UNIVERSIDAD DE COSTA RICA
SISTEMA DE ESTUDIOS DE POSGRADO

ANÁLISIS Y FORMALIZACIÓN DE ASPECTOS TEÓRICOS Y EPISTEMOLÓGICOS DE LA TRANSICIÓN ENTRE TEORÍAS CIENTÍFICAS MEDIANTE LÓGICA NO-MONOTÓNICA: EL CASO DE LA TEMPRANA REVOLUCIÓN EN CIENCIAS COGNITIVAS.

Tesis sometida a la consideración de la comisión del Programa de Estudios de Posgrado en Ciencias Cognoscitivas para optar al grado y título de Maestría Académica en Ciencias Cognoscitivas

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Ciudad Universitaria Rodrigo Facio, Costa Rica

2016

Dedicatoria

Para Andrea M. con sincero amor.

Agradecimientos

Quisiera externar mi agradecimiento al Dr. Mauricio Molina, al Dr. Max Freund y al Dr. Daniel Campos por la ayuda brindada a lo largo de esta investigación. Ha sido un privilegio haber contado con la guía de un equipo de impecables y conspicuos investigadores. Sin duda alguna, cualesquiera méritos que tenga esta investigación, son gracias a la paciencia y minuciosidad con las cuales los miembros del comité asesor han guiado este trabajo. A los tres miembros del comité asesor debo externarles mi más sincero agradecimiento por haber sido guías y mentores a lo largo de esta investigación.

Esta tesis fue aceptada por la Comisión del Programa de Estudios de Posgrado en Ciencias Cognoscitivas de la Universidad de Costa Rica, como requisito parcial para optar al grado y título de Maestría Académica en Ciencias Cognoscitivas.

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Resumen

El presente trabajo investiga los límites y alcances del análisis lógico/formal de los principales aspectos teóricos y epistemológicos de las teorías científicas y la investigación científica como empresa epistemológica. Con el fin de poner en contexto lo que se presenta en esta investigación, cabe mencionar que la misma es una contribución a un proyecto más amplio (actualmente en curso) que analiza algunos de los principales sistemas lógico/formales para modelar y explicar el suceso científico de la transición entre teorías científicas, acontecida en lo que se conoce como la temprana revolución en ciencias cognoscitivas. En particular, dicha pesquisa de carácter más amplio, estudia la transición entre los paradigmas del conductismo y el temprano cognitivismo de E. C. Tolman (1886-1959), y su repercusión en el contexto del establecimiento las ciencias cognoscitivas.

En este momento de la investigación, damos cuenta de los prolegómenos teóricos de dicho proyecto de formalización. Asimismo, evaluamos los límites y alcances de los diferentes formalismos, así como las implicaciones de estos en algunos de los debates más importantes de la filosofía de la ciencia relacionados con el proyecto de formalización de diversos aspectos del quehacer científico. En particular, nuestra discusión se inscribe en el contexto de la distinción en filosofía de la ciencia que propone dos momentos distintos en el quehacer científico: el contexto de descubrimiento por un lado, y el contexto de justificación por el otro. En línea con lo anterior, la presente investigación ofrece un análisis sobre dicha dicotomía y argumentos en favor de una interpretación atemperada de dicha distinción y en contra de una lectura tajante de estas dimensiones de la ciencia. En particular, nos basamos en la familia de lógicas no-monotónicas, la dimensión social y colaborativa de la ciencia y en el paradigma de la cognición distribuida como correlato teórico de esta dimensión social de la ciencia.

1. Introducción

El presente trabajo se inserta en el contexto de una tradición de la filosofía de la ciencia que se ocupa por investigar la posibilidad de construir modelos según los cuales la producción de conocimiento científico pueda ser representada y consecuentemente automatizada. Por ejemplo, René Descartes (1596-1650) es uno más célebres hombres de ciencia que se han abocado a tal búsqueda y uno de los más paradigmáticos antecedentes históricos en este debate. El caso de su *Discurso del Método* es sintomático a este proyecto y es un referente teórico del debate en cuestión. En la misma línea, Francis Bacon (1561-1626) con su *Novum Organum* propició una pesquisa similar correlativa a la búsqueda por sentar la base de los principios fundantes de la investigación científica. No obstante, en la contemporaneidad ha persistido -hasta cierto punto- un mayor escepticismo sobre la utilidad y pertinencia de tal búsqueda. Dicho escepticismo con respecto a la formalización de la producción de conocimiento científico puede ser adscrito a filósofos de la ciencia tan dispares como Karl. R. Popper (1902-1994) y Paul Feyerabend (1924-1994).

En el seno de la anterior discusión, surge uno de los más importantes conceptos en el campo de la filosofía de la ciencia, que ha impactado no sólo las principales tesis sobre ciencia y el quehacer científico sino que ha influenciado la naturaleza misma de la filosofía de la ciencia como pesquisa normativa. Al mismo tiempo, dicho concepto ha segregado los problemas y debates que son considerados como problemas y objetos de la filosofía de la ciencia, dejando de lado todo aquel problema, debate o discusión que se tipifique como empírico, contingente o descriptivo; abocándose al estudio y análisis únicamente de la dimensión normativa de la ciencia Es decir, sobre la base de este constructo teórico se distinguen y diferencian las diversas tareas involucradas en la producción del conocimiento científico, y con base en dicha diferenciación, se separan los aspectos de la ciencia que le competen a la filosofía y al análisis lógico de aquellos que no son susceptibles de dicho análisis.

Dicho concepto puede ser rastreado desde Hans Reichenbach, quien en su trabajo de 1938 *Experience and Prediction* sostiene que, por un lado se encuentran los métodos de la psicología que corresponden a los procesos de raciocinio de los seres humanos y por otra parte están los métodos de la lógica que seguirían patrones formales de raciocionio. Este polémico constructo teórico del cual hemos estado hablando es la dicotomía que contrapone el contexto del descubrimiento al contexto de justificación. De acuerdo con esta distinción, el contexto de descubrimiento se relaciona con la dimensión psicológica del descubrimiento (la cual se tipifica como empírica, vaga y difusa), mientras que la epistemología estaría relacionada con los procesos y principios formales según los cuales se validan las teorías, ideas e hipótesis producidas por la dimensión psicológica (la cual se caracteriza cómo precisa, formalizable y que sigue patrones establecidos).

Este mismo concepto fue retomado por Feigl (1965), quien en virtud de la distinción de Reichenbach proponía una categórica e irreductible división de tareas y disciplinas en el estudio de la ciencia similar a la de Reichenbach. De acuerdo con Feigl, la filosofía de la ciencia estaría involucrada en el análisis del contexto de la justificación, mientras que la psicología estaría relacionada con el contexto de descubrimiento. Esta división de *contextos* en el quehacer de la empresa científica fue una noción que influyó en los problemas y las preguntas válidas en filosofía, y al mismo tiempo trazó una imagen de la ciencia, según la cual hay distintos momentos en la producción de conocimiento científico.

La anteriormente mencionada dicotomía, es el objeto central de análisis del presente trabajo. Sobre dicha distinción, se proponen dos argumentos, los cuales apoyan que la distinción propuesta como una dicotomía que contrapone dos momentos distintos e irreductibles del quehacer científico es insostenible. El primero de estos argumentos consiste en señalar un fuerte debilitamiento de dicha dicotomía, basado en la perspectiva de las lógicas no-monotónicas y otros formalismos no clásicos (como la abducción, y lógicas paraconsistentes, etc.) El segundo argumento, propone un debilitamiento de la dicotomía basado en la perspectiva de la cognición distribuida aplicado a las dimensiones sociales intrínsecas al proceso de producción de conocimiento científico.

Habiendo esclarecido el contexto bajo el cual surge y se desarrolla la presente investigación, procedemos a hacer una reseña de los aspectos más importantes de los argumentos propuestos en este trabajo.

2. Debilitamiento de la dicotomía descubrimiento/justificación basada en la lógica no-monotónica y otros formalismos no clásicos.

El primer argumento (desarrollado en el Anexo A), señala que uno de los aspectos básicos y cruciales contenidos en la dicotomía descubrimiento/justificación consiste en sostener una perspectiva bicéfala sobre el tipo de tareas y procesos que acontecen en las distintas etapas del quehacer científico. En particular, por un lado habrían procesos prescriptivos y por otro lado hay procesos descriptivos. Naturalmente, esta perspectiva dicta que, para cada uno de los momentos y etapas del quehacer científico, habrían métodos en instrumentos cabalmente distintos. Por ejemplo, la lógica y los métodos formales y precisos asociados a ella, estarían indefectiblemente ligados a la dimensión prescriptiva del quehacer científico, mientras que de manera irremediablemente opuesta la psicología, la sociología y otra serie de ciencias empíricas y de carácter contingente, serían las que corresponden al análisis y estudio de la dimensión descriptiva del quehacer científico.

No obstante, dicha perspectiva no toma en consideración algunos aspectos cruciales de esta separación tajante de momentos en la ciencia y métodos y disciplinas asociados a estos momentos. En particular, y con un carácter de brevedad en este punto (pero debidamente desarrollado en el respectivo Anexo A) sostenemos que la psicología también estudia procesos racionales de justificación. Por ejemplo, desde la psicología cognitiva existen paradigmas y aproximaciones mediante los cuales se podría modelar/entender este tipo de procesos como el de GENEPLORE de Ward et al. (1995) y Boden (2004 & 2009). Asimismo, sostenemos que no es el caso que la justificación se agote a través de los métodos de la lógica.

De la misma manera, debe acotarse que los mecanismos de descubrimiento también pueden ser abordados desde una perspectiva prescriptiva y los procesos de justificación se pueden abordar descriptivamente, así como hacer prescripción del método de descubrimiento. Estos sostienen que el contexto de justificación sería asequible a través de métodos o modelos lógico-formales (por ejemplo el falsacionismo popperiano que hace uso del *modus tollens* de la lógica clásica), pero que por otra parte, el contexto de descubrimiento no lo sería (i.e. que sería completamente ajeno y fuera del alcance de modelos o sistemas formales).

Nosotros sostenemos que, basados en la perspectiva de la lógica no-monotónica dicha distinción se debilitaría por la siguiente razón: en la lógica clásica la adición de nueva evidencia (en la forma de enunciados) no podría invalidar razonamientos o inferencias previamente establecidas (sería una razón para despachar los instrumentos de la lógica clásica del contexto del descubrimiento tal y como se sostiene), pero ello no es el caso si nos basamos en la lógica no-monotónica, pues este sistema lógico permite precisamente la adición de evidencia (en la forma de enunciados) a una base de conocimiento, de tal modo que los enunciados adicionados revocan enunciados o inferencias previas. Esto último es una de las tareas antonomásticamente asociadas al contexto del descubrimiento, y por esta razón es que podría trazarse un proceso de continuidad entre los ámbitos de descubrimiento y justificación, es decir, a través de la lógica no-monotónica podríamos dar cuenta de procesos y tareas asociados a ambas dimensiones de la supuestamente categórica e irreductible dicotomía de contextos, todo a través de los métodos formales de la lógica. Es en virtud de lo anterior que pensamos que la lógica no-monotónica ofrecería insumos que debilitarían la tajante distinción de la mencionada dicotomía.

Precisamente, uno de los principales motivos por el cual se ha tomado esta lógica para llevar a cabo la tarea en cuestión radica en el hecho de que, los supuestos epistemológicos de dicho sistema lógico están fundamentados en preocupaciones de la cognición humana. Si se recaba en lo anteriormente señalado de Meheus y Nickles (1999) y en línea con Cheng (1996), el recelo por la lógica del descubrimiento científico está fundamentado en la aparente insuficiencia que ha tenido la lógica clásica para dar cuenta de los procesos de raciocinio de la cognición humana (Khemlani & Johnson-Laird, 2013).

Aunado a lo anterior, estudios empíricos evidencian la ineficiencia de la lógica clásica para captar los procesos de inferencia de la cognición humana (Byrne, 1989; Byrne, Espino & Santamaría, 1999) y ello refuerza la línea de investigación según la cual la familia de lógicas no-monotónicas podría solventar dicha insuficiencia (Makinson, 2005a). Es decir, al ampliar la capacidad de la clásica noción de consecuencia lógica, se podría dar cuenta descriptiva e inclusive razón normativa de múltiples procesos de inferencias; tal es el caso de las inferencias en el ámbito de la producción de conocimiento científico y la transición entre teorías científicas.

Ahora bien, es importante acotar que no sostenemos que la lógica no-monotónica sea un canon de producción de hipótesis científicas, pero aun cuando no se sostenga eso, sí mencionamos métodos o mecanismos contemporáneos mediante los cuales se pueden estudiar y analizar este tipo de procesos. Pero abstracción hecha de los mecanismos para generar hipótesis científicas (cosa que no sostenemos que lo supla la lógica no-monotónica), lo que sí sería el caso es que dicha lógica podría acomodar evidencia, enunciados o hipótesis nuevas que puedan ser incompatibles o puedan ocasionar la retractación de razonamientos anteriores, y eso es lo que ofrecería los insumos para debilitar la tajante distinción sostenida por la dicotomía.

Lo anterior hace una revisión crítica de algunos supuestos básicos de la dicotomía descubrimiento/justificación. No obstante, hemos tratado de hacer algunas acotaciones de orden constructivo que ponen en relieve el valor explicativo que contiene la mencionada distinción. En particular, no abogamos por la eliminación total de la distinción descubrimiento/justificación, sino que lo que habría que declinar es la interpretación según la cual la distinción implica una oposición categórica entre dos momentos en la ciencia. Ahora bien, lo que definitivamente debería rescatarse es la distinción como un binomio que alude a dos *enfoques* distintos del mismo proceso de descubrimiento científico.

Por una parte estaría el, *enfoque prescriptivo* o enfoque de justificación, según el cual se abocará a analizar en busca de razones lógicas, retóricas o argumentativas la validación de hipótesis, teorías o conjeturas científicas. Por otra parte, estaría el *enfoque descriptivo* o enfoque de descubrimiento, según el cual, lo primario sería la construcción de mecanismos computacionales del descubrimiento o sociológicos que den cuenta de los diversos factores involucrados en la formulación de hipótesis, teorías o conjeturas científicas. En virtud de lo anterior, señalamos que lo que Popper llama contexto del descubrimiento sería para nosotros un *enfoque descriptivo*, por su parte, y lo que él llama contexto de justificación para nosotros sería un *enfoque prescriptivo* de justificación.

Lo previamente descrito es una síntesis de lo desarrollado con mayor solvencia de espacio y detalle en el *Anexo A*. En la siguiente sección presentamos una similar síntesis del argumento desarrollado en el *Anexo B*, el cual pone en discusión las implicaciones para la dicotomía descubrimiento/justificación del carácter social colaborativo de la ciencia.

3. Debilitamiento de la dicotomía descubrimiento/justificación basado en el paradigma de la cognición distribuida y la dimensión sociológica de la investigación científica.

Este segundo argumento, se inscribe en el contexto de la dimensión social del quehacer científico. Esta denominada dimensión social del conocimiento científico se refiere a las interacciones entre los agentes en que acaece la ciencia como empresa humana. Así, en el marco de la producción de esta modalidad de conocimiento científico es posible discernir un muy importante componente social, de modo que sobre la base de esta naturaleza social del quehacer científico evaluamos algunas de las tesis e implicaciones que se siguen de la dicotomía descubrimiento/justificación.

A grandes rasgos, sostenemos que existe una profunda contradicción en la perspectiva según la cual, esta dimensión social del quehacer científico, no es susceptible al análisis lógico, racional o formal propio del contexto de justificación sino que, corresponde a los métodos y procedimientos propios del contexto de descubrimiento.

De acuerdo con la perspectiva previamente objetada, por un lado estaría el contexto del descubrimiento y por otro lado estaría el contexto de la justificación. Asimismo, esta separación no solamente es de carácter nominal, sino que, dicha dicotomía entraña una separación tanto de objetos de estudio y discusión correlativos a cada contexto, así como los métodos empleados en el estudio de los fenómenos acontecidos en dichos contextos. De este modo, la dimensión social de la ciencia sería parte de los fenómenos de la ciencia que le competen a otra serie de disciplinas distintas a la filosofía de la ciencia, pues esta, dado su carácter estrictamente normativo, no habría de lidiar con una dimensión ampliamente descriptiva.

Sin embargo, el estado de cosas anteriormente retratado implica de alguna manera aceptar una imagen controversial de la ciencia. En particular, si aceptamos lo previamente descrito, estaríamos compelidos a admitir que uno de los componentes cruciales e indefectibles de la producción de conocimiento científico no sería objeto de un análisis formal de reglas y principios que cimientan dicha colaboración. Por el contrario, nosotros abogamos por la tesis según la cual, aun en el contexto social del quehacer científico, existen normas, principios o reglas instanciadas por la misma institucionalidad científica, según las cuales este intercambio colaborativo entre pares se rige, mide y discierne. De no ser el caso que existan este tipo de normas principios o reglas institucionalmente insaturadas, nos veríamos compelidos a admitir una imagen irracionalista del quehacer científico, imagen que es amplia y abiertamente contraria a la perspectiva teórica desde la cual se propuso dicha dicotomía. Así, tendríamos razones para admitir que en el seno de la dimensión social del quehacer científico, el cual inicialmente concebíamos como estrictamente descriptivo, empírico y contingente existen en el fondo patrones normativos socialmente construidos e instaurados y autorregulados. Por ende, la tesis original se vería diezmada en favor de una interpretación más atemperada de la distinción descubrimiento/justificación.

Como corolario de lo anterior, construimos otro argumento en detrimento de otra de las tesis asociadas a la dicotomía descubrimiento/justificación, a saber, la distinción de tareas designadas a los científicos como individuos por un lado y a las tareas asociadas a la institucionalidad científica por otro lado.

Para este argumento, asumimos la distinción descubrimiento/ justificación en su sentido de dicotomía (i.e. la interpretación fuerte de la diferenciación) pero en virtud de ello -y sobre la base del paradigma en ciencias cognoscitivas de la cognición distribuida- mostramos un debilitamiento de este sentido fuerte de la diferenciación.

De acuerdo con el sentido fuerte de la distinción descubrimiento/justificación existirían tareas antonomásticamente asociadas al contexto de descubrimiento por un lado, y tareas asociados al contexto de la justificación por otro lado. El primer tipo de tareas, asociadas con los científicos como nodos individuales dentro de la red colaborativa de la ciencia estarían la recolección de evidencia, formulación de hipótesis, y corroboración/contrastación de teorías etc. El segundo tipo de tareas, tales como validación de teorías/hipótesis científicas, corrección de hipótesis se le suele atribuir a la dimensión de la ciencia como colectivo que comprende una multiplicidad de agentes o nodos individuales que colaboran interactivamente en el avance del conocimiento. Esta dimensión del quehacer científico la denominamos como *institucionalidad científica*.

Ahora bien, desde la perspectiva de la cognición distribuida, la denominada *institucionalidad científica* podría ser abordada como una entidad compuesta por una red de nodos más elementales que serían los individuos particulares. En línea con lo anterior, podríamos distribuir la asignación de tareas basadas en las fases o contextos (sea de descubrimiento o de justificación). En particular, diríamos que la comunidad científica como colectivo de agentes, sería la dimensión encargada de deliberar y validar las teorías científicas y los datos hallados por los científicos como agentes individuales. Así, podríamos decir que, la comunidad científica como colectiva sería parte del contexto de justificación, mientras que los científicos como nodos individuales estarían asociados al contexto de descubrimiento.

