On a Derivation of the Necessity of Identity

Saul Kripke's lecture "Identity and Necessity" [1971] begins with a short discussion of a formal derivation of a law of necessity of identity. The derivation has become rather well known, but perhaps not so well understood. The present commentary attempts to improve understanding of it by enlarging on four remarks of Kripke's:[1]

(A)   This is an argument which has been stated many times in recent philosophy.

(B)   Let us interpret necessity here weakly. We can count statements as necessary if whenever the objects mentioned therein exist, the statement would be true.

(C)   If … we can talk about modal properties of an object at all, that is, in the usual parlance, we can speak of modality *de re*… then [the conclusion of the derivation] has to hold.

(D)   [B]y itself [the conclusion of the derivation] does not… say anything about identity *statements* at all. It says that for every *object x* and *object y*, if *x* and *y* are the same object, then it is necessary that *x* and *y* are the same object.

1. What is the Source of the Derivation?

The derivation in "Identity and Necessity" proceeds in three steps:[2]

(1) $\forall x \Box x = x$

(2) $\forall x \forall y (x = y \rightarrow (\Box x = x \rightarrow \Box x = y))$

(3) $\forall x \forall y (x = y \rightarrow \Box x = y)$

Here (1), universal necessary self-identity, is simply postulated. (2) is an instance the axiom scheme of the *indiscernibility of identicals* from classical identity logic, also known as *substitutivity of identicals* and as *Leibniz' Law*:[3]

(4) $\forall x \forall y (x = y \rightarrow (\Phi(x/z) \rightarrow \Phi(y/z)))$

(3) follows by classical predicate logic.[4] The derivation (1)-(3) is one of those things that, though it may appear obvious once pointed out, requires some ingenuity for its initial discovery.

Kripke makes it clear in remark (A) that the ingenuity in question is not to be attributed to himself. He mentions one earlier source, [Wiggins 1965], but leaves us wondering: Who first found the derivation (1)-(3)? But let us first ask: Who first found the conclusion (3)? More specifically, let us ask after the sources of (3) and (1)-(3) *in formal systems of quantified modal logic*.

To begin at the beginning, the study of formal systems of quantified modal logic was launched by three papers that the late Ruth Marcus published, under the name Ruth Barcan, in the *Journal of Symbolic Logic* (*JSL*) in 1946-47. These derive from her Yale dissertation, written under the supervision of Frederic Fitch. Two systems are developed, based on the modal sentential logics **S2** and **S4**. The first Barcan paper [Marcus 1946a] is best remembered for the controversial *Barcan schemes*, converse and direct, one given as a theorem, the other taken as an axiom.

They are usually formulated today as follows:

(5) $\quad \Box \forall x \Phi \rightarrow \forall x \Box \Phi$ $\qquad$ (6) $\quad \forall x \Box \Phi \rightarrow \Box \forall x \Phi$

The groundwork laid in the first two Barcan papers is built upon in the third, [Marcus 1947], which adds second-order apparatus, and presents two main results about identity.

One main result is the equivalence of two candidate definitions of the identity predicate $x = y$ in second-order terms:

(7) $\quad \forall X(Xx \rightarrow Xy)$ $\qquad$ (8) $\quad \forall X \Box (Xx \rightarrow Xy)$

Definition (7) is Russell's, while definition (8), which results from it on replacing "material implication" by "strict implication" in the manner of C. I. Lewis, is Marcus's. In the **S2**-based system she obtains the material equivalence of (7) and (8), and in the **S4**-based system their strict equivalence. Fitch, in a paper [1949] that is largely a defense of his student's work, suggests (footnote 3) that the failure to get everything one would want in the **S2**-based system should be taken as an indication that **S2** is inappropriately weak. He in effect suggests working in the system **T**, stronger than **S2** but weaker than **S4**, which has the rule of *necessitation*:

(9) $\quad$ from Φ to infer $\Box \Phi$

but not the *doubling law*:

(10) $\Box\Box\Phi \Leftrightarrow \Box\Phi$

T indeed suffices in place of **S4** for the strict equivalence of (7) and (8).

The other main result is a version of the law of necessity of identity (3), and so the source of the conclusion (3) appears to be Marcus. Today, when the third Barcan paper is more often cited than read, there seems to be a widespread misimpression that it contains not just the conclusion (3), but also the derivation (1)-(3). Even Marcus herself, by four decades later, had formed such an impression. She forcefully expresses it at the beginning of her well-known letter on the relationship between her work and Kripke's:[5]

> In 1947 in a paper on identity in modal logic in <u>The Journal of Symbolic Logic</u> (12) (JSL), there is a formal proof of the necessity of identity for systems of quantified modal logic. It follows in the way later represented by Wiggins & Kripke. I no longer have copies of that paper but I enclose a reference from Hughes and Cresswell <u>Introduction to Modal Logic</u> (1968) (enclosed labelled '1'.) As in subsequent discussions (Wiggins, Kripke) the proof is a straightforward application of the indescernibility [*sic*] of identicals and does not depend on any peculiarly modal assumptions.

Evidently Marcus is here attempting to summarize from memory a paper published forty years earlier, without having the text before her. The attempt, not very surprisingly, fails: The proof of the *other* main result of the third Barcan

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paper, which *does* invoke indiscernibility, has been confused with the proof of the necessity of identity, which does *not*. The derivation of the necessity of identity in the third Barcan paper is *not* straightforward, *does* involve peculiarly modal assumptions (so that it does *not* go through for **T** in place of **S4**), and is altogether *different* from the derivation (1)-(3) to be found in Wiggins and then Kripke.

It may be summarized as follows:[6]

(11) $\forall x \forall y (x = y \Leftrightarrow \forall X \Box (Xx \to Xy))$

(12) $\forall x \forall y (\Box \forall X (Xx \to Xy) \Leftrightarrow \forall X \Box (Xx \to Xy))$

(13) $\forall x \forall y (\Box \Box \forall X (Xx \to Xy) \Leftrightarrow \Box \forall X (Xx \to Xy))$

(14) $\forall x \forall y (\Box \forall X \Box (Xx \to Xy) \Leftrightarrow \forall X \Box (Xx \to Xy))$

(15) $\forall x \forall y (\Box x = y \Leftrightarrow x = y)$

Here (11) is Marcus's distinctive definition of identity. (12) is an instance of the two Barcan schemes (5) and (6) (in second-order versions) combined into one. (13) is an instance of the **S4** doubling law (10). (14) follows from (12) and (13), and (15) from (11) and (14), by replacement of provable equivalents, a metatheorem in most modal systems, but taken as a primitive inference rule in the Barcan papers.

