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REVIEW

R.A. Daly's early model of seafloor generation 40 years before the Vine–Matthews hypothesis: an outstanding theoretical achievement inspired by field work on St. Helena in 1921–1922

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Abstract: Large-scale lateral mobility of the Earth's lithosphere (mobilism) was a hotly debated issue in Earth Sciences during some two decades following publication of Wegener's (1912) theory of continental displacement. The final acceptance of lithospheric mobility was brought about with the plate tectonics revolution during the late 1960s. Support for mobilism was rather popular in certain European countries during the 1920s, whereas the reactions in North America were mostly hostile. One of the very few influential mobilists in the New World was Reginald Aldworth Daly of Harvard University. The present paper discusses his model of continental displacement which is very remarkable in many aspects. We focus on the hitherto neglected fact that Daly proposed in the mid-1920s a mechanism to create oceanic crust which would have been totally consistent with the Vine–Matthews hypothesis of seafloor generation published in 1963. It is furthermore suggested that Daly's geotectonic proposals were inspired by small-scale analogues of lava flows and multiple dike swarms he observed on Atlantic islands such as St. Helena and Ascension. His model to account for the construction of new oceanic crust is reminiscent of the models of Vine and Moores (1972) and Cann (1970) which eventually led to the "Penrose-definition" of ophiolites in 1972. As these scientists arrived at their conclusions absolutely independently of Daly, this episode is an instructive example of a multiple or repeated discovery in the Earth Sciences which renders it difficult to believe certain theories of science which assume scientific models to depend mostly on social factors.

Résumé : La mobilité latérale à grande échelle de la lithosphère (le mobilisme) était une question chaudement débattue en sciences de la Terre durant les deux décennies suivant la publication de la théorie de Wegener (1912) à propos du déplacement des continents. L'acceptation finale de la mobilité de la lithosphère a été obtenue avec la révolution de la tectonique des plaques à la fin des années 1960. Le support pour le mobilisme était plutôt populaire dans certains pays européens durant les années 1920 alors que les réactions en Amérique du Nord étaient surtout hostiles. L'un des quelques rares mobilistes d'influence dans le Nouveau Monde était Reginald Aldworth Daly de l'Université Harvard. Le présent article discute de son modèle de déplacement des continents, lequel est très remarquable selon de nombreux aspects. Nous ciblons le fait, jusqu'à présent négligé, que M. Daly a proposé, au milieu des années 1920, un mécanisme pour créer de la croûte océanique qui aurait été tout à fait en accord avec l'hypothèse de Vine–Matthews de génération de plancher océanique publiée en 1963. Il est de plus suggéré que ses propositions géotechniques étaient inspirées par des faits analogues, à petite échelle, d'écoulements de lave et d'essaims de dykes multiples qu'il a observés sur des îles de l'Atlantique telles que Sainte-Hélène et l'Ascension. Son modèle pour tenir compte de la construction de nouvelle croûte océanique fait penser aux modèles de Vine et Moores (1972) et Cann (1970), lesquels ont éventuellement conduit à la définition « Penrose » d'ophiolites en 1972. Alors que ces scientifiques sont arrivés à leurs conclusions de façon absolument indépendante de Daly, cet épisode constitue un exemple instructif d'une découverte multiple ou répétée en sciences de la Terre qui fait qu'il est difficile de croire certaines théories scientifiques qui assument que les modèles scientifiques reposent principalement sur des facteurs sociaux. [Traduit par la Rédaction]

Introduction

The development of plate tectonics in the second half of the 1960s represents one of the most outstanding periods in the history of Earth Sciences (Frankel 2012*c*). The fast acceptance of this new model of global tectonics went hand in hand with the dismissal of the orthodox view that the global configuration of continents and oceans has been unchanged during Earth's history (fixism). However, as pointed out by Menard (1986), the plate tectonics model had much more profound consequences for geology than merely accepting the mobility of continents (mobilism). It added a hitherto unknown degree of quantification, predictability, and hence testability on a global scale to the Earth Sciences (Le Grand 1988). It has been justly pointed out later by Frankel (2012*b*) that the same does also hold true for the discipline of land-based paleomagnetism which developed during the 1950s and helped fundamentally to prove and to track former motions of continents.

Mobilism had many well-known adherents prior to the plate tectonics revolution (e.g., Wegener 1912; Argand 1924; Holmes 1931; Du Toit 1937; cf. Frankel 2012*a*), but the geotectonic models proposed by these scientists did generally not have much in common with plate tectonics except for the fact that they accounted for the lateral movement of continents. Nevertheless, several peculiar concepts or techniques which later formed the very base of plate tectonics can be found — albeit in often somewhat different contexts — in some of these early mobilist writings. A particularly

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intriguing example is Wegmann's (1943) early use of a Mercator projection to document relative motions of crustal blocks (see Frankel 2012*a* and Letsch 2013) thereby anticipating the later seminal work of McKenzie and Parker (1967) on the Pacific plate.