Sin embargo, desde esta misma perspectiva, los científicos a su vez representan un todo (i.e. un sistema) y los diversos procesos/mecanismos cognitivos serían nodos de procesamiento más elementales, etc. Así, si tomamos a los agentes científicos como sistemas por sí mismos diremos que en ellos intervienen tanto procesos asociados al contexto de descubrimiento, como procesos asociados al contexto de justificación. En virtud de que, en este análisis del quehacer científico, los nodos del sistema que es la comunidad científica, pueden ser a su vez analizados como sistemas por sí mismos, en los cuales intervienen procesos asociados a ambos contextos, encontramos razones por las cuales, la interpretación fuerte de la distinción descubrimiento/justificación no resulta la lectura óptima. Así, dicha interpretación en su sentido de dicotomía que contrapone dos momentos distintos del quehacer científico se debilitaría. En línea con lo anterior, los aspectos involucrados en la distinción descubrimiento/justificación serían mejor vistos como niveles descriptivos que podrían variar de acuerdo a lo que se define como conjuntos y nodos.

Habiendo expuesto lo anterior, sentamos los prolegómenos del proyecto general bajo el cual se inscribe la presente investigación, y procedemos en la siguiente y última sección a exponer uno de los fenómenos históricos en la ciencia que podría ser objeto de análisis lógico y ulteriormente objeto de modelación formal.

4. La temprana revolución en ciencias cognoscitivas y los estudios de E.C. Tolman sobre los mapas cognitivos: construcción de una base de conocimiento.

Como se ha mencionado previamente, los temas y debates analizados con anterioridad se enmarcan en el seno de una investigación más amplia que busca analizar los límites y alcances de la formalización de los principales aspectos de las teorías científicas y la transición de las mismas. Así pues, todo lo anterior ha de ser visto como un análisis prolegomenal de las condiciones de posibilidad de llevar adelante un esfuerzo de esta naturaleza. En el seno de la aludida investigación más amplia, nos abocamos por el estudio de un caso particular en la historia de la ciencia, a saber, la *temprana revolución en ciencias cognitivas* como un caso paradigmática de transición y cambio entre teorías científicas.

La denominada *temprana revolución* en ciencias cognoscitivas alude a la transición del paradigma conductista hacia el paradigma mentalista en psicología. En particular, analizamos el periodo en el que las posiciones anti-mentalistas de los modelos conductistas en boga durante la primera mitad del siglo XX, dan paso a la postulación de estados internos y de las llamadas variables intervinientes. Dicha transición es estudiada en el contexto de las investigaciones de E. C. Tolman (1886-1959), desarrolladas entre los años 1930 y 1960, las cuales junto con los trabajos de Hull (1884-1952), aun dentro de la tradición conductista recurren a este tipo de estrategias más propias de la psicología cognitiva posterior. Ahora bien, tanto las causas como los efectos de esta *revolución* rebasan el campo de la psicología, incidiendo en disciplinas afines a la cognición humana. En este sentido, utilizamos los términos *revolución en ciencias cognoscitivas* y *revolución en psicología* de manera intercambiable.

El mencionado episodio en la historia de la ciencia, además de ser relativamente reciente, representa un caso icónico de transición entre paradigmas científicos. Asimismo, es importante recalcar que este fenómeno de transición entre teorías científicas, además de ser un momento clave en la historia de la psicología, es uno de los capítulos que reafirma a las ciencias cognoscitivas como disciplina científica propiamente, mediante el restablecimiento del mentalismo como supuesto científico válido.

Ahora bien, para analizar este episodio en la historia de la ciencia, recurrimos al esquema y la taxonomía desarrollada por K.B. Madsen (1988), quien en su obra *A history of Psychology in a Metascientific Perspective* ofrece un esquema tripartito bajo el cual se puede analizar diversas teorías científicas. Dicho esquema consiste en discernir, para cada teoría o escuela científica tres estratos distintos: el meta-estrato, el estrato de las hipótesis y el estrato de los datos.

En primer lugar se identifica el meta-estrato, en el cual se ubican las tesis filosóficas más generales que subyacen a los diversos programas de investigación. En segunda instancia, se postula el estrato de las hipótesis, en el cual se ubican los enunciados, hipótesis y explicaciones científicas que identifican a los diversos programas de investigación. En este sentido, se dice que en el estrato de la hipótesis se ubican los aspectos más centrales y característicos de las teorías científicas. El tercer y último estrato se denomina el estrato de los datos, se ubican la información más concreta y específica de las teorías científicas, i.e. los datos, hechos y demás información recabada en el núcleo de los diversos programas de investigación científica.

En línea con el trabajo de Madsen (1988), en esta etapa de la investigación, hemos llevado a cabo un detallado análisis de las principales perspectivas científicas y meta-científicas, así como de las tesis tanto del conductismo como del temprano cognitivismo de los trabajos de E.C. Tolman de 1948. Asimismo, cabe mencionar que dicho análisis ha sido llevado a cabo a la luz del esquema conceptual y la taxonomía propuesta en la obra de K.B. Madsen (1988). Esto con el fin de hacer una reconstrucción lo más precisa de la base de conocimiento de cada uno de los mencionados paradigmas científicos. Sin embargo, a diferencia del conspicuo y detallado trabajo de K.B. Madsen, nuestra investigación ha sido enmarcada en el contexto de la construcción de una base de conocimiento (desarrollado en el artículo del Anexo C) de las diferentes etapas de la empresa científica, a saber, tanto del descubrimiento, como de la justificación y teniendo en mente la posibilidad que dicha base de conocimiento pueda ser el insumo de la construcción de un modelo formal de este episodio en la historia de la ciencia.

Así, en el presente trabajo se busca sentar las condiciones de posibilidad que permitan la aplicación de recursos propios de la lógica formal y otros paradigmas de la ciencia cognitiva al problema de formalizar y modelar la transición entre dos programas de investigación científica, tópico que está ausente en la obra de Madsen. Por ende, nuestra iniciativa, se puede comprender como un esfuerzo que complementa el trabajo de Madsen (1988).

5. Conclusiones.

En la presente investigación nos hemos abocado por dar cuenta de las condiciones de posibilidad de la formalización y automatización de los diversos aspectos y dimensiones que involucran las teorías científicas y la transición entre las mismas. En línea con lo anterior, hemos transitado por la discusión de las implicaciones de los modelos lógico-formales para el estudio de algunos de los procesos de la cognición humana (en la presente investigación el conocimiento científico), y hemos puesto de manifiesto la relevancia que tiene las propiedades de estos sistemas formales (como por ejemplo la no-monotonía, paraconsistencia, etc), y sus tesis filosóficas subyacentes.

En particular, hemos analizado el problema de la formalización de algunos aspectos de las teorías científicas en el contexto de uno de los conceptos más arraigados en una predominante tradición de filosofía de la ciencia, como lo es la distinción entre el contexto de descubrimiento y el contexto de justificación.

En un primer momento, hemos puesto bajo escrutinio la interpretación fuerte de la distinción descubrimiento-justificación en el quehacer científico. En línea como ello, señalamos que esta diferenciación en su sentido dicotómico (en el sentido de que existen dos dimensiones antagónicas en el proceso de descubrimiento científico) se ve debilitado si tomamos en consideración una serie de formalismos no clásicos. En línea con lo anterior, hemos propuesto lógicas no-monotónicas, lógicas paraconsistentes y algunos otros formalismos no clásicos desde los cuales, la mencionada dicotomía no sería plausible.

En un segundo momento, hemos traído a discusión, en el mismo contexto de la distinción descubrimiento/justificación en filosofía de la ciencia, la dimensión social del quehacer científico. En este apartado de la investigación, hemos puesto en discusión los límites y alcances de la mencionada dicotomía en virtud del carácter intrínsecamente social, colaborativo y multi-agente de la empresa científica.

En virtud de este rasgo característico de la ciencia hemos postulado que despachar el análisis de la dimensión social de la ciencia al contexto de descubrimiento por mor de su carácter difuso, ambiguo y contingente, deviene en una postura contraria al ideal mismo de ciencia que se promueve en el seno de la filosofía de la ciencia que propuso dicha distinción. En particular, si despojamos al análisis de la dimensión social de la ciencia de los métodos, instrumentos y procedimientos propios del contexto de la justificación, se terminaría defendiendo una imagen relativista e inclusive irracionalista de la ciencia misma. Asimismo, y como corolario de la analizada dimensión social del quehacer científico, hemos analizado la plausibilidad de la distinción descubrimiento/justificación en virtud del paradigma de la cognición distribuida.

Cabe mencionar, como lo hemos recalcado a lo largo del presente trabajo, que nuestra postura no es la de despachar por completo la diferenciación descubrimiento/justificación en filosofía de la ciencia, sino que hemos tratado de evidenciar la existencia de dos sentidos distintos de dicha diferenciación: el sentido fuerte o dicotómico, y el sentido moderado. Así, rescatamos el valor explicativo de la distinción en su sentido moderado, i.e. en tanto distinción según la cual pueden haber diferentes *enfoques* y *aproximaciones* dentro del quehacer científico, pero señalamos la inconsistencia de dicha distinción en su sentido dicotómico, i.e. en tanto que separa de manera categórica e irreductible dos momentos en el proceso de descubrimiento científico.

Finalmente, abordamos el análisis del episodio de la historia de la ciencia denominado la *temprana revolución en ciencias cognitivas*. En particular, hemos aplicado la taxonomía y esquema de análisis propuesto por K.B. Madsen en su obra de 1988, con el objetivo de construir una base de conocimiento tanto del conductismo como del temprano cognitivismo de E.C. Tolman, específicamente en contexto de su trabajo de 1948 sobre mapas cognitivos en ratas y seres humanos. En particular, hemos llevado a cabo dicho análisis y reconstrucción de los principales aspectos de cada uno de los programas de investigación (el conductismo y el temprano cognitivismo de E.C. Tolman) con el fin de que este puede ser usado en la construcción de un modelo formal de este episodio en la historia de la ciencia.

Así, el trabajo prolegomenal que se ha llevado a cabo con el escrutinio de los límites y alcances de la dicotomía contexto de descubrimiento versus contexto de justificación, se presenta necesario en orden al estudio de un formalismo óptimo para modelar/representar la transición entre teorías científicas acontecida en el desplazamiento desde el conductismo al mentalismo tolmaniano.

En virtud de lo anterior, la presente investigación complementa iniciativas previas (por ejemplo, Madsen 1988) en las que se dilucidan los estratos científicos y meta-científicos, y propone la formalización de la transición de las teorías científicas previamente mencionadas, con el fin de analizar los límites y alcances de la posible formalización de estos aspectos para el caso de la temprana revolución en ciencias cognitivas.

A modo de conclusión, en el marco de la presente investigación deseamos poner en relevancia que en el tránsito desde los sistemas lógicos formales a modelación de distintos aspectos y/o procesos de la cognición humana (como por ejemplo el descubrimiento científico) no se limita a la mera descripción de un fenómeno en particular, sino que conlleva la siempre latente posibilidad de implementar computacionalmente dichos procesos. Así, los diversos formalismos lógicos conllevan intrínsecamente la disciplina del descubrimiento científico automatizado, en el cual, los modelos formales adquieren conspicua relevancia.

Como corolario de esta etapa de la investigación, queda abierta la posibilidad de establecer un vínculo entre el problema de la lógica del descubrimiento científico y los procesos de raciocinio involucrados en la producción de conocimiento científico. Así, se pretende elaborar un aporte que enlace las anteriores dimensiones, con el estudio de un caso en particular, a saber la temprana revolución en las ciencias cognoscitivas.

Anexo A

Lógica no-monotonica, formalismos no-clásicos: implicaciones para la dicotomía descubrimiento/justificación.

En este artículo se desarrolla el primer argumento que comprende esta investigación, en la cual se señala un fuerte debilitamiento de la dicotomía descubrimiento/justificación basado en la perspectiva de las lógicas no-monotónicas y otros formalismos no clásicos (como la abducción, y lógicas paraconsistentes, etc.).

The discovery/justification context dichotomy within Formal and Computational Models of Scientific Theories: a weakening of the distinction based on the perspective of non-monotonic logics.

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Abstract: The present paper analyses the topic of scientific discovery and the problem of the existence of a logical framework involved in such endeavor. We inquire how several non-monotonic logic frameworks and other formalisms can account for such a task. In the same vein, we analyze some key aspects of the historical and theoretical debate surrounding scientific discovery, in particular, the context of discovery and context of justification context distinction. We present an argument concerning the weakening of the discovery/justification context dichotomy based on the descriptive accent contained in the non-monotonic logic perspective together with its epistemological concerns.

Keywords: philosophy of science, logic of discovery, scientific models, scientific method, scientific discovery, context of justification, context of discovery, cognitive science, scientific revolutions, non-monotonic logic, defeasible logic.

1. Introduction

One of the core concerns regarding the process of scientific discovery is the rationality of such process. That is, whether the process of such knowledge production is a rational one, or if it is beyond the realm of rationality and belongs to the mysterious genius of a few selected individuals. In the aforementioned dilemma there lies a deep contradiction due to the fact that none of the historical figures in the field of science can be regarded as accidental figures with only luck on their side (this is something to which most, if not all, philosophers and especially scientist themselves would dispute), but if the reasoning process of scientific discovery occurs as a sophisticated reasoning process, then it should be rational and systematic procedure (Alai, 2004).

Hence, towards the problem of scientific discovery, one is able to find two key aspects. First, we have profound esteem and intellectual respect to the great minds which have proposed the great body of knowledge that is science and we also believe that these great minds did not achieve what they did on a mere account of luck. Second, there is a long list of philosophers and scientists who have disregarded the possibility that the process of scientific discovery has a specific logic or set of rules, which can account for such a rational endeavor. These two perspectives leave the door open solely to the mysterious geniality of some scientific minds as an explanation of the process of scientific discovery and the great achievements of it.

The aforementioned state of affairs leaves many doubts of whether this is necessarily the case. Furthermore, it seems that there is plenty of room to approach this debate trough contemporary advances in long-standing disciplines such as logic, philosophy of science and even from novel disciplines such as computer science and cognitive science. In this paper, we analyze some advances and efforts within a specific debate concerning the problem of scientific discovery, namely, the problem of whether there exists or can exist a logic of scientific discovery. We frame this inquiry in the context of one of the most influential distinctions in philosophy of science, which is the dichotomy that contrasts and opposes two dimensions within scientific endeavor: the context of justification and the context of discovery. We will further analyze the impact this dichotomy has had in the construction of logic and computational models of scientific discovery. In virtue of the aforementioned survey, we propose an argument concerning the weakening of the discovery/justification context dichotomy based on the perspective of non-monotonic logics and similar non-classical logical formalisms. Before tackling our core object of inquiry, we provide a very brief historical sketch surrounding some of the relevant aspects of the debate regarding the problem of scientific discovery.

We start our survey recalling that during the seventeenth and eighteenth centuries, there was a positive attitude towards the existence of a logic of scientific discovery and/or a set of rules leading towards scientific discoveries, to this aspect, one could point a long and exemplary series of philosophers and scientists that like Rene Descartes (1596-1650) and

Francis Bacon (1561-1626), all of which embraced the search for a formal and abstract systematization of the crucial rules concerning the production of scientific knowledge. Nevertheless, during the nineteenth century, this optimism shifted towards the opposite side, in which the prevailing attitude was that scientific discovery is not and cannot be grasped by a set of mechanical rules, or logical principles (Meheus & Nickles 1999). This latter period would ground the historical antecedent that led to the somewhat romantic view of scientific discovery as only available through insights and scientific genius.

In line with this historical antecedents of the problem of scientific discovery, Gottlob Frege (1848-1925) is one the most influential philosophers in analytical philosophy who portrayed and spread the view that there is no room for a logic of discovery and that the aim of logic itself is the problem of deduction, which has nothing to do with the intricate process of scientific discovery and/or its method. The impact Frege's view had on the topic of the logical method of scientific discovery was deeply rooted in his view towards the anti-psychological nature of logic and in virtue of that, Frege defers the study of scientific discovery to the field of psychology withholding its inquiry in logical terms (Celluci, 2013). In the same line, Tarski (1994) held the view that there is no way of merging the study of logic and the procedure or methods employed in the process of scientific discovery.

One of the most influential movements that carried with the previously mentioned conviction was a highly influential group of philosophers and scientists established during the first half of the twentieth century known as the Vienna Circle. Among the various themes and concerns surrounding this intellectual group, the problem of scientific discovery was systematically left behind due to its elusive character and reinforced the "fregean" towards the scientific method. This state of affairs prevailed over a long period of time and has been preserved into our current set of open debates through the analytical tradition of philosophy, which was undisputedly influenced by Vienna's Circle (Noé, 1998).

In the same context as the Vienna Circle, Karl Popper's view on philosophy of science has played a crucial role. Although Popper very much disputed a large array of tenets held by the logical positivists, he did help spread the perpetuation of the attitude towards scientific discovery that completely disregards the existence of any logic of scientific discovery (Celluci, 2013). Hence, the view of scientific discovery as foreign to a logic has an overwhelming and boldly uncontested tradition in analytic philosophy.

Nevertheless, the whole attitude towards scientific discovery inherited by the logical positivists and Popper changed radically thanks to an emerging and opposing movement in the philosophy of science. In the 1960's the prevailing trend in philosophy of science shifted somehow by the appearance of a critical movement within philosophy of science, which opposed the views entertained up to the moment by the logical positivists and Popper concerning the nature of scientific discovery. This opposing view, which was composed of authors such as Norwood Hanson, Thomas Kuhn, and others, approached the sociological dimension of the topic of scientific discovery, which at the time was a whole new perspective that was historically unaddressed (Noé, 1998). This critical school of the previously established philosophy of science contested a long range of assumptions that both the positivists and Popper held regarding scientific endeavor. Among the new approaches to previous longstanding problems in philosophy of science was the treatment of scientific discovery.

Although this contesting movement of philosophy of science did not hold a positive view towards the existence of a logic of scientific discovery (they did not uphold the view that scientific discovery can be subject to some rules or sound logical heuristics) it did served the logic approach a great deal by reinstating the topic of scientific discovery and its method as an important subject within philosophy of science, which as we said before, was deeply unaddressed (Noé, 1998).

Be it as it may, the very succinct picture we have previously sketched somehow depicts in a broad sense a twofold approach towards the main topics and aspects of scientific discovery. One of the most important theoretical distinctions in philosophy of science emerges with the previous historical background. This matter will be the topic of the next section.