So the source of the derivation (1)-(3) appears after all *not* to be Marcus. It is, of course, the rule rather than the exception for the first proof of a theorem to be superseded eventually by a simpler proof subsequently discovered. In this instance,

the source of the simpler proof (1)-(3) appears to be W. V. Quine.

Quine's first *published* statements of the derivation date from 1953, but there is reason to speculate that he may have arrived at the derivation (1)-(3) as early as 1946, by pondering the complicated Barcan derivation (11)-(15) and asking himself whether it could be replaced by something simpler. For Quine must have been one of the first persons outside Yale to see the material in the second and third Barcan papers: When Marcus submitted those papers to the *JSL*, the editor, Alonzo Church, assigned them to Quine as referee.

I have been unable to locate his referee's report in the as yet incompletely catalogued Quine *Nachlaß* in the Houghton Library at Harvard, but Church's reply is available to scholars in the Church *Nachlaß* in the Firestone Library at Princeton (Box 26, Folder 1). From this reply (dated June 9, 1946) one can gather that Quine's report was not encouraging, but that Church as editor was prepared to overrule his referee. The operative passage reads as follows:

> The fact that you point out, that her methods are laborious and often rather obvious, while she seems to avoid the more difficult and interesting questions, leaves me still in doubt whether we should decline her papers, and I think that I ought to get another opinion on them before deciding. There are degrees of uninterestingness, and it is not clear just how far we ought to go in publishing material which is actually new but in method and outlook not very novel or interesting.

Given the general character of Quine's complaint, as paraphrased by

Church, there is reason to speculate that already in 1946 Quine may have had in

mind the replacement of the "laborious" (11)-(15) by the "obvious" (1)-(3). But if

one so speculates, one will need to explain the curious fact that when Quine came

to write a review [1947b] of the third Barcan paper for the *JSL*, he made *not one*

mention of the result on the necessity of identity. I think this *can* be explained, but

the story is neither short nor simple.

The first thing that needs mentioning is that Quine's review also made *not*

one mention of the other main result of the third Barcan paper, the coextensiveness

of "material" identity as defined by Russell with "strict" identity as defined by

Marcus. Quite the contrary, after noting that there are two identity definitions (7)

and (8) in play from early in the paper, Quine launches into a sermonette against

the idea of "degrees of identity", as if Marcus were advocating such a notion, and

as if her two definitions were intended as characterizations of two different

"degrees". Quine here is overlooking the fact that Marcus's explicit goal is

precisely to prove the two definitions coextensive.

Quine's oversight is pointed out in the Fitch footnote cited earlier. Fifteen or

so years later the review apparently still rankled, since in her much-discussed

February, 1962 talk at the Boston Colloquium for the Philosophy of Science,

Marcus goes out of her way to work in a semi-jocose allusion to that old episode.[7]

By that time Quine had already published a retraction, euphemistically entitled a "correction" [1958].

There Quine tries to explain how his embarrassing oversight could have come about. Apparently he wrote his review of the paper even before it was published, working from printer's proofs, and he suggests that he may simply never have seen the last page of the proofs, containing the last 20 lines of the paper. Among these lines are the enunciations of the two main results that Quine fails to mention, and I believe we have here the correct explanation both for Quine's confusion about the status of the two definitions of identity, and for his silence about the necessity of identity, whatever he may have thought about it.

For there is one consideration that makes Quine's partial excuse plausible. Fitch (in the same footnote already twice cited) notes two serious typographical errors in the third Barcan paper: The printer has made nonsense of Marcus's contrast between the **S2** material equivalence of the two second-order definitions of identity (formula 2.31 in the paper) and their **S4** strict equivalence (formula 2.33*, the star indicating dependence on **S4**) by printing the main connective as strict equivalence both times, and has made a mystery of the necessity of identity (formula 2.32*) by omitting its main connective altogether. Both these bad typos occur in the last 20 lines of the paper, whereas there do not seem to be any comparably serious printer's errors that slipped through without being caught at the

proofreading stage elsewhere in the three-paper series; and all this suggests that perhaps even the *author* never saw these lines in proof, let alone the reviewer.

Turning from historical speculation to documentable history, Quine, late in section 3 of [1953a], mentions "Miss Barcan's pioneer papers on quantified modal logic" and specifically her theorem, which he displays, as his formula (38), in the form of our version (3) above rather than of her version (15). After other brief remarks he adds:

> Note incidentally that (38) [our (3)] follows directly from (36) [our (1)] and a law
> of substitutivity of identity for variables:
>
> $$(x)(y)[(x = y \,.\, Fx) \supset F\text{y}]$$

The displayed formula amounts to our (4), and thus Quine is in effect giving the derivation (1)-(3) above, characterizing it as "direct" in implicit contrast to the proof given in the "pioneer paper". This is the closest he comes in print to contrasting "laborious" and "obvious", but I think we may conclude that, whenever exactly it was that Quine arrived at the derivation (1)-(3), his knowledge that Marcus had already given a derivation of the same conclusion (3) played a role in his seeking a simpler one.

In [1953b], section III, Quine also indicates the derivation, this time without allusion to the Barcan article. After mentioning our (4), under his numbering (51), he gives our (3), under his numbering (52), writing as follows:

...since surely 'nec $(x = x)$' is true for all x we have

(52) $(x)(y)[x = y \supset.$ nec $(x = y)]$,

I.e., identity holds necessarily if it holds at all.

The derivation is briefly indicated yet again, fairly late in his reply to Marcus's Boston Colloquium talk [Quine 1961]:

> In particular, the criterion makes no doubt of Professor Marcus's law for modal logic:
>
> $(x)(y)[x = y \supset.$ necessarily $x = y)$
>
> It follows from 'necessarily $x = x$' by substitutivity.