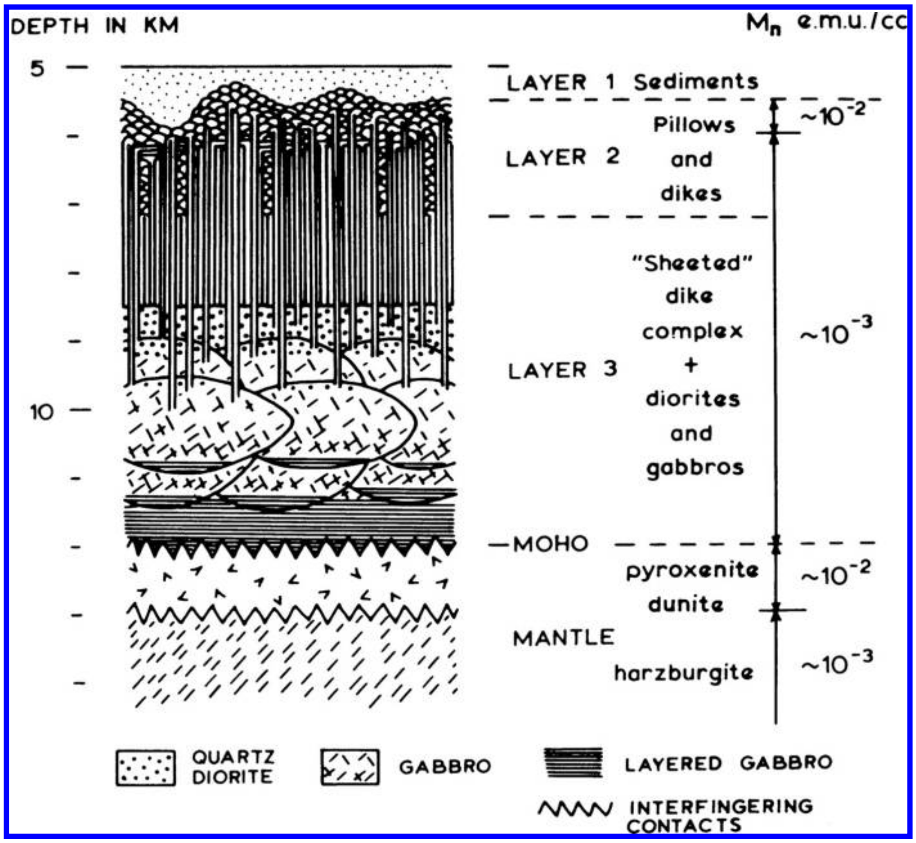
The purpose of the present article is to propose that Daly (e.g., 1926) was the first to develop a model of the generation of oceanic crust which would have been consistent with the famous Vine–Matthews hypothesis to explain the magnetic stripe patterns on the ocean floors (Vine and Matthews 1963). The article furthermore draws attention to the hitherto somewhat neglected or even misunderstood fact that the modern concept of a distinct, thin but rigid oceanic crust (or lithosphere) was not at all anticipated by Wegener in 1915 (as proposed by Oreskes 1999 and implicitly assumed by many others). It is argued that Daly made first steps towards a correct identification of the constitution and origin of the oceanic crust and that these steps were important, up to now not duly appreciated achievements on the long way to the plate tectonics revolution. To fully appreciate the importance and originality of Daly's conjectures, the article first summarizes the developments leading from the first proposal of seafloor spreading (by Hess 1962 and Dietz 1961) to the actual deciphering of the process generating new oceanic crust and the structure of its product (the oceanic crust) during the 1960s and early 1970s.

From seafloor spreading (1960) to the "Penrose-type" oceanic crust (1972)

Plate tectonics (McKenzie and Parker 1967; Morgan 1968) is not a dynamic or geologic but a kinematic and geometrical theory (e.g., Le Pichon et al. 1973; McKenzie 2003; Pilger 2003; Frankel 2012*c*). The global distribution of seismicity and volcanic activity as well as the age-pattern of the oceanic lithosphere could be elegantly described by early plate tectonicians without specifying the detailed processes taking place at convergent, divergent, and conservative plate boundaries and also without even addressing the cause and mechanism of plate tectonics (Frankel 2012*c*). Even though Hess (1962), Dietz (1961), and Vine and Matthews (1963) proposed the construction of new oceanic crust (or actually lithosphere) along divergent plate boundaries, it was not at all clear how this process worked in detail. Additionally, there was no consensus on the mineralogical composition of the oceanic crust. Hess (1962) favored a model of seafloor spreading according to which warm mantle peridotite ascended beneath mid-ocean ridges and got serpentinized by juvenile water above the 500 °C isotherm. The depth of this isotherm beneath mid-ocean ridges (some 5 km) supposedly determined the thickness of the oceanic crust which he considered to be mainly composed of serpentinite. Many other marine geologists and geophysicists (notably at Cambridge University and the Imperial College London, see, e.g., Vine 2003) maintained that at least the upper parts of the oceanic crust was predominantly composed of basalts (as Hess did too during the 1950s, e.g., Hess 1955). Thus, there were contradicting views on fundamental questions mostly due to sparse, equivocal, or ambiguous data from marine geophysics and numerical (magnetic) modeling (e.g., Vine and Moores 1972). A way out of these problems could be provided by field studies of presumably oceanic rock complexes. The proposal that Alpine-type ophiolite complexes might indeed represent former oceanic crustal or mantle material (de Roever 1957; Brunn 1959; Dietz 1963; see also Bernoulli and Jenkyns 2009), led Fred Vine and Eldridge Moores to direct their attention to the Troodos ophiolite complex of Cyprus (Moores 2003; Vine 2003). Detailed geological mapping and geophysical surveying during the 1950s had previously revealed a very high positive gravity anomaly and the lack of any granitic (continental) crustal material ("the sialic crust") in the mafic–ultramafic Troodos massif (Wilson 1959). Gass and Masson-Smith (1963) and Gass (1968) accordingly proposed that the Troodos massif represented a slice of oceanic crust of the former Tethys ocean. Moores' and Vine's work on the Troodos massif was especially motivated by the (then) unique "sheeted intrusive complex" overlain by pillow lavas (Moores 2003, p. 23). This very peculiar mafic complex which at places is almost exclusively built up by basaltic dikes of some 0.5–3 m width, hence implying a wholesale replacement of the original host rock, was independently linked to magmatic injections at mid-ocean ridges by Gass (1968) and Moores and Vine (1971). This correlation allowed for the first time the proposal of a petrologically well-constrained model of the oceanic crust (Vine and Moores 1972), which agreed remarkably well with a model proposed independently by Cann (1970) on the basis of theoretical considerations and from the results of marine dredging and geophysical measurements of mid-oceanic ridges. Petrological observations from the Troodos massif and the Semail ophiolite in Oman (e.g., Reinhardt 1969) were combined with seismic studies mainly from the Pacific to propose an ideal section of the oceanic crust and hence also ophiolite complexes (e.g., Dilek 2003). This ideal section assumed a laterally homogeneous, layer-cake, structure of the oceanic crust and became quickly known as the "Penrose definition of ophiolites" since it had been worked out before and during the GSA Penrose Field Conference on ophiolites in September 1972 (Anonymous 1972). The Penrose definition was obviously much influenced by Vine and Moore's (1972) model and Fig. 1 can be considered a fair graphic representation of the definition given in Anonymous (1972). Even though a fuller treatment of the complexity of the ophiolite concept is beyond the scope of this paper (see, e.g., Dilek 2003; Nicolas and Boudier 2003; Dilek and Furnes 2014 for further discussions), it should be pointed out that despite the usefulness of the Penrose definition for standard oceanic crust, it is rather an inappropriate model for many ophiolite complexes. Especially Alpine geologists pointed out shortly after the Penrose conference that many Alpine-type ophiolites (apart from the Troodos and Semail) do not fit into the new definition (e.g., Trümpy 1975). Many of them are dominated by abundant ultramafics (serpentinites) but do not contain any sheeted-dike complexes (Bernoulli and Jenkyns 2009). Moores (2003) distinguished ophiolites formed in magma-rich and magma-starved environments and he aptly referred to them as "Penrose-type" and "Hess-type" ophiolites, respectively. While magma-rich oceanic crust with well-developed sheeted-dikes and gabbro intrusions supposedly forms at fast-spreading ridges, incomplete oceanic crust (mostly composed of exhumed mantle) may form at slow-spreading mid-ocean ridges or at continent–ocean transitions during continental rifting and early drifting (e.g., Bernoulli et al. 2003).