2. The discovery/justification context dichotomy in philosophy of science.

In the 20th century a very influential distinction regarding scientific inquiry emerged, such distinction was based on the existence of two different and opposing dimensions within scientific endeavor: the process of producing scientific theories and hypotheses on the one hand, and the aspects concerning the validation and/or justification of such findings on the other. According to this differentiation, proposed by Hans Reichenbach in his work of 1938 *Experience and Prediction* one dimension of science is the process of conceiving new ideas and theories, and a different dimension is the process of grounding and justifying those ideas. The former is denominated the context of discovery and the latter is the context of justification. As Reichenbach (1938) states it:

> There is a great difference between the system of logical interconnections of thought and the actual way in which thinking processes are performed. The psychological operations of thinking are rather vague and fluctuating processes; they almost never keep to the ways prescribed by logic and may even skip whole groups of operations which would be needed for a complete exposition of the subject in question. ... It would be, therefore, a vain attempt to construct a theory of knowledge which is at the same time logically complete and in strict correspondence with the psychological processes of thought. […] The only way to escape this difficulty is to distinguish carefully the task of epistemology from that of psychology. Epistemology does not regard the processes of thinking in their actual occurrence; this task is entirely left to psychology. What epistemology intends is to construct thinking processes in a way in which they ought to occur if they are to be ranged in a consistent system; or to construct justifiable sets of operations which can be intercalated between the starting-point and the issue of thought-processes, replacing the real

> intermediate links. Epistemology thus considers a logical substitute rather than real processes. (5)

This discovery/justification context dichotomy would boldly influence the field of philosophy of science, as it would shape out some of the key debates in philosophy of science such as whether there can be a logic of scientific discovery or what was the role of philosophy in regards to scientific endeavor. Furthermore, the distinction would be retaken from its conception in Reichenbach's works of 1938 all through contemporary philosophy of science. For example, Feigl (1965) underscored the irreducible and categorical differentiation among the different dimensions of scientific enterprise:

> There is a fair measure of agreement today on how to conceive of philosophy of science as contrasted with the history, the psychology, or the sociology of science. All these disciplines are about science, but they are 'about' it in different ways. ... In the widely accepted terminology of Hans Reichenbach, studies of this sort pertain to the context of discovery, whereas the analysis pursued by philosophers of science pertain to the context of justification. It is one thing to ask how we arrive at our scientific knowledge claims and what socio-cultural factors contribute to their acceptance or rejection; and it is another thing to ask what sort of evidence and what general, objective rules and standards govern the testing, the confirmation or disconfirmation and the acceptance or rejection of knowledge claims of science. (472)

Based on the previously historical formulations and precisions of the discovery/justification context dichotomy, one can point out that it grounded the view that there is no room for logic regarding the process of scientific discovery or its method, but only in the context of justification such discussions and methods were legitimate. Furthermore, epistemology and philosophy of science should only concern to the rational reconstruction of the context of justification, since its discovery counterpart would be an object of psychology or some other descriptive enterprise, but it would certainly not be an object of inquiry in the highly normative field of philosophy. In the same vein as Feigl, Karl R. Popper conspicuously reinstated the dichotomy in one of his most influential work concerning the very core discussion of the existence of a logic of scientific discovery, in which he stated the following:

> The initial state, the act of conceiving or inventing a theory, seems to me neither to call for logical analysis not to be susceptible of it. The question how it happens that a new idea occurs to a man— whether it is a musical theme, a dramatic conflict, or a scientific theory—may be of great interest to empirical psychology; but it is irrelevant to the logical analysis of scientific knowledge. This latter is concerned not with *questions of fact* (Kant's *quid facti*?)*,* but only with questions of *justification or validity* (Kant's *quid juris*?). Its questions are of the following kind. Can a statement be justified? And if so, how? Is it testable? Is it logically dependent on certain other statements? Or does it perhaps contradict them? […] Accordingly I shall distinguish sharply between the process of conceiving a new idea, and the methods and results of examining it logically. As to the task of the logic of knowledge—in contradistinction to the psychology of knowledge—I shall proceed on the assumption that it consists solely in investigating the methods employed in those systematic tests to which every new idea must be subjected if it is to be seriously entertained. (2002, 7-8)

At this point, it is important to make a clear statement concerning the scope and limitations of the notions surrounding the discovery/justification dichotomy or distinction. Throughout this paper, we will make a difference concerning the terms ‘dichotomy’ and ‘distinction’. In the sense of a discovery/justification *dichotomy*, both discovery and justification refer not only to different process and/or tasks but it also refers to different moments in scientific endeavor, which are ultimately irreducible. To the contrary, in the sense of a discovery/justification *distinction*, both dimensions would make reference to different processes involved in scientific inquiry but they would not imply the bold and irreducible differentiation of both dimensions. Up until now, we have presented in a very succinct manner the discovery/justification context debate, the two senses of such differentiation (i.e. 'dichotomy' and 'distinction') and its supposed mandate for different questions and methods concerning the different dimensions of scientific endeavor. Nevertheless, some of the basic presumptions are highly debatable in the context of philosophy of science, cognitive science, and logic.

It is important to point out how one can read a strong sense of the discovery/justification distinction in the previously surveyed philosophers of science. For example, Reichenbach's stance is undoubtedly a strong version of the distinction, i.e. he would be placed among the authors who defend the dichotomy aspect of the distinction. This is due to his characterizations of the inner process of scientific reasoning as "vague", "fluctuating" and that it "almost never keep to the ways prescribed by logic". In the same quite strong and bold position, one can find Popper's view concerning the process of producing scientific knowledge which he characterizes as being "in contradistinction to the psychology of knowledge". Therefore, one can arrive at the conclusion that in the context in which the dichotomy emerged, the prevailing approach to such distinction was grounded in the view that the two dimensions of the process of scientific inquiry were not only categorically apart but that they were opposing moments in the production of scientific knowledge and each dimension should be addressed with very distinct criteria and methods. Based on the above precisions, we can sketch such dichotomy as follows:

Table1. Main distintions between Context of Discovery and Context of Justification.

Context of Discovery	**Context of Justification**
1. Vague. 2. Irrelevant to logical analysis. 3. Subject of psychology and /or other empirical sciences. 4. Does not follow any set of normative pattern or other formal rules. 5. Involves historical, social and other ambiguous aspects.	1. Precise. 2. Relevant to logical analysis. 3. Subject of Philosophy of science as a normative and prescriptive discipline. 4. It follows a set of normative pattern or other formal rules. 5. Does not involve in any form or manner historical, social and other ambiguous aspects.

With such background, in the next section, we will address some basic problematic issues concerning the dichotomy of contexts presuppositions, and we will construct a taxonomical sketch of such dichotomy in the context of formal models of scientific knowledge.

3. Some general debates surrounding the discovery/justification dichotomy and the construction a taxonomy of such dichotomy in the context of formal models.

First, we want to point out that one of the core topics concerning the aspects of the context of discovery lies in the comprehension of the nature of creativity in the context of scientific inquiry. This is due to the fact that the extent and limitations of understanding and -even more- modeling such a reasoning process can account for the acceptance or dismissal of the discovery/justification dichotomy. To this matter, it can be stated that creativity, in the context of scientific inquiry, can generate a myriad of plausible ideas but scientists as a norm do not take them up to inspection on a random or careless basis. Contrary to the above, there seems to be a decision making process that rules some out and keeps others, by its very nature, this decision making process of ruling hypothesis or ideas out and keeping others must have some sound criterion by which the process goes along no matter how creative the roots of those ideas can be (Celluci, 2013).

In particular, the model of Ward, Finke & Smith (1995) GENEPLORE, provides a framework under which such psychological creative process can be understood. According to the GENEPLORE model, the creative process is divided into two main phases. On a first phase, an individual generates an idea. To this aim, one only needs to search for simple cognitive processes such as recalling information from previous experiences or any other sort of elementary cognitive abilities. The outputs of this phase are the building blocks that are involved in the later phase. On the second phase, the individual explores the creative and combinatorial possibilities attached to the ideas previously produced. According to Picciuto & Carruthers (2014), this twofold model can shed light towards the emergence and development of creative processes in human infants and to the existence of such faculties in other non-human animals. Hence, in virtue of this framework of understanding the creative component in human reasoning, one can draw the thesis that, for the production of new hypotheses or theories, in the context of scientific discovery (a particular dimension of human creativity), there is no need to rule out any form of systematic rule following schema or rational approach, such as tree searches, conceptual frameworks, production systems, etc. This would leave the possibility for a rational approach to understand and model inferences involved in scientific discovery.

In line with the above, and despite the creative ingredient that might be involved in the production of knowledge -even in the realm of scientific knowledge- one can hold the acceptance of a rational scheme in this reasoning process based on the fact that even such creative components of scientific inquiry are subject to a problem-solving procedure that requires the proposal of inferences (Meheus, 1999). Hence, based solely one of the key ideas of this theoretical framework of cognitive psychology one finds grounds to question the boldness contained in the strong interpretation of the discovery/justification distinction (for example, Reichenbach's interpretation). This last thesis would be supported by the fact that even the creative dimension of scientific reasoning process would be subject to some form of rational and precise analysis, which stand in direct contradiction of what is stated in the classical and strong version of the distinction.

Leaving the ‘discovery’ side of the debate, there remains the important discussion concerning the context of justification in the studied dichotomy. One of the main topics of discussion regarding the problem of the justification in the context of scientific inquiry lies in the possibility of modeling this process, whether it is through the methods of classical logic or some other formal systems: or if this side of the scientific enterprise is doomed to be far beyond any rule-following, systematic and rational approach. To this matter, it has been pointed out that the process involves inferences in which a conclusion is derived or sought to derive on the basis of previous knowledge or evidence (Cherkassky, 2012). Furthermore, those inferences must be ground on some sound and valid procedure. Therefore, if at the core of the process of justification of scientific theories lies the production and manipulation of inferences and other human reasoning mechanisms, it follows that there has to be some kind of rule-following procedures or rational deliberation at least that lies beneath this inference process conducive to scientific discovery. Furthermore, one can point out that in the context of this debate, several frameworks such abduction and induction severely diminishes the strong and clear-cut dichotomy interpretation of the discovery/justification distinction. At this stage of our paper we only point out the subject matter, in later sections, we shall undertake the issue with more detail.

Following the previous discussion, there is also the question of whether any set of mechanisms or rules involved in the context of justification is congruent with classical logic. To this matter, it has been pointed out by Meheus (1999) that classical logic does not provide the tools and resources to cope with the reasoning process involved in much of the scientific deliberation and this is in part due to the fact that information involved in this process is more often than not either incomplete or inconsistent and these two aspects are not fully approachable from the standpoint of classical logic. But, the fact that standard or classical logic is not sufficient in the context scientific inquiry, does not implies that any other logical formalism whatsoever will also fail to grasp this aspect of human reasoning or that scientific deliberation is foreign to logic at all. But if this is the case, then again, we find reasons and evidence that displace the bold sense and interpretation of the discovery/justification dichotomy. That is, if the justification process is restrained by contextual contingencies such as limited cognitive resources or some other form of knowledge constraints (due to some external or internal limitation), and even more, the presence of inconsistent information, then the ideal presented in the so-called strong interpretation of the distinction would be severely undermined, because from the strong interpretation of the discovery/justification distinction the process of justification is not subject to such contingencies. Hence, if the non-classical logic frameworks better capture the deliberation process involved the context of justification, the sense of such dichotomy would be displaced in favor of a more tempered one.

In the context of the aforementioned debate concerning the nature of these systematic approaches to scientific deliberation, one crucial aspect lies in how one view the steps involved in the justification of scientific theories. If one regards them as rational procedures foreign to logic then one must question what kind of logic one has in mind to have such as stance. If one's view of logic is restrained to classical logic then various reasoning processes will be categorized as foreign to logic. Therefore, it stands out that one's view of logic is crucial to the methodological analysis of the process of scientific discovery (Meheus, 1999).

As example of the debate surrounding the context of justification within scientific inquiry, Popper (2002) did held the conviction that this aspect of scientific inquiry can be scrutinized in terms of logical procedures, specifically and according to Popper falsasionism can give an account of the process that lies at the core of the context of justification. Contrary to the above, one can point out how Kuhn (1970) held a very skeptical attitude towards the existence of a logic procedure concerning the comparison, contrasting and weighing of the different scientific theories, views or research programmes (i.e. aspects concerning scientific revolutions which can be said to belong to the context of justification). Kuhn (as other philosophers of science of with similar approaches) was more inclined towards historical and sociological explanations of the processes involved in scientific deliberation, which according to him would give a more rich and precise picture of what really happened in the process of scientific inquiry.

Following the aforementioned debate, one can point out that there is another alternative between the opposing views (Poppers defense of classical logic and Kuhn's sociological account) regarding the process of justification. For example, there are approaches to this aspect of scientific inquiry that although not belonging to sociological or historical accounts give a rational schema according to which this dimension of scientific inquiry can be scrutinized.

In line with the above, there are *rhetoric* approaches to deliberation that although do not fall under the procedures and method of classical logic, they do provide a rational and systematic framework for understanding such processes involved in the context of justification, that is, the stage of scientific discussion in which theories and statements concerning those theories are scrutinized in order to validate those theories. The deliberative or rhetoric approach towards the problem offers a halfway solution that lies between the logical/sociological dichotomy previously remarked.

One paradigmatic example of such rhetorical approaches to argumentative deliberation can be traced back to Aristotle's *Rhetoric* (1990), in which the Greek philosopher undertakes the systematic study of the art and means of persuasion in a sound and demonstrative manner. In the aforementioned work, he posits the *enthymeme* as one of the core structures under which the persuasive enterprise is carried out (1354a1-4). Aristotle understands such deliberative and persuasive task as *rhetoric demonstrations* or *rhetoric syllogisms* (1355a4-8). Nevertheless, Aristotle himself points out the elusive nature of enthymemes from the perspective of a logical structure and how its value lies between the notions of truth and plausibility (1355a16), which is far apart from the exact nature the syllogism of the *Analytics*. Despite the elusive nature of enthymeme's Aristotle does not dismiss its theoretically demonstrative capability. Hence, one can characterize the deliberative task concerning the justification of the scientific enterprise with the same elusive character as Aristotle had towards the *rhetoric syllogism*, without sacrificing any form of systematic or rational foundation.

Once again, in light of the previous perspective, one can point out that the discovery/justification as *a bold dichotomy* seems diminished. Supporting evidence for such stance lies in the fact that this kind of rhetorical/argumentative approach to rational deliberation (even in the context of scientific inquiry) acknowledges being far from the kind of irrevocable and infallible nature of demonstrative knowledge, given space to some degree of retractability. That is, even the justification context of scientific knowledge is far from the depiction made in the classical interpretation of the discovery/justification distinction.

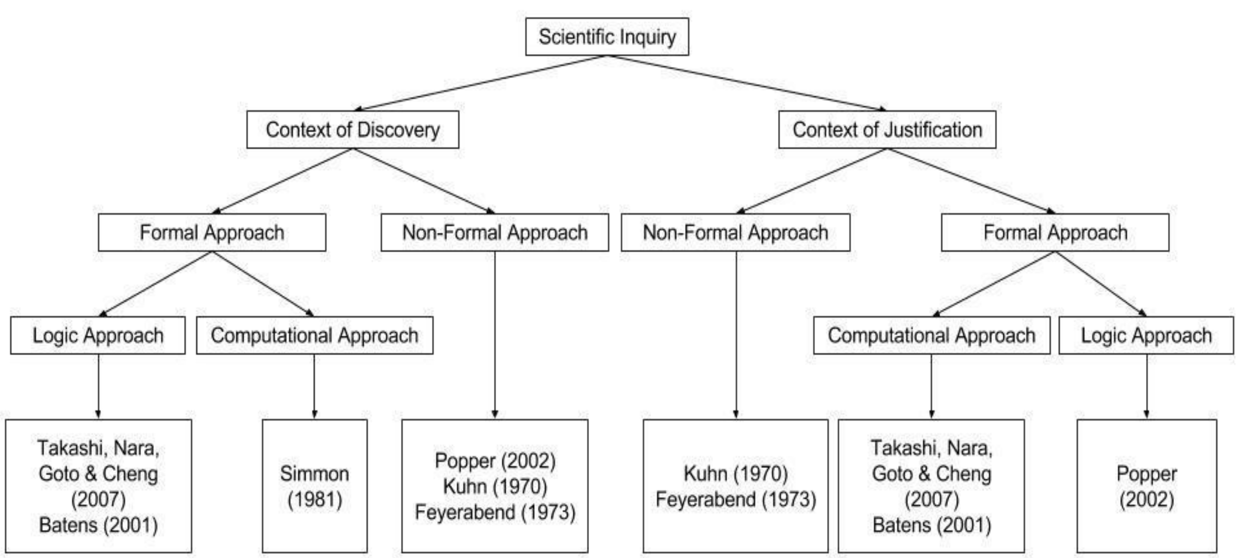
Lastly, it is of substantial importance to mention one of the core debates surrounding the discovery/justification dichotomy, and that is: the existence of a logic, a formal framework or any form of rule-following and systematic approach for tasks associated with each of the dimensions. As we mentioned earlier, traditionally it is held that one can apply such formal and logical methods to the context of justification, but such methods shall not be applied in the analysis and scrutiny of the context of discovery. Naturally, one key aspect of such stance lies in the extent and limitation of notions such as logic, logic framework, and formal systems. Hence, before drawing the final remarks concerning the expositions of the dichotomy and some of the key aspects surrounding such distinction, we must mention some differences to be made regarding the notion of a logic framework and a formal system.

As Schickore, (2014) has pointed out, there is a clear distinction between heuristic procedures and analytical algorithms. Examples of the former are statistical methods, production systems and in general, mechanisms to which, a precise solution to a given problem is far from being guaranteed, nevertheless this kind of tentative search and the problem-solving procedure is more efficient than the analytical algorithms counterpart. On the other hand, analytical procedures to problem-solving are much more exhaustive and furthermore, given a problem it will brute force all the possibilities in search for the appropriate answer. The downside of the analytical way is that its applicability if far narrower than the approach of heuristics. To this matter, it is necessary to point out the fact that a combination of heuristics plus analytical procedures can be combined to obtain far better results than any of the two approaches on its own. In spite of the aforementioned differences, both modes of problem-solving in the context of scientific discovery are rule following procedure that can be computationally implemented.

Based on the above distinction, there are two different senses of the concept of "formal models". The narrow sense of the term only includes a reduced and precise set of formal rules to conduct a deductive process in an analytical form. On the other hand, the broad sense of the term logic includes the narrow sense, plus any other form of heuristics or rule following procedure to tackle a given problem (Schickore, 2014). Examples of the narrow sense might include what is known as classical logic, relevant logic, etc. and concerning the broad sense of the term, we can take into account production systems or statistical models, etc. In this context, we will generally reserve the term logic framework to the narrow sense of the notion of logic and a computational system will be used in a way that can include a given set of heuristics to approach a specific problem.

Having in mind the above distinctions and the previous discussion of the dichotomy we sketch out in Fig. 1. a taxonomy of the discovery/justification context dichotomy and how it relates to the logical and heuristic formalization stance in connection with the various philosophical stances towards the same dichotomy.

Fig 1. The justification/discovery dichotomy in the context of the formalization of scientific inquiry.



The taxonomy of Fig. 1. can be understood as follows: the root of the tree would be the notion or object of inquiry of science as a whole; from there we would locate on the one side the context of discovery and another branch to the context of justification.

In the case of the context of discovery, we would have two attitudes, one holding that such dimension of scientific endeavor cannot be modeled. Exemplar representatives of this stance would be K. Popper, P. Feyerabend and T.S. Kuhn with their sociological and historical reconstructions of scientific endeavor (although P. Feyerabend and T.S. Kuhn would not conceive their stance in the terms of this dichotomy they would certainly not sustain the possibility of formalization of scientific endeavor). On the other hand, within the context of discovery branch, we would have the positive assumption that this aspect of scientific endeavor can be formalized. Regarding this stance, we would further have two possibilities: the heuristics formalization and the logical formalization. Exemplars of the former would be Herbert Simon (1981) with his BACON computational framework and similar computational architectures devoted to the computational automation of scientific discovery, and of the latter would be Batens (2001) with the *Adaptive Logics* research programme and Takashi, Nara, Goto & Cheng (2007) with their *EPLAS* epistemic programming language.