Kripke was aware early in his career of the Quinean origin of the derivation of (1)-(3). In his first publication [1959, bottom p. 9] he cites an instance as

> a theorem of Quine [1953b] p. 80, formula (52)

This is the same formula (52) [our (3)] I have just displayed in the quotation above.[8] By the time of the 1971 lecture, Kripke may have forgotten that before Wiggins there was Quine; or perhaps Kripke simply thought the history of too little importance to be worth mentioning. But it will after all be of some importance, when evaluating the status of derivation (1)-(3), to know that the person who first mentioned it in print was none other than Quine, notorious archenemy of quantified modal logic.[9]

2. What is the Status of the Derivation?

Quine and Kripke simply postulate (1), and the question of the soundness of the derivation (1)-(3) for them more or less reduces to the question of the plausibility of this postulate. Kripke is willing to endorse the postulate only subject to two *caveats*, explicit or implicit in his remarks (B) and (C): The box symbol must be given a *weak* reading and a *metaphysical* reading. Each point requires elaboration.

As to the first *caveat*, we may begin by noting that the well-known textbook of Hughes and Creswell [1968] gives a derivation of (3) similar to the one Kripke discusses,[10] but not quite the same, since the joint authors in effect derive (1) from other principles that they presumably take to be more basic. They are able to do so because they work with a version of quantified modal logic in which open formulas, containing free variables, can appear in derivations, and the rule of necessitation (9) can be applied to such formulas. Kripke [1963] by contrast argues for allowing only closed formulas, with all occurrences of variables bound, in derivations.[11]

Application of necessitation to open formulas is, as Kripke explains, more or less equivalent to assuming the converse Barcan scheme (5): It permits the easy derivation of (5), while inversely, in a system where only closed formulas may appear in derivations, assumption of (5) can in effect replace applications of

necessitation to open formulas.

In particular, it makes available the following derivation of (1):

(16) $\forall x\ x = x$

(17) $\Box \forall x\ x = x$

(18) $\Box \forall x\ x = x \rightarrow \forall x \Box x = x$

[(1)] $\forall x \Box x = x$

Here (16) is just the law of universal self-identity from classical identity logic; (17) follows by necessitation; (18) is the pertinent instance of the converse Barcan scheme (5); and the conclusion, identical with hypothesis (1) above, follows by modus ponens. One can then go on to (2) and (3).[12]

But Kripke [1963] shows the Barcan schemes (5) and (6) to express the assumptions, respectively, that everything that exists exists necessarily, and that everything that possibly exists exists, and he rejects both schemes. That is why he just starts from (1), offering no derivation of it. But if one thus rejects what the Barcan schemes express, one must distinguish three readings of the premise $\Box x = x$, which may be stated in Leibnizianokripkean "possible worlds" jargon as follows:

(19) strong reading:

in any possible world, x exists there and is self-identical there

(20) intermediate reading:

in any possible world, whether or not x exists there, it is self-identical there

(21) weak reading:

in any possible world, if x exists there, then it is self-identical there

Kripke rejects the strong reading, and indicates that he does not want to go into the tricky issues that arise in connection with the intermediate reading, and so in his remark (B) endorses (1) only if the box symbol is given the weak reading. Having noted this once and for all, in what follows we may ignore distracting complications about possible nonexistence.

As to the second *caveat*, we may begin by noting that it is significant that the discoverer of the derivation (1)-(3) was Quine, because no one would imagine that this militant critic of quantified modal logic could be advocating its conclusion (3) categorically. Rather, Quine says that it is a conclusion one must accept *if* one is to go in for quantified modal logic at all. The hypothetical "if" here is the same one that begins Kripke's remark (C). And though Kripke does very much go in for quantified modal logic while Quine very much does not, still one must not imagine that Kripke simply endorses what Quine opposes: There are distinctions to be made.

Quine in his most famous book [1960, chapter II, section 13, p. 59] remarks:

Philosophical tradition hints of three nested categories of firm truth: the analytic, the a priori, and the necessary. Whether the first exhausts the second, and the second the third, are traditional matters of disagreement.

But Quine's criticism of quantified modal logic is largely directed against (C. I. Lewis and) Rudolf Carnap, who effectively identified or conflated the three categories. More precisely, Carnap thought the metaphysical notion of necessity and the epistemological notion of aprioriness were to be explained or "explicated" in terms of the semantical notion of analyticity or "L-truth", which he took to be clearer. Quine's criticism of quantified modal logic is largely premised on this Carnapian assumption, Quine's claim that quantification into modal contexts makes no sense boiling down to the claim that quantification into the context "it is analytic that…" makes no sense. Dagfinn Føllesdal puts the matter succinctly in the new introduction (p. xxi) that accompanies the reprinting [2004] of his dissertation:

Quine saw that Carnap and Lewis's linguistic conception of necessity was untenable if one wanted to quantify into modal contexts, and that their position was therefore incoherent.[13]

Kripke insists on distinguishing the three categories, leaving us with three conceivable readings of the box, all conflated by early workers in modal logic under the label "alethic necessity":

(22)   metaphysical reading: *it is necessary that…*

(23)   epistemological reading: *it is apriori that…*

(24)   semantical reading: *it is analytic that…*

For Kripke, (22) is explained, not in terms of (23) or (24), but in terms of counterfactual or subjunctive conditionals: The metaphysically necessary is that which, *no matter how the world might have been, would still have been the case*, and metaphysical possibilities are *the way the world is, and all the ways the world actually isn't, but might potentially have been*, or the actual situation and all counterfactual situations. By contrast, the other readings pertain only to the way the world actually is: Epistemological possibilities are *ways that for all that is knowable independently of sense experience (that is, apriori) the world may actually be*, while semantical possibilities are *ways that for all that follows just from the meanings of words (that is, analytically) the world may actually be*.

Where Quine follows Carnap in not really distinguishing the three readings, but generally assuming the third, Kripke by contrast insists on the distinction, and the first reading is what *he* always means by "necessity".[14] Kripke in remark (C) in effect acknowledges that the notion of necessity involved must be one for which quantification into modal contexts makes sense, and he claims that it *does* make sense for metaphysical modality. He does not disagree explicitly with Quine, and

on the contrary appears to agree implicitly with Quine, about the status of quantification into the contexts "it is analytic that…" and "it is apriori that…". The fact that Kripke endorses the postulate (1) only for a certain specific reading of the box symbol will be of considerable importance as we now turn to issues about identity *statements*.