Summing up this short discussion of ophiolite research in the 1960s and early 1970s, it is important to realize that plate tectonics had also a tremendous impact on petrology. Whereas there was neither any consensus on age and composition nor any coherent model of the Earth's crust beneath the ocean basins in the early 1960s, by 1972 there was considerable agreement among most geoscientists on these issues. It was generally accepted that the oceanic substratum is built by a crustal layer which is totally different in terms of composition, structure, and age from continental crust and which is constantly produced anew along mid-ocean ridges in form of discrete increments. Implicit in this new model was the view that continental separation by rifting and later spreading involves a wholesale replacement of continental crustal material by oceanic crustal material. Trivial as these assumptions may seem today, they were not at all obvious and straight-forward as the following historic discussion will show. And it will be proposed that it was Daly who first arrived at almost all of these conceptions.

Fig. 1. The layer-cake model of oceanic crust ("Penrose-type") which was proposed in the early 1970s based on ophiolite studies and marine geophysical and dredging data (from Vine and Moores 1972, fig. 1).



Who came first up with a seafloor generation model compatible with Vine–Matthews?

In 1968, while the plate tectonics revolution was in full play, Arthur Meyerhoff, an eminent American petroleum geologist with a well-developed hostility against the newly emerging concepts in global tectonics, suggested to look for possible forerunners of Hess' and Dietz's seafloor spreading mechanism (Meyerhoff 1968). He identified Holmes (1931) as the true originator of the concept, but Dietz (1968) and Hess (1968) objected that Holmes did not actually conceive the seafloor spreading mechanism with the production of a new crust (a necessary precondition for the Vine–Matthews hypothesis) as a consequence of large-scale mantle convection. He rather envisaged the creation of a new ocean basin by means of stretching of a basaltic crust of some 100 km thickness and an overlying continental block of some 40 km thickness. Hess (1968) proposed the term "sea-floor stretching" for Holmes' mechanism. Both Hess' and Dietz's objections were justified when dealing with Holmes' early views (1931), but, as pointed out by Menard (1986) and Frankel (2012*a*), Holmes published a modified version of his mechanism in 1944 which admittedly came closer to seafloor spreading as envisaged by Hess (1962) and Dietz (1961). However, Oreskes' (1999) claim that he had proposed a mechanism for creating new oceanic crust reminiscent of Hess' seafloor spreading mechanism is neither corroborated by Holmes' figures nor his text. Holmes still adhered to the assumption of a stretched but nevertheless pre-existing crust beneath ocean basins. This "torn and outstretched crust" became invaded by basaltic magma "through innumerable fissures, spreading out as sheet-like intrusions within the crust, and as submarine lava flows over its surface." (Holmes 1944, p. 508). Clearly, Holmes thought of widespread basaltic magmatism in the stretched crust underlying newly formed ocean basins, but this basaltic magmatism was neither restricted to a narrow region, nor was it thought to fully replace a former continental crust by a new oceanic crust. Both assumptions are necessary conditions for the Vine–Matthews hypothesis (Frankel 2012*a*). Similar views of a partial crustal displacement by means of diffuse basaltic intrusions were also proposed by Ampferer (1924, p. 1014, 1925) some five years before Holmes (see also Schaer 2011). However, even though his model is more reminiscent of seafloor spreading than Holmes', it nevertheless fails to fulfill the requirements for the Vine–Matthews hypothesis.

