation-index, an initial value or a final value.

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