3. What is the Significance of the Derivation?

Here an *identity statement* is something of the form $a = b$, wherein a and b are singular terms. Our interest will be in the question: For which classes of singular terms a and b, and for which senses of the word "necessary" or readings of the symbol $\square$ is the case that whenever $a = b$ is true, then it is necessarily true, and hence $\square a = b$ true?

The derivation (1)-(3), depending on the indiscernibility law (4), might be thought indirectly relevant here, by suggesting a parallel derivation

(25) $\square a = a$

(26) $a = b \rightarrow (\square a = a \rightarrow \square a = b)$

(27) $a = b \rightarrow \square a = b$

depending on a parallel indiscernibility law

(28) $a = b \rightarrow (\Phi(a/z) \rightarrow \Phi(b/z))$

Or the derivation (1)-(3) might be thought more directly relevant, with (27) being immediately inferable from its conclusion (3).

Kripke's remark (D) indicates that he does *not* take the derivation (1)-(3) to be thus relevant to identity statements. And indeed, to get (27) from (3), or (28) from (4), in a formal system, one would need, beyond having the terms *a* and *b* in the language, a rule of *instantiation*

(29)   from $\forall x \Phi(x)$ to infer $\Phi(t)$

for singular terms *t* of a class to which *a* and *b* belong. But one doesn't get (29) automatically just because one has the singular term *t* in one's formal language.

For instance, in systems with the Russell iota-operator, if *t* is an iota-term $\iota y \Theta(y)$, which is to say, a description "the unique *y* such that $\Theta(y)$", then in general $\Phi(t/z)$ is not even a well-formed formula unless "scope" markers are added. And whether when markers are added the result follows from $\forall x \Phi(x)$ as per (29) depends on where the markers are placed. Such facts were well enough understood by workers on quantified modal logic. A general awareness of this sort of point is shown already in the first Barcan paper, while a specific application of this sort of point to puzzles about "the morning star" and "the evening star" was made in response to Quine by Arthur Smullyan [1947, 1948]. Kripke himself discusses the matter (crediting Smullyan) in "Identity and Necessity", shortly after his discussion

of the derivation that is the topic of this note.[15]

Once one realizes that (29) represents a substantial additional assumption, it becomes clear that there is no short, easy route from the derivation (1)-(3) to a conclusion like (27).[16] To be sure, Kripke does indeed recognize the truth of (27) for certain classes of singular terms, and a metaphysical reading of the symbol $\square$, but this recognition is not on his view justified by appeal to derivation (1)-(3) or anything like it. Kripke himself enlarges somewhat on this matter later in his lecture, and I will devote the remainder of the present note to enlarging upon it further.

To begin with, a more than sufficient condition for the truth of the necessary identity statement $\square a = b$, or what comes to the same thing, for the necessary truth of the identity statement $a = b$, is that the denotations of a and b should be actually the same, and that the denotation of a and likewise of b should be *invariant* or unchanging across the range of whatever kind of "possibilities" are in question. That this condition suffices is shown, not by some formal derivation in the object language, but by an argument in the metalanguage. The argument does not rely on previous establishment of any indiscernibility law (28). It can be phrased in terms of a comparison between what a singular term denotes in the actual world @ and what it denotes in a given possible world w.

(30)   *what a denotes in @*   =   *what b denotes in @*

(31) *what a denotes in w*   =   *what a denotes in @*

(32) *what b denotes in w*   =   *what b denotes in @*

(33) *what a denotes in w*   =   *what b denotes in w*

Here (30) is the hypothesis of the *actual* truth of $a = b$, the truth of $a = b$ at @. (31) and (32) are instances of the invariance assumptions for *a* and for *b*. (33) follows by the symmetry and transitivity of identity, and gives us the truth of $a = b$ at *w*. Since this holds for *arbitrary w*, we thus have the *necessary* truth of $a = b$, hence the truth of $\Box a = b$. Since we have derived $\Box a = b$ on the hypothesis $a = b$, we have derived (27).

According to a commonly used formulation, a Kripkean *rigid designator* would be a singular term that "denotes the same object in all possible worlds". This formulation is potentially misleading, but acceptable provided it is understood as follows: Whatever metaphysically possible world or counterfactual situation or way the world could potentially have been we may be speaking of, the term denotes the same object that it denotes when we are speaking of the actual world or actual situation or way the world actually is. More simply, a Kripkean rigid designator would be a singular term that is invariant across the range specifically of "metaphysical" possibilities, ways the world could potentially have been. By the foregoing argument, (27) would hold then for rigid designators provided the box $\Box$ is read specifically as "metaphysical" necessity. The expression "rigid

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designator" comes from Kripke's "Naming and Necessity" lectures in 1970. The notion of an *invariant* denoting expression appears earlier, in Føllesdal's 1961 dissertation; but it is too early to speak of *rigid* designation at that date, since "metaphysical" necessity was not systematically distinguished from other kinds of "alethic" modality in the pre-1970 literature.[17]

So far, all this has been about what Kripkean rigid designators by definition are like if there are any. But are there any in natural language? It has by now become comparatively uncontroversial that a minority of definite descriptions with certain special features are such rigid designators. These include descriptions containing the adverb "actually", as in *the candidate who actually came in second.* Even when speaking of some way the world might potentially have been under which a given description, with "actually" omitted, would have been met by some different individual, as in *If he had not been so burdened by associations with an unpopular war, the candidate who actually came in second might have become president*, insertion of the adverb pulls us back to the actual situation, so to speak, so that the description continues to denote the same individual that it denotes when speaking of the actual situation, as in *The candidate who (actually) came in second was the democratic nominee*. But Kripke has his eye on a more interesting but more controversial class of singular terms: names in the ordinary sense, the expressions traditional grammar calls "proper nouns", as in *Hubert Humphrey*.

Thus overall Kripke insists on a threefold distinction. In his lecture he endorses all three of the following, but each on different grounds:

(34)   The relations of identity and (metaphysically) necessary identity are coextensive: Of either it may be said that any object stands in that relation to itself and to nothing else.

(35)   A true identity statement $a = b$ is necessarily true if a and b are rigid designators, and metaphysical necessity is in question.

(36)   Ordinary proper nouns are rigid designators (and hence by (35) a true identity statement $a = b$ is necessarily true if a and b are ordinary proper nouns, and metaphysical necessity is in question).