The search for possible forerunners who had proposed a model of the generation of oceanic crust compatible with the Vine–Matthews hypothesis can also be viewed as a search for geoscientists who early grasped the true constitution and origin of the oceanic substratum. With Wegener's (1915) displacement theory and the extensive discussions which accompanied its proposal, it became apparent that at least two totally contrasting schools of thought concerning the nature of the oceanic substratum existed before the advent of marine geophysics during the 1950s. Fixists, representing the majority of the geological establishment on a global scale, viewed the oceanic substratum to be represented by a relatively thick and strong crustal layer exhibiting a rigid behavior against both long- and short-term stresses. As far as the chemical and mineralogical composition of the oceanic crust was concerned, the fixist camp was roughly divided between those assuming a basically continental-type composition (e.g., Emile Haug and his ideological offsprings Kober 1921; or Bucher 1939; cf. Şengör 1999), and those who assumed a different, more basic, denser, and more rigid crustal layer beneath the oceans as, e.g., Barrell (1927), Chamberlin (1928), Jeffreys (1959), Longwell (1945), or Hess (1946). Wegener, on the other hand, questioned the existence of any crust (i.e., a crystalline layer with a finite strength, cf. Holmes 1931, p. 569) or even a lithosphere beneath most ocean basins which would exhibit rigid behavior against long-lasting stresses (e.g., Wegener 1915, pp. 15, 19). Instead he proposed that the heavy, relatively hot, and highly viscous substratum itself formed the ocean floor (possibly covered by stretched and thinned continental crust beneath the Atlantic ocean). This substratum he variously called barysphere or sima and he pointed out that the Earth's surface topography and bathymetry is dominated by these two contrasting materials: the granitic (sialic) crust forming the continents and their denser substratium, the sima, which is supposedly fluid in the sense that it is not crystalline and has no strength (Holmes 1931, p. 569). Wegener assumed the continents to plough through the yielding sima which allegedly did not exhibit any strength against long-lasting stresses. Other proponents of a certain mobility of the continents, as, e.g., Joly (1923), Ampferer (1925), van Waterschoot van der Gracht (1928), or von Bubnoff (1931), expressed similar views. Hence, Wegener's version of continental displacement did neither imply any kind of production of new oceanic crust at the trailing edge of a continental block, nor any destruction (subduction) of oceanic crust at its leading edge (Letsch 2013). In fact, Wegener (1912, 1915, 1929) suspected that basaltic magmatism will accompany the denudation of sima behind a moving continent (as, e.g., beneath the Atlantic, see also Jacoby 1981). But this magmatism was neither thought to occur in discrete, spreading-centre like zones, nor to produce a chemically or mineralogically distinct new crust. Wegener's assumption of a zero-strength sima yielding against small stresses applied over long time spans beneath the oceans exposed his whole theory of continental displacement to sharp but essentially justified criticism (e.g., Chamberlin 1928; Schwinner 1936; Jeffreys 1959). Wegener could not explain permanent ocean floor topography such as deep sea trenches or oceanic islands (Daly 1927) unless the topography would be due to sialic relics such as the mid-ocean ridge. Wegener's fundamental mistake was that he assumed a rheology for the uppermost oceanic substratum which may be appropriate for deeper parts of the mantle but certainly not for the ocean floors ("the idea of weak ocean floor", Jeffreys 1959, pp. 367–368). It seems (Paul F. Hoffman, written communication) that Wegener was influenced by C. Doelter's 1906 textbook "Petrogenesis" (e.g., Wegener 1915, p. 28) which stated that sialic matter (granite) possessed a higher melting temperature than simatic matter (basalt). Hence, he argued, liquid sima and solid sial could coexist and rafts of the latter (continents) could plough through the yielding "sima sea". In doing this, Wegener implicitly denied that the ocean floor would possess any kind of "geological memory", i.e., the movement of continents through the weak sima would not leave the slightest trace on the ocean floor. For many geologists used to read Earth's history by studying rocks, such an assumption must have been untenable as it virtually undermined the basic underpinnings of geological research. Ironically, it was exactly the appreciation that past plate movements are recorded in the ocean floor that led to the breakthrough of the Vine–Matthews hypothesis in the 1960s.

Interestingly, some major protagonists of the later plate tectonics revolution do not seem to have noticed this profound difference between Wegener's drift theory and plate tectonics and instead assumed in a somewhat anachronistic way that he had already believed in the modern concept of a thin rigid crust or lithosphere beneath the oceans (e.g., Hess 1946, p. 787; Menard 1986, p. 25; Vine 2003, p. 65). And even some prominent historians of science have failed to appreciate this difference (e.g., Oreskes 1999, pp. 77–78) and thereby misinterpreted Wegener's figures and texts (see Letsch 2013). However, Wegener's assumption of the "sima sea" without a real geologic record was probably one of the main aspects of opposition against Wegener's theory of continental displacement.

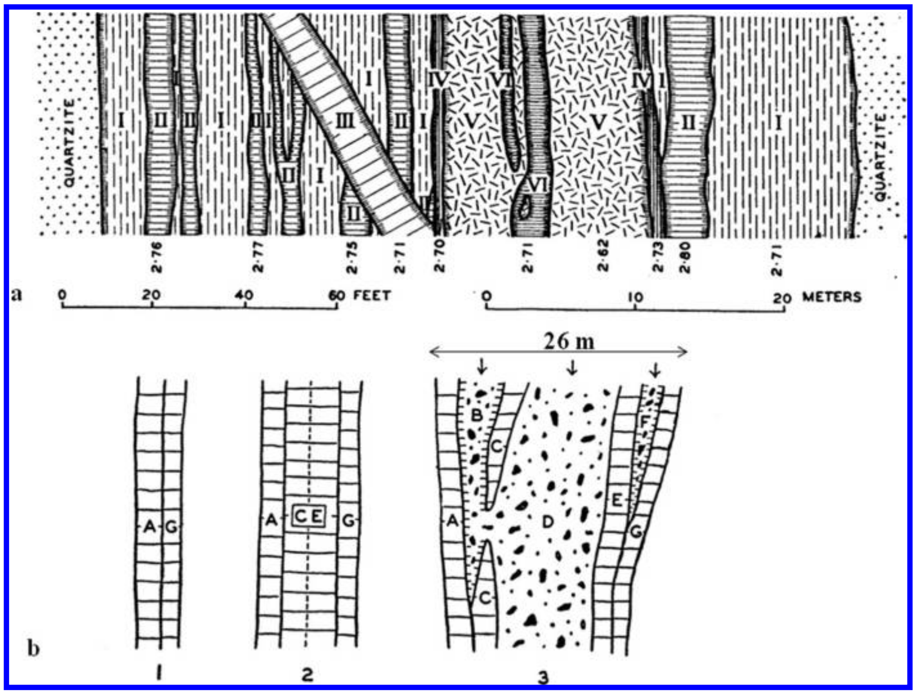
This was the situation in the mid-1920s when R.A. Daly entered the stage of the continental displacement debate.