In the case of the context of justification, we would have two attitudes. The first position would hold that such dimension of scientific endeavor cannot be formalized. Exemplars of this position would be again P. Feyerabend and T.S. Kuhn. A second position within the context of justification branch would have the positive assumption that this aspect of scientific endeavor can be formalized. This stance further bifurcates towards the heuristic formalization and the logical formalization: Exemplars of the former would be K. Popper with his falsifiability which is based on the *modus tollens* logical schema and of the latter we can again place Batens (2001) and Takashi, Nara, Goto & Cheng (2007).

Based on the previous schematization of the discovery-justification dichotomy, we proceed in the next section with the survey some logic frameworks that –as we will later argue- can give a satisfactory account of tasks associated with both dimensions of the distinction.

4. Research endeavors regarding the logical approach towards the process of scientific inquiry.

It has been pointed out that scientific discoveries more often than not involve a kind of reasoning that has to deal with inconsistent information, and furthermore, this is not an isolated occurrence but the norm. This particularity can be attributed to the immense complexity of inferences in the context of scientific discovery, in which some parts have to be rejected, modified and/or replaced, all of which must involve a rational process. This further vouches for the idea that the manipulation of inconsistent data is necessary but it must be a logical, rational or rule-following kind of inconsistent information manipulation (Meheus, 1999).

Thus, it has become a common ground that the production of knowledge in the context of scientific inquiry is one of those many cases in which classical logic may not be suited for modeling this inference process (Meheus1999). In light of this, there have been several proposals of what might be such a logic of scientific inquiry. On this latter topic, there is no clear consensus, and there are several proposals of how such a logic might look like. In the following subsections, we will survey some logical frameworks that have aimed to provide the foundations to the formalization of the process of scientific inquiry.

4.1. Non-Monotonic Logics and the reasoning process within scientific theories.

It is common ground that knowledge in the context of scientific theories is one of the most paradigmatic cases in which a set of beliefs in a given point of time may not remain firm with further advancement of the process of scientific inquiry. Furthermore, the process of gathering further information may further diminish a set of beliefs once held. This points to a more fundamental aspect of human reasoning, that is, besides the ability to make novel inferences we seem to also possess the ability to retract the derived information in light of new data or evidence. (Kraus, Lehmann & Magidor, 1990).

In classical logic, there is a key property that contraries this view. The property that a belief cannot be withdrawn if it was it is derived at some earlier point in the inference process is known as the monotony principle. It has been pointed out that the property of monotony is too restrictive in the sense that although it prevents us from reaching to false conclusions (by having a strict restriction on the deducibility relation that guarantees that when one proposition is inferred it can no longer be retracted) this is really counterproductive in the context of everyday common sense reasoning in which we do not possess this kind of certainty nor is it beneficial and can represent an epistemic cost to restrain to engage in new knowledge that can be true (Nute, 2003). This monotony property may be one of the key factors that at some point favored the view that the study of scientific discovery requires moving beyond classical logic (Meheus, 1999).

The non-monotonic mode of reasoning was largely dismissed and unattended in favor of deductive reasoning, at least the kind of (monotonic) deductive reasoning that infers propositions in an absolute and irrevocable way. This attitude has not been without cost in the epistemological realm in which we bluntly recognize the fallibility of our inferences and the revocability of our beliefs. There are historic and theoretic reasons of why this is the case, as Koons (2013) points out Aristotle's view of science was of an endeavor that constructs universal laws that hold no matter what. Nevertheless, a whole range of everyday inferences relies on common sense generalizations that do not follow the irrevocable character of classical logic.

The same epistemological concerns emerge in the context of the reasoning process involved in scientific inquiry, and to this aim there is a need for reasoning systems that can handle this process of making tentative inferences and correcting them in light of new evidence (Nute, 2001) since the process of manipulating inconsistent belief sets is a maneuver that cannot be undertaken in the context of classical logic, which render this inference process as insufficient within classical logic (Mcdermott & Doyle, 1980).

Hence, regardless of the specific logical framework to deal with the process of scientific endeavor, there is the conviction that this process is inherently non-monotonic. This is based on the fact that scientific theories may be inconsistent in a very specific way, one of which can be the addition of new data or evidence, which makes it necessary to be able to withdraw any belief at any given time based on the correct criterion. Therefore, the whole process of accepting or rejecting theories or beliefs in the context of tentative but scrutinized scientific theories has a rational or logical procedure to it, that is, this deliberation process is not a random or arbitrary one, and as such, we can establish and identify rules and reasons on which scientific theories are dismissed or upheld. Hence, the inference process and conclusions within scientific discovery are regarded as intrinsically non-monotonic.

4.2 Defeasible Reasoning and Defeasible Logic.

Defeasible reasoning is a model of deduction in which when we make an inference, the conclusions that we reach could be retracted later on. This kind of reasoning has the base presumption that at any given time we make inferences on a *ceteris paribus* clause, that is, all thing being equal we reach the conclusions we make, but there could exist a given situation that invalidates our ceteris paribus presumption (Nute, 1988).

In the same vein, Tohmé, Delrieux & Bueno (2011) have pointed out that one of the key distinctive features of scientific reasoning lies not only in the less than absolutely certain inferences but the fact that those inferences or information can be further corrected. Based not only on the fallibility but also the necessity of correcting mechanisms, they argue that defeasible logic is the most suited framework to accomplish such a task. Hence, the use of defeasible modes of reasoning and their logical formalisms counterparts might offer a satisfactory foundation for logic and computational models of episodes in scientific inquiry.

In the context of defeasible reasoning, when an argument is strongly or at least sufficiently supported by its premises we can defeasible sustain the respective conclusions, but this connection between premises and conclusions is not a definite or absolute in nature, it is just tentative (Koons, 2013). Now, it's important to underscore the fact that for a given proposition to be defeasibly inferred does not implies that the proposition is somehow false, it just means that the particular inference *could* be further defeated in light of new information or new rules connecting previously held information.

Defeasible logic is essentially a non-monotonic logic that deals with the problem of revising inconsistent sets of beliefs or information through the notion of defeasible inferences and defeasible rules, which by all means is one of the many cumbersome inconveniences contained in classical logic. In what follows, we will make a further exposition of the key aspects of defeasible logic based on an illustrative example given by Ewa Madalińska.

First of all, in defeasible logic we have the notion of *facts*, which state some proposition or known information. For example, we can state that fact that "Marco is Italian" and the fact that "Marco is a communist" as follows:

$$\mathit{Italian}(\text{Marco}) \quad (1)$$

$$\mathit{Communist}(\text{Marco}). \quad (2)$$

Now, the core idea of this logic framework is the existence various kinds of rules: strict rules and defeasible rules. Strict rules are the kind of monotonic rules that one can formulate in standard classical logic. For example, the rule that states, "If x is Italian then x is European" would be formalized as follows:

$$\mathit{Italian}(\text{x}) \rightarrow \mathit{European}(\text{x}). \quad (3)$$

Contrary to these classical and standard rules, there is the case of defeasible rules. For this kind of rules the inference is only defeasible, meaning that other rules can further defeat this inference. For example "If x is Italian then x is Catholic" and "If x is Communist then x

is not Catholic" would be formalized as follows:

$$r1: Italian(\mathrm{x}) \Rightarrow Catholic(\mathrm{x}). \quad (4)$$

$$r2: Communist(\mathrm{x}) \Rightarrow \neg Catholic(\mathrm{x}). \quad (5)$$

The defeasible rules exhibit the non-monotonic properties offer the flexible and revisable mechanism by which a set of beliefs can be revised and in which any particular belief can be further retracted at any given point in virtue of its defeasible nature. In the same vein, defeasible logic also provides a way to handle inconsistent sets of data and beliefs inasmuch that the beliefs that make up the system are not irrevocable; to the contrary, they can be revised and compared in virtue of defeasible rules. To this end, it posits the notion of *defeaters*. The role of defeaters is to defeat inferences (i.e. the deduction of a given atomic literal) that without the existence of the respective defeaters would be legitimately justified. For example, one can formalize the notion that "If *x* is a communist then the conclusion that *x* is Catholic should be blocked" as follows:

$$Communist(\mathrm{x}) \rightsquigarrow \neg Catholic(\mathrm{x}). \quad (6)$$

Hence, defeaters do not provide positive support for any inference; on the contrary, they can block the application of a given defeasible rule. In the above scenario, the defeasible rule (6) would prevent the inference stating that Marco is Catholic from the previously known fact (1) (i.e. the fact that state Marco is Italian).

In addition to the existence of defeasible rules, defeasible logic uses a superiority relation > concerning the available rules. Hence, solely based on our potentially contradictory set of defeasible rules (4) and (5), a decision of whether Marco is or is not catholic cannot be made. Therefore, if one establishes that *r2* > *r1* then some intended conclusion can follow in virtue of the primacy of some rule over other. In this case, with the specified superiority relation one is enabled to infer that Marco is not a catholic.

One final remark is to be made in connection with superiority rules within defeasible logic, and how this superiority relation has to be established. In particular, we point out the fact that a computational system can outsource this task. That is, to establish the different weights and levels of precedence among different defeasible rules (i.e. the details of the superiority relation >), a computational approach can be used. For example, statistical mechanisms can be used to determine with exact precision (and even with a dynamic nature) the details of the superiority relation in any given defeasible context. Hence, in such a setting, one can easily see how computational methods could be used to support a logical framework. In this sense, one could speak of hybrid models of the process of scientific inquiry, in which one or other approach could be used in the general framework to further optimize the model.

4.3. Adaptive Logics

In congruence with the notion that within the process of scientific discovery one has to deal with a great amount of data and some pieces of information within that data can be inconsistent, it has been argued that paraconsistency is a necessary condition that any logic formalism must satisfy in order to model this process of scientific inquiry. In this context, paraconsistency should be regarded as a property concerning logic formalisms that allows the occurrence of contradictions without any arbitrary proposition being able to follow from such inconsistency, as is the case in classical logic. To this, there has been intensive work in a research program devoted to the construction of a formal system that can provide the tools for this task. This formalism is denominated adaptive logics (Meheus, 1999).

Adaptive Logics restricts the applicability of the rules of inference, i.e. when a rule of inference is violated its applicability is restricted. In this way, one is not entitled to deduce any sentence from an inconsistency, rather its use and extent in virtue of such inconsistencies are restricted and limited. That is, when an inconsistency is found or involved in some step of the inference process, adaptive logic restricts the applicability to avoid triviality, but when no inconsistency is involved then rules of inferences are applied with their full-fledged strength. One of the most important aspects of adaptive logics, besides being a logic aimed at the proper manipulation of inconsistent information, is that it does so in a very precise way, in a non-monotonic way. This is important to point out due to the fact that simply paraconsistent logics only rules out some tenets contained in classical logic (most exemplary *ex falso quodlibet* among others) but this has been pointed out to be insufficient to explain inferences made in the context of scientific inquiry (Meheus, 1999). As such, adaptive logic is said to provide a much richer consequence relation than the ordinary monotonic paraconsistent logics (Batens & Meheus, 2001).

One prominent example is in which the importance of this kind of adaptive logic is put in the context of scientific theories. If for a particular theory *T* we assume a classical logic (CL) as its grounding framework and, such a theory would produce an inconsistency (i.e. inferring *A* and *~A* at the same time) then the original theory *T* should be fully disregarded as trivial in favor of some other theory *T'* that does not contain any inconsistency. But, if for the same scenario as described above we assume a monotonic paraconsistent logic (*MPL*) as its grounding framework, then the sole occurrence of an inconsistency in *T* is no longer a reason for the dismissal of this theory based on the fact that we now assume a logic framework that can handle such inconsistencies. Nevertheless, the monotonic property of such a paraconsistent logic would come at a price, namely, the fact that there are some rules of inference that are not accepted in the context of a monotonic paraconsistent logic, such as Disjunctive Syllogism. Hence, if the scientific theory *T* needed such a rule or any other that is available in classical logic (CL) but unavailable in a monotonic paraconsistent logic (MPL) then there would emerge a shortage of inference power in the formalized theory *T* (i.e. in this context it would no longer be the case that *B* follows from *~A* and *A v B*). Thus, with this change in our logical framework, our former theory *T* would not be trivial anymore, but the downside is that some rules of inference would cease to be available. Therefore, the sole change from classical to paraconsistent logic comes at a significant cost. But if we push forward our grounding formalism to be a non-monotonic paraconsistent logic, we would not only be entitled to avoid the dismissal of *T* based on some particular inconsistency but we would also retain the inference power available in classical logic (for example we would be able to work with rules such as the Disjunctive Syllogism. Hence, the change in viewpoint from classical logic to adaptive logics permits the reuse of the original theory *T* in such a way that all inference rules of classical logic are still available in the cases were those inference rules do not produce an inconsistent set of information (Batens & Meheus, 2001).

Meheus (1999) has pointed out, that adaptive logics can provide a logical formalism resource in the context of scientific inquiry, where some given inconsistency does not rule out a whole scientific theory, but scientists themselves only restrain the inconsistency to a particular fraction of the theory. That is, if from a given theory, a consistency might show up, that does not grant license to produce or infer any arbitrary number of inferences with no bearing on the original theory; to the contrary, scientists only restrain the implications of an inconsistency as much as possible, ruling out any more trivial inferences to the broad extent of the theory. Hence, in light of this common schema in the context of scientific inquiry, adaptive logics can accommodate to the dynamics within the process of scientific discovery.

As an example of the above, Meheus (1999) points out the demonstration of Carnot's theorem carried out by Rudolf Clausius (1822-1888). The theorem was conducted in such a way that involved inconsistent belief sets regarding the production of work in heat engines. On one side, Sadi Carnot proposed that this work production was derived from the heat transfer from a hot to a cold reservoir, but the opposing view of James Prescott proposed that such work production in heat engines was a result of the conversion of heat into work. Given this scenario, Rudolf Clausius developed two different proofs, both of which were conducted through *Reductio ad Absurdum*, that is, starting from the hypothesis that the negation of Carnot's theorem was true plus the corresponding premises he could reach a contradiction, so Carnot's theorem must be true, but Rudolf Clausius only considered his second proof to be valid. Now Meheus (1999) points out that, the first proof produces an inconsistency just from the premises, that is, the inconsistency at hand does not involve the hypothesis of Carnot's theorem, but the second proof, on the other hand, produces an inconsistency in such a way that such inconsistency does involve the statement of Carnot's theorem.

From the above scenario, Meheus (1999) remarks two key aspects. First, in the context of a monotonic paraconsistent logic, the rule of *Reductio Ad Absurdum*, is not admitted, therefore neither proof of Carnot's theorem would be valid. Second, and most importantly, assuming a non-monotonic paraconsistent logic, in which such rules of inference like *Reductio Ad Absurdum* are available, the question remains which of the two available proofs is valid. To this, Meheus notes that the second proof would be more relevant towards the demonstration of Carnot's theorem, due to the fact that it is in this second proof that the contradiction emerges not only from the premises themselves, but from the premises plus the hypothesis stating the negation of Carnot's theorem, whereas the first proof produces a contradiction but such contradiction emerges from the premises, and do not involve or use the statement regarding Carnot's theorem. Hence, only from this second proof, would be reasonable to infer that the negation of Carnot's theorem is false, i.e. Carnot's theorem is true. In light of this kind of episodes in scientific discovery, Meheus states that there is a need not only for a simple paraconsistent logic that can handle inconsistencies, because such framework would rule out important inference rules such as *Reductio Ad Absurdum* (as well as some others), but it would also need a non-monotonic paraconsistent logic that can handle inconsistencies without reducing the available inferences rules which could be needed in the context of scientific knowledge production.

It also pointed the need of such adaptive style of inferences for computational environments such as databases in which inconsistent information is an ongoing possibility but nevertheless rules of classical logic cannot be taken away in the global or general scenario (Batens & Meheus, 2001). The above naturally leads to the consideration of such a logical framework in the context of inferences in scientific inquiry.

4.4. Abduction

A final logical framework to be mentioned is the undoubtedly paradigmatic scheme to approach the topic of reasoning in the context of scientific discovery: abduction. In general terms, abduction is the mode of reasoning in which one infers possible hypotheses based on some given data or phenomena (Schickore, 2014). The abductive mode of reasoning is usually one of the three major modes of reasoning (the other two being induction and deduction), nevertheless, abduction is sometimes placed within the inductive category (Douven, 2013).

Nevertheless, definitions as the above lack a precise nature than can render a clear-cut formal criterion (general o narrow). To this matter, Douven (2013) notes that abduction can be defined as follows: given some set of evidence E and a list of possible explanations $H_1, H_2, \ldots H_n$ regarding E, if some hypothesis H_i better explains the given data E than the other competing hypothesis, H_i is accepted as a satisfactory and appropriate explanation of E.

There are several historical examples of how abduction is the functioning reasoning process concerning some scientific discovery. For example, Douven (2013) recalls how at the beginning of the 19th century the orbit of Uranus did not comply with Newton's theory of universal gravitation. At the time, it was held that Uranus was the seventh and last planet in our solar system. In this scenario, both Couch Adams (1819-1892) and Leverrier (1811-1877) instead of dismissing Newton's highly successful theory on the face of its poor explanation of Uranus orbit, proposed some other more parsimonious explanation (that there was an eighth planet: Neptune). In the context of the aforementioned scenario concerning the postulation of an eighth planet, one could see the Uranus' orbit irregularity, which departed in such a way that did not fully complied with predictions grounded on Newton's theory of universal gravitation as the available evidence set E, and the existence of an eighth planet as a hypothesis H_1 that better suits the state of affairs than the hypothesis H_2 that dismisses Newton's theory of universal gravitation. This abductive process was a more economical and precise explanation (than completely dismissing Newton's theory) and showed how intrinsic this mode of reasoning is to scientific endeavor.

Several shortcomings have been pointed out towards the abductive mode of reasoning. One of them consists in the fact that there are considerable number of hypothesis to explain a given phenomenon, in this sense, the sole fact of producing or proposing explanatory hypothesis towards a particular set of data or some phenomenon is not the whole story to scientific inquiry, and a further selection criterion is required to evaluate the hypothesis.

Furthermore, it is important to point out that although abduction has a longstanding tradition as an object of inquiry within contemporary logic and it can be sketched according to some general schema, it is “logical” in an extremely broad sense of the term. Thus, it might be too ambiguous for a formal and precise definition as other logical systems can be (Schickore, 2014). Due to the ambiguity of its formal status, this reasoning framework can also be classified –and has been done so- within the family of heuristic processes. Despite the categorical debates of the abductive mode of reasoning, it is without a question that is has been a prominent mode of understanding the process of scientific discovery

In the same vein as the above, it is important to point out that, abduction as a logical framework further supports the case of the weakening between computational and logical systems distinction. That is, in terms of the general sketch of the logical/computational distinction within the discovery/justification dichotomy illustrated previously in Fig 1. we see that abduction would incur in the same elusiveness as to whether it could be exclusively belonging to either the logical or computational branch. This, we stress, is not a shortcoming of abduction as such, but to the contrary, the shortcoming would be placed on any attempt to sustain a hard set and hidebound dichotomy concerning the logic and computational distinction. Hence, as we previously stated, abduction further illustrates the elusive nature of the logical/computational distinction.

5. The weakening of the discovery/justification context dichotomy based on the non-monotonicity perspective of logic and other non-classical formalisms.

In this section, we undertake a new approach to the previously surveyed discovery/justification dichotomy. In particular, we analyze such dichotomy and propose our argument supporting the weakening the aforementioned dichotomy, that is, declining the strong and bold interpretation of the discovery/justification distinction in favor of a moderate and tempered sense of such distinction.