And he emphasizes the distinctness of the three claims.

(34) is the only one of the three to which the derivation (1)-(3) may be relevant, according to Kripke's remark (D) and further elaborations. (35) is a direct consequence of the stipulative definition of the term of art "rigid designator". (36) is an empirical claim about natural language. It is argued for by elicitation of an intuition on the part of speakers with native fluency in our language to the effect that when we use an ordinary proper noun in speaking of some imagined counterfactual situation, as in *If he had not been so burdened by associations with an unpopular war, Hubert Humphrey might have become president*, that noun

continues to denote the same individual that it denotes when speaking of the actual situation, as in *Hubert Humphrey (actually) came in second, behind Nixon, but ahead of Wallace*.

If one is to understand fully how the derivation (1)-(3) is *not* relevant to (35) and (36), one must go into a matter not taken up by Kripke in his lecture, and distinguish the Kripkean notion of a *rigid designator* from the Russellian notion of a *logically proper name*, to which the derivation (1)-(3) is more relevant.

For Russell, a *logically proper name* is a singular term whose presence in a sentence contributes nothing to the proposition expressed thereby except the object that it denotes. An immediate consequence is that for logically proper names (28) would hold for *arbitrary* contexts Φ, since Φ(*a*/*z*) and Φ(*b*/*z*) would express the very same proposition. Two logically proper names for the same object are intersubstitutable in arbitrary contexts, the only exception being for contexts such as that of quotation, where the names are not being *used* to denote, but rather *mentioned* as words. Given (28) in this way, (27) could be derived for logically proper names in the manner indicated earlier, parallel to the derivation (1)-(3).[18]

This derivation of (27) for Russellian logically proper names would be entirely independent of any considerations about what sense of "necessity" the box symbol represents. Thus it would seem that if *a* and *b* are Russellian logically proper names, and *a* = *b* is true, then so are all three of the following:

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(37)   It is (metaphysically) necessary that $a = b$.

(38)   It is a priori that $a = b$.

(39)   It is analytic that $a = b$.[19]

Kripke indicates towards the end of the preface to the book edition of *Naming and Necessity* [1980] that he suspects that the apparatus of "propositions" simply breaks down in certain puzzle cases (the allusion being to the kinds of examples discussed in Kripke [1979]). Since the very definition of Russellian "logically proper names" presupposes the notion of "proposition", Kripke cannot accept the Russellian notion. Hence it is important to distinguish (35) from the following:

(40)   A true identity statement $a = b$ is necessarily true if a and b are logically proper names, whatever kind of "necessity" is in question.

Kripke *a fortiori* must reject any claim that ordinary proper nouns are logically proper names. In particular, he famously rejects (38) and (39) — even while accepting (37) — in the case of "Hesperus" and "Phosphorus". Thus besides distinguishing (35) from (40) we must distinguish (36) from the following:

(41)   Ordinary proper nouns are logically proper names (and hence by (40) a true identity statement $a = b$ is necessarily true if a and b are ordinary proper

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nouns, whatever kind of "necessity" is in question).

In any case, (40), a consequence of the stipulative definition of the term of art "logically proper name", and (41), an empirical claim about natural language, need to be distinguished from each other, just as (35) and (36) needed to be distinguished from each other. This should be especially clear since Russell himself famously held that ordinary proper names are *not* logically proper names, the former being for him "truncated descriptions", and the only real examples of the latter being for him "this" and "that" used in reference to sense data.[20]

Though the notion of a Russellian logically proper name plays no role in Kripke's thought, that notion did play a role in the pre-Kripkean literature on the philosophical interpretation of quantified modal logic. In order to enhance understanding of what is distinctive about the views expressed in Kripke's lectures in the 1970s, let me before closing turn back to the 1940s through 1960s and the debates between quantified modal logic's critic Quine and its various defenders.

The notion of Russellian logically proper name was invoked in that debate under one label or another — sometimes "logically proper names", sometimes just "proper names", sometimes just "names", and sometimes instead "tags" — by Arthur Smullyan [1947], and following him Fitch [1949, 1950, both citing Smullyan], Marcus [1960, 1961, 1963, the first citing Fitch, the second describing the point as "familiar"], and Prior [1963, 1967, the latter citing Smullyan]. The line

of thought leading to claims of intersubstitutability in arbitrary contexts is nicely summed up in the last-cited work (pp. 10-11):

> Alternatively, it may be argued (as it has been by Arthur Smullyan) that Leibniz' law holds only for x's and y's that directly *name* whatever they do name, not for x's and y's that pick out what they name merely as the thing, whatever it is, that answers to some definite description, such as "the morning star." If x and y simply tag the same object, so that "x is y" comes to "This is this," then this (when it is true at all) is a necessary truth....
>
> It seems clear, on any view, that when modal logic and identity theory are brought together Leibniz' law must be applied with caution, but we may be watchful at different points. We may look, with Quine, at the "contexts" we wrap around our identicals—the things that we say are true of y because they are true of x, which is identical with y—and insist that modal contexts, for example, are not covered by the law. In this case we need not be very meticulous about the expressions we take x and y to do duty for. We may, on the other hand, confine x and y rigorously to Russell's "logically proper names," and we can then afford to be free and easy with our "contexts."[21]

The most explicit endorsements of the consequences (38) and (39) are to be found, however, not in Prior but in Marcus: first in her Boston Colloquium talk, [Marcus 1961]; second, in the discussion following the talk, [Marcus et al. 1962]; and third in her talk of six months later at the famous conference on modal logic in Helsinki [Marcus 1963].[22] The background is as follows. Quine in his commentary

(immediately after his citation of Marcus for the law (3)) objects to (38) in the case of "tags" for the planet Venus:

> I think I see trouble anyway in the contrast between proper names and
> descriptions as Professor Marcus draws it. Her paradigm of the assigning of
> proper names is tagging. We may tag the planet Venus, some fine evening, with
> the proper name 'Hesperus'. We may tag the same planet again, some day before
> sunrise, with the proper name 'Phosphorus'. When at last we discover that we
> have tagged the same planet twice, our discovery is empirical.

Quine adds, "And not because the proper names were descriptions."[23] In the ensuing discussion Kripke asked a question about (39), whether it was Marcus's view that when two tags denote the same object, the identity statement $a = b$ is analytic.[24] Marcus took the occasion of Kripke's question to respond to Quine's objection.