R.A. Daly and the sliding continents

Reginald Aldworth Daly, a Harvard geologist of Canadian origin (for biographical details see Natland 2006), was one of the very few — and certainly the most renowned — North American geologist who was favorably inclined to Wegener's (1915) continental displacement theory. He developed his own version of it, the continental landslide hypothesis (first published in Daly 1923), which can be "represented in the following form: The continents appear to have slid down-hill, to have been pulled down, over the earth's body, by mere gravity; mountain structures appear to be the product of enormous, slow landslides." (Daly 1926, p. 263). Thus, Daly identified gravity as the driving mechanism which caused the continental displacements proposed mainly on geological grounds by Wegener (1912, 1915). Daly (1925*a*, 1926) suspected that large-scale distortions of the Earth's surface (i.e., deviations from the surface of a rotational ellipsoid), which he ascribed to either thermal contraction or a change of rotational velocity of the Earth, were responsible for these sliding movements of the continents. The continental crust allegedly slid over a hot and glassy ("vitreous") basaltic substratum which, due to its extreme mechanical weakness, allowed large-scale shearing. Anticipating the later distinction of divergent and convergent plate boundaries, Daly distinguished magmatic and tectonic activities occurring "downstream" and "upstream" of the sliding continental block, respectively. Mountain building accompanied by crustal contraction, gravity-induced foundering of the heavy crust in the lighter substratum, and plutonic activity due to mantle and crustal melting took place downstream of the continental block (see the many discussions, e.g., in Marvin 1973; Oreskes 1999; Frankel 2012*a*). It is very probable that Daly's views of these "downstream" processes were influenced by his observations of plutonic intrusions and the mechanisms which create space for their emplacement in the crust such as overhead stoping (J. Natland, written communication, see also Daly 1906). Taking this into account, his descriptions and schematic figures of downstream processes may only accidently resemble modern concepts such as subduction, slab pull, or slab rollback. Nevertheless, some of these resemblances (see especially Daly 1923) are striking and could provide material for future historic research. One quotation is chosen to illustrate Daly's views concerning the driving mechanism of continental sliding: "On the other hand, if tangential compression causes the suboceanic crust to be locally depressed, ruptured, and underthrust, the downward pull due to differential density becomes increasingly important. The immersed part of the crust tends to pull down with it the horizontal part to which it is solidly attached." (Daly 1923, pp. 364–365).

Highly original were Daly's speculations on the processes operating on the upstream edge of the sliding continent and they form the core of the present article. It is astonishing that this important and most powerful part of Daly's model has only received very scanty and incomplete attention by historians of science. According to Daly, a former continent like Gondwana which had been under tension due to gravitational pull, would eventually have been broken apart and "through-going, more or less vertical cracks were developed. Into those cracks the weak material of the substratum was injected, to freeze quickly as dikes." (Daly 1925*a*, p. 298). In his book "Our mobile Earth", written for a general audience, Daly became more explicit when describing the processes taking place at the upstream end ("upper edge") of a sliding continent: "Obviously the sliding of a continental block must be accompanied by tension on the upper edge of the block, the side opposite the mountain range. Vertical cracks open along that upper edge, cracks analogous to the tension-fissures or crevasses in a glacier. Basaltic lava from the substratum rises into the cracks, and flows out over the surface. […]. Both dike and lava-flow quickly freeze. Renewed tension, cracking, and eruption of lava cause other dikes and flows; the dikes side by side, the flows

Fig. 2. Examples of composite dikes reproduced by Daly in his textbook "Igneous rocks and the depths of the Earth" (1933, figs. 17 and 18). (*a*) Glen Etive, Scotland, the roman numerals indicate the order of injection of different varieties of rhyolite (I oldest, VI youngest) into the totally displaced (i.e., pushed away) country rock (quartzite). (*b*) Field sketch of an Icelandic composite dike with AGCE: dolerite, and BFD: rhyolite.



superposed. Thus a new crust is formed along the upper edge of the moving continent." (Daly 1926, pp. 277–278). In the second edition of his influential textbook on petrology "Igneous rocks and the depths of the Earth", Daly restated his views of the processes taking place on the upper edge of sliding continents. He wrote: "On the "upstream" side of the moving continent the earth's crust has been under tension, a condition changed by vertical fracturing (abyssal fissuring) when migration begins. The fissure is immediately filled with basaltic magma, risen from the substratum. The liquid may flow out at the surface, but that quickly frozen in the dikelike fracture represents a new, narrow element of the earth's crust. Further migration leads to a succession of such fracturing and dike extensions of the new crust, probably capped with floods of basaltic lava." (Daly 1933, p. 260).