To illustrate our stance, we recall Feigl's (as well as Reichenbach's and others alike) views on the distribution of the methods to approach the different tasks of the discovery and justification contexts. According to this perspective, which we will denominate the classical perspective on the subject matter, on the one hand, the context of discovery deals with the psychological, political and contingent aspects by which scientific theories and/or hypothesis are produced and on the other hand the context of justification deals with the rules, standards, and procedures according to which some hypothesis and/or theories are accepted or rejected. Just as we mentioned earlier in this paper, this perspective makes a bold assumption according to which logic is of a prescriptive/normative nature while other empirical disciplines such as psychology are descriptive and deal with matters of contingent and elusive nature. This approach can be sketched as follows:

Table 2. Classical perspective towards Computational and Sociological approaches.

	Computational	**Sociological**
Validation	Justification *Moment*	----
Generation	----	Discovery *Moment*

We argue that such state of affairs is incorrect. First of all, as we previously remarked (in section 3) psychology possess the methods and instruments to undertake the study and analysis of deliberative processes involved in the context of justification. Second, the process of justification does not end nor it's exhausted in the logical dimension, that is, one can make descriptive remarks of the justification process, and furthermore aspects of the process of discovery can also be approached and undertaken in a prescriptive manner. Therefore, we argue that the tasks and processes involved both in the production and validation of scientific knowledge can be subject to a descriptive or prescriptive analysis (again, earlier in this paper we have reviewed several frameworks and theoretical stances that can accomplish such tasks). In virtue of the above, we decline the thesis according to which neither the discovery nor the justification context refers to different or opposing moments in the process of scientific inquiry.

To support our perspective, one can notice how the dispute over the logic of scientific discovery has had a significant shift towards its rationalization as a rule following procedure (whether a heuristic or a logical one), which is a significant hallmark of contemporary philosophy of science. That is, the view that logic is beyond the realm of the descriptive process of human reasoning has been abandoned and to the contrary logic models have been investigated to explain human psychology (Garcez, Lamb & Gabbay, 2008). For example, there has been an enormous shift towards the descriptive value of logical models in contemporary of logic, which has been largely ignited by the Artificial Intelligence community (Dix, Pereira & Przymusinski, 1997). A particular example of the above, we point out precisely to some of the formalisms we have surveyed in previous sections, such as non-monotonic logics.

In the same vein, and as we previously stated, one of the core difficulties concerning the process of scientific endeavor is that this enterprise, more often than not, involves modes of reasoning that deal with inconsistent or incomplete information. The fact that scientific theories may be inconsistent in some way, one of which can be the addition of new data or evidence, makes it necessary for any formal framework to model such a phenomena to be able to withdraw any belief at any given time based on the correct criterion (Cheng, 2000). Furthermore, and in the same vein as Popper (2002), scientific beliefs are tentative conjectures, which can be further, diminished in light of new information. That is, a set of beliefs once held may be further diminished or revised by the process of gathering further information. According to these properties, one can easily see that classical logic is saliently insufficient to account in a comprehensive manner the distinct process and functions involved in the endeavor of scientific inquiry, given that, for example, if a belief is derived at some point, it cannot be further withdrawn. This is one of the crucial aspects or particularities of the family of non-monotonic logics, that is, they aim to construct a framework in which the addition of further information can interfere with previously held knowledge and/or handle inconsistent information can be captured. Scientific reasoning seems one of the paradigmatic cases in which the inference process loudly fails to satisfy this monotony requirement contained in classical logic. Hence, the family of non-monotonic logic is a case example of a formalism that can account for various aspects involved in the production of scientific knowledge, and in doing so supports our claim concerning the weakening of the hardcore dichotomy such the one expressed by Feigl,

As an example of the previous argument, one can imagine an instance of a logical model of a scientific discovery computationally implemented. If one takes as valid the justification/discovery dichotomy, then one would have to assume that the same scientific episode would require two separate descriptions and process to be satisfactorily modeled, one description and set of procedures concerning the processes of discovering and proposing new empirical laws, relations among the available data and generalizations, and on the other hand another set of processes that handle the evaluation and justification of the output produced by the first processes. In this sense, the discovery/justification dichotomy would provide an insightful and useful distinction.

Nevertheless, one could argue that in virtue of the loudly contested descriptive component present in non-monotonic logics, with the same specifications and with the same inference process the model would carry out not only the validation of inferences corresponding to the context of justification but also tasks strictly associated to the context of discovery, the reason being that non-monotonic logics is, after all, supraclassical (i.e. it extends classical logic without invalidating elementary inferences from classical logic). Hence both the task of producing inferences and validating those inferences could be addressed within the same logical framework. That is, within a non-monotonic framework, the computational instance of such a model would not only produce new and unbeknown statements that can match and extend any given knowledge base (a discovery context related task), but it would also have the available mechanisms to compare contrast between two possible theories in the face of an specific set of data (a justification context related task). This multiplicity of resources would be grounded on the fact that, for the first set of tasks (the discovery dimension) the model would provide all the available tools and features contained in classical logic and in virtue of the non-monotonic nature of the logical framework, then it would provide the mechanisms to retract information in light of new data, premises or some other kind of information (the justification dimension).

In virtue of the above, if the logical framework one uses to model the episode has not only a normative but also a descriptive component –as it is the case for non-monotonic logics-, then tasks of both dimensions of the proposed dichotomy of discovery/justification context could be addressed with the same logical framework, further diminishing the value of such dichotomy. Hence, we argue that, if one assumes the non-monotonic view concerning inferences in the process of scientific endeavor one could be labeled to be working on the context of justification rather than on the context of discovery, nevertheless, if the logic behind the proposed framework is descriptively significant to the agents involved in the process of constructing such scientific theories, then a link between the two dimensions of scientific discovery (discovery and justification) is satisfied.

Having made a case against the strong interpretation of the discovery/justification distinction, we wish to further advance some more constructive remarks concerning the subject matter. To attend this is important to recall that we previously distinguished two senses of the discovery/justification differentiation. Up until now, we have argued against the strong sense of the differentiation, that is, the *dichotomy* sense. Nevertheless, we will argue that there is much value in the differentiation, but we appreciate its value in the more temperate sense, i.e. in the distinction sense. That is, we do not see the existence of two distinct and separate moments (i.e. one of discovery and one of justification) we do see two different *approaches* concerning the endeavor of scientific inquiry.

On the one hand, we would have a *justification approach* to scientific endeavor. In line with this approach, what would be emphasized would be the analysis of the logical, rational or rhetorical reasons by which a given theory and/or scientific hypothesis is accepted, retained or dismissed. Exemplary frameworks that can serve the purpose of this approach can be the rhetorical deliberation of scientific knowledge or the means of inference validations contained in non-monotonic logics (such as defeasible or adaptive logics), with the much esteemed non-monotonic character of such logical framework. On the other hand, we would have a *discovery approach* to scientific endeavor. In line with this approach, the emphasis would be placed on the computational aspects and/or mechanisms by which a given theory and/or scientific hypothesis is generated and/or proposed. In this approach to scientific endeavor, further emphasis would be placed on computational models by which such processes can generate scientific theories and/or hypothesis. Some frameworks that would be of particular interest in this approach to the process of scientific endeavor can be Simmon's (1981) BACON series of computational discovery architectures and again, the non-monotonic logic framework, in that being an intrinsically non-monotonic family of logics, it can broaden the set of inferences and statements from classical logic to a much more dynamic and in doing so, extend the set of available scientific statements and hypothesis.

Based on our previous remarks, and in contrasts with the classical view (such as Feigl's and Reichenbach's) we would propose the following sketch of the discovery/justification differentiation:

Table 3. Our proposal regarding the Computational and Sociological approaches.

	Computational	**Sociological**
Validation	Justification *Approach*	
Generation	Discovery *Approach*	

According to the previous scheme, some aspects of scientific endeavor that are labeled as belonging to the context of discovery would be in our terms aspects emphasized in a justification or prescriptive approach and aspects that are labeled as belonging to the context of discovery would only amount to a descriptive character. But in our terms, the existence of distinct approaches does not imply that any of the tasks and aspects involved in scientific endeavor belong to separate and distinct *moments*, but aspects of scientific endeavor are analyzed through different *approaches*, whether a descriptive or prescriptive one.

6. Conclusions

In this paper, we have exposed some of the core controversies surrounding the discovery/justification context debate, and how it has shaped the philosophy of science as a discipline both in a methodological and theoretical sense. We have furthermore drawn a plausible taxonomy surrounding the discovery/justification dichotomy in the context of contemporary philosophy of science.

We have proposed, from the context of modern approaches to long-standing disciplines (such as logic and philosophy of science) and also from novel and evolving disciplines (such as computer science and cognitive psychology) several conceptual and theoretical frameworks to approach each of the dimensions conforming the dichotomy. In particular, we analyzed the family of non-monotonic logics as a plausible basis for the reasoning process concerning inferences in the context of scientific discovery. We have argued that such approaches and frameworks can give a satisfactory account of tasks associated with both dimensions in the discovery/justification dichotomy. In doing so, we have not only provided what we consider substantial evidence of the contemporary shift towards the logical/formal approaches to the process of scientific discovery, but we take this aspect as supporting evidence concerning our thesis about the weakening of the bold and clear-cut distinction portrayed in the original dichotomy. As we have argued throughout our current work, it seems that such distinction under scrutiny only refers to two (but not opposite) descriptions levels and that they do not hold up to the trial of a categorical and irreducible differentiation. Nevertheless, we also sustained the thesis that this distinction in a tempered and moderate interpretation does offer some explicative and illustrative value.

Despite our critic of the discovery/justification dichotomy, it is important to underscore that we do not dismiss the distinction but what we argue against is the bold and clear-cut dichotomy. That is, the discovery/justification as a distinction between two different approaches to the same object of study is of great explanatory value, but the existence of two different moments in scientific inquiry does not hold such explanatory value. Much to the contrary, the bold dichotomy of the discovery/justification differentiation seems to obscure the study and analysis of scientific endeavor in philosophy of science.

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Anexo B

La dimensión sociológica de la investigación científica y el paradigma de la cognición distribuida.

En este artículo ponemos de relieve la dimensión social, colaborativa y multi-agente de la ciencia que fundamenta la institucionalidad científica. Asimismo, y contrario a algunas tesis asociadas a la dicotomía descubrimiento/justificación, proponemos que esta dimensión social de la ciencia, denominada institucionalidad científica, es partícipe de procesos deliberativos racionales propios del contexto de justificación. Esta tesis viene a contrariar la idea de que la dimensión social del quehacer científico está allende de estos procesos, modelos y métodos formales de análisis. Por otra parte, proponemos la existencia de un debilitamiento de la anteriormente citada dicotomía que se fundamenta en la perspectiva de la cognición distribuida aplicado a las dimensiones sociales intrínsecas al proceso de producción de conocimiento científico.

The discovery/justification context dichotomy and the social nature of scientific endeavor: a weakening of the distinction based on the perspective of distributed cognition.

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Abstract: The present paper surveys some key aspects of the historical and theoretical debate surrounding scientific discovery. In particular, we analyze the context of discovery and context of justification distinction within philosophy of science. Furthermore, we advance an argument concerning the weakening of the discovery/justification context dichotomy based on the distributed cognition approach to scientific discovery endeavor as a collective and collaborative task among multiple agents.

Keywords: philosophy of science, logic of discovery, scientific models, scientific method, scientific discovery, context of justification, context of discovery, cognitive science, scientific revolutions, distributed cognition.

1. Introduction

The precise nature of the production of scientific knowledge is one of the long-standing debates within philosophy of science. One of the core controversies of the production of scientific knowledge is the constant interconnection between the individual and the community, that is, the interplay between knowledge produced by individual agents within a scientific community and the scope and extent of the community in the production of this kind of knowledge. This topic has a strong and important connection with the debate about the existence of a logic of scientific discovery (or lack of it), which ultimately sets the stage for contemporary research efforts directed towards the design and implementation of automated architectures and frameworks that could undertake the task of scientific discovery. This matter offers new insights as to which part of the production of scientific knowledge belongs to individual agents and which part belongs to the scientific community as a whole and if there is at all a clear-cut separation of processes and tasks concerning one and the other.

In this paper, we analyze one of the most influential distinctions in philosophy of science, which is the dichotomy that contrasts and opposes two dimensions within scientific endeavor: the context of justification and the context of discovery. In particular, we frame this inquiry in the context of the social and collective nature of scientific inquiry. In virtue of the above, we propose an argument concerning the weakening of the discovery/justification context dichotomy based on the distributed cognition approach to scientific discovery endeavor as a collective and collaborative task among multiple agents.

2. Scientific inquiry and the discovery/justification context dichotomy.

Concerning the precise nature of the reasoning process involved in scientific knowledge, it has been pointed out that in the seventeenth and eighteenth centuries there was a positive attitude towards the idea of the existence of a logic of scientific discovery. Exemplars of such stance are saliently conspicuous, in the history of philosophy. For example, both Rene Descartes (1596-1650) and Francis Bacon (1561-1626) embraced the quest of formal and abstract systematization of the guidelines and principles that would ground and facilitate the production of scientific knowledge. Nevertheless, such a positive attitude would cease favoring the skeptics of such project. This skeptical held that the process scientific discovery is not susceptible to a set of mechanical rules, or logical principles (Meheus & Nickles 1999).

To the contrary, the late 19th century and a large part of the 20th century have been portrayed as a period with a skeptical attitude towards the existence of a logic of scientific discovery was formed, sustained and developed throughout mainstream philosophical thought. To this matter, one can point out figures such as Gottlob Frege (1848-1925) who portrayed and spread the view that there is no room for a logic of discovery and that the aim of logic itself is the problem of deduction, which has nothing to do with the intricate process of scientific discovery and/or its method. In the same vein, Frege postponed the study of scientific discovery to the field of psychology withholding its inquiry in logical terms.

Needless to say that Frege was one of the most influential philosophers in analytical philosophy, which would later become one of the leading schools of modern philosophy that would undertake science as an object of study. Hence, Frege's anti-psychological view regarding the nature of logic served as one of the commonplaces in philosophy of science concerning the debate of the existence of a logic of scientific discovery. In the same vein as Frege's attitude towards the subject Alfred Tarski (1994) held the view that there is no way of merging the study of logic and the procedure or methods employed in the process of scientific discovery. This attitude would further ground what is called the "divorce" of logic from the philosophical study of scientific discovery (Celluci, 2013).

As Noé (1998) points out, one of the specific schools within analytical philosophy, denominated the Vienna Circle, was formed by notorious scientists and philosophers that not only undertook science as an object of study but also undertook the skeptical perspective towards the existence of a logic of scientific discovery. Within the context of the views of the Vienna Circle towards the matter of scientific endeavor, emerged Karl R. Popper's view of philosophy of science, which although significantly differed and contested some important tenet's in philosophy of science, it did hold the strong disregard of the possibility of a logic of scientific discovery. This state of affairs influenced many important views in the debate on the existence of a logic of scientific discovery, as well as mainstream perspectives in philosophy of science in general (Celluci, 2013).

Precisely, with the aforementioned state of affairs as historical background, one of the key theoretical distinctions in philosophy of science emerged: the context of discovery and context of justification dichotomy. The discovery/justification dichotomy held the existence of two different and opposing moments within scientific endeavor: the discovery and/or generation of theories and hypothesis on the one hand, and the validation and/or justification of those theories on the other. The genealogy of such dichotomy can be traced back to Hans Reichenbach work of 1938 *Experience and Prediction*, in which Reichenbach states the following:

> There is a great difference between the system of logical interconnections of thought and the actual way in which thinking processes are performed. The psychological operations of thinking are rather vague and fluctuating processes; they almost never keep to the ways prescribed by logic and may even skip whole groups of operations which would be needed for a complete exposition of the subject in question. ... It would be, therefore, a vain attempt to construct a theory of knowledge which is at the same time logically complete and in strict correspondence with the psychological processes of thought. […] The only way to escape this difficulty is to distinguish carefully the task of epistemology from that of psychology. Epistemology does not regard the processes of thinking in their actual occurrence; this task is entirely left to psychology. What epistemology intends is to construct thinking processes in a way in which they ought to occur if they are to be ranged in a consistent system; or to construct justifiable sets of operations which can be intercalated between the starting-point and the issue of thought-processes, replacing the real intermediate links. Epistemology thus considers a logical substitute rather than real processes. (5)

According to Reichenbach's view, one moment in scientific inquiry deals with the process of conceiving new ideas and theories, and a different moment handles the process of validating and justifying those theories. The former is denominated the context of discovery and the latter is the context of justification. This discovery/justification context dichotomy would irremediable influence, shape and organize the subjects and themes admitted within philosophy of science. An example of this claim concerning the influence of Reichenbach's dichotomy of 1938, Herbert Feigl (1965) stated:

> There is a fair measure of agreement today on how to conceive of philosophy of science as contrasted with the history, the psychology, or the sociology of science. All these disciplines are about science, but they are 'about' it in different ways. ... In the widely accepted terminology of Hans Reichenbach, studies of this sort pertain to the context of discovery, whereas the analysis pursued by philosophers of science pertain to the context of justification. It is one thing to ask how we arrive at our scientific knowledge claims and what socio-cultural factors contribute to their acceptance or rejection; and it is another thing to ask what sort of evidence and what general, objective rules and standards govern the testing, the confirmation or disconfirmation and the acceptance or rejection of knowledge claims of science. (472)

According to Feigl's view, there is not only irreducible and categorical differentiation among the different moments of the scientific enterprise, but the theoretical tools and instruments by which one approaches one context or the other differs. In line with this view philosophy of science would deal with a specific set of inquiries and mechanisms to address those questions (i.e. philosophy deals with the context of justification with the tools of logical analysis) and psychology and sociology would address another set of aspects of science (i.e. psychology and/or sociology addresses the context of discovery through historical mechanisms and so on).

In the same vein as Feigl, Karl R. Popper conspicuously reinstated the dichotomy in one of his most influential work concerning the very core discussion of the existence of a logic of scientific discovery, in which he stated the following:

> The initial state, the act of conceiving or inventing a theory, seems to me neither to call for logical analysis not to be susceptible of it. The question how it happens that a new idea occurs to a man— whether it is a musical theme, a dramatic conflict, or a scientific theory—may be of great interest to empirical psychology; but it is irrelevant to the logical analysis of

> scientific knowledge. This latter is concerned not with *questions of fact* (Kant's *quid facti*?), but only with questions of *justification or validity* (Kant's *quid juris*?). Its questions are of the following kind. Can a statement be justified? And if so, how? Is it testable? Is it logically dependent on certain other statements? Or does it perhaps contradict them? […] Accordingly I shall distinguish sharply between the process of conceiving a new idea, and the methods and results of examining it logically. As to the task of the logic of knowledge—in contradistinction to the psychology of knowledge—I shall proceed on the assumption that it consists solely in investigating the methods employed in those systematic tests to which every new idea must be subjected if it is to be seriously entertained. (2002, 7-8)

The aforementioned state of affairs was -during a great part of the 20th century- the mainstream and predominant view in philosophy of science, due to the influence of logical positivism in the Vienna Circle and later by Karl R. Popper who held some important tenets of the logical positivists. Nevertheless, in the 1960's some important aspects of such state of affairs would be significantly contested in the field of philosophy of science.