The operative material from her response, which was her last speech (apart from a brief interjection) in the discussion at the Boston colloquium, is as follows:

> Let's talk about proper names not descriptions in this ideal sense of proper names.
> Now presumably in this ideal sense of proper names and tagging there would be a
> dictionary.… [F]inding out whether two things have the same tag … [is] like
> looking up a word in a dictionary. Do these two words — does the dictionary tells
> us they have the same meaning? And this is what we would mean by analytic
> truth.

or again:

> Presumably, if a single object had more than one tag, there would be a way of finding out such as having recourse to a dictionary or some analogous mode of inquiry, which would resolve the question as to whether the two tags denote the same thing. If 'Evening Star' and 'Morning Star' are considered to be two proper names for Venus, then finding out that they name the same thing as 'Venus' names is different from finding out what is Venus' mass, or its orbit. It is perhaps admirably flexible, but also very confusing to obliterate the distinction between such linguistic and properly empirical procedures.

to which she added in Helsinki (p. 132):

> [T]o discover that we have alternative proper names for the same object we turn to a lexicon, or, in the case of a formal language, to the meaning postulates. … To resolve the puzzle [about different names for the same planet], one doesn't investigate the planets, but the accompanying lexicon.

It is in such terms that (38) and (39) are endorsed by Marcus, making explicit what is implicit in Prior's claims of "free and easy" intersubstitutability.[25]

Kripke at the Boston Colloquium responded to Marcus's "dictionary" remark by calling it "a perfectly valid point of view". This is perhaps the most surprising statement in the Boston discussion for those familiar only with Kripke's mature views. For in the celebrated 1970 lectures "Naming and Necessity" and the

1971 lecture "Identity and Necessity" he is by contrast quite critical of the "dictionary" remark. When did he change his mind? He dates the main ideas of his "Naming and Necessity" lectures to the academic year 1963-64, and thus to a year or two after the Boston Colloquium. But let us for the moment stick with the earlier history, before Kripke became a serious contributor (except on the technical, model-theoretic side).

So far I have been discussing (40) (as contrasted with (35)). Let me turn now to (41) (as contrasted with (36)), and specifically to the issue of going beyond (40) to (41). Was any of the early responders to Quine's criticisms willing to take that step, and not merely posit logically proper names, but claim that ordinary proper nouns are logically proper names? The question whether there is any class of expressions of natural language that function as Russellian logically proper names was first brought into the debates over quantified modal logic by Church [1950], who did not believe that ordinary proper nouns formed such a class, and challenged Fitch and like-minded theorists to identify such a class if they could. There was no response to the challenge in the 1950s.

Moving on the 1960s, readers may form their own judgments about the disputed question of whether (41) is being advocated in addition to (40) in Marcus [1961], where different passages seem to point in different directions.[26] Prior, when speaking at Helsinki, took a line on the issue that to some extent sidesteps

Church's challenge (while dispensing with the Marcusian "dictionary" or "lexicon"). The claim of Prior [1963] is that there is no fixed class of expressions of natural language that *must always* function as Russellian logically proper names, but that expressions of various different classes (including not only ordinary proper nouns but also other substantival phrases such as definite descriptions, and even demonstrative pronouns) *may sometimes* so function:

> It is not necessary, I think, for philosophers to argue very desperately about what is in fact 'ordinary' and what is not; but let us say that *a name in Russell's strict sense* is a simple identifier of an object … [T]here is no reason why the same expression, whether it be a single word like 'This' or 'Tully,' or a phrase like 'The man who lives next door' or 'The man at whom I am pointing,' should not be used sometimes as a name in Russell's strict sense and sometimes not. If 'The man who lives next door' is being so used, and successfully identifies a subject of discourse, then 'The man who lives next door is a heavy smoker' would be true if and only if the subject thus identified *is* a heavy smoker, even if this subject is in fact a women and doesn't live next door but only works there. And if 'Tully,' 'Cicero,' 'The Morning Star' and 'The Evening Star' are all being so used, then 'Tully is Cicero' and 'The Morning Star is the Evening Star' both express necessary truths, to the effect that a certain object is identical with itself.

So for Prior (40) remains important even though (41) is not defended. Clearly Prior is anticipating here what later came to be called (at least in the case of definite

descriptions) the "referential *vs* attributive" distinction. This distinction was developed and defended by Keith Donnellan [1966] (the *locus classicus* for it) and subsequently criticized by Kripke [1977].

We have now arrived back at the Kripke of 1970s again. Kripke's mature view, to reiterate, is that (41) is false and (40) vacuous, there being no such things as Russellian logically proper names; and this fact is significant in connection with the derivation (1)-(3) because *the derivation has a relevance and significance for Russellian logically proper names that it does not have for Kripkean rigid designators*. I have not here been arguing that Kripke's opinion is correct as against Prior's or anyone else's, but only urging that the multiple issues sometimes conflated under the heading "necessity of identity" should be properly distinguished. The positions of Kripke, Prior, and others need to be clearly understood and distinguished before an attempt is made to evaluate them.[27]

Notes

1 Remark (A) comes in the second sentence after Kripke's statement of the conclusion of the argument as his displayed formula (4), and remarks (B)-(D) come in the second paragraph thereafter. As the preceding sentence illustrates, when citing passages in works more readily available in reprints than in the original, I will generally locate them by internal divisions, such as section and formula numbers, since unlike pagination these remain the same from printing to printing.

2 Except in direct quotations I will use the modern notation →, ⇒, ↔, ⇔ in place of the older horseshoe and fishhook and triple bar and quadruple bar symbols for the truth-functional or material and modal or strict versions of the conditional or implication and the biconditional or equivalence. As the preceding sentence illustrates, when mentioning expressions of formal languages, I will generally let them name themselves, eschewing quotation or quasi-quotation marks.

3 Here Φ may be any formula having free occurrences of a variable z and no quantification on the variables x and y, while x/z and y/z indicate substitution of x and of y, respectively, for all free occurrences of z. (2) is the instance of (4) where Φ is $\Box x = z$.