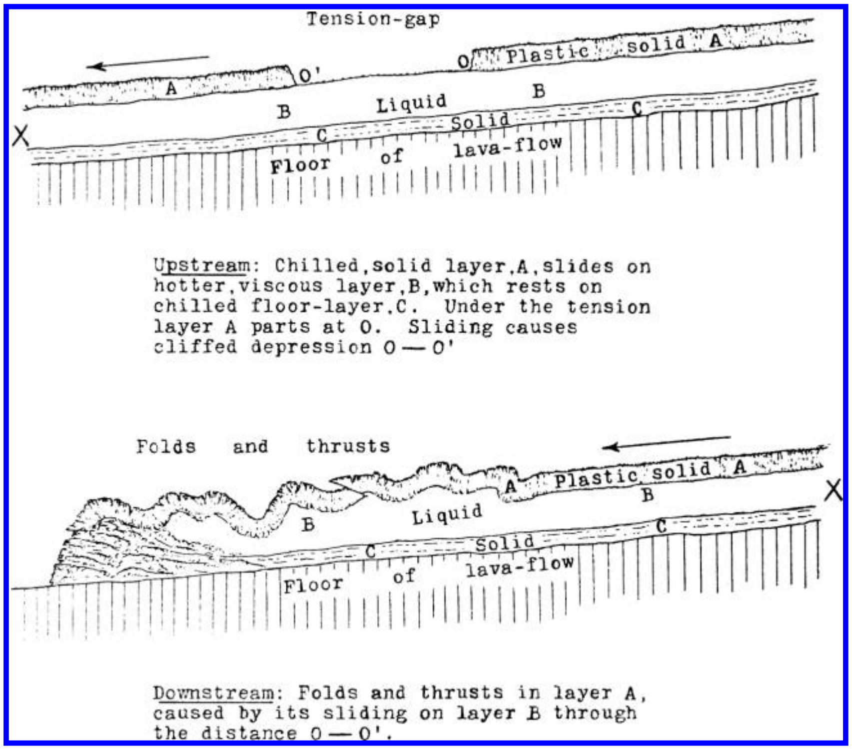
There are several remarkable points about these statements of Daly's theory of continental displacement. First, he proposed, to our knowledge for the first time ever in the history of geology, the continuous creation of a totally new crust, distinct from the continental crust but nevertheless exhibiting considerable strength against both long- and short-term stresses. Second, this new crust was supposedly built in a discrete way by basaltic dikes side by side thus fulfilling Frankel's (2012*a*) necessary condition for the Vine–Matthews hypothesis. And third, one can, by somewhat stretching the evidence, propose that he described in very general terms the peculiar "textbook" (i.e., Penrose type) structure of the upper oceanic crust (basaltic lava flows above a sheeted-dike complex, cf. Figure 1), which was only deciphered some 45 years later through studies of ophiolite complexes! Thus, Daly arrived around 1925 at most points which later characterized the new view of the ocean basins in the aftermath of the plate tectonics revolution around 1972 (see above). Nevertheless, two points should be mentioned which distinguish Daly's model and the modern models of the generation of oceanic lithosphere. First, Daly (1926, p. 280) maintained (in accordance with other eminent mobilists such as Argand 1924, p. 311) that the mid-Atlantic ridge was not composed of new oceanic crust but of an elongated slice or original continental material. The reason for this assumption was probably Daly's (1925*b*) observation of quartz-bearing, acidic rocks on the ridge-centered island of Ascension which he took as evidence for relics of continental rocks. Second, almost throughout his whole career Daly considered the oceanic crust (which corresponds to the oceanic lithosphere of modern terminology) to be thicker than the continental one (e.g., Daly 1926, 1940). This was a consequence of Daly's assumption that the oceanic crust (lithosphere) was exclusively composed of basalt instead of assuming a substantial amount of denser rocks such as peridotite (cf. also Holmes 1931).

As demonstrated above, the way to the Vine–Matthews hypothesis and its link to the sheeted dike complexes in ophiolites was by no means an obvious and direct track. One might thus wonder how Daly managed to propose almost by a stroke of genius such a concise description of this peculiar process and its product — oceanic crust. It will be proposed in the following that Daly took his inspiration from field examples which he encountered both in the literature and on two Atlantic islands — Saint Helena and Ascension.

Daly's inspiration: the volcanic rocks of Saint Helena and Ascension and multiple and composite dikes

According to Daly (1925*a*, 1926, 1933) the newly built oceanic crust on the upstream side of sliding continents would stand low due to its relatively high density and would eventually be covered by a new ocean and thus be inaccessible for further examination. However, he argued that "[…], the tensioned edge of the moving continent itself [i.e., the passive continental margins in modern terminology] is also liable to diking and flooding by lava, and the mechanism can there be studied." (Daly 1933, p. 261). Specifically, he referred to basaltic dike swarms on the Scottish Hebrides where the total extension of the sialic crust, however, only reached some 5%. A much more intriguing example was provided by Daly in his discussion of multiple and composite dikes in his petrology textbook (1933, p. 91; see our Fig. 2), which are small-scale analogues of the modern seafloor spreading model and

Fig. 3. Longitudinal section of a frozen basaltic lava flow on Ascension Island (reproduced from Daly 1925*b*, fig. 2). This flow, which is not drawn quite in true scale (the cliffs at O and O' are some 3–5 m high) was considered by Daly a possible small-scale analogue of his continental sliding theory even though the production of new oceanic crust (in the tension gap) does not take place according to Daly's model as described in the text.