The critical movement within philosophy of science that emerged in the 1960's consisted in a series of authors such as Norwood Hanson (1924-1967) and Thomas Kuhn (1922-1996) and others. One of the main focus of this opposing movement within philosophy of science was the study and treatment of scientific endeavor in integrative and novel manner, that is, an approach to scientific endeavor in a way that it did not separate and segregated the different aspects of scientific discovery, and tried to understand and explain scientific inquiry having in mind these various aspects when explaining a given scientific phenomenon. Through their work, these authors reinstated the study of some previously and conspicuously unattended aspects of scientific inquiry, such as the nature of scientific discovery and the sociological aspects of scientific discovery (Noé, 1998).

As an example of some of the contesting views of this critical movement of philosophy of science, one that notoriously stands out is T. S. Kuhn's work of 1970 *The Structure of Scientific Revolutions*, in which scientific inquiry and its process of development are analyzed in terms of revolutions or successions among competing scientific paradigms. Kuhn's work in analyzing the emergence and succession of scientific theories did not separate the psychological, sociological or historical aspects of it from the logical and justificatory process concerning the acceptance of such theories. To the contrary, T. S. Kuhn's thesis lies heavily on the fact that such aspects are of crucial importance and an inescapable ingredient in scientific endeavor (Kuhn, 1970). This movement and group of philosophers reinstated the study and philosophical treatment of all aspects of scientific discovery, including aspects that were previously dismissed towards psychology or sociology (Noé, 1998).

3. The discovery/justification context dichotomy and the "classical" perspective of philosophy of science.

As we previously mentioned, following the mainstream perspective in philosophy of science which was mainly composed of the Vienna circle and K. Popper's views, it emerged a "critical" movement within philosophy of science. Part of this contesting movement in philosophy of science revolved around some of it is supposed mandate for different questions and methods concerning the different dimensions of scientific endeavor. This critical movement within philosophy of science indirectly placed the discovery/justification dichotomy under scrutiny. In this section, we address a particular set of concerns of the aforementioned dichotomy that relates to the topic of this paper.

Regarding the context of discovery, one can point out that whether one admits or rejects the discovery/justification dichotomy, this stage has been accompanied by the strong recognition of the fact that scientific discovery involves a creative component. Hence, it has been a long-standing question as to whether this creative component can be grasped, analyzed and modeled through the methods of logic (whether it is through classical logic or some other form of formal frameworks), or if to the contrary, this creative component is far beyond the reach of rational and/or rule following procedures, whether they concern classical logic or some other formal systems. Hence, the extent and limitations of understanding and -even more- modeling such a reasoning process can account for the acceptance or dismissal of the discovery/justification dichotomy.

In line with the above, it can be stated that creativity, in the context of scientific inquiry, can generate a myriad of plausible ideas but scientists as a norm do not take them up to inspection on a random or careless basis. Contrary to the above, there seems to be a decision making process that rules some out and keeps others, by its very nature, this decision making process of ruling hypothesis or ideas out and keeping others must have some sound criterion by which the process goes along no matter how creative the roots of those ideas can be (Celluci, 2013). Precisely, Boden (2004 & 2009) has remarked that creative process deals with the production of new ideas in a twofold manner: a new idea to an individual no matter if it was already known to some other individual or groups of individuals (psychological creativity) or a new idea that no one else has had before (historical creativity). Furthermore, creativity would be seen as feature or faculty consisting of various components of different levels of complexity and variance, ranging from the simple combinations and associations of previously known commonplace notions and ideas to the disruptive change of rules and variations in the repertoire of commonplace notions themselves. As pointed out by Picciuto & Carruthers (2014), philosophers studying the scientific process are more likely attracted to the historical dimension of creativity, but the examination of psychological creativity could shed light into the comprehension of historical creativity.

On the other hand, and concerning the context of justification, one of the main topics of discussion lies in the possibility of modeling this process, whether it is through the methods of classical logic or some other formal systems or if this side of the scientific enterprise is doomed to be far beyond any rule-following, systematic and rational approach.

Following the aforementioned debate, one can point out that there is another alternative between the opposing views (Poppers defense of classical logic and Kuhn's sociological account) regarding the process of justification. For example, there are approaches to this aspect of scientific inquiry that although not belonging to sociological or historical accounts give a rational schema according to which this dimension of scientific inquiry can be scrutinized, without such scrutiny being proposed with the methods of logic.

In line with the above, there are *argumentative* approaches to scientific deliberation that although do not fall under the procedures and method of classical logic, they do provide a rational and systematic framework for understanding such processes involved in the context of justification, that is, the stage of scientific discussion in which theories and statements concerning those theories are scrutinized in order to validate those theories. The argumentative approach towards the problem offers a halfway solution that lies between the logical/sociological dichotomy previously remarked.

For a paradigmatic example of such systematic and rational approach towards scientific deliberation, one can point out Straßer's proposal of *Rational Disagreements*. Straßer coined the term rational disagreements to describe the process of dispute among opposing scientific views regarding a particular subject, which is characterized on the basis of two fundamental tenets as follows: "RD1 There is a disagreement concerning some issue A. RD2 There are reasons to suppose that the stance of each participant is the result of a rational deliberation." (Straßer, Šešelja & Wieland, 2015, 116). Accordingly, for any given pair of competing stances *A* and *B*, Straßer identifies two degrees of disagreement, one is a strong version of scientific disagreement and the other is a weak version of scientific disagreement. Regarding the strong version of scientific disagreement: *A* holds that *B's* stance is wrong and incompatible with *A's* position, and there are two key factors: (1) *B's* position although a wrong one is a product of a rational deliberation process in which reasons were taken into account but (2) the reasons taken into account are not enough to sustain or uphold *B's* stance. According to the weak form of disagreement: *A's* position can further understand *B's* position but nevertheless, *A* prefers its position although it recognizes *B's* stance as a plausible and attention worthy one. (Straßer, Šešelja & Wieland, 2015). Of course, one could intuitively hold the strong version of rational disagreement as for the most interesting and productive one since it pushes the boundaries of each of the stances involved and sharpens the clarity of the involved theories. Hence, Straßer's notion of scientific disagreements can also be seen as an underlying epistemic foundation concerning the process of contrasting and explaining the transition and justificatory deliberation within scientific endeavor.

The previous survey underscores that there is a large and important dimension concerning scientific inquiry that is left behind from the classical perspective of philosophy of science, that is, the sociological dimension of scientific enterprise is conspicuously unaddressed from the *classical* perspective. By "sociological dimension" of scientific discovery, we refer to the inescapable social nature involved in the collective process of the production of scientific knowledge, in which scientific communities play a crucial role in the advancement of such knowledge. To finish this section, we wish to analyze some of the controversial aspects contained in such "classical" perspective of philosophy of science, which will also serve to provide a better depiction of what it is when we talk about this *classical* perspective in philosophy of science.

As Cheng (2000) has pointed out, the endeavor of scientific discovery can be seen as a collective enterprise in which a whole set of individuals participate and collaborate collectively and more important, the construction of common scientific knowledge can be seen a collective and collaborative endeavor. In the following, we will place this sociological dimension of scientific inquiry in the context of the discovery/justification dichotomy. To expose the sociological dimension of the discovery/justification dichotomy one can imagine how scientific communities and individual scientists would be placed, and what part of their work would be assigned to the individual and which part would be assigned to the whole. This last matter is what we are going to label as the task and role distribution among individuals and the community in the context scientific endeavor.

According to the 'classical' division and assignations of tasks incarnated in the discovery/justification dichotomy, one can state the individual is concerned with the discovery dimension of science and on the other hand, the community, to a large extent, as a whole undertakes the rational deliberation process that evaluates such a proposed discovery. Nevertheless, we have to underscore that the above is what usually is the case, because we also have a large array of historical examples of simultaneous discoveries such as Darwin's and Wallace's discoveries and several examples in which single individuals carry out the task of providing scientific justification and/or validation to some particular theory or hypothesis. Having in mind that there are some cases that do not comply with such standard view (the extent and limitations of these escapes the point of our argument), we can posit a 'classical' view concerning the regular distribution of tasks within scientific endeavor as we previously remarked, i.e. the individual scientific agent seeks to discover and propose scientific knowledge in the form of data on scientific journals and so on, and on the other hand it's the scientific community as a collective through their congresses and discussion forums that provide validation that data, ideas, and theories.

Given the above, we can frame the discovery/justification dichotomy in terms of the tasks and roles distribution in the context of scientific endeavor. That is, one can understand such roles assignments as an immediate byproduct of the core and bold thesis in philosophy of science concerning the discovery/justification dichotomy. According to this, on the one hand, the individuals as single scientific entities would be concerned in the process of discovering and proposing new theories, data, and ideas, and on the other hand, the scientific community would be concerned with the collective validation and justification of such products of individual scientists. Hence, to a very large extent, this complies with the standard sense of the dichotomy.

Up to this point, we have advanced two ideas. The first consists on the standard or classical distribution of tasks and roles within scientific endeavor. The second idea frames such task distribution as a byproduct of the discovery/justification dichotomy. In the next section, we will discuss in more detail the sociological dimension of scientific inquiry, and what this aspect of scientific endeavor implies for the basic thesis and notions contained in the discovery/justification dichotomy.

4. The sociological dimension of scientific inquiry and the discovery/justification context dichotomy.

In this section, we want to pursue a larger and broader point concerning the discovery/justification dichotomy together with the collective nature of scientific inquiry and the sociological foundations of such enterprise.

The problem of scientific inquiry and the knowledge it produces has ignited a vast array of debates concerning the extent and limitations of the rationality involved in scientific discovery. As we have previously remarked, in the context of such debate there has emerged two opposing moments in scientific inquiry. One deals with the reasoning process involved in scientific knowledge production, and as such it does not have a specific set of rules or procedures and so it belongs to the ambiguous and elusive nature of the empirical studies of scientific knowledge. The other moment in scientific inquiry would entertain the aspects of scientific knowledge, which not only follows some set of rational guidelines and principles but it can be modeled and analyzed with formal methods. According to Karl R. Popper's (and others such as Feigl's and Reichenbach's) view, epistemology and philosophy of science should only concern to the rational reconstruction of the context of justification, since its discovery counterpart would be object of psychology or some other empirical enterprise, but it would certainly could not be an object of inquiry in the highly normative field of philosophy.

In the context of this dilemma there lies, we argue, a very basic contradiction concerning the social nature of scientific inquiry. In particular, we argue that the discovery/justification context dichotomy -in this methodological sense- fails to give a proper account towards the sociological nature of scientific inquiry.

To understand our argument, we need to posit the notion of *scientific institutionality* as referring to the collective and social nature of scientific enterprise. Scientific institutionality as an object of study within philosophy of science would be wrongly undertaken if we were to follow the classical tenets and views contained in the discovery/justification dichotomy. That is, if we try to analyze the social nature of scientific enterprise using precisely, the notion, concepts, and thesis contained in the discovery/justification dichotomy as is defended in the classical approach, we would immediate fall under a strange position from within the classical view of philosophy of science. According to the so-called classical view, the social dimension of scientific discovery would not be part of the rational and normative study, in other words, the social dimension would be left out from this justification context and displaced towards the discovery context in philosophy of science. But, if this is the case, one could not address the basic dynamic and interaction taking place in the social dimension of science from a normative approach (which could include rational and formal methods of analysis). In the same vein, one would displace the analysis of the social dimension of scientific endeavor towards a psychological or some other fuzzy field of study that would not belong nor have access to the formal methods of the context of justification. Hence, under this *classical* depiction of the discovery/justification dichotomy, the social dimension of science could not be –to a great and important extent- the subject of formal methods.

The problem arises precisely at this point, inasmuch as one seems to be drawn into a view of scientific endeavor that involves some contradictory characteristics. First, we would depict the knowledge (including theories, hypothesis, and data) of scientific agents as highly rational, but at the same time, we would depict the social interaction of such agents in the context of collaboration, discussion, and validation of their knowledge (theories, hypothesis and data) as belonging to a non-formal and even irrational one. Hence, this state of affairs displaces the study of the social dimension of scientific process farfetched from formal methods, because such methods are attributed only to the rational justification of scientists and their theories.

Furthermore, this depiction does seem quite opposite to the tenets and views by which one would characterize the long list of philosophers, scientists, and intellectuals involved in the classical view of philosophy of science (such as Popper, Feigl, Reichenbach, etc.). Such inconsistency is easy to foresee inasmuch as one can attribute such depiction of scientific endeavor to the kinds of views and thesis portrayed by the irrationalists analysis of philosophers such as Feyerabend (1993) and other alike (even those which undertake an extreme interpretation of Kuhn's work). Hence, this seems to suggest that some of the core tenets in the *classical* interpretation of the discovery/justification dichotomy seem incompatible with the very same philosophers who proposed it.

In our perspective, what seems more plausible is that the classical view in philosophy of science regarding the discovery/justification dichotomy, would defend a minimum ground of rationality to all aspects of scientific endeavor in which deliberation and validation of scientific theories, hypothesis and/or data is involved, even in the sociological realm of scientific endeavor. If this is so, what seems more consistent is a discovery/justification distinction in which scientific institutionality (and its sociological dimension) remains available to the rational and normative methods of the context of justification.

The above discussion relies heavily on the social dimension of scientific endeavor, and its role in the production of scientific knowledge. In the following section, we further analyze some of the implications of the collective and social nature of scientific endeavor in light of the discovery/justification dichotomy. In particular, we advance an argument proposing the weakening of the discovery/justification dichotomy based on the distributed cognition paradigm and the social dimension of scientific endeavor. We will argue that both dimensions (the discovery and justification) can be seen as two description levels rather than being two irreducible moments of the same process.

5. The weakening of the discovery/justification context dichotomy based on the perspective of distributed cognition.

The concept of distributed cognition has been largely attributed to E. Hutchins, who has done extensive and pioneer work on the subject. The topic is usually traced back to Hutchins work of 1995 *Cognition in the Wild*. In a general sense, some of the general features of distributed cognition relies on the units of analysis inquired and the nature of the cognitive mechanisms among the units of analysis; in particular, the distributed cognition perspective, emphasizes that a cognitive process can occur across several agents and that the processes involved in different cognitive tasks, such as representational states are not necessarily restrained to single and individual agents, hence a particular representational state can be shared among several agents. For the sake of precision, we can sketch the main thesis of distributed cognition as being the study of cognitive process in such a way that those processes are not necessarily constrained to individual agents. In this sense, a cognitive process may be distributed across a collection of agents or nodes, which each agent or node by itself can carry out some particular aspect of such distributed process. In this context, the task carried out by the individual is conducted in the context of a much wider collective process which encompasses various individual agents. In this scenario, factors such as coordination and structure between individual agents and their environment become crucial to understand the wider system. Furthermore, this collective process might not only be distributed among several agents but it can also be seen as being distributed over time (Hutchins, 2000).

It is of no surprise that one of the key endeavors that have received considerable attention in terms of the distributed cognition approximation within cognitive science is scientific endeavor itself. In particular, Hutchins (2000) has pointed out that scientific endeavor is a paradigmatic case of a cognitive task that is highly pronounced by its collaborative and distributed nature. Furthermore, within scientific endeavor, is clear how external conditions affect individual agents in a non-trivial and significant way (for example in their choices, decision process or perspectives towards a given problem), how facts in science are collaboratively constructed and sketched in such a way that it would not be possible to explain appealing solely to the individual agent by its own (Hutchins, 2000).

One of the key cornerstones of the distributed cognition paradigm is the use of different description levels through which a given phenomenon can be scrutinized and the interplay among the different aspects involved in the production of any cognitively oriented task. According to Bechtel (1994), description levels are levels of analysis by which a given cognitive process can be scrutinized. As a paradigmatic example of description levels in cognitive science is David Marr's (1982) three-way level of analyzing the information processing concerning vision. Descriptions can offer a different viewpoint to understand the same cognitive phenomenon, in a way that such viewpoints explain in a different layer of abstraction the same process. Hence, for a given cognitive system composed of human agents, it can be scrutinized in a twofold manner. On one hand, the general and wider system can be seen as emerging from the individual nodes, and on the other hand, the individual agents by themselves can be seen not only as nodes that form the wider system, but they themselves can be analyzed as a distributed system within (Hutchins, 2000).

Framing our object of inquiry (i.e. scientific knowledge and its production) in light of the above idea of description levels in the context of distributed cognition, we can posit on the one hand, a collective group of agents conducting a somewhat homogeneous inquiry towards the advancement of human knowledge of some predefined topics such as chemistry, biology, physics, etc. From this point of view, the individual agents are nodes that contribute to the wider system of human knowledge. But, on the other hand, the same individual agents that compose the aforementioned distributed whole, can be seen as *sub*systems on their own right in which a myriad of individual processes takes place within individual agents. As Hutchins (2000, 4) puts it: "Rather than using the language of mind to describe what is happening in a social group, the language of social groups can be used to describe what is happening in a mind.". In this sense, the nodes of the general distributed system are distributed systems in their own right.

In light of the above, one can argue that the scientific individual as a single agent has also a major role in the process of validation and justification of scientific theories and or hypothesis, as much as with the production of such body of knowledge (i.e. scientific discovery). That is, according to this approach, the individual itself would not only be preoccupied with the blind production that concerns the process of scientific discovery, but it would need to undertake as much of a rational deliberation associated with the justification dimension as the collective community composing the wider system. As remarked by Gigerenzer (2006), this rational deliberation does not directly correspond to logical systems or the estimation of probabilities, but with an array of the heuristic toolset. Now, from such a wide heuristic toolset, one can state that science as a human institution does make use of it, but only of a refined and curated subset of such heuristic resources. For example, we could point out to Gigerenzer's (2006) notions of ecological rationality and unbounded rationality.

In the context of scientific endeavor, one could be said to operate with an ecological rationality in the dimension of discovery on the one hand, and on the other, one could afford some form of relatively unbounded rationality in the reflexive process of justification of those previously produced theories, ideas, and statements. To this matter, one can point out that even Popper's notions of science exemplify the non-optimal use of this kind of heuristic and reasoning tools. In line with such state of affairs, the discovery and justification dimensions have an interesting parallel with Kahneman's (2011) mode or systems of reasoning and decision making, a fast and intuitive one which allows us to perform under demand, and a slower and more reflexive one that gives us the necessary space to reflect upon the matter within hands. We believe that such a parallel can be traced towards the discovery/justification dichotomy. On the one hand, we need to rely heavily on intuition and creativity to solve and perform under constrained environments such as incomplete information or inconsistent evidence (tasks associated to the context of discovery) and on the other hand, we also need a space under which the product of our ingenious creative process and be closely scrutinized (tasks associated with the context of justification).

The aforementioned state of affairs is consistent with the fact that the production of knowledge in the context of scientific discovery is not a blind set of outputs from individual agents given a specific topic. To the contrary, the individual agent not only deals with the production of relevant data, ideas and/or theories but in order to achieve and/or produce such contribution, the individual as an agent must involve with a deliberative process. Hence, one could rightfully ask, how one could ever distinguish whether an individual agent is involved or represents a single moment in scientific moment or aspect of the dichotomy. We answer that based on the distributed cognition paradigm, she is an individual node in the social nature of scientific knowledge, that not only is involved with the output of data, theories and or ideas towards the scientific community, but she is in her own right a distributed system in which a myriad of rational and psychological process take place into account to give an account of the scientific knowledge she is able to produce.

Let's take for example H. Simmon's (1981) BACON architecture. Such architecture is without question a system in its own right, with identifiable parts, and nodes that are involved in a particular task that contributes to the general purpose of discovering scientific laws and regularities. But, at the same time, a given implementation of the BACON architecture, can be embedded in a larger and wider system in which this particular device accounts for a particular output. As an example of the previous scenario, one can precisely think in terms of multi-agent systems. That is, one could approach the process within scientific discussion and deliberation according to which a set of agents scrutinize theories and/or scientific statements in order to provide a rational validation those theories. In this context, each node of the whole system could be involved in a particular task of the whole deliberation process.