4 In the sense that (1) & (2) → (3) is a substitution instance of a thesis of classical predicate:

$$(\forall x Px \mathbin{\&} \forall x \forall y(Qxy \to (Px \to Rxy))) \to \forall x \forall y(Qxy \to Rxy)$$

5 The significance of the letter is discussed by James Fetzer and Paul Humphreys in the editorial introduction (p. viii) to their collection [1998]. Marcus declined their invitation to have the letter included in their volume, but she had already by then circulated it widely, the present writer being one of its many recipients. There is a briefer remark tending in the same direction as the passage I am about to quote in the introduction (p. viii) to the collection [Marcus 1993].

6 For a more detailed analysis see footnote 4 of [Soames 1995].

7 See displayed formulas (25) and (26) in [Marcus 1961].

8 Kripke does not mention Marcus, and may not at the time have been aware of her role, since the particular formulation of Quine's that he cites is the one that happens not to mention her. In any case, Kripke really needed Quine's and not Marcus's version, since he was working in a first-order and not a second-order context, with identity as a primitive and not a defined notion.

9 Not everyone who studied with Quine realized that he was the originator of the derivation, since one published source misattributing the derivation (1)-(3) to Marcus is Føllesdal [2004], a reprinting of (a 1963 revision of) Føllesdal's 1961 doctoral dissertation, written under Quine's direction. There seems to have been a miscommunication between the doctoral candidate and his supervisor, since Føllesdal, when giving (on pp. 40-41) a derivation similar to (1)-(3), describes it as a Quinean *reformulation* of the argument of "Miss Barcan", when he ought to call it a Quinean *replacement* for that argument. He then goes on, in describing what is and isn't done in the derivation, to speak of what is and isn't done by "Miss Barcan" rather than by Quine.

10 At the beginning of their chapter 11. Since this is the presentation mentioned in the Marcus letter quoted earlier, it may be mentioned that Hughes and Creswell cite Marcus for the conclusion (3) and give no separate citation for their derivation. This might leave the careless reader with the impression that the derivation is also to be attributed to Marcus; but the careful reader will note that this is not something Hughes and Cresswell explicitly say.

11 The formulations Kripke presents actually require a slight amendment as per Fine [1983], but that matter does not affect any issue under discussion here.

12 Arthur Prior [1956] showed that (6), which unlike (5) is not derivable even in the Barcan system based on **S4**, becomes derivable if the underlying sentential modal logic is strengthened

to **S5**. Indeed, it is enough to have available the "Brouwerian" formula $\Diamond\Box\Phi \rightarrow \Phi$. Having this formula and (3), one can further derive the law of the necessity of distinctness and its contrapositive:

$$\forall x \forall y (x \neq y \rightarrow \Box x \neq y) \qquad\qquad \forall x \forall y (\Diamond x = y \rightarrow x = y)$$

[13] Perhaps the first to see that a linguistic conception of necessity admits at most *de dicto* and not *de re* modality was J. S. Mill, the well-known advocate of the principle that the only necessity is verbal necessity, who also held that there are no individual essences. For him "Philosophers are rational" can be necessary because "rational" is part of the connotation of "philosopher", but "Auguste Comte is rational" cannot be necessary because "Auguste Comte" has no connotation for "rational" to be part of. Of course, Mill here assumes his doctrine that the signification of a proper name consists solely of its denotation, that a proper name has no connotation. Quine's ambition was to make the case without any special assumption about proper names or other singular terms. It is one of the ironies of the debate that many of Quine's opponents thought they could answer his critique by positing Millian proper names, without explicitly arguing, as Kripke was to do, against Mill's assumption that all necessity is verbal. Unless that is done, the assumption of Millian names succeeds in reducing *de re* to *de dicto* modality only at the cost of making *de dicto* modality vacuous or a mystery for *dicta* involving proper names.

[14] Always, that is, after he had arrived at the doctrines expressed in "Identity and Necessity" and *Naming and Necessity*. We will see later that he had *not* yet arrived at them at the time he did his famous technical work on model theory for modal logic.

[15] For another instance, in systems of so-called free logic (whose use is nowadays often advocated in connection with quantified modal logic) there may be constants, but the instantiation rule (26) is rejected in favor of the following weaker rule, with an additional

premise asserting existence for *t*:

from $\forall x \Phi(x)$ and $\exists z\ z = t$ to infer $\Phi(t)$

The second premise may alternatively be expressed as E!*t*, using a special existence or "*ecce*"

predicate. But to pursue this matter further would take us back to distracting issues about

possible nonexistence, which I wish to set aside here.

[16] Even experts, however, sometimes forget this. Marcus in her Boston talk [1961] claims that if

a = *b* (her formula (13)) is true, then just like *a* = *a* (her formula (14)) it is necessarily true. She

further claims that this first claim "is precisely the import of" her theorem on the necessity of

identity (her formula (18), our formula (15)). But the claim about the necessary truth of true

identity statements does not follow from Marcus's theorem without the instantiation rule (29).

[17] Quine, who did not put much weight on the distinction even after 1970, early and late tends to

mention Føllesdal and Kripke in the same breath, as when he says, "Circumstances of this kind

have led Professors Føllesdal and Kripke to posit a special kind of singular term called a *genuine*

singular term or *rigid designator…*" in [Quine 2008, p. 339].

   I have been ignoring, as I earlier said I would, the distracting issue of possible non-

existence, but it is worth mentioning that if one is only taking the box in a *weak* sense, the

argument for (27) requires only that the denotation of *a* and likewise that of *b* should remain

invariant in all worlds in which *a* and *b* exists. A main reason Føllesdal undertook the 1963

revision of his 1961 thesis was to take account of this fact. If one seeks not merely a sufficient

but a necessary and sufficient condition, the obvious requirement is that the denotations of *a* and

b should be actually the same, and should *covary* across the whole range of whatever kind of

"possibilities" are under discussion, so that if the one changes, the other changes in the same

way. A case where we would presumably have covariance without invariance, regardless of what

kind of "possibilities" are under discussion, would be that of synonymous definite descriptions such as "the largest asteroid" and "the largest planetoid".