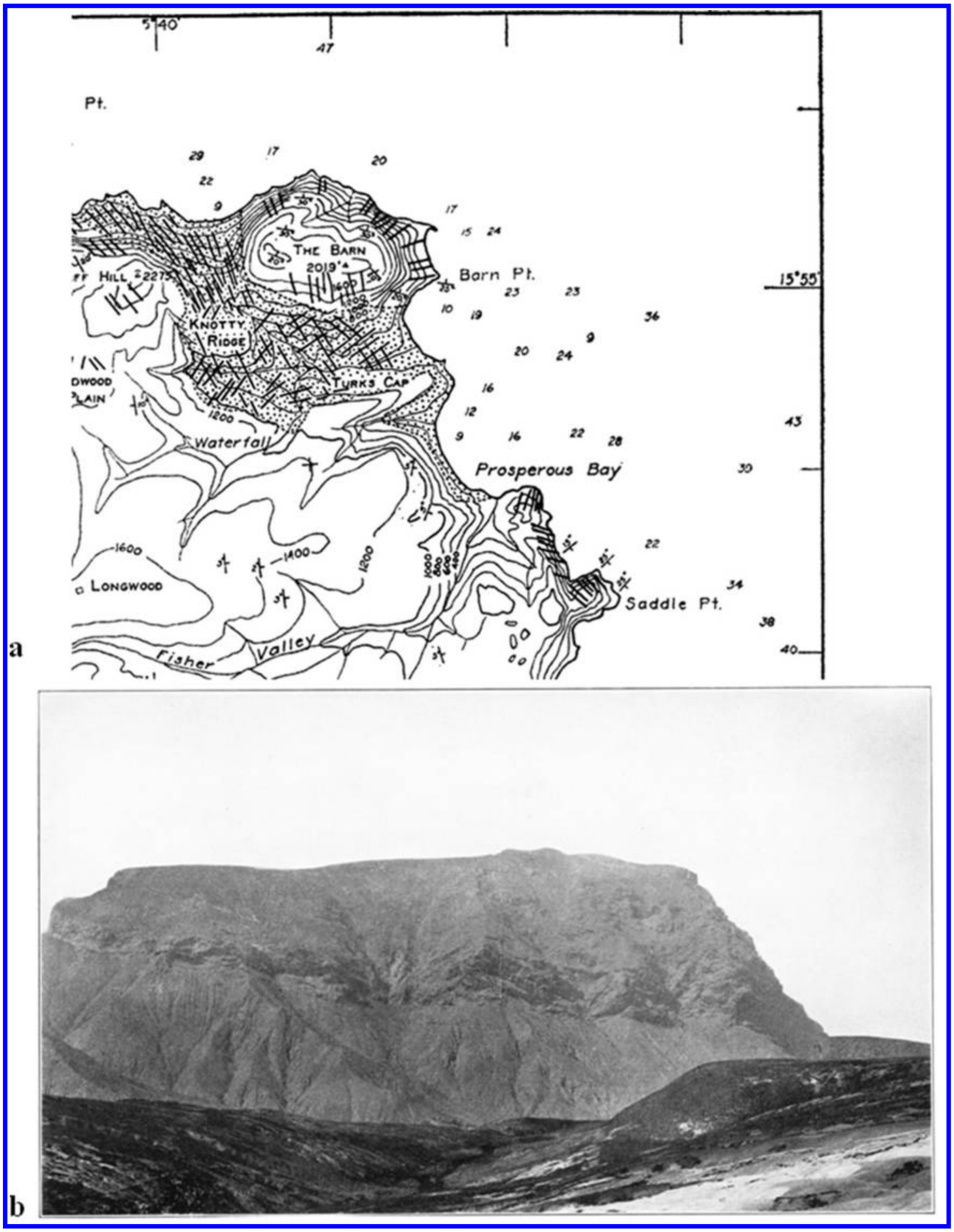
where the country rock has been totally displaced by repeated injections of magma in the middle of the composite dike. The rock pattern thus produced visually resembles the magnetic "zebra-patterns" on the ocean floors. It must have been clear to Daly that the stretched continental borders did only provide a rather incomplete representation of the processes occurring beneath the Atlantic. It is thus proposed that Daly took his inspiration mainly from his visits to two small South Atlantic islands — Saint Helena and Ascension — in winter 1921–1922. We shall first discuss his findings on Ascension. This young island, located near to the mid-Atlantic ridge, provided ample opportunity to study recently solidified basaltic lava flows. One of these flows, which Daly called the "comfortless cove" exhibited interesting combinations of viscous and brittle deformation upon cooling. His diagrammatic sketch (Fig. 3) displays some features reminiscent of his continental sliding hypothesis and he noted (1925*b*) that such lava flows might serve as useful small-scale analogues for much greater processes of Earth's tectonics.

Less obvious but probably more important were Daly's observations on the older and geologically more mature island of Saint Helena (Daly 1927). He was heavily impressed by a complex of volcanic rocks outcropping at the northeastern coast of the island, a complex which had already been noted by Charles Darwin at the occasion of his short visit to the island in 1836, and which Daly named "Knotty Ridge Complex". He described the complex as follows: "On the razorback ridge connecting The Barn and Flagstaff Hill is the outcrop of a multiple dike, which is made up of nearly 200 single dikes, between which no tuff or other different material could be found." (Daly 1927, p. 57). This huge composite basaltic dike was overlain by massive basaltic lava flows building up The Barn peninsula (Fig. 4). The mechanism which might have produced this complex was summarized by Daly in a way which is highly reminiscent to both Daly's theoretical model of "upstream" magmatism and to the mechanism to create oceanic crust implicitly assumed by the Vine–Matthews hypothesis: "After a dike has solidified, the earth's crust may be pulled apart, cracked open again at the same place, and a second dike injected alongside the first. A third parallel dike may be formed in the same way, and so on." (1926, p. 140). Even though Daly never stated it explicitly, it is beyond any reasonable doubt that the Knotty Ridge Complex served as a natural analogue for his conception of oceanic crust which is continuously produced on the upstream edge of a moving continental block.

Discussion

Even though Daly was a very renowned and influential figure of early 20th century geology, the specific and highly original details of his contributions to the continental displacement controversy were not really appreciated by most of his contemporaries (Frankel 2012*a*). Not even Wegener (1929, p. 53) grasped the importance of Daly's new model whose ideas might have substantially improved Wegener's own version of continental displacement. The reason for this neglect might have a least three aspects. First, Daly's gigantic contributions to other fields (as, e.g., petrology, global isostasy, or regional geology) for sure overshadowed his speculations on theoretical geodynamics. Second, since continental displacement was generally discarded by North American geologists and geophysicists before the 1960s, any discussion of the detailed processes which could bring about these hypothetical movements of the continents seemed superfluous to many scientists. Even if one was favorably inclined or at least unbiased towards mobilism, the particular process of formation of new oceanic crust proposed by Daly was by no means the only or the most plausible one to account for new oceanic basins. Diffuse magmatism (e.g., Ampferer 1925), exposure of the mantle (Wegener 1915), or stretching and thinning of continental crust (Holmes 1931) were as plausible as Daly's spreading mechanism.

Fig. 4. The "Knotty Ridge Complex" on Saint Helena where Daly supposedly took his inspiration to propose a model for the origin and structure of the uppermost oceanic crust. (*a*) Geological sketch map of The Barn peninsula at Saint Helena's northeast coast (from Daly 1927, pl. I) with basaltic flows and interbedded pyroclastics (white, dipping with 20° to 30° towards the NE) overlying the Knotty Ridge Complex (dotted). (*b*) Photograph showing the basaltic flows of The Barn (hill in the background) and its basement, the Knotty Ridge Complex (exclusively composed of subvertical basaltic dikes), in the foreground (from Daly 1927, pl. XVII). The individual dikes are running from the left to the right, or vice versa.