Given the above state of affairs, one immediately wonders how can a given instance (that is a computational instance of some scientific discovery-oriented architecture), be part of both one context and the other, at the same time. The only mode of such twofold and simultaneous adherence to the discovery/justification notions is through a tempered and modest sense of the discovery/justification differentiation.

Hence, what seems more consistent with the social nature of scientific enterprise is a tempered and modest sense of the discovery/justification differentiation, rather than strong and clear-cut dichotomy of contexts. Nevertheless, an important aspect of our stance is that we do not dismiss or disregard the moderate discovery/justification differentiation. What we do dismiss is the explicative or analytical value of the dichotomy insofar as it conveys us to take it as it was originally conceived, i.e. as a clear-cut distinction in which the dimensions to which it referred two seemed ultimately incompatible. On the other hand, we certainly admit and appreciate the distinction in a moderate and temperate sense, in which the differentiation refers to description levels of the same object of analysis (i.e. scientific inquiry).

Despite the above, we boldly underscore the fact that the dichotomy concerning the context of discovery and context of justification could be explicative and conceptually useful to understand and or illustrate the process and enterprise of scientific inquiry from the perspective of the scientific institutions, but we do not see as valuable to sustain any irreducible and categorical differentiation among these allegedly opposite dimensions of scientific inquiry. Furthermore, when we point out the weakening and collapse of the two distinct notions and dimensions, we do not propose that both are equal, or that they refer to the same aspect of scientific inquiry, but that they are ultimately correlated, much more than it would be esteemed in its genealogical conception within the logical positivists.

6. Conclusions

In our current work, we have inquired about the sociological nature of scientific inquiry, we have framed our analysis in the context of the discovery/justification context distinction. We have remarked its historical background and how it shaped and determined the view over the problem of a logic of scientific discovery (or the existence of it). In the same vein, we have reviewed some key aspects in contemporary philosophy of science and formal epistemology concerning the rational and rule-following approach to scientific discovery.

Based on the above precisions, we have analyzed the social dimension of scientific endeavor in light of the discovery/justification dichotomy. We have argued that the social nature of scientific inquiry should not be displaced or characterized as an ambiguous aspect of the production of scientific knowledge foreign to any set of formal methods or approaches. That is, we have argued in favor of the reinstatement of the dichotomy within the social aspects of scientific knowledge, and in doing so, we argue that an important aspect of scientific enterprise should be given a proper account (a rational and formal account) within philosophy of science, contrary to the more tradition or *classical* views in which the dichotomy emerged, which relayed such inquiries towards another set of empirical studies due to its supposed ambiguity and fuzziness.

Finally, and in virtue of the social nature of scientific endeavor, we have analyzed the discovery/justification dichotomy in light of the distributed cognition paradigm and the theoretical notion of description levels. In particular, we have argued that the discovery/justification dichotomy only amounts to mere descriptions levels and that they do not hold up to the trial of a categorical and irreducible differentiation, but they nevertheless offer some explicative and illustrative value. In particular, we have argued how such a distinction can also seem weak from the distributed cognition stance together with Marr's notion of description levels, in which scientific discovery endeavor can be approached as a collective and collaborative task among multiple agents. According to this last point, the dichotomy with amount no more than two descriptive levels of the same phenomenon. That is, our argument is that the distributed perspective of cognition together with the concept of description levels can provide a new approach to understand the discovery/justification context dichotomy, one in which both notions are just description levels more than irreducible categories.

Be it as it may, the whole range of previously mentioned dichotomies only amount to a illustrative and explicative set of distinctions which are embedded and in constant operation within agents, hence, we do not posit the idea of an identification of the discovery/justification dichotomy, but of a highly correlated phenomenon that when viewed from some specific perspectives (such as distributed cognition) they are severely weakened.

Our stance is that a proper analysis of some of the components involved scientific knowledge (usually displaced from philosophy of science) should be reinstated in the context of philosophy of science, and they can be approached through a series of paradigms and theoretical frameworks facilitated by the contemporary discipline of cognitive science. In this paper, we made a case concerning one of those aspects of scientific knowledge (the social dimension of scientific knowledge) in the context of one of the many paradigms available in the context of cognitive sciences (in our case the distributed cognition paradigm together with the notion of description levels).

This methodological approach to some long-standing problems of philosophy of science would not only reinstate some aspects of science previously disregarded but places those same problems in the light of new and novel perspectives.

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Anexo C

Base de conocimiento del conductismo y el temprano cognitivismo de E.C. Tolman.

En este artículo llevamos a cabo una reconstrucción racional de los principales aspectos científicos, metacientíficos y tesis involucradas en la temprana revolución en ciencias cognitivas, en particular, la transición del conductismo al temprano cognitivismo presente en el trabajo de E.C. Tolman de 1948 sobre mapas cognitivos en ratas y seres humanos.

Knowledgebase of the early revolution in cognitive science: the transition from behaviorism to cognitivism in E.C. Tolman's work on 1948.

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Abstract: The present paper seeks to construct a precise knowledge base of the paradigms involved the episode in the history of science known as part of the early revolution in cognitive science, in which cognitivism displaced behaviorist assumptions in modern psychology. The aim of this paper is to build the knowledge base of the aforementioned transition of scientific paradigms according to K.B Madsen's *systematology*, exposed in his work 1988 *A history of psychology in metascientific perspective*.

Keywords: philosophy of science, logic of discovery, scientific models, E.C. Tolman, behaviorism, cognitivism, mentalism, scientific revolutions.

1. Introduction

E.C. Tolman's 1948 groundbreaking paper *Cognitive maps in rats and men* set a foundational milestone in the rupture between behaviorism and cognitivism in psychology, in particular, it would advance the displacement of behaviorism in favor of cognitivism (which was significantly disregarded at the time). Furthermore, this episode in the history of science -which was part of the denominated early revolution in cognitive science- exemplifies several of the most important aspects of scientific discovery.

Our interest in the early revolution in cognitive science is placed within the context of the problem of formalizing and modeling scientific theories and the process involved in the transition between scientific theories. In particular, this paper deals with the construction of a knowledge base which can later serve as the basis of a formal model of the main aspects involved in shifts, changes, and transitions among scientific theories. Hence, Tolman's experiments of 1948 constitute an exemplary and relatively recent episode in the history of science in which a scientific theory vehemently displaces another and further illustrates the theoretical and pragmatic implications of such a transition.

This paper is organized as follows. First, we undertake the exposition of K.B. Madsen's *systematology*, i.e. the framework by which both behaviorism and cognitivism will be analyzed. Next, we analyze and construct the knowledge base of behaviorism according to Madsen's systematology. Finally, we go into the detail of the analysis and knowledge base construction of cognitivism in the context of E.C. Tolman's work of 1948, also in accordance with Madsen's systematology.

2. The theoretical basis of our logical framework proposal I: K.B. Madsen's Meta-Theory.

According to K.B. Madsen (1988), a systematic framework is needed in order to carry out a proper analysis of scientific theories. To this aim, he constructed a theoretical *systematology* under which scientific theories can be further decomposed in its basic and essential components. Hence, to address our current task of organizing, systematizing and constructing a knowledge base of behaviorism and Tolman's cognitivism, we are going to make use of K.B. Madsen's framework regarding the meta-theory of science.

Before proceeding with the exposition of Madsen's systematology, we further advance two main reasons why we ground our work in such a theoretical framework. First, Madsen's work is based on a series of well-established set of philosophical theories and debates concerning the field of meta-science, i.e. of science as an object of study, which is largely carried out in the realm of philosophy of science. Second, Madsen's systematology has been already applied to most important theories and schools of psychology. Taking this two aspects within the context of this paper, which is the rational reconstruction of the displacement of behaviorism in favor of cognitivism (as it can be found in E.C. Tolman's work of 1948), we see our current work as specific implementation and extension of Madsen's framework for the purpose of constructing formal models of the involved theories.

According to K.B. Madsen (1988), a scientific theory has three major layers or *strata*. First, there is the *meta-stratum*, which is composed of a coherent collection of philosophical theses that "steer" the whole scientific paradigm or theory, i.e. a set of philosophical and worldview beliefs that underlie the scientific theory. This meta-stratum is composed of *ontological* and *philosophical* theses. Concerning the *ontological meta-theses*, Madsen's divides them into: a) a conception of man b) a psycho-physical theory and c) a human freedom of action perspective. Concerning the *philosophical meta-theses*, Madsen's divides them into: a) epistemological theses b) meta-theoretical theses and c) methodological theses.

The second layer is the *hypothetical stratum*. This stratum is somehow the core of any given scientific theory and it basically consists of a set of explanatory propositions of the observed data (*data stratum*). The hypothetical stratum consists of *hypothetical terms* (the theoretical explanatory concepts) and the *scientific hypotheses*, which are the basis of scientific explanations and interpretations of the collected data. This second layer is what is commonly regarded as the *scientific theory* in a broad sense.

Finally, there is the *data stratum*, which is the lesser layer of abstraction. The data stratum component is divided into two distinctive sets, the abstract data stratum, and the concrete data stratum. The *abstract data* stratum consists of descriptive terms concerning empirical relations, which is linking experimental data with propositional beliefs. The *concrete data stratum* is the component of the scientific theory or text that holds the specific observations (specific data-theses).

Having addressed the theoretical and technical aspects regarding our framework of analysis, in the next sections we will proceed with the construction of each knowledgebase. We will construct the core beliefs, assumptions and epistemological foundations of each competing scientific paradigm in our case study: behaviorism and cognitivism. Since the aforementioned task can be stretched to unmanageable extents, we will limit ourselves to the transition occurred within the context of E.C. Tolman's experiments concerning spatial learning and cognitive maps of rats in mazes conducted in his 1948 paper.

3. Epistemic knowledge base of Behaviorism according to K.B. Madsen's Meta-Theory.

In this section, we offer a reconstruction of behaviorism as a paradigm within psychology according to K.B. Madsen's systematology. It is important to stress out the fact that this is a rational reconstruction of a broad and general scientific view, hence, there are various ways in which this systematic reconstruction might not reflect a specific type of behaviorism, but in a general sense, it tries to reflect the most important theses of such a stance.

To accomplish the aforementioned we seek into Watson's work of 1913 *Psychology as the Behaviorist Views it*, his work of 1930 *Behaviorism*, Graham's 2016 entry in the Stanford Encyclopedia of Philosophy *Behaviorism* and K.B. Madsen's work of 1998 *A History of Psychology in Metascientific Perspective*. We sustain that the aforementioned sources offer a balance of a primary source and propaedeutic view on the subject. Furthermore, throughout the reconstruction of both stances, we will further cite as footnotes the direct or indirect sources that sustain and further illustrate the reasons for our statements.

3.1 Behaviorism's Meta-stratum.

The meta-stratum, as we said before, is compromised by: (a) ontological meta-theses and (b) philosophical meta-theses. In the ontological meta-theses category we can find the following aspects: *the conception of man, psycho-physical theory* and the *human freedom of action*.

Concerning the *conception of man*, one can state that it is without question that behaviorism draws a picture in which both cognition and behavior are determined by the precise material configuration of the environment and how it interacts with the agent, and in doing so, it also denies that an agent's mental faculties have any role in behavior and cognition[1]. Hence, the following theses:

[1] "It would be better to give up the province altogether and admit frankly that the study of the behavior of animals has no justification, than to admit that our search is of such a 'will o' the wisp' character. One can assume either the presence or the absence of consciousness anywhere in the phylogenetic scale without affecting the problems of behavior by one jot or one tittle; and without influencing in any way the mode of experimental

a) Behavior is determined by the environmental factors (i.e. stimulus-response interactions).
b) Behavior is determined by the bearing of previous stimulus-response interactions places upon individuals to predict and understand their current and future behavior.
c) Behavior is not determined by mental representations.

The above theses are somewhat unclear as to the classification within Madsen's systematology. For once, Madsen himself notes that there is no clear stance within behaviorism to any particular perspective towards this category. Nevertheless, Madsen (1988) classifies it as a *biological* conception of man. To this matter, we may note that indeed no clear formulation of an ontological thesis regarding the conception of man seems available, but from Watson's work, one can infer that the biological component although is not the sole criterion by which organisms are to be analyzed, it does play a crucial role[2]. Furthermore, within Madsen's systematology, one can hardly argue to characterize the behavioristic view as *humanistic*, and the social aspect is conspicuously minor in the behavioristic discussion compared to the biological components that explain behavior.

Regarding the *psycho-physical theory*, in the context of behaviorism, one can point out that the mind and the subsequent agent's interaction within the environment are governed by a set simple and known laws, and the concept of the mind does not add any real significance in understanding cognition and behavior. In this sense, we posit the following theses:

a) The mind is a metaphysical entity exempt from any scientific or experimental analysis[3].

attack upon them." (Watson, 1913)

[2] "The psychology which I should attempt to build up would take as a starting point, first, the observable fact that organisms, man and animal alike, do adjust themselves to their environment by means of hereditary and habit equipments. These adjustments may be very adequate or they may be so inadequate that the organism barely maintains its existence; secondly, that certain stimuli lead the organisms to make the responses. In a system of psychology completely worked out, given the response the stimuli can be predicted; given the stimuli the response can be predicted." (Watson, 1913)

[3] "Far more damaging to an effective analysis is the internalization of the environment. The Greeks invented the mind to explain how the real world could be known. For them, to know meant to be acquainted with, to be

b) The mind does not add significance towards the understanding of behavior.

The above thesis can be said to adhere to what Madsen has denominated the *materialistic* view. To this matter, Madsen himself classifies the behavioristic approach as a materialistic view[4]. But, it is important to underscore the fact that, as it was with the ontological theses, there is not much of a clear statement on this matter, and we find that the hints to such problem are of a complete disregard of it[5], Be it as it may, we can point out either that: (1) such a problem is of no real significance[6] or any view one might have towards the subject is of no value in the context of psychology as a scientific discipline[7] or (2) any reference to mental or non-behavioral terms may be fully translated into behavioral terms[8]. Hence, a materialistic view is what seems a more plausible characterization of the behavioristic

intimate with. The term cognition itself is related to coitus, as in the Biblical sense in which a man is said to know a woman. Having no adequate physics of light and sound nor any chemistry of taste and odor, the Greeks could not understand how a world outside the body, possibly some distance away, could be known. There must be internal copies. Hence cognitive surrogates of the real world." (Skinner, 1977)

4 "This suggested elimination of states of consciousness as proper objects of investigation in themselves will remove the barrier from psychology which exists between it and the other sciences. The findings of psychology become the functional correlates of structure and lend themselves to explanation in physico-chemical terms." (Watson, 1913)

5 "The consideration of the mind-body problem affects neither the type of problem selected nor the formulation of the solution of that problem. I can state my position here no better than by saying that I should like to bring my students up in the same ignorance of such hypotheses as one finds among the students of other branches of science." (Watson, 1913)

6 "Will there be left over in psychology a world of pure psychics, to use Yerkes' term? I confess I do not know. The plans which I most favor for psychology lead practically to the ignoring of consciousness in the sense that that term is used by psychologists today. I have virtually denied that this realm of psychics is open to experimental investigation. I don't wish to go further into the problem at present because it leads inevitably over into metaphysics. If you will grant the behaviorist the right to use consciousness in the same way that other natural scientists employ it - that is, without making consciousness a special object of observation - you have granted all that my thesis requires." (Watson, 1913)

7 "The organism is regarded as a completely automatic entity; that in our approach to behavior theory there is no ***entelechy,*** no disembodied mind, soul, or spirit which in some ways tells the various parts of the body to co-operate behaviorally to attain successful adaption, i.e. how to achieve survival." (Watson, 1930)

8 "In the course of theory development in psychology, if, somehow, mental terms or concepts are deployed in describing or explaining behavior, then either (a) these terms or concepts should be eliminated and replaced by behavioral terms or (b) they can and should be translated or paraphrased into behavioral concepts." (Graham, 2016, 3)

approach.

Following with the next category *the ontological meta-theses*, we arrive at the *Human Freedom of Action* problem. Concerning this matter, one can characterize behaviorism as a view that specifies a reduced and fixed set of laws that govern all behavior and cognition. Hence, one can posit the idea that an agent in this highly deterministic and mechanical world a conduct or behavior cannot escape the aforementioned laws and would be constrained to such laws[9]. In a succinct way:

a) Behavior involves a reduced and fixed set of empirical laws.
b) Behavior involves a reduced and fixed set of environmental factors
c) Behavior does not involve mental factors.

In terms of Madsen's systematology, this perspective is denominated *mechanical determinism*, since actions of agents within the world can be reduced to a simple set of linear laws, such as the reinforcement and stimulus-response interactions, and this alone explains and predicts an agent's behavior.

Regarding the *philosophical meta-theses,* we have taken three categories: (a) the epistemological theses (b) the meta-theoretical theses and (c) the methodological theses. In the dimension of the epistemological thesis, we have two branches or subcategories: the *origin of cognition* and *cognitions relation to reality*. We will inquire them separately.

Before proceeding with the characterization of the aforementioned aspects of Madsen's systematology, it's important to underscore the fact that in the behavioristic approach, behavior is all that there is (i.e. no more and no less than behavior). Hence the concept of cognition as such is reduced to the category of behavior, that is, cognition is a form of

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[9] "Psychology as the behaviorist views it is a purely objective experimental branch of natural science. Its theoretical goal is the prediction and control of behavior. Introspection forms no essential part of its methods, nor is the scientific value of its data dependent upon the readiness with which they lend themselves to interpretation in terms of consciousness." (Watson, 1913)

behavior[10]. Given this state of affairs regarding some aspects of cognition and its properties within behaviorism, is important to take them into account and restraining oneself from ascribing any form of cognitivism in the behaviorist view.

Regarding the *origin of cognition,* we have that behaviorism would embrace a view in which an independent reality would definitely exist beyond an agent's existence and, this external world plus the agent's stimulus-response interaction within it, would explain all of its behavior and cognition. That is, there would be no room for any form of rational processing of the information within an agent's organic structure. In a schematic manner:

a) Behavior involves the material configuration of the environment in which an agent interacts.
b) Behavior does not involve mental representations nor any form of mental information processing.
c) Behavior is significantly determined and shaped by previous stimulus-response interactions and strengthened behavioral pathways.

The aforementioned thesis is identified in Madsen's systematology as *empiricism*. Now, it is clear that from an epistemological and philosophical standpoint, such categorization can be problematic since empiricism in the philosophical tradition represents a myriad of different positions. Be it as it may, all that we mean through the use of this characterization is that experience is the primary source of cognition, not some innate or internal structure within an agent's mind (most notably due to the fact that the concept of the mind within behaviorism if entertained is detached from any cognitivist view).

Regarding the *Cognition's relation to reality* we would have that the external world would not be subject to an agent's existence, and consequently, this reality is not subject to an agent's mental representations since behaviorism does not even contemplate such notion. Therefore:

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[10] Many mentalistic or cognitive terms refer not only to contingencies but to the behavior they generate. Terms like "mind," "will," and "thought" are often simply synonyms of "behavior." (Skinner, 1977)

a) There exists an external world independent of an agent's internal mental states.

In Madsen's systematology, this is identified as *realism*. That is, the view according to which there is such a thing as an objective reality, and furthermore, this objective reality is not subject to an agent's mental faculties, that is, it is completely independent of it.