[18] See (25)-(27) above. This is a kind of derivation that, to reiterate, played no role in establishing (27) for rigid designators by the argument (30)-(33). Now given (28) for *non*-modal Φ, Becker's rule, which is available even in the minimal modal logic **K**, gives us

$$\Box a = b \rightarrow \Box(\Phi(a/z) \rightarrow \Phi(b/z))$$

while

$$\Box(\Phi(a/z) \rightarrow \Phi(b/z)) \rightarrow (\Box\Phi(a/z) \rightarrow \Box\Phi(b/z))$$

is an instance of the sole axiom of **K**. With (27) for the metaphysical reading of $\Box$ we then get

$$a = b \rightarrow (\Box\Phi(a/z) \rightarrow \Box\Phi(b/z))$$

and this is the key step in a proof (by induction on logical complexity of Φ) of the indiscernibility law (28) for Φ containing metaphysical modalities (but no other non-classical logical operators). In this sense, indiscernibility follows from necessity of identity in a Kripkean context, rather than the other way around.

[19] Some recent defenders of Russellian logically proper names (nowadays often called Kaplanian *directly referential names*, from the terminology used in [Kaplan 1989] and related earlier work), such as Scott Soames, have rejected (39) while accepting (38), very roughly on the grounds that in contrast to "it is apriori that…", the context "it is analytic that…", though it does not overtly involve quotation, is covertly a context of mention rather than use. But it will not be needful to go into this issue for present purposes, since none of the early workers on quantified modal logic with whose views I will be concerned anticipated taking such a line.

[20] Russell's distinctive position is discussed by Kripke in the "Naming and Necessity" lectures, note 4, and by Føllesdal in his dissertation, *passim*. It is safe to say that all participants in early

debates about quantified modal logic were well enough aware of Russell's views.

21 Prior's formulations illustrate Quine's complaint that use/mention confusions were endemic in the modal logic of his day. By Quine's lights, one really should not be speaking about *x* and *y* as "naming" or "describing" objects, since variables range over a domain of objects but do not have specific objects as denotations; one should be speaking instead about *instantiating* the variables with "names" or "descriptions".

22 The expressions in question are called "tags" in [1961] and "names in an ideal sense" in [1962]. The original version of [1963], dating from before the *Naming and Necessity* lectures, is more revealing than the rewrite, [Marcus 1974], dating from after. In particular, the passages I will be quoting shortly disappear in the rewrite.

23 While Quine urged that names should be replaced by descriptions or iota-terms and then eliminated by Russell's method, he never held a "descriptivist" theory of names as that label is usually understood. As Delia Graff Fara has reminded the present writer, in §36 of his *Methods of Logic* [1950] Quine explicitly declares himself neutral. If "Pegasus" or "Socrates" is not synonymous with any existing description in the language, we can simply introduce a new predicate "Pegasizes" or "Socratizes", amounting to "is-Pegasus" or "is-Socrates" with the "is" of predication rather than of identity, and replace the names by "the Pegasizer" or "the Socratizer". Kripke in *Naming and Necessity*, footnote 5, explicitly exempts Quine's proposal from his critique of "descriptivism" or "the Frege-Russell theory". He does insist that Quine is only trading the question "How is the reference of 'Socrates' determined?" by the question "How is the extension of 'Socratizes' determined?" but adds "Of course I do not suggest that Quine has ever claimed the contrary."

24 The verbatim transcript of the audiotape of the discussion — though as is usual in such

transcripts it contains many false starts, incomplete sentences, and the like — is in some ways even more revealing than the heavily-edited published version, Marcus et al. [1962]. One noteworthy change between the verbatim transcript and the edited, published version of the discussion is that the word "analytic" in Kripke's question is changed to "necessary". The switch misled Soames [1995, note 26] into thinking that "Marcus responds to a straightforward question about the necessity of identities involving names…with remarks about epistemology and synonymy." In fact, the question to which Marcus was responding was phrased in terms of analyticity to begin with. Fetzer and Humphreys lament their inability to include a verbatim transcript — they had a new one prepared from the original audiotapes — in their collection [1998], but the partial catalogue seems to indicate that Quine's copy of the 1962 transcript is now available to scholars in his *Nachlaß* (folder 673) at the Houghton. Of the two Boston quotations below, the first is from the unedited transcript, the second from the published version, of Marcus's last speech.

25 To be sure, a quarter century later Marcus was to write as follows of the above quoted statement about "having recourse to a dictionary":

> I had in mind here a biographical "dictionary" which as linguists take it are not
> lexical dictionaries but encyclopedias. Such a "dictionary" might not resolve the
> question. Encyclopedias can be mistaken.

(The quoted formulation is from the letter cited earlier, but there are similar remarks in the preface and notes to the reprintings of Marcus [1961] and Marcus et al. [1962] in the collection Marcus [1993].) And this amounts, not merely to a retraction of the endorsement of (39) and even (38), but to a virtual denial that she ever meant to make such an endorsement. But what we have in this autobiographical reminiscence is clearly another instance of an unsuccessful attempt

to reconstruct decades-old thoughts without consulting the full range of contemporary documents. Specifically, Marcus seems not to have consulted the verbatim transcript as opposed to the edited, published version of [Marcus et al. 1962], or the original [Marcus 1963] as opposed to the rewrite [Marcus 1974]. For those sources, from which I have just quoted in the body of the text, clearly show that Marcus meant by "dictionary", as she used the term in Boston, a book that would tell us when two words have the same meaning — what she in Helsinki called a "lexicon", and not an "encyclopedia" — and that she was mentioning the "dictionary" in order to explain in what sense an identity statement would be analytic. No one, of course, explains the notion of analytic truth in terms of what non-lexical information or misinformation is to be found in an encyclopedia that may be mistaken.

26 Soames [1995] suggests that Marcus *would have liked to be able* to claim that ordinary proper nouns are Russellian logically proper names, but was aware of some difficulties or objections, and so wavered between bolder formulations that clearly seem to be about "names" in the ordinary sense and more guarded formulations that almost equally clearly seem to be about "names" only in some idealized sense. The "dictionary" passage seems of the latter kind.

27 The philosophical issues can hardly be satisfactorily sorted out if doxographical issues remain in confusion. Nothing has been more damaging to philosophical understanding of the issues in this area than sensationalistic "revisionist" attempts, of the kind objected to in [Soames 1995], to read doctrines expressed in later writings by Kripke back into earlier writings of Marcus and others. For this sort of thing can be accomplished only by willfully conflating notions and theses — such as various of (35)-(41) above — that crucially need to be distinguished.

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