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Kitsault, Lime Creek 103P/6W

J. R. Woodcock Consultants Ltd.

1521 PEMBERTON AVENUE NORTH VANCOUVER, B.C., CANADA TELEPHONE: 604-988-2171

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Mr. Nick Carter
Mineralogical Branch
Department of Mines and Petroleum Resources
Victoria, B. C.

Dear Nick:

Thank you for sending the copy of the proposed publication "Geology and Isotope Geochemistry of the Lime Creek Molybdenite Ore Body" by D. L. Giles and D. E. Livingstone. I have reviewed the paper briefly and have a few comments to make, especially in relation to our forthcoming paper.

After reading this very detailed report on the mineralization and other aspects of the geology, I find that my short report submitted to you on November 8th is somewhat brief in places. I omitted some of the details of my previous reports that should probably have been included. Such examples are:

(a) The control of the molybdenite mineralization by the northern contact of the stock, resulting in higher grade ore within the intrusive at this contact.

(b) Some carbonate-filled fractures which are later than the polymetallic quartz veins.

(c) The fact that the late stage alaskite lenses and dykes, in places, contain disseminated rosettes of molybdenite and fluorite.

(d) The fact that clay and sericite alteration are most intense along shear zones.

(e) The east-west structure of the intruded Bowser sediments (this might be better included in your discussion of the regional geology).

(f) The fact that there are several phases of quartz-molybdenite, molybdenite, and quartz-pyrite-molybdenite mineralization.

(g) The presence of abundant fluorite.

With regard to the overall zoned nature of the main intrusive complex, Ben Bradshaw who took over the exploration project after I left, believed that there was an outer zone of quartz diorite. However he attributed this to assimilation of hornfels. My impression was that there was a southern and western outer part of quartz diorite. Some of the drill hole intersections of quartz diorite from the central eastern part of the stock, I attributed to local lenses or dykes. However I realize that, with an open pit to study, the general distribution of rock types should be considerably improved over what I suggested. Possibly our report should,

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in some place, include a statement of the general distribution of quartz diorite vs. the granodiorite quartz monzonite. However I do not have sufficient data to call this a "zoned intrusive".

Based on a number of thin section studies, I concluded that there was a recrystallization of the porphyries in the area of intense hydrothermal alteration and that the rock was acquiring a seriate texture by breakdown of phenocrysts and increase in size of matrix crystals. Giles apparently includes this seriate type of rock within his general picture of a zoned intrusive. He may be right. However I don't think we need to confirm or refute his idea of a zoned intrusive within our paper unless we have our own reasons to do so.

On page 3, Giles states that the Hazelton rocks are metamorphosed to the green schist facies and that the Bowser Group are unmetamorphosed. My work showed chlorite and albite in the Bowser sediments near the Lime Creek intrusive and I have suggested that they have been regionally metamorphosed to the green schist facies. Whether this green schist facies is merely adjacent to the stocks and the batholith, I do not know. If you have additional data, then my statement might need changing.

With regard to the alaskite dykes, I believe that our overall report should mention the fact that these dykes, both at Lime Creek and at Roundy Creek, contain molybdenite that appears to be of magmatic origin. At Lime Creek there are disseminated rosettes throughout the unaltered alaskite and in some of the alaskite bodies there is disseminated fluorspar. At Roundy Creek the high grade ore zone is centered on an alaskite intrusion that contains molybdenite intergrown with the feldspar in a feathery graphic sort of texture. I have specimens of this Roundy Creek material if you wish to see it.

One of the things that bothers me about Giles' data is his interpretation of secondary hydrothermal biotite. He refers to secondary biotite within the ore zone along the northern margin which is interpreted as mainly hydrothermally reconstituted biotite in the high mafic dioritic host rock (page 10). He also refers to minor secondary biotite within the central barren zone of intense quartz-orthoclase alteration (page 12). I do not recall having seen any secondary biotite in my studies of the central zone of intense hydrothermal alteration. I did see hornblende crystals change to fine-grained secondary biotite within some of the quartz diorite in the southern part of the stock. This was outside of the zone of alteration in an area where even the sericite alteration of the plagioclase is too slight to be noticeable in hand specimen. This alteration of hornblende phenocrysts to fine-grained biotite is found in many igneous rocks away from ore deposits and I attributed it to "deuteric alteration". However, Giles states that the biotite in the quartz diorite border phase has been mainly chloritized, possibly by deuteric alteration. Thus I am perplexed. Is Giles mistaking some incorporated biotite hornfels along the northern contact for hydrothermal biotite? It may be necessary for me to incorporate some statement to the effect that secondary biotite within the intrusive is present; however much of this is outside the zone of intense hydrothermal alteration and may be of deuteric origin.

The deep alteration has shown that the northern contact of the stock is practically vertical to a depth of 2000 feet or more and that abundant

anhydrite occurs at depth. These facts should definitely appear in my report and it will be necessary for me to make reference to someone else's work. Possibly I could make a reference to this forthcoming publication by Giles and Livingstone, both as to the anhydrite content and the attitude of the northern contacts and also as a reference for some detailed descriptions of the mineralogy.

On page 13, Giles refers to the intramineral quartz monzonite porphyry, and on page 16 he again refers to a porphyry body of probable intramineral age. This is probably the barren post-molybdenite intrusive that I referred to. However, Giles states that it is weakly altered and MoS_2 - mineralized. Do you recall seeing any molybdenite in this post ore intrusive?

The main area of conflict, as you have already mentioned, involves Giles' age date of 63 my. This appears to be based on one age date only and it could be interpreted in many ways, including the possibility of an error in age dating. Giles has acknowledged the more basic nature and the slightly more diabasic texture of the quartz diorite of the East stock and I have suggested that the East stock was emplaced prior to the Main stock. However, in the core that I logged there were short sections of fairly basic quartz diorite (similar to that of the East stock) occurring right within the Main stock. This in itself could lead to many interpretations or rationalizations of Giles' one age date. The fact that so many of your age dates within the general region are in agreement makes me somewhat skeptical of Giles' age date until he has confirmed it with additional dated samples. However it is nice to know the possibility of an older date for some of the stock -- your statements can have loopholes!

Except in the discussion of the age relationships of the region, Giles' paper is not a duplication of our paper. And even if it is, our brief paper is being compiled for a different purpose. The paper by Giles contains sufficient useful data that it should be included in our bibliography.

Yours very truly,

Dick Woodcock

J. R. Woodcock

JRW:mb