Moving on to the *meta-theoretical theses* we easily identify that the behavioristic approach to psychology as a scientific endeavor is grounded on the view that psychology is simply the objective science of studying and predicting behavior and as such it must be subject to intersubjective revision. In line with this view of psychology as a strictly scientific endeavor, the object of such inquiry would be to determine and precise a fixed set of laws that govern, explain and predict behavior. The statements are constructed as follows:

a) Psychology is the science of studying and predicting the behavior of human and non-human animals.
b) Psychology must be the subject of intersubjective revision.
c) Strictly behavioral models can account for all relevant aspects of an agent's behavior.
d) A set of laws such as stimulus-response interactions and operant conditioning can explain and predict behavior in human and non-human animals.

In Madsen's view, this is denominated the *nomothetic ideal*, that is, a view that seeks to construct the discipline of psychology as an exact and natural science in which all behavior and any other entertained concepts or phenomena can be explained and reduced to a set of precise empirical laws.

Finally, we arrive at the *methodological theses*. In this subcategory, we have two further divisions: *research methods* and *data language*. We will inquire them separately.

Concerning the *research methods* -and in direct connection with the meta-theoretical theses we have that animal experimentation and other forms of empirical research methods must be the basis of scientific inquiry. Furthermore, in behaviorism there lies the view that there is absolutely no distinction between human and non-human animals and as such, experimental enterprises that inquire non-human and human are of equal scientific importance[11]. Therefore, we propose the following theses:

a) Psychological research is optimally conducted through the behavioral study of human and non-human animals.
b) There is absolutely no distinction in the study of both human and non-human animal in order to understand and predict behavior[12].
c) The study of non-human animals can extend our understanding of human animals.
d) Psychological research is optimally conducted through specifically designed experimentation.

In this aspect, behaviorism is a well-defined and precise research programme in which the research methods and procedures are strictly defined. Furthermore, this set of research method has a direct and absolute connection to the set of more abstract categories reviewed above.

Concerning the *data language*, in the context of behaviorism, we arrive at the conclusion that all language and concepts must restrain themselves from making any reference to some mental or non-behavioral content, since all mentalist terms can be eliminated or reduced and/or translated to behavioristic terms[13], hence:

a) Mental terms can be eliminated and replaced by behavioral terms.
b) Mental terms can be translated into behavioral terms.

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11 "The man and the animal should be placed as nearly as possible under the same experimental conditions." (Watson, 1913).

12 "The behaviorist, in his efforts to get a unitary scheme of animal response, recognizes no dividing line between man and brute. The behavior of man, with all of its refinement and complexity, forms only a part of the behaviorist's total scheme of investigation." (Watson, 1913)

13 "I believe we can write a psychology, define it as Pillsbury [the science of behavior], and never go back upon our definition: never use the terms consciousness, mental states, mind, content, introspectively verifiable, imagery, and the like." (Watson, 1913)

c) Mental terms are irrelevant to characterize behavior and cognition.

In Madsen's framework, this is evidently a behavioral language, i.e. a set of linguistic terms that evades all possible references to mental, or otherwise non-behavioral language. Furthermore, all references to such kind of concepts are taken either as empty or as reducible to behavioral terms.

3.2 Behaviorism's Hypothetical stratum.

Regarding the hypothetical stratum, we can locate two major subcategories: the *hypothetical terms* and the *scientific hypothesis*.

Regarding the *hypothetical terms*, in behaviorism, there is the view that mental states solely are a set of behavioral dispositions that explains behavior. Hence, we can define the following set of hypothetical terms:

a) Innate Reflex
b) Conditioned Reflex
c) Unconditioned Reflex
d) Stimulus-response processes.
e) Law of Reinforcement.

In the same vein, and based on the fact that according to behaviorism mental states are not a component of belief attribution and cannot be inferred from belief attribution, the following set of terms are disregarded in the behavioristic approach:

a) Mental states.
b) Mental representation.
c) Consciousness.
d) Intervening variables.

According to Madsen, behavioristic hypothetical terms are classified as *organismic terms*. That is, the set of terms employed in the behavioristic approach refer to non-mentalist aspects of the agents under study, that is, there is no reference to any kind of terms that cannot be reduced to some behavioral process.

Regarding the *scientific hypothesis*, we identify the following:

a) Non-rewarded rats will wander around the maze facing blind and open spots in a random fashion, as they will not be directed towards any specific reward in the maze.
b) Non-rewarded rats will not strengthen a particular set of nervous connections due to the fact that their behavior is not oriented to some particular goal.
c) Non-rewarded rats will show a steady performance improvement only after the rewarded trials set in and the respective connections begin to further strengthen.
d) Non-rewarded rats will not learn about the particular setting of the maze during the non-rewarded trials, as there will be no strengthening or weakening of any particular connections in the central nervous system.

e) Rewarded rats will wander around the maze facing blind and open spots in a goal-oriented fashion as they will be directed towards a specific reward at some point of the maze.
f) Rewarded rats will strengthen a particular set of nervous connections due to the fact that their behavior is oriented to some particular goal.
g) Rewarded rats will show a steady improvement throughout the trials, as they will always be directed by a reward.
h) Rewarded rats will learn about the particular setting of the maze during all the trials, as there will be strengthening and weakening of some particular connections in the central nervous system.

3.3 Behaviorism's Data stratum.

Finally, in this subsection, we present the *data stratum*. To this matter, one key aspect has to be mentioned, and that is, since our analysis concerns a specific episode in the history of psychology (that is Tolman's work of 1948 with rats behavior in mazes) the concrete data stratum will be the shared (i.e. the same) both for behaviorism as for Tolman's cognitivism.

For the sake of simplicity, we will take under scrutiny Tolman's experiment concerning *latent learning*, which illustrates key aspects in Tolman's cognitivism. In his work of 1948, the experimental setting is as follows: there is one control group (group 01) and two experimental groups (groups 02 and 03). The rats in each of these groups were run one trial a day and found food in the goal box at the end of each trial with the following guidelines: group 02 found food at the end of the maze for the first time and continued to find it on subsequent days. This group was not fed in the maze for the first six days but only in their home cages some two hours later. Group 03 first found food at the end of the maze on the third day and continued to find it there on subsequent days. This group was not fed in the maze for the first six days but only in their home cages some two hours later.

The results were as follows: for group 01 and group 03 as long as they were not finding food did not appear their error curves did not drop. For group 02 and group 03 on the days immediately succeeding their first finding of the food their error curves did drop astoundingly.

All of the above can be summarized as follows:

Group 01:

1. Run one trial a day.
2. Found food at the end of the maze on day 1.
3. Performance was regular (somewhat linear) trough out the trials (from a 3.0 error score on day 1 to less than a 0.5 error score on day 7).

Group 02:

1. Run one trial a day.
2. Found food at the end of the maze on day 7.
3. Before food reward performance was poor (from 3.0 error score on day 1 to approximately 2.5 error score on day 7).
4. After food reward performance was optimal (from approximately 2.5 error score on day 7 to less than 0.5 error score on day 9).

Group 03:

1. Run one trial a day.
2. Found food at the end of the maze on day 3.
3. Before food reward performance was poor (from 3.0 error score on day 1 to an error score above 2.5 on day 3).
4. After food reward performance was optimal (from an error score above 2.5 on day 3 to less than 0.5 error score on day 7).

In light of the above information is important to notice that all groups ended their trials with almost the exact error score, regardless of their rewarded or non-rewarded time frame. Furthermore, the improvement in the non-rewarded rats was conspicuously steep after the reward took in (both in day 3 for group 03 and in day 7 for group 02).

4. Epistemic Knowledge Base of Cognitivism in the context of Tolman's experiments work 1948 according to K.B. Madsen's Meta-Theory.

In this section, we construct a knowledge base of E.C. Tolman's cognitivism according to K.B. Madsen's systematology. As we did for the case of behaviorism, is important to stress out the fact that this is a rational reconstruction of a general scientific view, hence, there are various ways in which this systematic reconstruction might not reflect every type of cognitivism, but the aim is to reflect the most important theses of E.C. Tolman's cognitivism. Nevertheless, and differing very much from behaviorism, the reconstruction we undertake in this section concerns a very specific author's view, and as such the range of variation or divergence is less. To undertake the reconstruction of E.C. Tolman cognitivism, we seek the author's work of 1948 *Cognitive Map in rats and Men*, which was the groundbreaking work of the author concerning the displacement of behaviorism by the cognitivist approach in psychology and we also seek into K.B. Madsen's work of 1998 *A History of Psychology in Metascientific Perspective* when we consider it appropriate.

4.1. Cognitivism's Meta-stratum.

As it was conducted in the case of behaviorism, we will inquire into each of the two subcategories of the *meta-stratum*: the *ontological meta-theses* and the *philosophical meta-theses*. Regarding the ontological meta-theses, we have the following: *the conception of man*, *psycho-physical theory* and the *human freedom of action*. We will proceed to analyze them in detail.

Concerning the *conception of man* in the context of Tolman's cognitivism, it is clear that behavior and cognition are still influenced by the precise material configuration of the environment and how it interacts with the agent, but it does not rule out the fact that mental representations encoded by the agent's mental faculties also play an important role in understanding behavior. Therefore, one can draw the following theses:

a) Behavior is partially determined by the environmental factors (i.e. stimulus-response interactions).
b) Behavior is partially determined by the bearing of previous stimulus-response interactions places upon individuals to predict and understand their current and future behavior.
c) Behavior is partially determined by mental representations and the processing of those mental representations by an agent.

The above thesis can be said to follow what Madsen has described as a *biological conception of man*, that is, the view that sees human organisms solely compromised by the biological and/or empirical factors. Although one can claim that Tolman's cognitivism presents a rupture with the behavioristic approach, one cannot really sustain that this type of cognitivism entertains any social or cultural conception of human beings. Although such a thesis can be further constructed, it is certainly not present in Tolman's early cognitivism.

In regards to the *psycho-physical theory*, in the context of Tolman's cognitivism, one can point out that it can be regarded as congruent with behaviorism inasmuch as it doesn't disregard the material basis of an agent's behaviors, nevertheless, one cannot state that all that can explain an agent's behavior is contained within a set of reduced and fixed set of laws, hence, mental factors that are not reducible to the empirical basis of behaviorist understanding come into play. In particular, we posit the following theses:

a) The mind is a significant factor towards the understanding of behavior.

In virtue of the aforementioned conditions, we infer that some sort of *epiphenomenalism* can be attributed to Tolman's cognitivism, at least as it is sketched out in Madsen (1988) in which correlates 'consciousness' to a twofold occurrence emerging from brain processes. Nevertheless, we believe that this matter is open to discussion and it is far from being clear in the context of Tolman's work of 1948.

Regarding the final division within the ontological meta-theses, that is the problem of the *human freedom of action*, we identify the following theses to the cognitivist view:

a) Behavior partially involves a set of empirical laws.
b) Behavior partially involves a set of environmental factors.
c) Behavior partially involves mental factors, i.e. internal mental representations and the processing of those mental representations by an agent.

In terms of Madsen's systematology, this perspective is denominated *dynamic determinism*. According to this view, actions of human agents are not entirely determined by a reduced set of empirical laws, but from the interaction of various sets of factors, in which of course, empirical causes are involved but do not represent the whole picture. In the case of Tolman's cognitivism, we can state that the mental representations would come into play in determining an agent's behavior in a non-fixed-deterministic manner.

Continuing with the *philosophical meta-theses* we identify: (a) the epistemological theses (b) the meta-theoretical theses and (c) the methodological theses. As we previously mentioned in the case of behaviorism, in the dimension of the epistemological thesis, we have two branches or subcategories: the *origin of cognition* and *cognition's relation to reality*. We will inquire them separately.

Regarding the *origin of cognition* we have that an independent reality would most definitely exist beyond an agent's mental representation, but in opposition to behaviorism, as much as there exist mental representations within an agent's cognitive faculties, a twofold relation between the empirical and mental factors take place. In this sense, there would be room for some form of information within an agent's organic structure. In a succinct manner:

a) Behavior involves the material configuration of the environment in which an agent interacts.
b) Behavior involves mental representations and the subsequent mental information processing.
c) Behavior is significantly determined and shaped by previous stimulus-response interactions and strengthened behavioral pathways, in coordination with internal mental representations.

This is identified in Madsen's systematology as *rationalistic empiricism*. This view can be best compared to Kant's synthesis of empiricism and rationalism, in the sense, that not only does the empirical configuration of the agent and the environment plays a role, but also of the cognitive faculties within the agent must be taken into account to provide a satisfactory description of an agent's behavior.

Regarding the *Cognition's relation to reality* we have that -in congruence with behaviorism- the external world would not be subject to an agent's mental representations, hence:

a) There exists an external world independent of an agent's internal mental states.

This identified in Madsen's systematology as *pragmatism*. In this view, Tolman's cognitivism seems to be fully compatible with the behavioristic approach. That is, although there is an objective reality, we cannot state -within cognitivism- that this objective reality is subject to an agent's mental representations.

Regarding the *meta-theoretical theses* we have, that psychology as a scientific endeavor is grounded on the view that its the objective science of studying and predicting behavior and as such it must be subject to intersubjective revision. Furthermore, -and this would be the cognitivist turn- this endeavor can take into account models of information processing within any given agent in order to understand and predict behavior. The statements are constructed as follows:

a) Psychology is the science of studying and predicting the behavior of human and non-human animals.
b) Psychology must be subject to intersubjective revision.
c) Strictly behavioral models cannot account for all relevant aspects of an agent's behavior since there are some aspects of behavior that cannot be reduced to simple stimulus-response models (i.e. cognitivist models must be incorporated in psychology to successfully explain behavior).
e) A set of laws such as stimulus-response interactions and operant conditioning can partially explain and predict behavior in human and non-human animals.

In Madsen's view, this is denominated the *nomothetic ideal*. Again, this is a point of congruence between Tolman's cognitivism and the behavioristic approach inasmuch that both programs seek to build the discipline of psychology as an exact and natural science, with the notable difference that in the cognitivist view, such laws, factors, and causes are not as simple and reduced as in behaviorism due to the intervening variables. In the same vein, the cognitivist approach would include an extended set of concepts, laws, and constructs not taken into account within behaviorism.

Regarding the *methodological theses*, we identify two subcategories: *research methods* and *data language*:

Concerning the *research methods,* we have that -in congruence with behaviorism- cognitivism would also hold that animal experimentation and other forms of empirical research methods must be the basis of scientific inquiry. In a schematic form we posit the following theses:

a) Psychological research is optimally conducted through the behavioral study of human and non-human animals.
b) There is absolutely no distinction in the study of both human and non-human animal in order to understand and predict behavior[14].
c) The study of non-human animals can extend our understanding of human animals.
d) Psychological research is optimally conducted through specifically designed experimentation.

[14] "The behaviorist, in his efforts to get a unitary scheme of animal response, recognizes no dividing line between man and brute. The behavior of man, with all of its refinement and complexity, forms only a part of the behaviorist's total scheme of investigation." (Watson, 1913)

In this aspect, cognitivism has, by all means, a congruent and consistent connection with the behavioristic research programme. Nevertheless, and despite that congruency, it departs on what this set of research tools can show and manifest (i.e. cognitive structures). It is in this latter sense that cognitivism departs from behaviorism, but be it as it may, the two research programmes follow the same set of research methods. In this sense, the same starting point leads to inconsistent views and approaches.

Concerning the *data language,* we have that all language and concepts do not need to restrain from making any reference to some mental or non-behavioral content, since these kind of terms are necessary to give a satisfactory account of some sophisticated forms of behavior and cognition, and they cannot be obtained from the behavioristic repertoire. In particular, we identify the following theses:

a) Mental terms cannot be eliminated and replaced by behavioral terms.
b) Mental terms cannot be translated into behavioral concepts.
c) Mental terms are not irrelevant to characterize behavior and cognition.

In Madsen's perspective, this is evidently a *mentalist language*. That is, a set of linguistic terms that on top of all the behavioral concepts adds a series of constructs that do not refer directly to observational or empirical content and furthermore it cannot be reduced to such categories.

4.2 Cognitivism's Hypothetical stratum.

Regarding the hypothetical stratum, we have two subcategories: the *hypothetical terms* and the *scientific hypothesis*. Regarding the *hypothetical terms*, we can define the following as part of the cognitivist approach:

a) Innate Reflex
b) Conditioned Reflex
c) Unconditioned Reflex
d) Stimulus-response processes
f) Law of Reinforcement
g) Mental states.
h) Mental representation.
i) Consciousness.
j) Intervening variables.

According to Madsen, these are described as *mentalist terms*. In this approach, there is some reference to a set of mental or otherwise non-behavioral terms. In this sense, cognitivism extends the set of concepts entertained in the behavioristic approach with mental and cognitive terms that are non-reducible to behavioral process.

Regarding the *scientific hypothesis*, we identify the following:

a) Non-rewarded rats will wander around the maze facing blind and open spots in a random fashion, as they will not be directed towards any specific reward in the maze.
b) Non-rewarded rats will strengthen a particular set of nervous connections although their behavior is not oriented to some particular goal.
c) Non-rewarded rats will show a steep performance improvement after the rewarded trials set in and the respective connections begin to further strengthen.
d) Non-rewarded rats will learn about the particular setting of the maze during the non-rewarded trials, as there will occur the construction of cognitive map within in the

central nervous system.

e) Rewarded rats will wander around the maze facing blind and open spots in a goal-oriented fashion as they will be directed towards a specific reward at some point of the maze.
f) Rewarded rats will strengthen a particular set of nervous connections due to the fact that their behavior is oriented to some particular goal.
g) Rewarded rats will show a steady improvement throughout the trials, as they will always be directed by a reward.
h) Rewarded rats will learn about the particular setting of the maze during through all the trials, as there will be strengthening and weakening of some particular connections in the central nervous system.

4.3 Cognitivism's Data stratum.

As we stated before since our analysis concerns a specific episode in the history of psychology (that is Tolman's work of 1948 concerning cognitive maps in rats and men) the concrete data stratum is the same for Tolman's cognitivism as it was for the behaviorist scenario. Hence, just for brevity sake, we refer the reader to the data stratum presented in section *3.3* were we reconstructed Tolman's experimentation with latent learning.

5 .Conclusions

In this paper, we have undertaken the rational reconstruction of two major schools of modern psychology: behaviorism and cognitivism. Furthermore, we have undertaken the rational reconstruction of both paradigms as knowledge bases and point out how in the context of E.C. Tolman's work 1948 regarding cognitive maps in rats and men one can see a clear-cut transition from behaviorism to cognitivism.

The aforementioned effort has been sketched along the lines of K.B Madsen's work of 1988. We have tried to retain the most important aspects of Madsen's theoretical framework concerning the most important parts of scientific paradigms. Nevertheless, we have filled what was missing in Madsen's work concerning this particular endeavor, and we have conducted such rational reconstruction having in mind that Madsen's account is far from complete, since his work of 1988 accounted for all of the majors schools in modern psychology and as such, it could not address every episode and aspect with full attention and detail.

Furthermore, the task undertaken in this paper has been addressed with the aim of using this knowledge base as the foundation of formal models of such episode in the history of science. Nevertheless, we have limited our current work to the construction of the aforementioned knowledgebases. Hence, the construction of formal models based on the rational reconstruction addressed in this paper is a matter of further research.

Finally, it is important to mention that we have taken the case study of a specific episode in the history of science (the case of the transition from behaviorism towards cognitivism), as a proof of concept. Nevertheless, this particular case serves as an initiative to further investigate the limitations and possibilities offered by this approach to the formalization of scientific theories.

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