Not even Daly's open-minded friend Chester Longwell mentioned this aspect of Daly's model when discussing the sliding hypothesis in the "Daly-Volume" of the *American Journal of Science* (Longwell 1945). And third, as we tried to show above, the mechanism of generation of oceanic crust which was implicitly assumed by the Vine–Matthews hypothesis, i.e., discrete production of thoroughly new crust along a continuously re-opened crustal fracture zone, was and is a process so peculiar that it first needed the pertinent evidence of magnetic stripe patterns on the ocean floor. Nevertheless, it seems remarkable that Harry Hess who was wellversed in petrologic literature and who often cited later works by Daly, did not mention Daly's model for the generation of oceanic crust in his seminal paper "History of ocean basins" (Hess 1962). At this point it seems appropriate to point out a further asset of Daly's theory of continental displacement which made it even more powerful than Hess' and Dietz's versions (seafloor spreading). Whereas the latter two authors firmly linked the generation of new oceanic crust to mantle convection with the mid-ocean ridges tied to upwelling limbs of huge convection cells, Daly's model neither invoked nor demanded any large-scale mantle convection. The motor of his model was cooling of oceanic and continental crust and the accompanied density increase eventually leading to crustal delamination (large-scale "stoping") triggered by inequalities of the geoid (Daly 1923). His model can thus be classified as a "top-down" mechanism according to Anderson's terminology (Anderson 2007). As Daly separated continental movements and the generation of oceanic crust from mantle convection, his model would have been immune against the justified criticism of the 1960s concept of seafloor spreading (Hess 1962; Dietz 1961). The main point of this criticism was that, because kinematic considerations demanded the oceanic ridges (e.g., around Africa, or Antarctica) to move relative to the mantle (e.g., Wilson 1961, following earlier criticisms by Menard, Heezen, and Ewing), these supposed movements would lead to unreasonably complex movements of the underlying convection cells as well (see the discussion in Frankel 2012*c*) and would hence render seafloor spreading itself as rather improbable. Plate tectonics changed the view of mid-ocean ridges. They were no longer considered as areas of active mantle upwelling due to rising limbs of huge convection cells but rather passive features where differential plate motions lead to passive mantle upwelling (Morgan 1968; Pavoni 1969). Surprisingly, as it has been justly criticized by Pavoni (2005), most recent textbooks on geology and geophysics still implicitly display in figures the old model of a mid-ocean ridge situated above the two rising limbs of a giant convection cell in the mantle (classical seafloor spreading). Thus, they implicitly assume a simple correspondence between surface tectonics and mantle convection. Making the implicit assumption of a simple correspondence between plate motions and mantle convection implies denying the main advantage of plate tectonics as developed by McKenzie and Parker (1967) and Morgan (1968) over its precursor theory, i.e., seafloor spreading (the mental decoupling of plate motions and mantle convection, cf. Frankel 2012*c*, p. xviii). Also in this respect, Daly's theory was remarkably prophetic.

Finally, one might speculate that widespread knowledge of Daly's mechanism in the late 1950s and early 1960s might have accelerated both the formulation of the Vine–Matthews hypothesis and the recognition of the true nature of ophiolites, such as the Troodos massif. However, none of these speculations did actually take place. Hence, the formulation of the seafloor spreading mechanism in 1960 (by Hess and Dietz), the proposal of the Vine–Matthews mechanism to generate oceanic crust, and the subsequent deciphering of the general genesis and structure of the oceanic crust was in a way a repeated but seemingly independent rediscovery of a discovery made by Daly in the 1920s. This episode in the history of Earth's sciences lends additional support to Şengör's (2003) claim for repeated rediscoveries of identical features of nature and consequently for the existence of objective evidence in scientific enterprise which is for large parts independent of any social factors (cf. also Menard 1986, p. 296).

Conclusions

The presentation and discussion of the repeated discovery of seafloor generating mechanisms by Reginald Daly in the 1920s and Hess, Dietz, Vine and Matthews in the 1960s allows drawing some general conclusions.

1. Wegener's most profound mistake was his failure to present a realistic picture of the geological processes taking place beneath the oceans by denying the very existence of any rigid oceanic crust capable of recording geologic history. Misunderstanding of Wegener in this respect by both important protagonists of the plate tectonics revolution and some historians of science has led to the neglect of an important aspect of the continental displacement controversy (the discovery of the true nature of the oceanic lithosphere and its tremendous consequences for the continental displacement controversy). While fully appreciating Wegener's great genius, one might even arrive at the conclusion that the rejection of his theory was not a social and highly subjective phenomenon (Oreskes 1999), but rather a scientifically justified and objective decision.
2. Daly seems to have been the first to propose a model of oceanic crust generation which would have been compatible with the Vine–Matthews hypothesis of oceanic stripe patterns. In this respect, his model was even more powerful than the models proposed by the early proponents of seafloor spreading such as Hess (1962) and Dietz (1961).
3. Both, Daly and his scientific successors in the 1960s relied heavily on field observations of oceanic crust (on oceanic islands with inspiring small-scale abalogues, and ophiolite complexes, respectively) to arrive at a reasonable explanation for the large-scale processes taking place at divergent plate margins. Thus, inspiration through smaller-scale analogues seems sometimes a necessary ingredient for large-scale theories of Earth's behavior.
4. Daly presented a mechanism for continental displacement which did not imply any simple correspondence between the movements of continents (or plates) and mantle convection. In this respect he anticipated one of the most important improvements of plate tectonics over its precursor theory (seafloor spreading), which, however, is still not fully appreciated by many recent geoscientists. New textbooks on the subject should pay more attention to this important point (Pavoni 2005).
5. Daly and later geologists arrived independently on very similar models of the generation of oceanic crust. Thus, this episode of the history of geology can be taken as an example against those theories of science that assume a strong social control on scientific enterprise and try to downplay the importance of objective evidence which must eventually lead to repeated or multiple discoveries (Menard 1986; Şengör 2003).

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